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Stirling, Allan.....................216 Woodworth Avenue, Yonkers, N. Y.

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Stone, Joseph.................................The Warren, Roxbury, Mass.

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THOMAS, SAMUEL..Catasauqua, Pa
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THOMPSON, EDGAR B..............Chief D'tsman C. & N. W. R'y, and 547 Washington St., Chicago, Ill
THOMPSON, EDWARD P.....M. E. Sol. Pat. & Cons. Elect., Temple Court, New York City
THOMPSON, ERWIN W.........Man. Southern Cotton Oil Co., Montgomery, Ala
THORNE, WM. H........Wm. Sellers & Co., Incorporated, Phila., Pa., and Gowen Av., Mt. Airy
THURMAN, GALEN E..............Man. Ainslie, Cochran & Co., Louisville, Ky
THURSTON, ROBERT H........Director, Sibley College, Cornell University, Ithaca, N. Y.
TILDEN, JAMES A............Eng. and Des. for Hersey Bros., So. Boston, Mass
TOBEY, GEORGE A.......................Supt. U. S. E. L. Co., Newark, N. J
TOWNE, HENRY R..............Pres. Yale & Towne Mfg. Co., Stamford, Conn
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ULMANN, CHAS. J..Providence, R. I.
UNDERWOOD, F. H.................Pres. Underwood Mfg. Co., Tolland, Conn
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UPSON, LYMAN A.............Supt. Hartford Carpet Co., Thompsonville, Conn.

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WALL, EDWARD B ....Supt. M. P., P. C. & St. L. R. R., Columbus, Ohio.

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Warren, John E.........Agt. Cumberland Paper Mills, Cumberland Mills, Me.
Warrington, James N....Sec'y Vulcan I. W., 86 No. Clinton St., Chicago, Ill.
Warrington, Jesse..............Nordyke & Marmon, 289 Union St., Indianapolis, Ind.
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Webb, John Burkitt....Prof. App. Math., Stevens Inst. Tech., Hoboken, N. J.
Webber, Henry, Jr....Dept. Coal Mine Mach., Dickson Mfg. Co., Scranton, and Dunmore, Pa.
Webber, Samuel..................Consulting Engineer, Charlestown, N. H.
Webber, Samuel S...Supt. Scott Foundry, Reading Iron Works, Reading, Pa.
Webber, William Oliver.....V. P. and Eng., Lawrence Machine Shop, Lawrence, Mass.
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Webster, J. F............Supt. Champion Bar & Knife Co., Springfield, Ohio.
Webster, John H.....Supt. Standard Ref. Co., 24 Broad Street, Boston, Mass.
Weeks, George W...................Agent, Lancaster Mills, Clinton, Mass.
Weightman, Wm. H........Mech. Eng. and Patents, 24 Park Place, New York City.
Wellman, Samuel T...........Supt. Otis Iron and Steel Co., Cleveland, Ohio.
Wells, Eben F....Brattleboro, Vt.
Wells, J. Leland...Gillis & Geoghegan, and 875 East 169th Street, New York City.
West, Thomas D........Iron Founder, Thomas D. West Foundry Co., Cleveland, Ohio.
Westinghouse H. Herman..V. P. Westinghouse Machine Co., 17 Cortlandt Street, New York City.
Weston, Edward.................. Elec. Eng., 645 High St., Newark, N. J.
Wheeler, Frederick M.....George F. Blake Mfg. Co., 95 & 97 Liberty Street, New York City.
Wheeler, Herbert A................Washington University, St. Louis, Mo.
Wheelock, Jerome......................................Worcester, Mass.
Whitaker, Ezra J.....Chief Eng. U. S. N., Steamer "Adams," Care U. S. Consul, Panama, U. S. C.
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White, Maunsel.......................Bethlehem Iron Co., Bethlehem, Pa.
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Whitehill, Robert.............Newburgh Steam Eng. Co., Newburgh, N. Y.
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Whiting, S. B........Gen. Man. Phil. & Reading Coal and Iron Co., Pottsville, Pa.
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Ridgely, Charles... ....Prest. Springfield Iron Co., Springfield, Ill.
Rupley, Geo....Gen'l Supt. L. S. Elevator Co., and W. S. & E. Co., Duluth, Minn.
Russell, C. M..Massillon, Ohio.
Sperry, Charles .... Port Washington, Queens County, L. I., N. Y.
Stockly, Geo. W... ....Pres't Brush Electric Co., Cleveland, Ohio.
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Wood, Walter......R. D. Wood & Co., 400 Chestnut Street, Philadelphia, Pa.
Zahm, A. F.. ....Prof. Math. and Mech. Eng., Notre Dame, Ind.

Juniors.

ARMSTRONG, E. J.................Supt. Weston Eng. Co., Painted Post, N. Y.
BABCOCK, W. IRVING.... ....Supt. Union Dry Dock Co., Buffalo, N. Y.
BEACH, CHARLES S.....................H. E. Bradford & Co., Bennington, Vt.
BURDSALL, ELWOOD, JR....... ....Portchester, Westchester Co., N. Y.
CALDWELL, ANDREW J..H. R. Worthington, Van Brunt & Rapelyea Sts., So. Brooklyn, N. Y.
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CONRAD, HUGH VINCENT...D'ftsman "Rand Drill Co.," North Tarrytown, N. Y.
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DENT, EDWARD LINTHICUM..............1413 G St., N. W., Washington, D. C.
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GUTHRIE, EDWARD B.......................Deputy City Eng'r, Buffalo, N. Y.
HIGGINS, GEO. F.........Asst. M. E. Amoskeag Manuf. Co., Manchester, N. H.
HILL, WILLIAM...............................Collins Co., Collinsville, Conn.
LIDGERWOOD, WM. VAIL...Lidgerwood Mfg. Co., 96 Liberty Street, N. Y. City.
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MARX, HENRY.. Res. Man. Hill, Clark & Co., 805 N. Main Street, St. Louis, Mo.
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MORAN, DANIEL E.....................252 West 15th Street, New York City.
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NEFTEL, KNIGHT....... ....32 Liberty Street, New York City.
NORRIS, R. VAN A.....Asst. Eng. Susquehanna Coal Co., Box 726, Wilkesbarre, Pa.
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ROWLAND, CHAS. B..................Continental Iron Works, Brooklyn, N. Y.
SMITH, ALBERT W........Asst. Prof. Mech. Eng. Cornell University, Ithaca, N. Y.
SMITH, JOHN WM.........................Solvay Process Co., Syracuse, N. Y.
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TOMPKINS, S......Supt. Miller Manual Labor School, Crozet, C. & O. R. R., Va.
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WILLIAMS, HARVEY D...............Yale & Towne Mfg. Co., Stamford, Conn.

Deceased.

Henry R. Worthington	Dec. 17, 1880.
Theodore R. Scowden	Dec. 31, 1881.
Alexander L. Holley	Jan. 29, 1882.
Erastus W. Smith	June 12, 1882.
Peter Cooper, Honorary Member	April 4, 1883.
James Park, Jr	April 21, 1883.
W. K. Seaman	July 2, 1883.
Redmond J. Brough	July 21, 1883.
C. W. Siemens, Honorary Member	Nov. 20, 1883.
Henry F. Snyder	Nov. 25, 1883.
O Hallauer, Honorary Member	Dec. 5, 1883.
William Atwood	Feb. 16, 1884.
Wilmer G. Cartwright	Feb. 23, 1884.
Theodore H. Risdon	May 19, 1884.
Isaac Newton	Sept. 25, 1884.
J. H. Burnett	Jan. 31, 1885.
Horace Lord	Feb. 28, 1885.
D. H. Hotchkiss	April 29, 1885.
Henri Tresca, Honorary Member	June 24, 1885.
Henry H. Gorringe	July 6, 1885.
Wilbur H. Jones	July 29, 1885.
Frederic E. Butterfield	Sept. 5, 1885.
Wm. Cleveland Hicks	Oct. 19, 1885.
D. S Hines	Nov. 9, 1885.
Theodore Bergner	January 5, 1886.
Emile F. Loiseau	April 30, 1886.
John C. Hoadley	Oct. 21, 1886.
Homer Hamilton	Nov. 29, 1886.
John B. Root	Dec. 11, 1886.
Bishop Arnold	Feb. 16, 1887.
B. F. Emerson (*Associate*)	April 5, 1887.
Wm. L. Nicoll	July 2, 1887.
Jackson Bailey (*Associate*)	July 7, 1887.
James Sheriffs	July 18, 1887.

RULES

OF THE

AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

[*Adopted November 5th, 1884.*]

OBJECTS.

ART. 1. The objects of the AMERICAN SOCIETY OF MECHANICAL ENGINEERS are to promote the Arts and Sciences connected with Engineering and Mechanical Construction, by means of meetings for social intercourse and the reading and discussion of professional papers, and to circulate, by means of publication among its members, the information thus obtained.

MEMBERSHIP.

ART. 2. The Society shall consist of Members, Honorary Members, Associates and Juniors.

ART. 3. Mechanical, Civil, Military, Mining, Metallurgical and Naval Engineers and Architects may be candidates for membership in this Society.

ART. 4. To be eligible as a *Member*, the candidate must have been so connected with some of the above-specified professions as to be considered, in the opinion of the Council, competent to take charge of work in his department, either as a designer or constructor, or else he must have been connected with the same as a teacher.

ART. 5. *Honorary Members*, not exceeding twenty-five in number, may be elected. They must be persons of acknowledged professional eminence who have virtually retired from practice.

ART. 6. To be eligible as an *Associate*, the candidate must have such a knowledge of or connection with applied science as qualifies him, in the opinion of the Council, to co-operate with engineers in the advancement of professional knowledge.

ART. 7. To be eligible as a *Junior*, the candidate must have been in the practice of engineering for at least two years, or he must be a graduate of an engineering school.

The term "Junior" applies to the professional experience, and not to the age of the candidate. Juniors may become eligible to membership.

ART. 8. All Members and Associates shall be equally entitled to the privileges of membership. Honorary Members and Juniors shall not be entitled to vote nor to be members of the Council.

ELECTION OF MEMBERS.

ART. 9. Every candidate for admission to the Society, excepting candidates for honorary membership, must be proposed by at least three members, or members and associates, to whom he must be personally known, and he must be seconded by two others. The proposal must be accompanied by a statement in writing by the candidate of the grounds of his application for election, including an account of his professional experience, and an agreement that he will conform to the requirements of membership if elected.

ART. 10. All such applications and proposals must be received and acted upon by the Council at least thirty days before a regular meeting, when the Secretary shall at once mail to each member and associate, in the form of a letter ballot, the names of candidates recommended by the Council for election.

ART. 11. Any member or associate entitled to vote may erase the name of any candidate, and may, at his option, return to the Secretary such ballot enclosed in two envelopes, the inner one to be blank and the outer one endorsed by the voter.

ART. 12. The rejection of any candidate for admission as member, associate, or junior, by *seven* voters, shall defeat the election of said candidate. The rejection of any candidate for admission as honorary member by *three* voters shall defeat the election of said candidate.

ART. 13. The said blank envelopes shall be opened by the Council at any meeting thereof, and the names of the candidates elected shall be announced in the first ensuing meeting of the Society, and also in the first ensuing list of members. The names of candidates not elected shall neither be announced nor recorded in the proceedings.

ART. 14.—Candidates for admission as honorary members shall not be required to present their claims; those making the nominations shall state the grounds therefor, and shall certify that the nominee will accept if elected. The method of election in other respects shall be the same as in case of other candidates.

Art. 15. All persons elected to the Society, excepting honorary members, must subscribe to the rules and pay to the Treasurer the initiation fee before they can receive certificates of membership. If this is not done within six months of notification of election, the election shall be void.

Art. 16. The proposers of any rejected candidate may, within three months after such rejection, lay before the Council written evidence that an error was then made, and if a reconsideration is granted, another ballot shall be ordered, at which thirteen negative votes shall be required to defeat the candidate.

Art. 17. Persons desiring to change the class of their membership shall be proposed in the same form as described for a new applicant.

FEES AND DUES.

Art. 18. The initiation fees of members and associates shall be $15, and their annual dues shall be $10, payable in advance. The initiation fee of juniors shall be $10, and their annual dues $5, payable in advance. A junior, being promoted to full membership, shall pay an additional initiation fee of $5. Any member or associate may become, by the payment of $150 at any one time, a life member or associate, and shall not be liable thereafter to annual dues.

Art. 19. Any member, associate or junior, in arrears may, at the discretion of the Council, be deprived of the receipt of publications, or stricken from the list of members, when in arrears for one year. Such person may be restored to membership by the Council on payment of all arrears, or by re-election after an interval of three years.

OFFICERS.

Art. 20. The affairs of the Society shall be managed by a Council, consisting of a President, six Vice-Presidents, nine Managers, and a Treasurer, who shall be elected from among the members and associates of the Society at the annual meetings, to hold office as follows:

Art. 21. The President and the Treasurer for one year; and no person shall be eligible for immediate re-election as President who shall have held that office for two consecutive years; the Vice-Presidents for two years, and the Managers for three years; and no Vice-President or Manager shall be eligible for immediate re-election to the same office at the expiration of the term for which he was elected.

ART. 22. A Secretary, who shall be a member of the Society, shall be appointed for one year by a majority of the members of the Council at its first meeting after the annual election, or as soon thereafter as the votes of a majority of the members of the Council can be secured for a candidate. The Secretary may be removed by a vote of twelve members of the Council, at any time after one month's notice has been given him by a majority of its members to show cause why he should not be removed, and he has been heard to that effect. The Secretary may take part in any of the deliberations of the Council, but shall not have a vote therein. His salary shall be fixed for the time he is appointed by a majority vote of the Council.

ART. 23. At each annual meeting, a President, three Vice-Presidents, three Managers and a Treasurer shall be elected, and the term of office of each shall continue until the end of the meeting at which their successors are elected.

ART. 24. The duties of all officers shall be such as usually pertain to their offices or may be delegated to them by the Council or by the Society. The Council may, in its discretion, require bonds to be given by the Treasurer.

ART. 25. The Council may, by vote of a majority of all its members, declare the place of any officer vacant, on his failure for one year, from inability or otherwise, to attend the Council meetings, or to perform the duties of his office. All such vacancies and those occurring by death or resignation shall be filled by the appointment of the Council, and any person so appointed shall hold office for the remainder of the term for which his predecessor was elected or appointed; *provided* that the said appointment shall not render him ineligible at the next annual meeting.

ART. 26. Five members of the Council shall constitute a quorum; but the Council may appoint an Executive Committee, or business may be transacted at a regularly called meeting of the Council, at which less than a quorum is present, subject to the approval of a majority of the Council, subsequently given in writing to the Secretary and recorded by him with the minutes. Absent members of the Council may vote by proxy upon subjects stated in the call for a meeting, said proxy to be deposited with the Secretary.

ART. 27. The President on assuming office shall appoint a Finance Committee and a Publication Committee and a Library Committee of five members each. The appointment of two members of each Committee shall expire at the end of each year. The Secretary shall, *ex officio*, be a member of all three Committees.

ART. 28.—The Finance Committee shall have power to order all ordinary or current expenditures, and shall audit all bills therefor. No bill shall be paid except upon their audit. When special appropriations are ordered by the Society, they shall not take effect until they have been referred to the Council and Finance Committee in conference.

ART. 29. It shall be the duty of the Publication Committee to receive all papers contributed, to decide which shall be published in the *Transactions*, and which shall be read in full at the meetings.

ART. 30. It shall be the duty of the Library Committee to take charge of the collection of all material for the Library of the Society, and to supervise all regulations for its use.

ELECTION OF OFFICERS.

ART. 31. At the regular meeting preceding the annual meeting a nominating committee of five members, not officers of the Society, shall be appointed, and this committee shall, at least thirty days before the annual meeting, send to the Secretary the names of nominees for the offices falling vacant under the rules. In addition to such regularly appointed committee, any other five members or associates, not in arrears, may constitute an independent nominating committee, and may present to the Secretary, at least thirty days before the annual meeting, all the names of such candidates as they may select. All the names of such independent nominees shall be placed upon the ballot list with nothing to distinguish them from the nominees of the regular committee, and the Secretary shall at once mail the said list of names to each member and associate in the form of a letter ballot, it being understood that the assent of the nominees shall have been secured in all cases.

ART. 32. In the election of Vice-Presidents, each member and associate may cast as many votes as there are Vice-Presidents to be elected. He may give all these votes to one candidate, or distribute them among more, as he chooses. Managers shall be voted for in the same way.

ART. 33. Any member or associate entitled to vote may vote by retaining or changing the names on said list, leaving names not exceeding in number the officers to be elected, and returning the list to the Secretary—such ballot inclosed in two envelopes, the inner one to be blank and the outer one to be indorsed by the voter. No member or associate in arrears since the last annual meeting shall be allowed to vote until said arrears shall have been paid.

ART. 34. The said blank envelopes shall be opened by tellers at the annual meeting, and the person who shall have received the greatest number of votes for the several offices shall be declared elected.

MEETINGS.

ART. 35. The annual meeting of the Society shall be held on the first Thursday in November of each year, in the City of New York, unless otherwise ordered, at which a report of proceedings and an abstract of the accounts shall be furnished by the Council. The Council may change the place of the annual meeting, and shall, in that case, give timely notice to members and associates.

ART. 36. Other regular meetings of the Society shall be held in each year at such time and place as the Council may appoint. At least thirty days' notice of all meetings shall be mailed by the Secretary to members, honorary members, associates and juniors.

ART. 37. Special meetings may be called whenever the council may see fit; and the Secretary shall call a special meeting at the written request of twenty or more members. The notices for special meetings shall state the business to be transacted, and no other shall be entertained.

ART. 38. Any member, honorary member or associate may introduce a stranger to any meeting; but the latter shall not take part in the proceedings without the consent of the meeting.

ART. 39. Every question which shall come before the Society shall be decided, unless otherwise provided by these rules, by the votes of a majority of the members and associates present, provided there is a quorum.

ART. 40. At any regular meeting of the Society thirteen or more members and associates shall constitute a quorum.

ART. 41. Unless otherwise ordered, papers shall be read in the order in which their text is received by the Secretary. Before any paper appears in the *Transactions* of the Society a copy of the paper shall be sent to the author, and, so far as possible, a copy of the reported discussion shall be sent to every member who took part in the same, with requests that attention shall be called to any errors therein.

ART. 42. The Society shall claim no exclusive copyright in papers read at its meetings, nor in reports of discussions, except in the matter of official publication with the Society's imprint, as its *Transactions*. The Secretary shall have sole possession of papers between the time of their acceptance by the Publication Committee and their reading, together with the drawings illustrating the same; and at the time of such reading, or as soon thereafter as practicable, he shall cause to be printed, with the authors' consent, copies of such papers, "subject to revision," with such illustrations as are needed for the *Transactions*, for distribution to the members and for the use of technical newspapers, American and foreign, which may desire to reprint them in whole or in part. The policy of the Society in this matter shall be to give papers read before it the widest circulation possible, with the view of making the work of the Society known, encouraging mechanical progress, and extending the professional reputation of its members.

ART. 43. The author of each paper read before the Society shall be entitled to twelve copies, if printed, for his own use, and all members shall have the right to order any number of reprints of papers at a cost to cover paper and printing; *provided*, that said copies are not intended for sale.

ART. 44. The Society is not, as a body, responsible for the statements of fact or opinion advanced in papers or discussions, at its meetings; and it is understood that papers and discussions should not include matters relating to politics or purely to trade.

AMENDMENTS.

ART. 45. These rules may be amended, at any annual meeting, by a two-thirds vote of the members present; *provided*, that written notice of the proposed amendment shall have been given at a previous meeting.

CONTENTS OF VOLUME VIII

LIST OF ILLUSTRATIONS IN VOLUME VIII.

ERRATA.

Page 220. First formula should be :

$$r = \frac{1.226\,W_a R N^2}{40A}.$$

Page 231, line 27, *for* carbonic oxide, *read* carbonic acid.

PAPERS

OF THE

NEW YORK MEETING

(XIVth),

NOVEMBER, 1886.

[Seventh Annual Meeting.]

CCXXV.

PROCEEDINGS

OF THE

SEVENTH ANNUAL MEETING

OF THE

AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

(XIVth)

New York, November, 1886.

LOCAL COMMITTEE OF ARRANGEMENTS:—WM. H. WILEY, *Chairman;* F. R. HUTTON, *Secretary;* J. C. Bayles, Wm. Lee Church, Jas. A. Crouthers, Charles E. Emery, Thomas A. Edison, H. S. Hayward, Wm. Hewitt, Henry Morton, Edward Weston.

THE opening session was called to order in the hall of the New York Academy of Medicine, No. 12 West 31st Street, at 8.30 P.M., on Monday, November 29th. The chair was taken by Vice-President Henry R. Towne, of Stamford, in the absence of the President, Mr. Coleman Sellers, of Philadelphia. The latter was in Europe on account of ill health, and had sent a letter of regret, which was read by the Secretary.

The acting president in calling the meeting to order, spoke of the regret with which the meeting had to forego the pleasure of listening to the usual address of the President of the Society, which is presented at this time, but announced that such an address would be printed as a paper in the volume of Transactions when the strength of the President should enable him to complete it. The veteran engineer, Mr. Horatio Allen, honorary member of the Society was then introduced, and spoke of the early engineering enterprise of this country in the locomotive and steamship development. Prof. R. H. Thurston, at the close of the address, moved a vote of thanks to Mr. Allen, which was seconded by Prof. W. P. Trowbridge and Mr. H. R. Towne. After some announcements by the Secretary, and the appointment by the chair of Messrs. Van Winkle, Heminway and White to act as tellers to count the ballots cast for officers to be chosen at this meeting, the Society adjourned for a social reunion.and supper in the lower room and parlors.

Second Day. Tuesday, November 30th.

The second session was called to order at 10 A.M., in the Hall of the New York Academy of Medicine.

The Secretary's Register showed the following members in attendance, during the successive meetings, and quite a number of ladies joined the excursions:

Alden, G. I.	Worcester, Mass.
Allen, Francis B.	Hartford, Conn.
Allen, Horatio (*honorary member*)	Orange, N. J.
Allen, John F.	New York City.
Almond, T. R.	Brooklyn, N. Y.
Ashworth, Daniel	Pittsburgh, Pa.
Babcock, Geo. H.	New York City.
Baker, W. S. G.	Baltimore, Md.
Bailey, Jackson	New York City.
Baldwin, S. W.	New York City.
Barnard, Geo. A.	New York City.
Bayles, Jas. C.	New York City.
Beardsley, Arthur	Swarthmore, Pa.
Betts, William	Wilmington, Del.
Bellhouse, R. W.	Syracuse, N. Y.
Bilgram, Hugo	Philadelphia, Pa.
Bond, Geo. M.	Hartford, Conn.
Booraem, J. V. V.	Brooklyn, N. Y.
Borden, T. J.	Fall River, Mass.
Brady, Jas.	Brooklyn, N. Y.
Brooks, Morgan	Boston, Mass.
Bulkley, H. W.	New York City.
Butterworth, Jas.	Philadelphia, Pa.
Campbell, A. C.	Bridgeport, Conn.
Capen, T. W.	Stamford, Conn.
Carpenter, R. C.	Lansing, Mich.
Church, W. L.	New York City.
Clarke, C. L.	New York City.
Clarke, S. J.	New York City.
Cole, J. W.	Columbus, O.
Colin, Alfred.	New York City.

Collins, C. C.	Newark, N. J.
Colwell, A. W.	New York City.
Corbett, C. H.	Greenpoint, N. Y.
Cottrell, C. B.	New York City.
Couch, A. B	Philadelphia, Pa.
Crane, T. S.	Newark, N. J.
Cremer, Jas	Brooklyn, N. Y.
Cullingworth, G. R	New York City.
Davis, D. P	New York City.
Davis, I. H	Dorchester, Mass.
Denton, Jas. E.	Hoboken, N. J.
Doane, W. H	Cincinnati, O.
Dodge, Jas. M	Philadelphia, Pa.
Douglas, E. V	Philadelphia, Pa.
Du Faur, A. F.	New York City.
Durfee, W. F.	New York City.
Edson, J. B	New York City.
Emery, A. H	Stamford, Conn.
Emery, C. E.	New York City.
Ewer, R. G.	Brooklyn, N. Y.
Falkenau, Arthur	Scranton, Pa.
Forney, M. N.	New York City.
Foster, C. H.	Chicago, Ill.
Fowler, Geo. L	New York City.
Fritz, John	Bethlehem, Pa.
Gibson, Wm	New York City.
Giddings, C. M	Massillon, O.
Gobeille, Jos. Leon	Cleveland, O.
Gowing, E. H	Boston, Mass.
Grant, Jno. J	Philadelphia, Pa.
Hall, John H	Portland, Conn.
Harmon, O. S	Jersey City, N. J.
Hartshorne, W. B	Lawrence, Mass.
Hawkins, G. C	Boston, Mass.
Hawkins, J. T.	Taunton, Mass.
Heminway, F. F.	New York City.
Henney, John, jr	New Haven, Conn.
Henning, G. C.	New York City.
Herman, Ludwig	Cleveland, O.
Herrick, J. A.	New York City.
Hewitt, Wm	Trenton, N. J.
Higgins, Geo. F.	Manchester, N. H.
Higgins, M. P.	Worcester, Mass.
Hill, C. C.	Chicago, Ill.
Hill, H. A.	Boston, Mass.
Hill, Wm	Collinsville, Conn.
Hillmann, Gustav	City Island, N. Y.
Hobbs, A. C.	Bridgeport, Conn.
Hollerith, Herman	Washington, D. C.
Hollingsworth, Sumner	Boston, Mass.
Hornig, Julius L.	Jersey City, N. J.

Hunt, Chas. W	New York City.
Hunt, Robt. W	Troy, N. Y.
Hutton, F. R. (*Secretary*)	New York City.
Hyde, C. E	Bath, Me.
Illingworth, J. J	Utica, N. Y.
Kent, Wm	New York City.
Kerr, Walter C	New York City.
King, Charles C	W. New Brighton, S. I.
Kirchhoff, Charles	New York Ciiy.
Le Van, W. B	Philadelphia, Pa.
Lewis, Wilfred	Philadelphia, Pa.
Livermore, Chas. W	New York City.
Low, F. A	Boston, Mass.
Mackinney, Wm. C	Philadelphia, Pa.
Manning, Chas. H	Manchester, N. H.
Martens, Ferdinand	College Point, L. I.
McBride, James	Brooklyn, N. Y.
Miller, Alex	New York City.
Minot, H. P	Columbus, O.
Monaghan, W. F	New York City.
Moore, Jno. W	New York City.
Moran, D. E	New York City.
Morgan, Jos., jr	Johnstown, Pa.
Morgan, Thos. R	Alliance, O.
Morton, Henry	Hoboken, N. J.
Muller, M. A	Newark, N. J.
Murphy, E. J	Hartford, Conn.
Murray, S. W	Milton, Pa.
Nason, C. W	New York City.
Odell, Wm. H	Yonkers, N. Y.
Parker, Chas. D	Worcester, Mass.
Parker, Chas. H	Cambridgeport, Mass.
Parks, E. H	Providence, R. I.
Parsons, Henry	Newark, N. J.
Partridge, W. E	New York City.
Philips, Fredinand	Philadelphia, Pa.
Phillips, Franklin	Newark, N. J.
Phillips, George H	Newark, N. J.
Porter, Chas. T	New York City.
Porter, H. F. J	New York City.
Potter Chas., jr	Plainfield, N. J.
Pusey, C. W	Wilmington, Del.
Raynal, A. H	New York City.
Robinson, A. W	Bucyrus, O.
Robinson, J. M	New York City.
Roche, John A	Chicago, Ill.
Rowland, T. F	Greenpoint, N. Y.
Rowland, T. F., jr	Greenpoint, N. Y.
Rowland, C. B	Greenpoint, N. Y.
Sanguinetti, P. A	Philadelphia, Pa.
Saunders, W. L	New York City.

Schleicher, A. W	Philadelphia, Pa.
Schuhmann, George	Reading, Pa.
See, Horace	Philadelphia, Pa.
Smith, C. A	Providence, R. I,
Smith, Oberlin	Bridgeton, N. J.
Snell, Henry I	Philadelphia, Pa.
Sperry, Chas	Port Washington, N. Y
Spies, Albert	New York City.
Sprague, W. W	Lake, Ill.
Stearns, Albert	Brooklyn, N. Y.
Stevens, W. N	Brooklyn, N. Y.
Stiles, Norman C	Middletown, Conn
Stirling, Allan	Yonkers, N. Y.
Sunstrom, Karl J	Worcester, Mass.
Swasey, Ambrose	Cleveland, O.
Sweet, Jno. E	Syracuse, N. Y.
Tabor, Harris	New York City.
Taylor, F. W	Nicetown, Pa.
Taylor, Stevenson	New York City.
Thompson, E. P	New York City.
Thurston, R. H	Ithaca, N. Y.
Torrance, Kenneth	Brooklyn, N. Y.
Towne, H. R. (*Acting President*)	Stamford, Conn.
Trautwein, A. P	Greenpoint, N. Y.
Trowbridge, W. P	New York City.
Ulmann, C. J	Providence, R. I.
Underwood, F. H	Tolland, Conn.
Upson, L. A	Thompsonville, Conn.
Vanderbilt, Aaron	New York City.
Van Winkle, Franklin	New York City.
Ward, W. E	Portchester, N. Y.
Warren, B. H	Boston, Mass.
Watson, Wm	Boston, Mass.
Webb, Jno. Burkitt	Hoboken, N. J.
Webber, S. S	Reading, Pa.
Webber, W. O	Lawrence, Mass.
Webster, John H	Boston, Mass.
Weeks, Geo. W	Clinton, Mass.
Weightman, W. H	New York City.
Wells, J. Leland	New York City.
West, Thos. D	Cleveland, O.
Westinghouse, H. H.	New York City.
Weston, Edward	Newark, N. J.
Wheeler, F. M	New York City.
White, H. C	New York City.
White, Maunsel	Bethlehem, Pa.
Whitney, Baxter D	Winchendon, Mass.
Whitney, Wm. M	Winchendon, Mass.
Wilder, Moses G	Philadelphia, Pa.
Wiley, Wm. H	New York City
Williams, Harvey D	Stamford, Conn

Williams, J. Newton	Brooklyn, N. Y.
Williams, S. T.	Philadelphia, Pa.
Winter, Herman	New York City.
Wolff, A. R.	New York City.
Woolson, O. C.	Newark, N. J.
Wright, J. Q.	New York City.
Wyman, H. W.	Worcester, Mass.
Zell, R. R.	New York City.

A number of gentlemen were also registered as guests of the Society during the convention.

The first business in order was the Report of the Council, which was read by the Secretary as follows:

REPORT OF THE COUNCIL.

The Council would present its Annual Report to the Society under the Rules. It has held seven meetings during the year for the transaction of business, and the following is a summary of its action:

The Committee on Rules for Debate presented a scheme for conducting this business, which went into effect at the XIIIth meeting in Chicago in May, 1886, and will be found in the VIIth Volume of the Transactions of the Society, page 408.

The Council has directed, further, that is not necessary, under Rules 10–13, to hold back the election of all candidates for membership until within thirty days of a meeting, but have established the precedent of ordering a ballot whenever twenty applications are received.

The Council has, moreover, ordered that the pin-badge for members be made, hereafter, of a higher grade of workmanship than heretofore, albeit at an advanced cost. These badges are now furnished at $4.50 under contract with the Society, and by a special system of order as called for.

A committee has also reported upon a system of precedents in reference to members delinquent in dues under Art. 19 of the Rules. Their report recommends that as in the Rule the names of members who have not paid dues for any year be dropped from the Catalogue at the end of the first year of delinquency. Such names, however, are to be retained upon a "Suspended List" for two more years, during which time their membership may be resumed on payment of arrears. After three years, however, membership can only be resumed by re-election, as provided in the Rule. Members on the Suspended List receive no Transactions, nor can they vote, but may attend meetings, and take part therein.

This same committee has also considered the advisability of a change in the form of blank for application for membership, in view of the growing necessity for close scrutiny of candidates by the Council. The new form will be shortly issued for the use of members and new applicants.

The Council would present the Report of its Tellers as follows:

The undersigned were appointed a Committee of the Council to act as tellers under Rule 13, to count and scrutinize the ballots cast for and against the candidates proposed for membership in the Society of Mechanical Engineers, and seeking election before the fourteenth meeting of the Society, in November, 1886.

They would report that they have met upon the designated days in the office of the Society, and proceeded to the discharge of their duties.

They would certify, for formal insertion in the Records of the Society, to the election of the appended named persons to their respective grades upon the Lists Number One and Number Two, colored, respectively, pink and yellow.

There were 293 votes cast in the ballot upon the pink list, of which 11 were thrown out because of informalities, and there were 334 votes cast using the yellow ballot, of which 8 were thrown out because of informalities.

The list is appended below.

W. F. Durfee, } *Tellers of the*
Wm. Kent, } *Council.*

AS MEMBERS.

Briggs, Charles C.	**Pittsburgh, Pa.**
Bellhouse, R. Wynyard	**Syracuse, N. Y.**
Blake, Francis C	**Mansfield Valley, Pa.**
Cook, Frederic	**New Orleans, La.**
Corliss, William	**Providence, R. I.**
Cottrell, Calvert B.	**New York City.**
Cullen James K.	**Chicago, Ill.**
Davidson, Marshall T.	**New York City.**
DeMontmorency, Carl G. L.	**Warren, Mass.**
Dicey, Elmer C.	**Chicago, Ill.**

Folger, William M.	Washington, D. C.
Gale, Horace B.	St. Louis, Mo.
Goodale, A. M.	Waltham, Mass.
Goodfellow, George	Steelton, Pa.
Hanson, Augustus	Chicago, Ill.
Hornung, George	Cincinnati, Ohio.
Hunt, Joseph	Catasaqua, Pa.
Kebler, J. A.	Ottumwa, Iowa.
King, Charles C.	New Brighton, N. Y.
Lanphear, O. A.	Columbus, O.
Lavery, George L.	Chicago, Ill.
Ludlam, Jos. S.	Lowell, Mass.
McBride, James	Brooklyn, N. Y.
Packard, L.	West Albany, N. Y.
Pearson, W. B.	Cleveland, O.
Plamondon, Ambrose	Chicago, Ill.
Plamondon, Charles A.	Chicago, Ill.
Pope, Samuel I.	Chicago, Ill.
Ramsay, H. A.	Baltimore, Md.
Roberts, F. C.	Trenton, N. J.
Ruth, W. M	Fort Wayne, Ind.
Sanguinetti, Percy A.	Phila., Pa.
Sheriffs, James	Milwaukee, Wis.
Sorge, A., Jr.	Rochester, N. Y.
Steward, John F	Chicago, Ill.
Taylor, J. Archie	Wilmington, Del.
Ulmann, Charles J.	Providence, R. I.
Unger, John S.	Steelton, Pa.
Waddell, J. A. L.	Phœnixville, Pa.
Walworth, Arthur C.	Boston, Mass.
Webber, Henry, Jr.	Dunmore, Pa.

AS ASSOCIATES.

Dick, John	Meadville, Pa.
Rupley, George	Duluth, Minn.

AS JUNIORS.

Armstrong, E. J.	Painted Post, N. Y.
Dixon, W. F.	New York City.
Lidgerwood, Wm. V.	New York City.

PROMOTIONS TO FULL MEMBERSHIP.

Roche, John A., Associate of the A. S. M. E.	Chicago, Ill.
Greene, Isaac C., Junior of the A. S. M. E.	Baltimore, Md.

The Council would also report the following summary of the membership:

Honorary Members	15
Life Members	7
Members	590
Associates	25
Juniors	31
Total	668

Increase at this meeting :

Members	41
Associates	2
Juniors	3
Grand Total	714

The losses by death since the last annual meeting have been as follows:

Frederick E. Butterfield	Member.
Dauphine S. Hines	"
Theodore Bergner	"
Emile F. Loiseau	"
John C. Hoadley	"

The Report of the Finance Committee to the Council at its final meeting was also read, giving the summary of their work during the fiscal year just closed. This report was as follows :

The Finance Committee, would respectfully report to the Council the following statement of the receipts and expenditures of the Society, under their direction, for the twelve months ending November 1st, 1886.

The receipts have been as follows :

Initiation Fees	$1,835 00
Current Dues	6,242 00
Past Dues	273 00
Advance Dues	57 10
Paper Sales	572 03
Binding	322 10
Library (Permanent)	125 43
Library (Current Expenses)	248 30
Badges	242 78
Engraving	193 17
Life Membership	150 00
Contingent	22 77
Balance on hand November 1st, 1885	789 43
	$11,073 11

The expenditures have been as follows:

Printing and Stationery	$545 18
Postage	564 46
Library	137 20
Salaries	2,210 00
Expense and Rent	960 82
Engraving	848 10
Binding	329 60
Meetings	375 50
Work of Committees	41 55
Office Furniture and Fixtures	50 98
Badges	157 50
Traveling	138 15
Printing Transactions	3,695 51
Refund Advance of Treasurer	22 77
Refund Advance Dues paid	11 10
Total Expenditures of Year	$10,088 42
Balance on hand November 1, 1885	984 69
	$11,073 11

Of this balance, $831.73 stands to the credit of the Society in the Bleecker Street Savings Bank, and $152.96. is in the hands of the Treasurer as a Balance to carry forward to the next fiscal year.

There remains $554.37 on the books, outstanding, as due to the Society, from the membership. Of this sum, over $270 comes over from last year, and $320 of it is probably uncollectable. There is every probability that the rest will be paid in when the financial embarrassments are removed from the indebted members.

The increase in the expenditures over previous years is due to the growing size of the volumes of Transactions, and the entailed expense for printing, engraving and distribution. During this year also the Society has paid for many items, for which the bills have been presented in previous years, after the date of the Annual Report. This is due to the earlier date at which papers are issued before the meeting, and there are so many less bills outstanding against the Society chargeable to next year's income.

Respectfully submitted,

By the Finance Committee.

The Library Committee Report was also presented by its Chairman, and read by the Secretary, as follows:

In accordance with the resolutions adopted at a previous meeting of the Society, active measures have been taken for the creation of a library, in the manner recommended by the report of the Committee submitted at that meeting.

To this end the Secretary issued a circular to the membership, explanatory of the scheme, and soliciting subscriptions in any of the three following forms, viz.:

(*a*) Annual subscriptions to a permanent fund, of $10 or upwards (payable in installments if preferred).

To this there have been responses since the last Report, Vol. VII. Transactions, page 13, from six members, viz.:

C. C. Hill	$25.00	
G. R Cullingworth	10.00	
Ferd. Martens	10.00	
Horace See	10.00	
W. P. Parsons	5.00	
A. H. Raynal	5.00	
		$65.00
Previously reported in last annnal report		618.40
Total		$678.40

(*b*) Annual subscriptions to an amount of $2, payable at the same times as the annual dues.

To this there have been responses since the last report above referred to, from thirty-one members, viz.:

A. H. Raynal,
Albert W. Jacobi,
Ferd. Phillips,
C. C. Hill,
W. H. Doane,
Geo. H. Perkins,
F. D. Cummer,
Gaetano Lanza,
Ferd. Martens,
F. M. Wheeler,
W. F. Rumely,
J. F. Sorzano,
O. W. Kelly,
Wm. Sellers,
Chas. A. Moore,
E. K. Sancton,
W. H. Inslee,
N. C. Stiles,
Chas. P. Howard,
Isaac C. Greene,
C. Potter, Jr.
Frank Grinnell,
Harry C. Francis,
Chas. H. Loring,
Theo. N. Ely,
Geo. Hayes,
J. N. Barr,
W. H. Bone,
Eckley B. Coxe,
John Walker,
Hillary Messimer,

Whose annual subscriptions form a total of	$62.00
The previous subscriptions, given in the last report were..	169.00
Making a total yearly fund to draw upon, of	$231.00

There are therefore 114 members now regularly contributing to this fund by this plan of small increase in the dues, and it is urged that all others who can conveniently do so, should co-operate in the further extension of this plan, and thus induce a more general interest among the members.

(*c*) Direct contributions of books and papers of value.

To this there have been a number of responses during the year, particularly by members residing abroad. The following list contains the contributions not catalogued in the previous report.

By Samuel McElroy:
- Original and Present State of Brooklyn Water Works.
- Water Power at Niagara Falls.

By G. A. Hirn:
- La Cinetique Moderne et la dynamisme de l'Avenir.
- La Conservation de l'Energie Solaire.
- Notice sur les Lois du Frottement.
- Recherches sur les Lois de l'écoulement et du Choc des Gaz.
- Remarques relatives a une Critique du Zeuner.
- Biographie de O. Hallauer.
- Recherches Experimentales sur la Limite de la Vitesse que prend un Gaz.
- Relations reciproques des grands Agents de la Nature.
- Remarques par M. Faye sur la Vitesse de Gaz.
- Reflexions sur une Critique de M. Hugioniat.

By G. A. Hirn and O. Hallauer:
- Refutations, une Critique de M. Zeuner.

By O. Hallauer (Deceased):
- Etude Critique sur les Moteurs a Vapeur. 7 Pamphlets.
- Resume de la Theorie Mecanique de la Chaleur. 2 Pamphlets.
- Compression de la Vapeur.
- Note sur les Variations du Vide.
- Note sur une Modification de l'Indicateur de Watt.
- Note sur la Construction du Thermometre differential a Air.
- Analyse de deux Machines Corliss.

By J. T. Henthorn:
- Report upon the Pumping Engine at Pettaconset Station.

By William Kent:
- Engineering as a Profession.

By J. Burkitt Webb:
- The Second Law of Thermodynamics.

By George H. Bleloch:
- The Art of Needle Making.

By W. H. Durfee:
- The Mitis Process of Producing Wrought Iron and Steel Castings.

By William A. Rogers:
- The German Survey of the Northern Heavens.
- On the Method of Determining the Index Error of the Meridian Circle.
- Studies in Metrology.
- The Reduction of the Different Star Catalogues to a Common System.
- Micrometry.

By J. Bauschinger:
- Protokoll of the Second and Third Sessions of the Conference in Reference to Testing of Materials.

By Henri Schneider :
    Essais Effectues sur un Machine Corliss a Creusot.
By R. Clausius :
    Zur Theorie der Kraftubertragung durch Dynamo-electrische Maschinen.
2 Pamphlets.
By H. Tresca (Deceased) :
    Resultats sur les Machines et les Regulateurs d'Electricite a courant continu.
    Conference sur la Transmission du Travail mechanique par les Courants electriques.
    Experiences faites a l'Exposition Electrique.
By Francis Reuleaux :
    Dampfkessel Explosionen im Deutschen Reiche, 1877–1881.
    Cultur und Technik.
    Neuerungen in Ferntriebwerken.
By J. J. Kunstadter :
    Report of Experiments with U. S. S. S. "Nina," with rudder alone and Kunstadter's Screw.
By William Cowles :
    The "Oram System" of Marine Propulsion.
By Charles T. Main :
    Relative Costs of Steam and Water Power.
By J. C. Hoadley (Deceased) :
    Steam Engine Practice of the United States.
By Horace See :
    Utilizing Steam of the Higher Pressure.
By F. R. Hutton :
    Improvement in Locomotive Engines and Railways, by George E. Sellers.
By C. J. H. Woodbury :
    Reports 1 and 2 on Automatic Sprinklers.
    Electric Lighting in Mills.
    The Relation of Electric Lighting to Insurance.
    Protection of City Warehouses from Loss by Fire.
By Dwelshauvers-Dery :
    Les Decouvertes recentes concernant la Machine a Vapeur. Calcul du rayon des Bobines des Machines d'Extraction.
    Sur la Forme du Tambour Regulateur des Machines d'Extraction.
    Note sur la Compression de Vapeur.
    De la Regulation de Machines.
    Le Regulateur de Buss.
    Report sur la Section de Mechanique de l'Exposition Nationale de Milan.
    Theorie des Moteurs a Vapeur.
    Revue des Machines thermiques motrices exposes a Paris.
    Revue des Machines thermiques motrices exposes a Vienne.
    Programme du Cours de Mechanique appliquee et de Physique industrielle.
    Principes de la Resistance des Materiaux.
W. S. Auchincloss :
    Practische Anwendung der Schieber und Coulissensteuerungen.
Henry Metcalfe :
    The Cost of Manufactures and the Administration of Work Shops.

J. Bauschinger:
- Mittheilungen aus dem Mechanisch Technischen Laboratorium. Parts, 1 to 15.

John H. Cooper:
- Ruskin's Modern Painters. 5 vols.
- Time and Tide. L. Ruskin.
- Life and Travels of Von Humboldt.
- Together with twenty other books which have not been as yet put upon the Library Catalogue, in view of their more remote connection with the special objects for which the Library is being collected.

ANNUAL REPORTS.

City Engineers	Toronto, Canada.
Harbor Commission	Montreal, Canada.

PROCEEDINGS OF SOCIETIES.

Associated Societies of County Surveyors and Civil Engineers of Indiana.
American Water Works Association.
Michigan Association of Surveyors and Civil Engineers.
Michigan Engineering Society.

It is known, however, that many members have issued books or professional publications during the year, which are not represented in the Library. The advantage to the Library of such gifts from the members is so obvious that it is only necessary to ask your more particular attention to the matter in the future than it has received in the past.

Moreover, since the previous report, the Society has acquired by purchase, vols. 4 to 12 inclusive, of London Engineering, which were lacking at that time, and their set is now complete with the exception of vols. 1, 2 and 3, and vols. 29 to 33 inclusive, which the Committee would be very glad to obtain, so as to make their set complete from the beginning.

During this year also efforts have been made to complete the back files of the periodicals taken at the office, and in addition to the files of current numbers given below, the Library contains back issues as follows:

American Engineer, Chicago. Complete from beginning of vol. 2, 1881.
Electrical Review, New York. Complete from beginning of vol. 3, 1883, and vol. 2 is complete except the first six numbers.
Manufacturers' Gazette, Boston. Complete from the beginning of vol. 6, 1884.
American Machinist. Complete from the beginning of vol. 1, except No. 1, of vol. 1.
American Journal of Railway Appliances. Complete from vol. 1, 1883.

Cotton, Wool and Iron, and Boston Journal of Commerce. Complete from beginning of vol. 23, 1884.
Chicago Journal of Commerce. Complete from beginning of vol. 44, 1884.
Electrical Review of England. Complete vol. 13, 1883.
Mechanics. Complete from vol. 1.
Official Gazette, Patent Office. Complete from vol. 26, 1884.
Engineering and Mining Journal. From vol. 29, 1880.
National Car Builder. From vol. 2, 1880.
Railroad Gazette. From vol. 15, 1883.
Van Nostrand's Engineering Magazine. From vol. 22, 1880.
Industrial World, Chicago. From vol. 20, 1883.
The Engineer of England, begins with vol. 58, 1884.
Iron, England, begins with vol. 24, 1884.
Mechanical World and Steam Users' Journal, England, begins with vol. 17, 1884.
Engineering News, begins with January, 1886.
American Miller, begins with January, 1886.
Railway News, begins with January, 1886.

If any of the members know of any facilities for supplying any of the previous issues of the above mechanical journals, they will confer a favor by putting the Committee in the way of securing them.

LIST OF EXCHANGES.

Zpravy Spolku Architektu a Inzenyru. Prague.
Ingenioers-Foreningens Forhandlinger. Stockholm.
Mining Institute of Scotland. Hamilton.
North of England Institute of Mining and Mechanical Engineering. Newcastle.
Institute of Mechanical Engineering of Great Britain. London.
Institute of Civil Engineering of Ireland. Dublin.
Mechanical World. London.
Engineering. London.
The Engineer. London.
Iron. London.
Electrical Review. London.
Master Car Builders' Association. New York.
Engineers' Club of Philadelphia. Philadelphia.
Engineers' Society of West Penn. Pittsburgh.
United States Naval Institute. Annapolis.
Franklin Institute. Philadelphia.
American Society of Civil Engineers. New York.
American Institute of Mining Engineers. New York.
Associated Engineering Societies. St. Louis.
American Journal of Railway Appliances. New York.
Electric Review. New York.
Chicago Journal of Commerce. Chicago.
Boston Journal of Commerce. Boston.
Industrial World. Chicago.
American Engineer. Chicago.
Manufacturers' Gazette. Boston.
American Machinist. New York.

Mechanics. New York.
Railroad Gazette. New York.
Engineering and Mining Journal. New York.
Institution of Engineers and Shipbuilders of Scotland. Glasgow.
Northeast Coast Institute of Engineers and Shipbuilders. Newcastle on Tyne.
Societe des Ingenieurs Civils de France. Paris.
National Car Builder. New York.
American Miller. Chicago.
Engineering News. New York.
United States Patent Office. Washington, D.C.
Van Nostrand's Magazine. New York.
Railway News. New York.
Conservatoire des Arts et Metiers. Paris, France.
Polytechnic Society of Norway. Christiana.
Liverpool Engineering Society. Liverpool, Eng.
The Society of Arts. Massachusetts Institute of Technology. Boston.

Respectfully submitted
by the LIBRARY COMMITTEE.

The Committee on securing the passage of a bill for the appointment of a U. S. Commission to test materials, etc., presented its final report as follows:

To The American Society of Mechanical Engineers.

GENTLEMEN:—

Your Committee beg respectfully to report that since the last Annual Meeting they have been actively engaged in endeavoring to further the object of their appointment. They were in conference with members of Congress before it met, and immediately after its opening a bill for the appointment of a Test Commission was introduced. Engineers were then written to all over the United States, asking them to urge their representatives in Congress and their senators to vote for the bill. Many assurances of sympathy in the bill and determination to work for it were received from members of Congress and from all parts of the United States as well. After some delay a canvass was made of the opinions of the various members in the House of Representatives, and it was found that there was a large majority in favor of the bill. Notwithstanding all this, it was found to be impossible (for what reason your Committee cannot state) to get the bill before the House for discussion, although it was on the calendar. The reason which was given by the persons in charge of the bill was that there was no politics in it. Congress adjourned without reaching the bill. Any further effort that is made in this direction will therefore have to be made *ab initio.* In view of the fact that your Committee has worked earnestly at this matter and has used every honorable means within their knowledge to promote the passage of this bill, and has failed for several successive years, owing to no fault of their own, your Committee suggest that if this subject is to be further continued it be committed to other hands, and that this Committee be discharged from any further consideration of this subject.

Yours respectfully,
THOS. EGLESTON.
C. J. H. WOODBURY.
OBERLIN SMITH.

This report having been presented, it was moved and seconded that it be accepted and adopted and the Committee discharged.

This motion was carried.

The report of the Committee of the Society on securing uniformity in standards for pipe and pipe-threads was next presented by the Secretary of the Committee, Mr. Geo. M. Bond, and received brief, favorable and complimentary discussion. Mr. Kent moved a vote of thanks, which was most cordially seconded by Mr. Doane, and the motion was carried.

The report, as amended in conformity with Mr. Kent's suggestion, is of so important a nature, and particularly in view of added matter which could not have been included in it at the date of its presentation, that it is printed in full as one of the contributed documents of the meeting, with appended discussion, as paper No. CCXXVI.

The Chairman: * The next report is that of the Committee on Uniform Methods of Testing. As a member of that committee I will report briefly that we have made some progress. Prof. Egleston, who was chairman of the committee, has been compelled by ill health to resign the chairmanship. The purpose sought in the appointment of the committee was to arrive, if possible, at some uniform method of making tests of materials whereby the immense amount of this kind of work, which is constantly being done all over the country, may be made comparable, so that the work which each person does shall be available not only to him, but to all others, and *vice versa.* The first step taken by the committee was the sending out of a request, accompanied by samples of material—bars of iron and steel, round, square, and flat—to some ten or twelve different concerns having testing machines, with the request that they would test these specimens and make a report on them and return them. The reports received in response to that request were so variable in form and statement, that a tabulation of them, which was made by Mr. Henning, one of the committee, required, if I remember, very nearly fifty columns, in order to cover every kind of report or statement made by each of the different investigators. It was simply impossible to make any general deduction from returns which were so variable in their bases of statement; and the next step of the committee was to consider carefully a form of standard report for this particular purpose; not for general adoption, but a form which would

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* Mr. Henry R. Towne.

give us identical kinds of information from each party who made the tests. Such a blank was prepared, printed, and sent out, in this case, to twenty-two concerns having testing machines, accompanied by specimens, consisting of three each, round and square wrought iron bars, and also a similar number of pieces of steel—all of the material, by the way, being contributed by manufacturers in aid of the work of the Society. These test pieces have now been out for a month or more; many of them have been tested and returned, and we believe that probably all of the parties addressed will respond by making the tests and giving us the reports. A few of them who have already reported have, in addition to furnishing the reports of the tests, given us considerable information as to the construction of their machines, as to their regular methods of test, and other data which are of interest. When all of this information shall have been received, the committee proposes to tabulate it and put it in form for submission to the Society, and, if the committee can arrive at an agreement as to a standard form for a report blank for tests, they will append that, presumably also adding some brief statement of the rules that should govern tests in order to make them comparable. If all this can be accomplished, the work of the committee will undoubtedly be of value and interest. We hope to have the work so far completed as to be able to make a report at the next meeting.

The last report was the report of the tellers appointed at the previous session to count the ballots for officers:

November 30*th*, 1886.

The tellers, appointed at the meeting last evening, have to report that they have finished the duties assigned them, and report the ballot as follows: Whole number of votes cast, 353, 14 of which not indorsed.

PRESIDENT.

George H. Babcock	335
Scattering	3

VICE-PRESIDENTS.

Joseph Morgan	341
Chas. T. Porter	337
Horace S. Smith	334
Scattering	2

MANAGERS.

Frederick G. Coggin	339
John T. Hawkins	337
Thos. R. Morgan, Sr.	339
Scattering	None

TREASURER.

William H. Wiley	339
Scattering	None

F. VAN WINKLE,
F. F. HEMINWAY, } *Tellers.*
H. C. WHITE,

The chairman then inquired if there was any new business to be presented by any one, before commencing the reading and discussion of papers.

He then briefly reminded the members of the new rules adopted last year, and which were put into effect in Chicago, and which proved satisfactory to all the members. The underlying motive of those rules is simply fair play—that each paper shall have its proportion of time, and that each member discussing a paper shall have his fair proportion of time. Five minutes are allowed for presenting a paper by abstract and five minutes to each member who speaks on a paper; ten minutes being allowed to those who present written discussion. Acting upon the evident sentiment of the Society, as expressed at the last meeting in Chicago, the chair will have to call the time when it has elapsed.

The first professional paper was by Prof. Francis Reuleaux, of Berlin, Germany, honorary member of the Society, entitled "Friction of Toothed Gearing." Messrs. Lewis, Bilgram and C. A. Smith presented written discussions, and Messrs. Stirling and Raynal also spoke. Prof. Thurston's paper on the "Friction of the Non-Condensing Steam Engine," was discussed by Messrs. Barrus, Lanza, Kent, Alden, Wolff, and Raynal. The paper by Mr. A. Wells Robinson, of Philadelphia, entitled "Recent Improvements in Dredging Machinery," was discussed by Messrs. Hill and Towne. At the close of these papers, a lunch was served in the session rooms, tendered by the local committee, and an opportunity was given for social intercourse.

AFTERNOON SESSION.

The afternoon session was called to order at half-past two. Three of the topical queries were taken up first. Messrs. Lanza, Alden, and Towne spoke on the question, "How should a laboratory of mechanical engineering be equipped?" Messrs. Crane, Kent, Lanza, Partridge, Henning, Sanguinetti, Gobeille, and Alden spoke on the query, "What are the best problems for students in mechanical engineering in the last year of their regular course?" The third question, "Can you give any data from your own experience as to the power required to drive modern American machine tools?" elicited discussion from Messrs. Lewis, Hobbs, Lanza, Crane, Babcock, Doane, Couch, Kent, Raynal, Ashworth, Fowler, Falkenau, Weightman, and Towne.

At the close of these discussions, and before resuming the consideration of the papers of the meeting, the Chairman announced that the scientific apparatus of Mr. John C. Hoadley, the well-known engineer and expert, had been purchased by Mr. Stephen W. Baldwin, of New York, as a gift to the American Society of Mechanical Engineers. Mr. Hoadley had been an honored member of the Society, and, upon his death in October, his estate had desired to dispose of his apparatus by sale. The announcement by the chair of this very graceful act of Mr. Baldwin was received with applause, and, on motion, a committee was appointed, consisting of Messrs. Babcock and Partridge, to draft suitable resolutions of thanks to the donor.

The next paper was by Professor Gaetano Lanza, of Boston, upon "The Strength of Shafting exposed to both Twisting and Bending:" Messrs. Kent, Henning, Hawkins, Crane, Hill, and Towne discussed it. The paper by Prof. G. I. Alden, entitled "Formulæ and Tables for Calculating the Effect of the Reciprocating Parts of High-Speed Engines," was discussed by Messrs. Lanza and Lewis.

The paper by Mr. Geo. H. Barrus, of Boston, giving results of investigations with his new calorimeter, was the last paper of the session. The rest of the time up to adjournment was taken up with the topical query: "What are the best conditions for flying rope for transmission of power? Are there limitations to its use?" Messrs. Crane, Hill, Sterling, Hawkins, Giddings, Raynal, Hobbs, Towne, Partridge, Sanguinetti, Durfee, and Hutton spoke in discussion.

At the close of this topic, a letter was read inviting the Society to visit the works of the National Car Spring Co., at Newark. The Secretary was requested to return the thanks of the Society for the courtesy. A telegram to the Secretary was also read, announcing the death, at his residence in Youngstown, Ohio, of Mr. Homer Hamilton, one of the interested members of the Society. At the next session it was moved that the Secretary be requested to send a message of sympathy from the convention to the family of Mr. Hamilton. The session then adjourned.

In the evening, by the courtesy of the Board of Managers of the American Institute, the Society attended the fair of the Institute. Complimentary tickets had been furnished, and were very generally used by the visiting members.

THIRD DAY, WEDNESDAY, DECEMBER 1ST.

The steamer *Pleasant Valley* had been chartered for this day to take the members and their friends to visit places of interest in Newark. On the way down the bay, a stop was made at Bedloe's Island, and by the courtesy of Captain Brewerton, U. S. A., the party were allowed to ascend the Bartholdi Statue of Liberty and get the view from it. Arriving at Newark, the party visited the Hewes and Phillips Iron Works, and were from thence escorted to lunch, tendered in one of the city armories. After lunch, carriages took the party to the works of the U. S. Illuminating Co., Watts Campbell and Co., Clark Thread Works, and to the private laboratory of Mr. Edward Weston. The lateness of the hour prevented a projected visit to the Howell Morocco Works. Parties went also to the Benjamin Atha Steel Works.

A professional session was held in the evening in the hall of the Academy of Medicine. This session was devoted to the two papers belonging to the Economic Section of the Transactions. These papers were by Mr. Oberlin Smith, of Bridgeton, N. J., entitled "Intrinsic Value of Special Tools," and by Mr. W. E. Partridge, of New York, entitled "Capital's Need of High-priced Labor." Messrs. Grant, Borden, Almond, Cole, Walworth, and Towne spoke on the first paper; and Messrs. Hawkins, Kent, Fowler, Almond, Wells, Oberlin Smith, Walworth, Grant, Borden, and Towne discussed the second.

FOURTH DAY, THURSDAY, DECEMBER 2D.

The fifth session was called to order in the lecture-room of Professor Wood, in Stevens Institute of Technology, in Hoboken. The Society had been invited by the President and Faculty of the Institute, and met, pursuant to call, at ten o'clock. The first paper was by Mr. Benjamin Baker, Engineer of the Forth Bridge Railway of England, and honorary member of the Society. It was entitled "The Working Stress of Iron and Steel," and Messrs. Kent, Bond, Henning, and Towne took part in the discussion, together with Prof. De Volson Wood, of the Institute, present by invitation.

The paper by Mr. Wm. Cowles, of New York, on Fire-Boats, was discussed only by Messrs. Le Van, and Cole.

The paper by Mr. Wm. Kent, of New York, "Is Water Gas an Economical Fuel," was discussed by Messrs. F. W. Taylor, Trautwein, Schuhmann and Towne.

The paper by Mr. Andrew C. Campbell, of Bridgeport, Conn., was entitled, "A New Conicograph," and he exhibited one of the instruments and its work and method of operation. Messrs. Couch, Kent, Sanguinetti, Henning, Webb, and Le Van spoke in the discussion, and also by invitation Professors MacCord and Wood of the Institute.

The topical query as to the practical value of the sand-blast process for sharpening files was then taken up and discussed by Messrs. S. T. Williams (who exhibited a file of which part had been treated and part had not), Towne, F. W. Taylor, Webb, Oberlin Smith, Doane, and Babcock, and Mr. Chas. N. Trump spoke by invitation.

An invitation to the Society to visit the warerooms of the Edison Electric Lighting Co. was received and responded to by telegram with thanks.

President Morton, of the Stevens Institute, had invited the members to a reception and lunch, which were much enjoyed by all present. An opportunity was given after lunch to visit the mechanical laboratories and shops of the Institute while the classes were at work there. Much of interest was going on, and the afternoon session did not come to order till three o'clock.

The paper of Mr. Thomas D. West, of Cleveland, entitled "Casting Aluminium Bronze and Other Strong Metals," was presented, and was discussed by Messrs. Le Van, T. R. Morgan, Oberlin Smith, Dodge, and Hill.

Thereupon the remaining topical queries were taken up, and filled the time up to the hour of adjournment. Messrs. West and Sanguinetti spoke on the query as to corrosion of aluminium bronze; Messrs. Wolff, Towne, Kerr, Giddings, Cole, and Le Van spoke of the reasons for preferring injectors or pumps for feeding boilers; Messrs. Dodge, A. H. Emery, Henning, and Towne discussed the best way to build annealing furnaces for small gray iron castings; on the matter of the best way to separate grit from grinding rooms and prevent its dissemination in yards and shops, Messrs. Dodge, Giddings, Partridge, and Towne spoke.

Messrs. Oberlin Smith, Webb, Partridge, Weightman, Babcock, Herman, Le Van, A. H. Emery, Webber, C. T. Porter, and Campbell gave their experience as to the expanding and contracting of drawing boards and paper, and the prevention of annoyance from it.

Messrs. Herman, Oberlin Smith, and Sanguinetti spoke of methods of cutting templets from very thin metal, and Mr. W. R. Jenkins had sent in a written reply to the question why an oil-well pump sucker-rod is prevented from unscrewing at the sockets by giving a twist to the rods. This was the last of the topical discussions.

The Chairman then called for the report of the committees on resolutions. Their reports were presented as follows:

Resolved, That the cordial thanks of this Society are tendered to the Pennsylvania Railroad for courtesies extended during the present session of this Society.

Resolved, That we tender our thanks to Messrs. Cooper, Hewitt & Co., for the invitation to visit their works at Trenton, and for entertainment and courtesies received on that occasion.

Resolved, That the thanks of this Society are due to Edward Weston, Watts, Campbell & Co., Hewes & Phillips, U. S. Elec. Illum. Co., Clark Thread Co., Benj. Atha Steel Works, Howell Morocco Works, and others, of the city of Newark, for the opportunity of visiting the several important and interesting establishments owned or controlled by them, and for the exceedingly hospitable entertainment provided on the recent visit of this Society to that enterprising city.

Resolved, That the thanks of the Society be cordially extended to Capt. Henry Brewerton, United States Army, in command at Bedlow's Island, for facilities afforded on the occasion of our visit to the island, and for the opportunity of inspecting the Statue of Liberty.

Resolved, That the Society extends its thanks to the Board of Managers of the American Institute Fair, for their kind and highly appreciated invitation to visit that very interesting and instructive exhibition of arts, manufactures, and mechanism, and for courtesies on that occasion.

Resolved, That this Society tender its hearty thanks to President Morton, the faculty and trustees of Stevens Institute of Technology, for the highly appreciated attention in inviting this Society to hold two of its sessions at Stevens Institute, and for their courtesy in exhibiting and explaining the abundant resources of the institution under their charge.

Resolved, That the cordial and hearty thanks of this Society are hereby extended to Dr. Henry Morton, President of the Stevens Institute of Technology, for his generous hospitality during the recent visit of the Society to the institution over which he presides.

Resolved, That we collectively and individually extend our thanks to our local committee, and especially to Messrs. Wiley & Crouthers and Hutton, for their admirable arrangements for the entertainment of the Society, and that we heartily appreciate the exceptionally successful manner in which these arrangements have been carried out.

Resolved, That the Secretary be instructed to send copies of these resolutions to the persons mentioned.

Messrs. Babcock and Partridge, the special committee to prepare resolutions of thanks to Mr. S. W. Baldwin for his gift to the Society of the scientific apparatus formerly belonging to the late John C. Hoadley, reported as follows:

Whereas, Mr. S. W. Baldwin has presented to the Society the technical apparatus formerly belonging to, and much of which was original with our late lamented member, Mr. J. C. Hoadley ; therefore,

Resolved, That the thanks of this Society be tendered to Mr. Baldwin for his generous and thoughtful gift, through which this Society comes in possession of so valuable a collection.

Resolved, That the Secretary be instructed to have these resolutions engrossed and presented to Mr. Baldwin as a token of our appreciation of his act.

The Committee also presented the following resolution, just as the hour for adjournment had arrived :

Resolved, That the thanks of the Society be tendered to Mr. Henry R. Towne for the able and faithful manner in which he has performed the duties of acting president during the sessions of the present meeting and throughout the illness of President Coleman Sellers.

The question on this motion was put President-elect Babcock, and Mr. Towne replied as follows from the chair:

The Chairman.—I appreciate very warmly indeed the compliment you have paid me, and am glad if anything I have done during the year has been of service to the interests of the Society.

During the two years that I have been in the Council, as one of the vice-presidents, it has been a pleasant thing to see that the Society has advanced and grown in many ways, and is now in very much better condition than it was two years ago, particularly in the quality of its work. I think that the last volume the Society issued is the best volume of its kind, and contains more material of value to the engineer than any other that I know of as embodying the transactions of a Society of this kind.

Two matters in particular have interested me during the year, and it is a pleasure to me that in each of them I have been able in a small way to contribute to the good work of the Society. One of them came up a year ago, at our meeting in Boston, where it seemed to me that a great deal of the time of the Society was taken up in listening to the reading of papers (which might have been prepared in advance), thereby limiting the discussion on them, which was often interesting and valuable, and which in that case was very much curtailed. Conferring with Prof. Hutton, our Secretary, about it, I found that our views coincided. The matter was brought to the notice of the Council, and a committee was appointed, consisting of Prof. Hutton and myself, the result of whose work was the drafting of the new rules, which were first put into effect at Chicago, and which have been continued here. The members of the Society at the Chicago meeting expressed by a vote their approval of these rules, and requested that in the future the presiding officers of the meetings should enforce the rules strictly. The fundamental idea of thus limiting the time allowed to each paper and to each speaker is fair play; that the first paper on the list shall have no greater privileges than the last.

The other matter to which I refer was introduced by the paper I read at Chicago, in which I modestly ventured to suggest that the Society might annex another subject to its discussions, bringing in industrial or economic questions. I had no thought then that such a proposition would be entertained except as a side issue, a matter which those of the members who took interest in such things could promote by attending a supplemental session. The proposition was received favorably in one sense and unfavorably in the other. The idea of introducing such matters in our discussions was accepted. The idea of relegating them to an annex or sub-section was negatived emphatically, and it was voted that such subjects should be introduced and made part of our regular transactions.

We have had some test of this policy in our present meeting, and it seems to be the sentiment of the members generally that the Society is fully justified in taking up the discussion of such subjects. I may add that it is more appropriate for this Society to consider such matters than any other of the engineering societies. We have in our membership, much more than have the Civil Engineers or the Mining Engineers, men who are managers of labor, who are either owners or representatives of owners, and who therefore control capital. There are fewer purely professional men, and men having no direct responsibility for the management of others, in this Society than in either of the other engineering societies. These economic questions come nearer to us, therefore, and to a larger number of our members, and in my judgment they can most properly be considered, to a reasonable extent, germane to the interests and duties of a large proportion of our membership.

Again thanking you for the compliment you have just tendered me, I repeat my appreciation of it. I hope that the Society's work will continue to grow in the future as well as it has during the past year.

The meeting then adjourned.

FIFTH DAY, FRIDAY, DECEMBER 3D.

A special train on the Pennsylvania Railroad conveyed the members on an excursion to Trenton, N. J., where they were the guests of Messrs. Cooper, Hewitt & Co. The cars were run on a siding to the lower mill, where large beams are rolled, and a visit was paid to the bridge-shop. Returning to the wire-mill, a lunch was spread by the hosts in one of the buildings, and after it the wire-mill with its belted Corliss engine running trains on continuous wire rods, and the other features of the manufacture were examined. A visit to one of the potteries was also made in the afternoon, and by some to the Phœnix works upon the river. The train brought a large number back to New York, while numbers went directly West from Trenton.

CCXXVI.

REPORT OF THE COMMITTEE ON STANDARD PIPE AND PIPE THREADS,

OF THE AMERICAN SOCIETY OF MECHANICAL ENGINEERS,

Submitted at the Seventh Annual Meeting, held in New York City, November 29 to December 3, 1886.

[NOTE.—This report is the one referred to on page 19 of the Proceedings. The importance of the matter of which it treats has induced the Publication Committee to order it issued as a separate paper contributed to the Society.

The report was as follows :]

GENTLEMEN: Your committee to whom was referred the consideration of a standard for pipe and pipe threads, have the honor to present the following report:

At a meeting of your committee held in Hartford, February 23, 1886, the request embodied in the following circular letter to the manufacturers of wrought-iron pipe in the United States was decided upon, and the letter was issued April 21, 1886, addressed to each of the companies composing the above association:

"At the Sixth Annual Meeting of the American Society of Mechanical Engineers, held in Boston, in November, 1885, a committee was appointed by the president, to confer with manufacturers of pipe, pipe dies, and pipe fittings, with a view of bringing about a uniformity in the sizes of pipe and pipe threads, and of maintaining it by the use of gauges which shall definitely represent standard sizes.

"A meeting of this committee was held in Hartford, February 23, 1886.

"The opinion of this committee is that the Briggs standard, which nearly all, if not all, of the pipe manufacturers once adopted, is the proper standard to be adhered to, and that it only requires definite co-operation on the part of pipe manufacturers with the committee, in order to bring their product strictly to that standard, and to adopt means of strictly adhering to it within practical limits.

"A copy of the minutes of the Boston meeting referred to is herewith mailed you, in which will be found the report of the discussion and subsequent recorded appointment of the committee. There is also sent you a copy of the paper upon this subject, which was read at that time before the Society, together with the report of the discussion which followed its reading.

"The committee request that the pipe manufacturers give this matter consideration, and would suggest that they appoint a committee to confer with them, with a view of bringing about the desired result, and to notify the secretary as to the date when this meeting may be held.

"Will you please give your individual aid in having such a committee appointed?

"An early answer will oblige,

"Yours very truly,

(Signed) "GEO. M. BOND, *Secretary.*

Frederick Grinnell, *Chairman.*
George Schuhmann.
Wm. J. Baldwin.
B. H. Warren.
Geo. M. Bond, *Secretary.*
HARTFORD, CONN., *April* 21, 1886."

There was also issued a circular letter to each of the members of the Associated Manufacturers of Cast and Malleable Iron and Brass Fittings in the United States. This letter was dated April 28, 1886, and is as follows:

"The enclosed communication, which has been sent to each of the pipe manufacturers, will explain itself.

"A copy of extract from minutes of the proceedings is also enclosed.

"We will be pleased to have your views upon the subject for our guidance in conferring with the proposed committee of pipe manufacturers.

"Yours very truly,

(Signed) "GEO. M. BOND, *Secretary.*"

As stated in the preliminary report of your committee, submitted at the meeting of this Society, held in Chicago, May, 1886,* a committee was appointed by the Manufacturers of

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* Vol. VII. Transactions A. S. M. E., p. 414.

Wrought Iron Pipe and Boiler Tubes in the United States, at their meeting held in Philadelphia, May 12, 1886, the members of which committee, as at that time given, being:

Mr. L. W. Shallcross, Chairman, representing Messrs. Morris, Tasker & Co., Limited, of Philadelphia.

Mr. J. H. Flager, representing The National Tube Works Co., of McKeesport, Pa.

Mr. L. J. Piers, representing The Allison Manufacturing Co., Philadelphia, and

Mr. Jas. H. Murdock, of Pittsburgh, Secretary to the committee.

The action taken by the Cast Iron Fittings Association is also here given:

"At a meeting of the Cast Iron Fittings Association, held in New York, May 19, 1886, the following resolution was unanimously adopted:

"'*Resolved*, That a committee of five (5) be appointed to take into consideration the matter of a standard gauge of thread.'

"The following gentlemen were named as such committee:

"Mr. R. T. Crane, President Crane Bros. Manufacturing Co., Chicago, Ill.

"Mr. C. C. Walworth, President Walworth Manufacturing Co., Boston, Mass.

"Mr. E. G. Burnham, Vice-President The Eaton, Cole & Burnham Co., Bridgeport, Conn.

"Mr. Charles Jarecki, President Jarecki Manufacturing Co., Erie, Pa.

"Mr. Carleton W. Nason, President Nason Manufacturing Co., New York City."

The action taken by the Manufacturers of Brass and Iron, Steam, Gas and Water Work of the United States, at a meeting of their Association, held in Pittsburgh, May 11, 12 and 13, 1886, was that the following resolution was unanimously carried:

"*Resolved*, That this association favors the establishment of a universal wrought-iron pipe gauge, to be used as a standard throughout the United States, and that any action taken by the manufacturers of wrought-iron pipe to accomplish this object shall have our hearty co-operation."

Soon after this preliminary report was submitted at the meeting of the Society in Chicago in May, a joint conference of the committee appointed by the manufacturers of wrought-iron pipe, and your committee, was held at the Fifth Avenue Hotel, New York, June 17, 1886. The result of this conference is clearly stated in the official notification issued by Mr. L. W. Shallcross, chairman of the conference, and which is here given :

"NEW YORK, *June* 17, 1886.

"At a meeting of the Standard Pipe and Pipe-Thread Committee of the Manufacturers of Wrought Iron and Boiler Tubes in the United States, held at the Fifth Avenue Hotel, this day, Thursday, June 17, 1886, at 11 o'clock A.M.

L. W. Shallcross in the chair.

Jas. H. Murdock, Secretary.

Present:—

Morris, Tasker & Co., Limited........L. W. Shallcross.
National Tube Works Co ....J. H. Flagler.
The Allison Manufacturing Co........L. J. Piers.

"Also, present in conference, members of the Standard Pipe-Thread Committee of the American Society of Mechanical Engineers, as follows :

"Frederick Grinnell, Chairman of Committee, President Providence Steam and Gas Pipe Co., Providence, R. I.

"Geo. M. Bond, Secretary of Committee, of The Pratt & Whitney Co., Hartford, Conn.

"Geo. Schuhmann, of the Reading Iron Works, Reading, Pa.

"Wm. J. Baldwin, 96 Fulton Street, New York, Steam Heating Engineer.

"On motion of Mr. Flagler, seconded by Mr. Piers, that each manufacturer send to The Pratt & Whitney Co., Hartford, Conn., sample pieces of their pipe from (6) six inches diameter down, threaded on one end, to be tested by The Pratt & Whitney Co., with the Briggs standard, and a report to be made by them to each manufacturer of the state of his gauges only, as compared with the Briggs standard.

"And the Secretary be hereby instructed to notify the manufacturers, and request them to comply with this resolution without delay, so that action can be taken at the meeting of July 20th, proximo.

"Unanimously carried.

"On motion, adjourned to meet at the call of the Chairman.

"Attest—JAS. H. MURDOCK, L. W. SHALLCROSS,
Secretary to Committee. *Chairman of Committee.*"

In accordance with the foregoing request, there were received by The Pratt & Whitney Co., for the required test, samples of pipe with threads cut on one end, which your committee has every reason to believe represented the average sizes of pipe and pipe threads, as ordinarily manufactured.

Samples were received from the following manufacturers of wrought-iron pipe, members of the association.

Messrs. The National Tube Works Co., McKeesport.
" Reading Iron Works, Reading.
" A. M. Byers & Co., Pittsburgh.
" Spang, Chalfant & Co., Pittsburgh.
" American Tube and Iron Company, Middletown, Pa.
" Conshohocken Tube Co., Conshohocken.
" Crane Bros. Manufacturing Co., Chicago.
" Morris, Tasker & Co., Limited, Philadelphia.
" Fieldhouse, Dutcher & Belden, Chicago.
" The Allison Manufacturing Co., Philadelphia.
" Jas. Hooven & Son, Norristown, Pa.
" Delaware Iron Co., New Castle, Del.

The Pittsburgh Tube Company, their new works not being at the time in operation, stated that their sizes would in general conform to those sent by the American Tube and Iron Co.

The examination and test of these sample pieces of threaded pipe, was not made until August 23 and 24, 1886, owing to the late arrival of several sets necessary to complete the list. After having had the set of Briggs standard reference gauges critically verified by The Pratt & Whitney Co., the test was conducted under the conditions of confidence which was accepted by your committee, the secretary only to have personal knowledge of each manufacturer's variation. The results were collected, and were arranged in tabulated form, each column being headed with a special number, which referred to the manufacturer there represented.

This tabulated report, with general deductions appended, was submitted to the Pipe Manufacturers' Association at their regular Convention held at the Continental Hotel, Philadelphia, August 25, 1886, and soon after, reports to manufacturers individually were sent by the secretary of your committee, as requested in the resolution adopted at the conference.

The variation from the Briggs standard, as found to exist under this test, did not seem to warrant a departure from the original standard represented by the Briggs gauges, and confirmed the opinion of your committee that the Briggs standard could be adhered to.

With the exception of the three-quarters and one inch sizes, but comparatively little change would be required in the dies used by the pipe manufacturers in cutting pipe made by them for the market.

Recognizing the interests of the manufacturers of brass and cast-iron fittings, a joint conference, appointed by these associations and your committee, was called, in order to harmonize all interests involved, and to adopt a resolution expressing the sense of such a meeting for the consideration of the Pipe Manufacturers' Association.

This conference was arranged for Monday, October 25, 1886, and was held at the Fifth Avenue Hotel, New York, at 11 o'clock A.M. of that date.

There were present at this conference: of the committee representing the Wrought-Iron Pipe Manufacturers' Association, Mr. J. H. Flagler, of the National Tube Works Co.; Mr. L. W. Shallcross, chairman of the conference: of the committee appointed by the Cast-Iron Fittings Association, Mr. Carleton W. Nason, President of Nason Manufacturing Co., New York, representing also Mr. Jarecki, President of The Jarecki Manufacturing Co., Erie, Pa. There was also present, representing this Association, Mr. W. H. Douglas, Corresponding Secretary. Also there were present, representing the Manufacturers' Association of Brass and Iron, Steam, Gas and Water Work of the United States, Mr. S. L. Morison, Secretary of the Association. Of your committee there were present, Mr. William J. Baldwin, and George M. Bond, secretary of the conference. Letters were read by Mr. Carleton W. Nason from Mr. R. T. Crane, Chicago, and Mr. C. C. Walworth, of Boston, members of their committee, who were unable to attend, stating their position in the matter. After considerable discussion the following resolution was unanimously carried:

> "That it is the sense of this meeting that a common standard be adopted, and that action should proceed first from the Pipe Manufacturers, and for that reason we recommend that it should be particularly brought to their attention at the meeting to be held in Pittsburgh this week."

The meeting of the Pipe Manufacturers' Association just referred to was held at the Monongahela House, Pittsburgh, October 27, 1886.

The official notification received by your committee of the action taken by the pipe manufacturers at this meeting, is here given:

"George M. Bond, Esq.,
"*Sec'y Committee Standard Pipe and Pipe Threads,*
"*American Society Mechanical Engineers,*
"*Hartford, Conn.*

"Dear Sir:—At a meeting of the Manufacturers of Wrought-Iron Pipe and Boiler Tubes in the United States, held at the Monongahela House, Pittsburgh, October 27, 1886, it was resolved that the Wrought-Iron Pipe Manufacturers of the United States hereby adopt the Briggs standard of gauges, and that where any manufacturer has from any cause got away from that standard, they be requested to get such corrections made as soon as possible, so as to conform to the Briggs standard.

"Yours truly,
(Signed) "James H. Murdock, *Sec'y*
"*Manufacturers Wrought-Iron Pipe and Boiler*
"*Tubes in the United States.*'
"Pittsburgh, *November* 4, 1886."

Your committee has notified officially of the above action of the Pipe Manufacturers' Association, Mr. W. H. Douglas, Corresponding Secretary Cast-Iron Fittings Association, New York; Mr. S. L. Morison, Secretary Brass Fittings Association, New York, and Mr. John Maneely, Corresponding Secretary Malleable Iron Association, Philadelphia, in the following letter, which was mailed to each from Pittsburgh, November 4:

"It is gratifying to me to advise you that at a meeting of the Manufacturers of Wrought-Iron Pipe and Boiler Tubes in the United States, held at the Monongahela House, Pittsburgh, October 27, 1886, it was resolved 'that the Wrought-Iron Pipe Manufacturers of the United States hereby adopt the Briggs standard of gauges, and that where any manufacturer has, from any cause, got away from that standard,' they be requested to get such corrections as soon as possible, so as to conform to the Briggs standard.'

"Yours truly,
(Signed) "Geo. M. Bond, *Sec'y*
"*Committee Standard Pipe and Pipe Threads,*
"*American Society Mech. Engineers.*"

In concluding this report, your committee wish to express their appreciation of the assistance kindly rendered by Mr. J. H. Murdock, Secretary Manufacturers Wrought-Iron Pipe and Boiler Tubes in the United States; Mr. S. L. Morison, Secretary of the Association of Brass and Iron Fittings Manufacturers of the United States, and Mr. W. H. Douglas, Corresponding Secretary of the Cast-Iron Fittings Association, in furnishing lists of the different manufacturers of wrought-iron pipe, cast-iron, and brass fittings, and who in many ways greatly facilitated the work of your committee. Respectfully submitted,

FREDERICK GRINNELL, *Chairman.*
GEO. M. BOND, *Secretary.*
GEORGE SCHUHMANN.
B. H. WARREN.
WM. J. BALDWIN.

NEW YORK, *November* 30, 1886.

ADDENDA.

The following communication, received subsequently to the presentation of the foregoing report, transmitting to your committee officially the resolution adopted by the Manufacturers' Association of Brass and Iron, Steam, Gas and Water Work, sustaining the action of the Manufacturers of Wrought-Iron Pipe, is herewith presented:

"MANUFACTURERS' ASSOCIATION
OF
BRASS AND IRON, STEAM, GAS AND WATER WORK.

"79 FULTON ST., NEW YORK, *December* 15, 1886.

"GEORGE M. BOND, *Secretary Committee*
"*Standard Pipe and Pipe Threads,*
"*American Society Mechanical Engineers,*
"*Care of Pratt & Whitney Co.,*
"*Hartford, Conn.*

"DEAR SIR:—At a meeting of the Manufacturers' Associatior of Brass and Iron, Steam, Gas and Water Work, held at the Fiftl Avenue Hotel, New York, on December 8th, 1886, the following resolutions were unanimously adopted:

"'That the action of the Wrought-Iron Pipe Association, in adopting the Brigg Standard as the Standard Iron Pipe Gauge of the United States, be indorsed, an

that as Manufacturers of Brass and Iron, Steam, Gas and Water Work we will act in conformity with the resolution adopted by them; and that as a measure of safety to ourselves, in order to avoid any difficulty with our customers at any time, that the Secretary of this Association be authorized to correspond with the Secretaries of the Wrought-Iron Pipe, Cast-Iron Fittings and Malleable Fittings Associations, and, if acceptable to them, that an order be given by these Associations to The Pratt & Whitney Co., Hartford, Conn., for one set of Standard Gauges, and that such set of Gauges be presented to the American Society of Mechanical Engineers as a matter of reference for all our united members, to be held by them in case of any dispute arising as to whether the members of these Associations are manufacturing to the standard or not.

" '*Resolved*, That the thanks of this Association are due to Mr. Bond for his enlightened, gentlemanly and courteous manner in presenting this matter for our attention.'

" Yours truly,
(Signed) " S. L. MORISON, *Secretary*."

For comprehensive information regarding the subject of standard pipe and pipe threads, as applied in American practice, your Committee would refer all who may be interested, to the Excerpt Minutes of Proceedings of the Institution of Civil Engineers of Great Britain, vol. lxxi., Session 1882–83, Part I. containing the paper of the late Robert Briggs, C. E., presented and read after his death, on "American Practice in Warming Buildings by Steam," which also includes the discussion which followed.

Referring specially to the subject your committee has had in hand, the following, from the text and tables of the paper of Mr. Briggs is here presented, giving completely the data upon which the Briggs standard pipe thread sizes are based. . . .

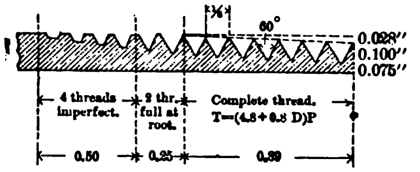
"The taper employed for the conical tube-ends is uniform with all makers of tubes or fittings, namely an inclination of 1 in 32 to the axis. Custom has established also a particular length of screwed end for each different diameter of tube. Tubes of the several diameters are kept in stock by manufacturers and merchants, and form the basis of a regular trade in the apparatus for warming by steam. A knowledge of all these particulars is therefore essential for designing apparatus for the purpose. The ruling dimension in wrought-iron tube work is the external diameter of certain nominal sizes, which are designated roughly according to their internal diameter. These nominal sizes were mainly established in the English tube trade between 1820 and 1840, and certain pitches of screw-thread were then adopted for them, the coarseness of the pitch varying roughly with the diameter, but in an arbitrary way utterly devoid of regularity. The length of the screwed portion on the tube end varies with the external diameter of the tube according to an arbitrary rule-of-thumb; whence results, for each size of tube, a certain minimum thickness of metal at the outer extremity of the tapering screwed tube-end. It is the determination of this minimum thickness of metal, for the tapering screwed end of a wrought-iron tube, which constitutes the question of mechanical interest.

"A longitudinal section of the tapering tube-end, with the screw-thread as actually formed, is shown full size in Fig. 105 for a nominal 2½ inch tube, that is, a tube of about 2¼ inches internal diameter, and 2⅞ inches actual external diameter.

"The thread employed has an angle of 60°; it is slightly rounded off both at the top and at the bottom, so that the height or depth of the thread, instead of being exactly equal to the pitch, is only four-fifths of the pitch, or equal to $0.8\ \frac{1}{n}$, if n be the number of threads per inch. For the length of tube-end throughout which the screw-thread continues perfect, the empirical formula used is $(0.8\ D + 4.8) \times \frac{1}{n}$, where D is the actual external diameter of the tube throughout its parallel length, and is expressed in inches. Further back, beyond the perfect threads, come two having the same taper at the bottom, but imperfect at the top. The remaining imperfect portion of the screw-thread, furthest back from the extremity of the tube, is not essential in any way to this system of joint; and its imperfection is simply incidental to the process of cutting the thread at a single operation. From the foregoing it follows that, at the very extremity of the tube, the diameter at the bottom of the thread,

Fig. 105.

(Thread of 2½ inch tube. Full size.)



$$D - \left[\frac{2 \times (0.8\ D + 4.8)}{32\ n} + \frac{2 \times 0.8}{n}\right] = D - (0.05\ D + 1.9) \times \frac{1}{n}.$$

The thickness of iron below the bottom of the thread, at the tube extremity, is empirically taken to be = $0.0175\,D + 0.025$. Hence the actual internal diameter d of any tube is found to be, in inches,

$$d = D - (0.05\,D + 1.9) \times \frac{1}{n} - 2 \times (0.0175\,D + 0.025),$$

or

$$d = 0.965\,D - 0.05\frac{D}{n} - \frac{1.9}{n} - 0.05.$$

For the various sizes of tubes, ranging from $\frac{1}{8}$ inch to 10 inches nominal internal diameter, with their corresponding numbers of screw-threads per inch, the actual internal diameter d is expressed by the following Table I. in terms of the actual external diameter D.

TABLE I.

DIAMETERS OF WROUGHT-IRON WELDED TUBES.

Nominal Internal Diameter of Tube.	Number of Screw-threads per inch.	Actual Internal Diameter d in Terms of Actual External Diameter D.
Inches.	*No.*	*Inches.*
$\frac{1}{8}$	27	$d = 0.9631\,D - 0.1204$
$\frac{1}{4}$ and $\frac{3}{8}$	18	$d = 0.9622\,D - 0.1556$
$\frac{1}{2}$ and $\frac{3}{4}$	14	$d = 0.9614\,D - 0.1857$
1, $1\frac{1}{4}$, $1\frac{1}{2}$ and 2	$11\frac{1}{2}$	$d = 0.9607\,D - 0.2152$
$2\frac{1}{2}$ to 10	8	$d = 0.9587\,D - 0.2875$

"The figures derived from this statement, which are of importance for practical use, are presented in detail in the accompanying Table II. in a convenient order for reference.

"The number of screw-threads per inch for the several sizes of tubes is here accepted from customary usage. It is the workman's approximation to the pitch practically desirable, and much reluctance must consequently be felt in calling it in question. Still it would have been better to investigate the general case upon the basis of a pitch ranging in closer accordance with the range of tube diameter. Thus the nominal $\frac{1}{2}$-inch tubes might have had 16 threads per inch; $\frac{3}{4}$ inch, 14 threads; 1 and $1\frac{1}{4}$ inch, 12 threads; $1\frac{1}{2}$ and 2 inches, 11 threads; $2\frac{1}{2}$ to $3\frac{1}{2}$ inches, 10 threads; 4 to 6 inches, 8 threads; 7 to 9 inches, 7 threads; and 10 inches, not more than 6 threads per inch. The existing numbers of threads however, as given in Tables I. and II., are now too well established to be disturbed; at all events they must be taken in any statement of present practice.

TABLE II.

STANDARD DIMENSIONS OF WROUGHT-IRON WELDED TUBES.

DIAMETER OF TUBE.			THICKNESS OF METAL.	SCREWED ENDS.	
Nominal Inside.	Actual Inside.	Actual Outside.		Number of Threads per Inch.	Length of Perfect Screw.
Inches.	Inches.	Inches.	Inch.	No.	Inch.
⅛	0.270	0.405	0.068	27	0.19
¼	0.364	0.540	0.088	18	0.29
⅜	0.494	0.675	0.091	18	0.30
½	0.623	0.840	0.109	14	0.39
¾	0.824	1.050	0.113	14	0.40
1	1.048	1.315	0.134	11½	0.51
1¼	1.380	1.660	0.140	11½	0.54
1½	1.610	1.900	0.145	11½	0.55
2	2.067	2.375	0.154	11½	0.58
2½	2.468	2.875	0.204	8	0.89
3	3.067	3.500	0.217	8	0.95
3½	3.548	4.000	0.226	8	1.00
4	4.026	4.500	0.237	8	1.05
4½	4.508	5.000	0.246	8	1.10
5	5.045	5.563	0.259	8	1.16
6	6.065	6.625	0.280	8	1.26
7	7.023	7.625	0.301	8	1.36
8	8.982	8.625	0.322	8	1.46
9	9.000	9.688	0.344	8	1.57
10	10.019	10.750	0.366	8	1.68

Taper of conical tube-ends, 1 in 32 to axis of tube.

DISCUSSION.

Mr. Oberlin Smith.—I want to ask Mr. Bond whether the committee have ascertained what the condition of pipe threads is in England—whether there is any general standard in use there, and how near the Briggs standard conforms to it. I also want to ask whether the brass valve manufacturers in this country have been generally approached on this subject. The report speaks of the cast-iron fittings men and the pipe men. It does not mention particularly the valve manufacturers.

Mr. Bond.—I can say, in answer to Mr. Smith, that the second circular letter referred to in the report which was addressed to the fittings manufacturers, included the names on the list received from Mr. S. L. Morison, Secretary of the Fittings Manufacturers' Association, and embraced nearly all the manufacturers of valves and fittings. There were over seventy firms and corporations represented, and quite a number of them were valve manufacturers, both of brass and iron.

In regard to the first question, Professor Hutton handed me a letter last evening which is of interest to our society and our committee, which refers to this very subject, from Dr. Chaney, Superintendent of the Standards in the Bureau of Weights and Measures of Great Britain. The letter speaks of the very unsatisfactory condition of the pipe-thread standards of that country, and inquires in detail about what the Society is doing, in the hope that they may be helped in a similar effort in England and its colonies. So that it would seem there is a tendency to make an effort to adopt some uniform standard which is probably not in existence there at the present time. Our committee has not undertaken to correspond with English manufacturers or to have anything to do with the English standard, because their standard is different from ours. The threads are cut straight and are different in pitch. All the work we had in hand was the pipe made or used in the United States.

Mr. Wm. Kent.—I think the committee deserve the thanks of the Society for the admirable manner in which they have conducted their duties, and for the presentation of their report, which will no doubt fix the question of standard threads for some time.

Mr. Stirling.—It seems to me that this report, so far as it goes, is very exhaustive, and leaves nothing to be desired as far as it concerns this country. But I had occasion within a month to send some machinery to a part of the British dominions, in Australia, and we had to send a piece of pipe with our thread cut on one end, leaving the other end to be cut to suit the fittings which would be found there. It occurs to me that it would be well for this Society to communicate with the sister societies in Great Britain, and to see about arranging for a uniform standard for all parts of the world. It seems to me that is a practicable thing and very desirable.

Mr. Oberlin Smith.—Mr. Stirling's ideas coincide with mine entirely. Are we so committed to this Briggs standard as to have actually recommended it to the manufacturers?

The Chairman.—It is the other way; I think they have adopted it.

Mr. Bond.—Your committee recommended the adoption of the Briggs standard. There is no doubt at all about the advisability, if it were possible to do so, of changing the pitches of the thread, because the two-inch pipe is eleven and one-half threads to the inch, while the next size larger is much coarser, being eight threads to the inch; so that with the thickness of pipe, as now made, it has a tendency to weaken the joint at that point. There is no doubt that at the time when the Briggs standard was carried out by Mr. Briggs himself, the iron for pipe was made of a heavier gauge; there was greater thickness of metal, and it would stand a deeper thread. But the process of manufacture now is so much improved that strong pipe is made without requiring it to be so thick, and of course, if too thin, eight threads to the inch cuts pretty deeply into the pipe. If your committee had undertaken to make any change in the pitches of the thread we would have met with an opposition that would have defeated everything for which we were working. Your committee took the ground that if the original Briggs standard thread could be adopted in the original pitches, and have the sizes made uniformly to gauges, so that they would be practically interchangeable, with the allowance of a variation of one turn either way, which is certainly close enough for interchangeability, the whole matter would be practically remedied. In the test made, the larger sizes and some of the smaller sizes exhibited a variation quite enough to throw out the Briggs standard; but at the same time, the average showed conclusively that it was to the interest of the different manufacturers to work to some definite standard, and as the Briggs standard covered the ground so completely your committee advocated its re-adoption. The only way to carry out this system, of course, is to work to gauges which are truly representative of the Briggs standard thread.

Mr. Smith.—It is very desirable that there should be a common standard all over the world. I do not know what is done in France and Germany; but even if we could coincide with England, it would be a good thing. It may be they will adopt the Briggs standard after we get it going; perhaps it is the best thing this Society can do to recommend this standard, although if it is going to keep back an international standard, I would be sorry.

Mr. G. L. Fowler.—I would like to ask if that standard includes the tube-casing such as is used in salt wells and oil wells. We have had a good deal of trouble in connection with that.

Mr. Bond.—I would say that our tests did not include casing for either salt or oil wells. There was one sample received which was intended to represent casing for oil wells. It had very much finer thread. It was thrown out because it was not included under the Briggs standard. I understand that in tube which is used for natural gas and petroleum pipe lines, where the pressure is very great, a finer thread is used on the larger pipe, but it is finer probably because the necessity for a tight joint is very obvious, and the thread of eight to the inch on three or four-inch pipe seems to require too much effort to make the joint sufficiently tight. The Briggs standard covers the ground for all threaded wrought-iron pipe for gas, steam and water work, unless i is outside of ordinary practice.

Mr. A. R. Wolff.—I just wish to suggest that it might be well if this Society would indorse the action of the committee on standard pipe and pipe thread in advocating the Briggs standard as the proper standard for this class of pipe that Mr. Bond has referred to. I would make that resolution if it be in order.

Mr. Kent.—I rise to protest against any such action. I think we established the precedent at the Atlantic City meeting that this Society would not indorse anything. As there is no other motion before the house, I move that the report of the committee be accepted with the thanks of the Society, and ordered printed in the transactions.

The Chairman.—Before putting the question, I would say with regard to Mr. Smith's inquiry, that it is my understanding that the matter of standard threads is much more chaotic in England than it is here. We are much in advance of them in that respect, and we have nothing to gain by following in their tracks. I think that is partly true of the continent also. It is a great pleasure to me personally, also, to see this recognition of the work done by my late friend, Mr. Robert Briggs, than whom we never had a more conscientious, faithful engineer in the profession in this country.

Mr. Fowler.—Would it not be well if the work of the committee should include that work of tubing? The engineers who have charge of sinking salt wells have great difficulty in that way. Manufacturers will send in 4½-inch casing cut with 10 and 12 threads. The pipe is expanded on one end with the thread cut on the inside of the pipe; the other end is cut with a thread which has a taper so that the full thread is not cut at the pipe end of the thread. I think it would be a great advantage if engineers who have charge of that kind of work could know that the pipe they ordered would be cut with the right kind of thread —the standard thread—and not be obliged to specify it in every instance.

The Chairman.—I suppose there is no objection to considering that a request to the committee.

Mr. W. H. Doane.—I want to add one word in seconding Mr. Kent's resolution. I think that the members of the Society may not appreciate how much of hard and laborious work has been expended by this committee, as I have had reason to know myself somewhat, and it has moreover been continued through many months. Some of the members of the committee, especially, have given a great deal of time, which they could ill afford, and I think, sir, if we can establish this uniform system in this country it will be a great thing, and when that has been accomplished I have no doubt in my own mind that the English people would be very glad to follow after us. I rise, sir, to second the motion made to adopt this report, with the thanks of the Society, and to move to continue this committee. They have this matter fully in hand, and when their final report is made up, I think it will be one that will give satisfaction to every engineer present.

The motion was carried with applause.

CCXXVII.

FRICTION IN TOOTHED GEARING.

BY PROF. F. REULEAUX, BERLIN, GERMANY.*

(Honorary Member of the Society.)

The following investigation relates to the friction of teeth in spur gearing; but it can readily be adapted to bevel gear as well. In a plane section normal to the axes of a pair of spur gears, let two curves be moved upon each other. For the least possible movement the amount of sliding at the point of contact of the curves is equal to the difference $ds - ds_1$, in the space passed over by that point on each curve when both of them move in the same direction. When they move in opposite directions, it is equal to the sum of their paths, $ds + ds_1$; so that we obtain the general expression for the differential dg of the amount of sliding,

$$dg = ds \pm ds_1.$$

In Figures 1 and 2, T and T_1 are the pitch circles, and E the curve on which the point of contact will always lie for two teeth in gear. A study of the movement shows that whatever be the shape of the teeth the profiles will always pass through the common point of contact in the same direction, so that here in every case we have

$$(1) \quad . \; . \; . \; . \; . \; . \; . \; . \quad dg = ds - ds_1.$$

With a working pressure Q, normal to the common tangent of

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* Translated from the original German by Mr. Wheaton Kunhardt, Engineer for the Bower-Barff Rustless Iron Co., of New York, acting in co-operation with the Secretary of the Society. The latter would put on record in this way the obligation under which the author and the membership have been put by the chief translator for the manner in which this work has been done, as a contribution to the transactions of the Society.

the tooth-profiles, and a coefficient of friction f, the work of friction for a pair of teeth is

$$(2) \quad . \; . \; . \; . \; . \; . \quad W = \int Q f \,(ds - ds_1).$$

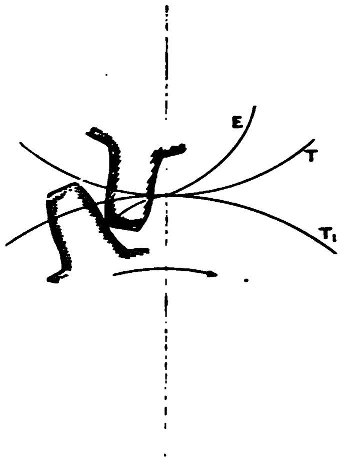


Fig. 1.

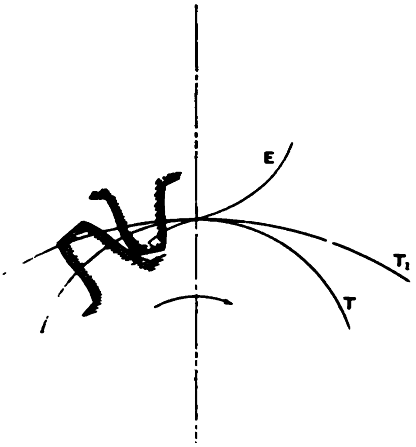


Fig. 2.

This general expression can readily be applied to the different kinds of toothed gearing.

EPICYCLOIDAL GEARING.

Geometrical Investigation.—In Figure 3, let T be the pitch arc and D the describing circle with which the profile of a tooth is drawn. Referring to Figure 3, the hurtful work of friction on one side of the line of centers OO_1 is

$$(3) \quad . \; . \; . \; . \; . \quad W' = f \int Q \,(ds' - ds_1');$$

and the useful work

$$(4) \quad . \; . \; . \; . \; . \quad U' = \int Q R \sin \alpha' \, d\theta',$$

in which $\theta' = \frac{r}{R} \varphi' = \frac{r}{R}(180 - 2\alpha');$

and hence

$$(5) \quad . \; . \; . \; . \; . \; . \; . \quad d\theta' = -\,2\frac{r}{R}\,d\alpha'.$$

Then, for the hurtful work relatively to the useful work, we obtain

$$p' = \frac{W'}{U'} = \frac{f\int Q\,(ds' - ds_1')}{\int -\,Q\,R\sin\alpha'\,2\frac{r}{R}\,d\alpha'} = f\frac{\int ds' - ds_1'}{2\,r\cos\alpha'};$$

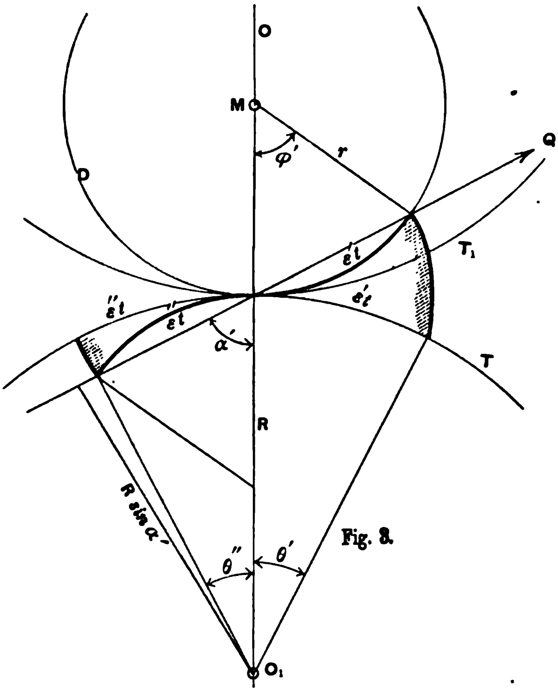


Fig. 3.

or, if the respective parts of the teeth which slide in contact are designated, as in Figure 4, we have, for an arc of approach,

$$(6) \quad . \; . \; . \; . \; . \quad p' = \frac{W'}{U'} = f\frac{s' - s_1'}{2\,r\cos\alpha'};$$

and for an arc of recess on the other side of the line of centers,

$$(7) \quad . \; . \; . \; . \; . \quad p'' = \frac{W''}{U''} = f\frac{s_1'' - s''}{2\,r\cos\alpha''};$$

and hence, for the total loss:

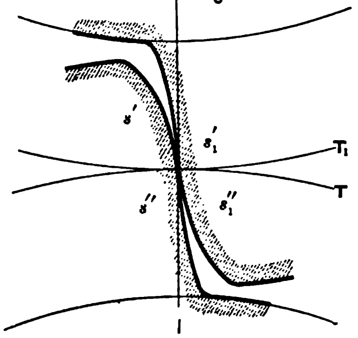
$$p=\frac{W}{U}=\frac{W'+W''}{U'+U''}=p'\frac{U'}{U'+U''}+p''\frac{U''}{U'+U''};$$

for which, by substitution of the values of U' from equation 4, and the corresponding value of U'', we can also put

$$p=p'\frac{\theta'}{\theta'+\theta''}+p''\frac{\theta''}{\theta'+\theta''}.$$

If the length of the whole arc of action measured on the pitch circle, on both sides of the line of centers, be denoted in terms of the circular pitch by ε*—equal to $\varepsilon'+\varepsilon''$—we have from the last equation and from Figure 3, since the arcs are proportional to the subtended angles,

Fig. 4.



(8) . . . . $p=p'\frac{\varepsilon'}{\varepsilon}+p''\frac{\varepsilon''}{\varepsilon}.$

Introducing into this equation the values of p' and p'' in (6) and (7), we have

$$p=f\left(\frac{s'-s_1'}{2r\cos\alpha'}\,\frac{\varepsilon'}{\varepsilon}+\frac{s_1''-s''}{2r\cos\alpha''}\,\frac{\varepsilon''}{\varepsilon}\right),$$

in which $2r\cos\alpha'$ and $2r\cos\alpha''$ are the chords of the two parts of the curve of contact on either side of the line of centers, OO'

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* See the author's *Konstrukteur*.

(see Figures 3 and 6). Designating the whole chord by S, and the parts respectively by S' and S'', we have

$$(9) \quad . \quad . \quad . \quad . \quad p = f\left(\frac{s' - s_1'}{S'}\frac{\varepsilon'}{\varepsilon} + \frac{s_1'' - s''}{S''}\frac{\varepsilon''}{\varepsilon}\right).$$

Here $\frac{\varepsilon'}{\varepsilon}$ and $\frac{\varepsilon''}{\varepsilon}$ are respectively the lineal ratios of the arc of approach and arc of recess to the whole arc of action on the pitch circle. But as these arcs, in epicycloidal gearing, which we are now considering, are of the same length as the corresponding parts of the curve of contact on the describing circles, the ratios are equally those of the two parts of the curve of contact to the whole curve. In formula (9) the value of the bracketed expression can be determined graphically, and when the quantities are found the value of p may be expressed in the following rule:

Find the difference in length between the two tooth-profiles which work in contact on the same side of the line of centers, and divide it by the chord of the respective curve of contact; multiply the quotient by the ratio between this curve and the whole curve of contact; add the quantities thus found for either side of the line of centers, and multiply the sum by the coefficient of friction to obtain the total loss of work, p.[*]

In Figure 6 the outside, or tip, circles[†] K and K_1 intersect the

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[*] We have in the above been proceeding on the assumption that only one pair of tooth-profiles is in gear at a time, whereas actually each pair is brought into engagement while the preceding pair is still in action. But this is as though we had, in Figure 5, for the pressure between the teeth on the curve of contact,

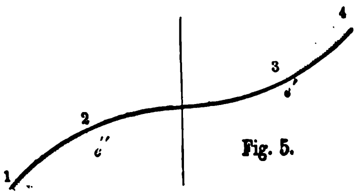

Fig. 5.

between points 1 and 2, $Q' = Q - Q''$
and, at the same time, " " 3 and 4, $Q'' = Q - Q'$;
or, taken together, $Q' + Q'' = Q$;

and hence our assumed case holds good in practice. To find points 2 and 3, make the portions 1-3 and 2-4 of the curve of contact equal to the pitch.

[†] In Brown & Sharpe's *Practical Treatise on Gearing*, the tip circle is denominated "addendum circle."

curve of contact in the points *I* and *II*, and so limit the line of

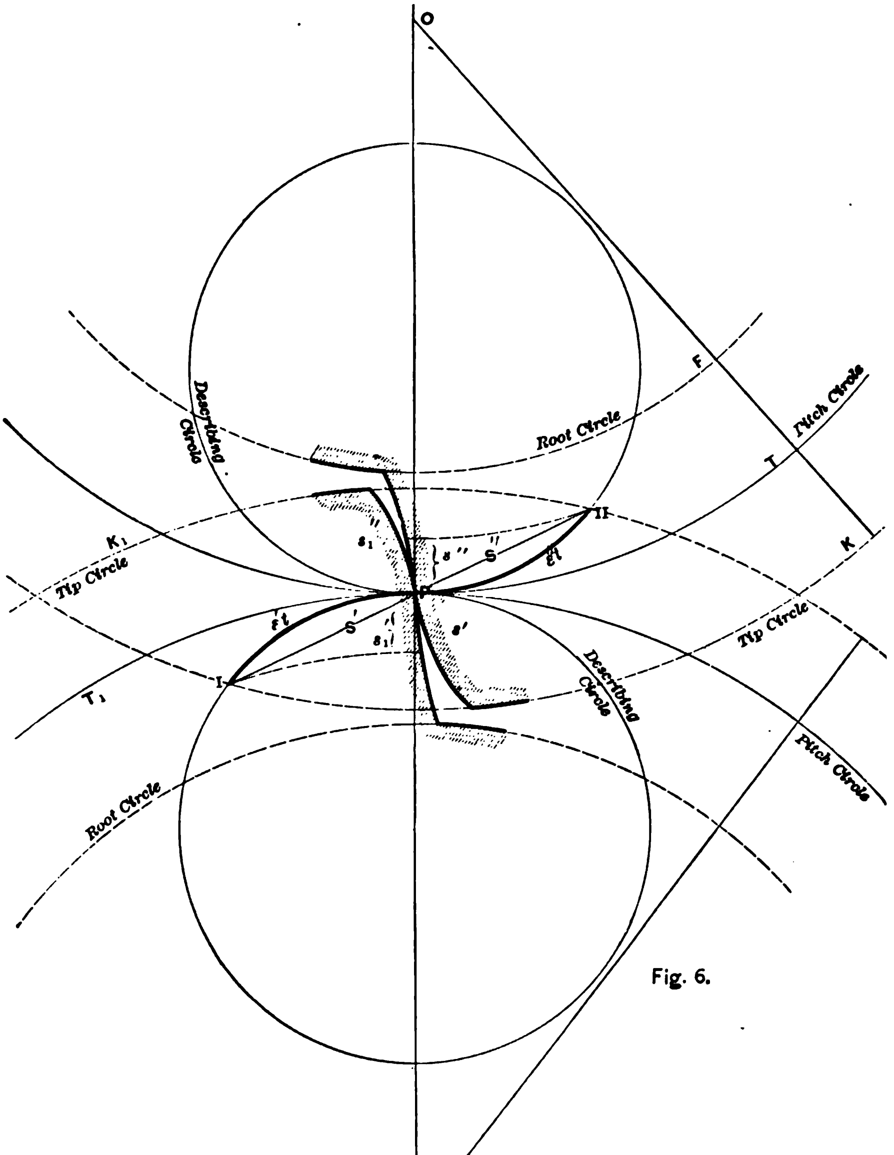

Fig. 6.

actual contact, which, divided by the circular pitch of the teeth, *t*, gives the value ε in terms of the pitch. Arcs described from the centers of the pitch circles, O and O_1, with OI and OII as radii, intersect the flanks of the teeth and determine the lengths of s_1' and s''. The arcs PI and PII, divided by the pitch of the teeth, t, give the value of the arcs of approach and recess, ε' and ε'' in terms of the pitch.

Analytical Investigation.—Retaining the same notation and referring to the former figures, we have, from the developments of analytical geometry for epicycloidal curves,

$$s' = 4r\left(1 - \cos\frac{\varphi'}{2}\right)\frac{R + r}{R} \text{ and } s_1' = 4r\left(1 - \cos\frac{\varphi''}{2}\right)\frac{R_1 - r}{R_1}.$$

Substituting these values in Equation 6:

$$\frac{p'}{f} = \frac{4r\left(1 - \cos\frac{\varphi'}{2}\right)}{2r\cos\alpha'}\left(\frac{R + r}{R} - \frac{R_1 - r}{R_1}\right);$$

or, since $r\cos\alpha' = 2r\sin\frac{\varphi'}{2}$,

$$\frac{p'}{f} = 2\,\frac{1 - \cos\frac{\varphi'}{2}}{\sin\frac{\varphi'}{2}}\left(\frac{RR_1 + R_1r - RR_1 + Rr}{RR_1}\right);$$

whence, by simple reduction,

$$(10)\quad .\ .\ .\ .\ .\quad \frac{p'}{f} = 2r\tan\frac{\varphi'}{4}\,\frac{R + R_1}{RR_1}.$$

But as angle φ' is small, we may consider $2r\tan\frac{\varphi'}{4} = \frac{2r\varphi'}{4} = r\frac{\varphi'}{2}$, and therefore

$$\frac{p'}{f} = r\frac{\varphi'}{2}\,\frac{R + R_1}{RR_1}.$$

Denoting the number of teeth in the two wheels in gear by Z and Z_1, and the circular pitch by t, we have $2\pi R = Zt$, and from Figure 3, $r\varphi' = \varepsilon' t$; introducing these two values into the last equation, we find

$$\frac{p'}{f} = \frac{\varepsilon' t}{2}\,\frac{(Z + Z_1)\frac{t}{2\pi}}{ZZ_1\left(\frac{t}{2\pi}\right)^2},$$

and reducing,

$$(11). \quad . \quad . \quad . \quad . \quad p' = f\pi\left(\frac{1}{Z} \pm \frac{1}{Z_1}\right)\varepsilon',$$

the negative sign within brackets being applied to internal, and the positive sign to outside gearing. For the total loss through friction we have, as in Equation 8,

$$p = p'\frac{\varepsilon'}{\varepsilon} + p''\frac{\varepsilon''}{\varepsilon},$$

or, multiplying the second member by $\frac{\varepsilon'}{\varepsilon'}$,

$$(12). \quad . \quad . \quad . \quad . \quad . \quad p = \frac{p'}{\varepsilon'}\left(\frac{\varepsilon'^2 + \frac{p''}{p'}\varepsilon'\varepsilon''}{\varepsilon}\right).$$

From Equation 8 and from the general value $p = p' + p''$, we deduce

$$\frac{p'}{p''} = \frac{\varepsilon'}{\varepsilon''}, \quad \text{or} \quad p'' = p'\frac{\varepsilon''}{\varepsilon'}.$$

Substituting this in (12),

$$(13). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad p = \frac{p'}{\varepsilon'}\left(\frac{\varepsilon'^2 + \varepsilon''^2}{\varepsilon}\right).$$

Introducing here the value of p' from (11) we obtain, finally,

$$(14). \quad . \quad . \quad . \quad . \quad . \quad p = f\pi\left(\frac{1}{Z} \pm \frac{1}{Z_1}\right)\frac{\varepsilon'^2 + \varepsilon''^2}{\varepsilon}.$$

The interpretation of this formula shows that a large value for ε, that is, a long contact, is, contrary to the practical impression received at first sight, unfavorable because the value of p increases with it; and also that the ratio $\varepsilon' : \varepsilon''$ exerts an important influence. To find a minimum value for p, we have the analogous general expression,

$$y = \frac{x^2 + (a - x)^2}{a}.$$

Differentiating:

$$0 = 2x - 2(a - x);$$

whence,

$$x = \tfrac{1}{2}a$$

or, in this particular case,

$$\varepsilon' = \varepsilon'' = \tfrac{1}{2}\varepsilon.$$

The most favorable case, therefore, will be that in which the contact is equally divided on either side of the line of centers OO'. This proportion has been so closely adhered to in epicycloidal gearing for standard wheels of my own design ($r = \tfrac{7}{8}t$, $k = 0.3t$, $f = 0.4t$),* that there is no appreciable inaccuracy in calling $\varepsilon' = \varepsilon'' = \tfrac{1}{2}\varepsilon$, and then we find for the lost work of friction,

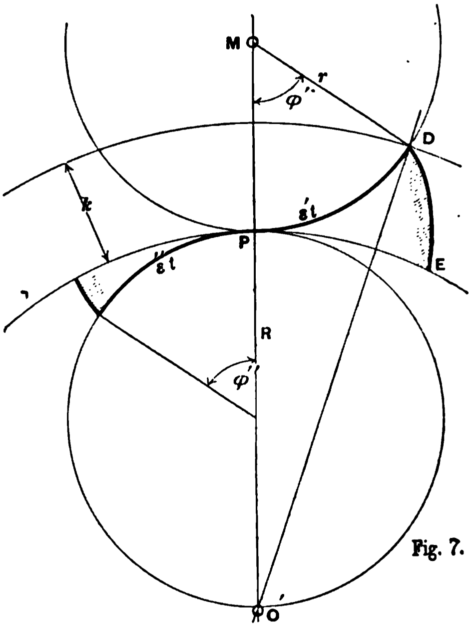

Fig. 7.

$$(15). \quad . \quad . \quad . \quad . \quad . \quad p = f\pi\left(\frac{1}{Z} \pm \frac{1}{Z_1}\right)\frac{\varepsilon}{2}.$$

Here, too, Z_1 denotes the number of teeth of the second gear, and is taken with the negative sign in cases of internal gearing,

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* See the author's *Konstrukteur*. Brown & Sharpe, in their treatise on gearing already referred to, make $k = \frac{t}{\pi} = 0.318t$, hence somewhat larger. The difference is trifling; the adoption of this value has certain practical merits.

showing that in such cases there is less friction than in ordinary spur gearing of the same number of teeth.

The value of ε in (15) is readily found by the graphic method, but an analytical determination is also possible, as follows:

The two outside, or tip, circles of a pair of engaging gear-wheels always intercept the shorter portions of the curve of contact and so determine the lengths of the two arcs of actual contact in this curve, these being, as above noted, equal in length to the corresponding arcs of approach and recess on the pitch circles, or, in Figure 7, $PD = PE$. But, from the figure,

$$(16) \quad . \quad \varepsilon = \varepsilon' + \varepsilon'' = \frac{r(\varphi' + \varphi'')}{t} = \frac{Z}{4\pi}(\varphi' + \varphi''),$$

in which $t = \dfrac{2\pi R}{Z}$ = the circular pitch, and $R = 2r$, so that the teeth have radial flanks. Again, to find a value for φ' we have from the triangle $O'MD$, $\overline{OM}^2 + \overline{MD}^2 - 2\overline{O'M} \cdot \overline{MD} \cos \varphi' = \overline{O'D}^2$.
or

$$\cos \varphi' = \frac{(R + r)^2 + r^2 - (R + k)^2}{2(R + r)r}$$

$$= \frac{R^2 + r^2 + 2Rr + r^2 - R^2 - k^2 - 2Rk}{2Rr + 2r^2}$$

$$(17) \quad . \quad . \quad . \quad . \quad = \frac{Rr - Rk + r^2 - \frac{1}{2}k^2.}{Rr + r^2},$$

Introducing into (17) the values $R = \dfrac{Zt}{2\pi}$, $r = \dfrac{Zt}{4\pi}$, $Z = 11$, $k = 0.3t$, and substituting in (16) the value of $\cos \varphi'$ thus found, we have, approximately

$$(18) \quad \varepsilon = \tfrac{7}{8}\left(\text{arc cos}\,\frac{1.27Z + 10}{2Z + 11} + \text{arc cos}\,\frac{1.27Z \pm 10}{2Z_1 \pm 11}\right),$$

the negative signs being used when Z_1 belongs to an internal gear-wheel. Formula (18) can very well be approximated for all values from $Z \geqq 6$ upwards, as follows:

a), for spur-wheels, $\varepsilon = 1.246 + 0.00048(Z + Z_1)$.

b), when Z_1 refers to an internal gear, $\varepsilon = 1.431 + 0.00048Z - 0.0001Z_1$.

c), for a rack and pinion gear, $\varepsilon = 1.396 + 0.00048Z$.

We obtain further, for

$Z = Z_1 = 7$, the value $\varepsilon = 1.25$
$Z = 7$, $Z_1 = \infty$, " " $\varepsilon = 1.40$
$Z = 7$, $Z_1 = -60$, " " $\varepsilon = 1.43$
$Z = 60$, $Z_1 = -300$, " " $\varepsilon = 1.43$

We are now enabled, as in the following examples, to find numerical values for the loss, p.

Example 1.—Number of teeth, $Z = 7$ and $Z_1 = 7$; coefficient of friction 0.15 (introduced here with rather a high value on account of the narrowness of the surfaces in contact). We have for this case, as above shown, $\varepsilon = 1.25$; and hence, from formula (15): $p = 0.15 \cdot 3.14 \left(\frac{1}{7} + \frac{1}{7}\right) \frac{1.25}{2} = 0.0628 \cdot 3.14 = 0.08419$, or 8.4 per cent.

Example 2.—$Z = 7$, $Z_1 = \infty$ (rack). Here ε is $= 1.40$, and $p = 0.15 \cdot 3.14 \left(\frac{1}{7} + 0\right) \frac{1.40}{2} = 0.015 \cdot 3.14 = 0.047$, or 4.7 per cent.

Example 3.—$Z = 28$, $Z_1 = \infty$, whence $\varepsilon = 1.396 + 0.00048 = 1.41$, and $p = 0.15 \cdot 3.14 \left(\frac{1}{28} + 0\right) \frac{1.41}{2} = 0.0038 \cdot 3.14 = 0.0119$, or 1.19 per cent.

Example 4.—$Z = 30$, $Z_1 = 30$. We find $\varepsilon = 1.346 + 0.00048 (30 + 30) = 1.275$, and then $p = 0.15 \cdot 3.14 \left(\frac{1}{30} + \frac{1}{30}\right) \frac{1.275}{2} = 0.006375 \cdot 3.14 = 0.02$, or 2 per cent.

Example 5.—$Z = 30$, $Z_1 = -40$ (annular wheel). We find $\varepsilon = 1.431 + 0.00048 \cdot 30 - 0.0001 \cdot 40 = 1.44$, and $p = 0.15 \cdot 3.14 \left(\frac{1}{30} - \frac{1}{40}\right) \frac{1.44}{2} = 0.0009 \cdot 3.14 = 0.0028$, or 0.28 per cent.

INVOLUTE GEARING.

Geometrical Investigation.—We have here, as in equations (3) and (4), for the work of friction and the useful work respectively,

$$W' = f\int Q\,(ds - ds_1), \text{ and } U' = \int QR \sin \alpha' d\theta'.$$

In the present case, however, α' is a constant, so that $U' = QR \sin \alpha'\theta'$, and

$$(19) \quad \ldots\ldots \quad p' = \frac{W'}{U'} = f\,\frac{\int ds' - \int ds_1'}{R \sin \alpha'\theta'}.$$

But $R \sin \alpha'\theta'$ is the arc of recess measured on the root circle r, and hence, from the property of the involute, it is equal to the portion E', Figure 8, of the curve, or path, of contact, coinciding here with the straight line of pressure BQ. Equation (19) therefore becomes

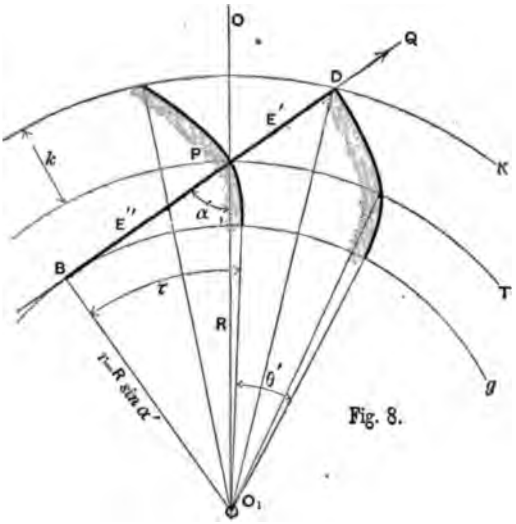

Fig. 8.

$$p' = f\frac{s' - s_1'}{E'};$$

and, similarly,

$$p'' = f\frac{s_1'' - s''}{E''}.$$

These expressions correspond with formulæ (6) and (7)—the chords of the curve of contact, $2r \cos \alpha'$ and $2r \cos \alpha''$, of the former case being identical, in the latter, with the line of contact itself. But again, as in Equation 8,

$$p = p'\frac{\varepsilon'}{\varepsilon} + p''\frac{\varepsilon''}{\varepsilon}.$$

Substituting in this the above values of p' and p'',

$$p = f\left(\frac{s' - s_1'}{E'}\frac{\varepsilon'}{\varepsilon} + \frac{s_1'' - s''}{E''}\frac{\varepsilon''}{\varepsilon}\right).$$

Here, as before, ε' and ε'' are the parts, and ε the whole, of the curve, or line, of contact expressed in terms of the pitch. If, therefore, t_0 denote the root-pitch (measured on the root circle r), then

$$\frac{E'}{t_0} = \varepsilon', \quad \frac{E''}{t_0} = \varepsilon'', \quad \text{and} \quad \frac{E' + E''}{t_0} = \varepsilon.$$

Introducing these values of ε, ε' and ε'' into the last equation, we obtain

$$p = f\left(\frac{s' - s_1'}{E' + E''} + \frac{s_1'' - s''}{E' + E''}\right) = f\,\frac{(s' + s_1'') - (s'' + s_1')}{E}.$$

An easy graphic solution of this expression results in the following rule:

Add together the lengths of the two faces (s' *and* s_1'', *Figure* 9), *and also the lengths of the active portions of the two flanks* (s'' *and* s_1', *Figure* 9); *subtract the second sum from the first, and divide the remainder by the length of the line of contact* ($I - II = E$); *multiply the quotient by the coefficient of friction*, f, *and the product will be the total loss*, p.*

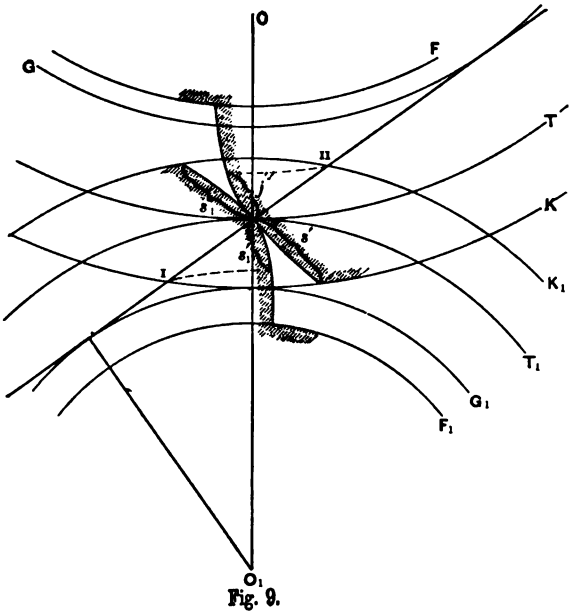

Fig. 9.

Analytical Investigation.—If τ and θ' denote the angles of ap-

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* To find the values of s_1', s'', E, etc., set a pair of compasses to quite a small opening—say $\frac{1}{8}$ to $\frac{1}{16}$ inch—and space off the curves which are to be rectified, keeping count of the number of spaces measured.

proach and recess, both taken with reference to the root circle, as represented in Figure 8, then we may put for Equation (19):

$$\frac{p'}{f} = \frac{\int_{\tau}^{\tau+\theta} r\theta' d\theta' - \int_{\tau_1 - \theta_1}^{\tau_1} r_1\theta_1' d\theta_1'}{R \sin \alpha'\theta'}.$$

Integrating, and substituting for R sin α' its value r, we have

$$\frac{p'}{f} = \frac{\frac{r}{2}(\theta'^2 + \tau^2 - 2\theta'\tau - \tau^2) - \frac{r_1}{2}(\tau_1^2 - \tau_1^2 - \theta_1'^2 + 2\tau_1\theta_1')}{r\theta'}.$$

Reducing:

$$(20) \quad . \quad . \quad . \quad \frac{p'}{f} = \frac{\frac{r}{2}\theta'^2 + r\tau\theta' + \frac{r}{2}\theta'^2_1 - r_1\tau_1\theta_1}{r\theta'}.$$

But $r\theta' = r_1\theta_1'$, both being equal to E' (Figure 8); and $\tau = \tau_1$, both being equal to cot α' (because arc $r\tau = E'' = R \cos \alpha'$, and $r = R \sin \alpha'$, whence $\tau = \frac{\cos \alpha'}{\sin \alpha'} = \cot \alpha'$).

Substituting in (20):

$$\frac{p'}{f} = \frac{r}{2}\theta' \frac{\left(1 + \frac{\theta_1'}{\theta'}\right)}{r} = \frac{r}{2}\theta' \frac{\left(1 + \frac{r}{r_1}\right)}{r} = \frac{r}{2}\theta' \frac{r + r_1}{rr_1}.$$

Replacing r by its value R sin α':

$$\frac{p'}{f} = \frac{R \sin \alpha'\theta'}{2} \frac{R + R_1}{RR_1} \frac{\sin \alpha'}{\sin \alpha'^2} = \frac{R + R_1}{RR_1} \cdot \frac{R\theta'}{2}.$$

Here, as before, $R\theta'$, the arc of recess measured on the pitch circle, is equal to $\varepsilon' t$, so that

$$\frac{p'}{f} = \frac{R + R_1}{RR_1} \cdot \frac{\varepsilon' t}{2};$$

from which, reducing as for equation (11), Z and Z_1 denoting the numbers of teeth of the two wheels,

$$(21) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad p' = f\pi \left(\frac{1}{Z} \pm \frac{1}{Z_1}\right) \varepsilon';$$

and finally, deducing p as in Equations 12, 13, and 14, we find

$$(22) \quad . \quad . \quad . \quad . \quad p = p' + p'' = f\pi\left(\frac{1}{Z} \pm \frac{1}{Z_1}\right)\frac{\varepsilon'^2 + \varepsilon''^2}{\varepsilon},$$

which is identical with the value of p for epicycloidal wheels—the negative sign and number Z_1 referring to internal gearing.

As in the case of epicycloidal gear the values of ε' and ε'' in involute gearing can be analytically deduced as follows:

The two tip circles of a pair of engaging gear-wheels limit by intersection with the describing circles the length of the active portions of the curve of contact now considered. We have, as before,

$$\varepsilon = \frac{R\theta}{t} + \frac{R_1\theta_1}{t} = \varepsilon' + \varepsilon''.$$

To find a value for ε' we have from the triangle $O'PD$, Figure 10,

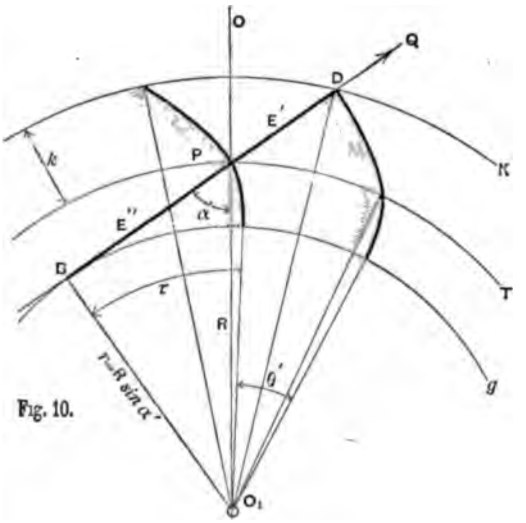

Fig. 10.

$$(R + k)^2 = R^2 + E'^2 + 2E'E'' = R^2 + R^2\theta^2 \sin\alpha^2 + 2R^2\theta \sin\alpha \cos\alpha,$$

from which we deduce

$$\varepsilon' = \frac{-Z\cot\alpha}{2\pi} \pm \sqrt{\frac{1}{\sin\alpha^2}\cdot\frac{1}{\pi^2}\left(\frac{Z}{2} + 1\right) - \frac{Z^2}{4\pi^2}};$$

or

$$\varepsilon' = \frac{-Z\cot\alpha}{2\pi} \pm \sqrt{Z^2\left(\frac{1}{4\pi^2\sin\alpha^2} - \frac{1}{4\pi^2}\right) + \frac{1}{\sin\alpha^2\pi^2}\left(Z + 1\right)};$$

and for the particular value $\alpha = 75°$, whence $\sin \alpha = 0.96593$ and $\cot \alpha = 0.26795$,

$$(23) \quad . \quad . \quad \varepsilon' = -0.043Z + \sqrt{0.0018Z^2 + 0.108\,(Z+1)}.$$

This formula applies to outside spur gearing; for practical purposes it is approximated with sufficient accuracy in the expressions:.

$\varepsilon' = 0.8 + 0.0036Z$, $\varepsilon'' = 0.8 + 0.0036Z_1$, and $\varepsilon = \varepsilon' + \varepsilon'' = 1.6 + 0.0036\,(Z + Z_1)$; and for the rack, $\varepsilon' = 1.20$.

Example 1.—In a pair of gear-wheels assume $Z = 20$, and $Z_1 = 100$:

$$\begin{aligned}\varepsilon = \varepsilon' + \varepsilon'' &= 0.8 + 0.0036 \cdot 20 + 0.8 + 0.0036 \cdot 100 \\ &= 1.6 + 0.0036 \cdot 180 = 1.6 + 0.331 = 1.932.\end{aligned}$$

Example 2.—Assume $Z = 20$, $Z_1 = 20$:

$$\varepsilon = 1.6 + 0.0036 \cdot 40 = 1.744.$$

Example 3.—Assume $Z = 28$, $Z_1 = \infty$:

$$\varepsilon = 0.8 + 1.20 + 0.0036 \cdot 28 = 2.0 + 0.09 \doteq 2.09.$$

When Z_1 refers to an internally geared wheel, Equation (23) becomes

$$\varepsilon_1' = 0.043Z_1 - \sqrt{0.0018Z_1^2 - 0.108(Z-1)},$$

which yields imaginary values for all cases in which the number of teeth in the second member is less than 59. The formula is approximated with sufficient practical accuracy in the expression

$$\varepsilon_1' = 1.7 - \frac{Z_1}{1000}.$$

Example 1.—Assume $Z_1 = -100$: then $\varepsilon_1' = 1.7 - 0.1 = 1.6$.

Example 2.—Assume $Z_1 = -200$: then $\varepsilon_1' = 1.7 = 0.2 = 1.5$.

In usual cases ε' differs appreciably from ε'', and consequently the mean value of the factor $\dfrac{\varepsilon'^2 + \varepsilon''^2}{\varepsilon}$ is greater than $\dfrac{\varepsilon}{2}$. For example, for

$$\begin{aligned}&Z = 20 \text{ and } Z_1 = +200,\ \varepsilon' = 0.8 + 0.0036 \cdot 20 = 0.872, \\ &\varepsilon'' = 0.8 + 0.0036 \cdot 200 = 1.52; \text{ and} \\ &\varepsilon = 0.872 + 1.52 = 2.392; \text{ and hence the factor}\end{aligned}$$

$$\left[\left(\frac{\varepsilon'}{\varepsilon}\right)^2 + \left(\frac{\varepsilon''}{\varepsilon}\right)\right]\varepsilon = \left[\left(\frac{0.872}{2.392}\right)^2 + \left(\frac{1.52}{2.392}\right)^2\right]\varepsilon =$$
$$(0.36^2 + 0.64^2)\,\varepsilon = (0.1296 + 0.4096)\,\varepsilon = 0.5392\,\varepsilon,$$

which is a little greater than $\frac{1}{2}\varepsilon$.* We may therefore put for formula (22),

$$p = f\pi\left(\frac{1}{Z} \pm \frac{1}{Z_1}\right)\frac{\varepsilon}{2},$$

always bearing in mind, however, that in involute gearing ε is generally greater than in epicycloidal gearing of the same number of teeth, and that in the approximation which we adopted, the value $\frac{\varepsilon}{2}$ is for most cases a trifle too small.

It will not be uninteresting to examine a few numerical applications of the last formula, as this will enable us to make a comparison between the two systems of gearing as regards friction. Supposing the true involute curves applied, we must employ numbers of teeth greater than 19 and 28 respectively, as stated above.

Example 1.—$Z = 30$, $Z_1 = 30$. This gives $\varepsilon = 1.6 + 0.0036\,(30 + 30) = 1.816$, and $p = 0.15 \cdot 3.14\left(\frac{1}{30} + \frac{1}{30}\right)\frac{1.816}{2} = 0.00908 \cdot 3.14 = 0.0285$, or 2.85 per cent. (as against 2 per cent. in epicycloidal gearing).

Example 2.—$Z = 28$, $Z_1 = \infty$ (rack). ε, as given above, $= 2.09$, which makes $p = 0.15 \cdot 3.14\left(\frac{1}{28} + 0\right)\frac{2.09}{2} = 0.0056 \cdot 3.14 = 0.0175$, or 1.75 per cent. (as against 1.19 per cent. in epicycloidal gearing).

Example 3.—$Z = 30$, $Z_1 = -40$; then $\varepsilon = 0.8 + 1.7 + 0.0036 \cdot 30 - 0.001 \cdot 40 = 2.5 + 0.068 = 2.568$, and $p = 0.15 \cdot 3.14\left(\frac{1}{30} - \frac{1}{40}\right)\frac{2.568}{2} = 0.0016 \cdot 3.14 = 0.005$, or 0.5 per cent. (as against 0.28 per cent. in epicycloidal gearing).

If we give up the accurate involute curves for wheels under 20 and 23 teeth respectively, and apply a sufficient correction to the faces, we will find a greater value for ε than in epicycloidal gearing, and therefore greater friction. Many engineers imagine that the reverse should be the case.

* * * * * * *

The action, or contact, in involute gearing is not always good,

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* In my *Konstrukteur* I have taken $\frac{3}{4}\varepsilon$ as the mean value of this factor, which is, therefore, too large.

for with certain proportions between the numbers of the teeth in two engaging gears, a failure of action will occur. This takes place when that portion of the curve of contact which corresponds with the flank of a tooth is shorter at its maximum than the portion corresponding with the face of the tooth of the engaging gear. In such a case, the action, while geometrically correct, is physically impracticable. For example, the tooth F_1IK_1 of

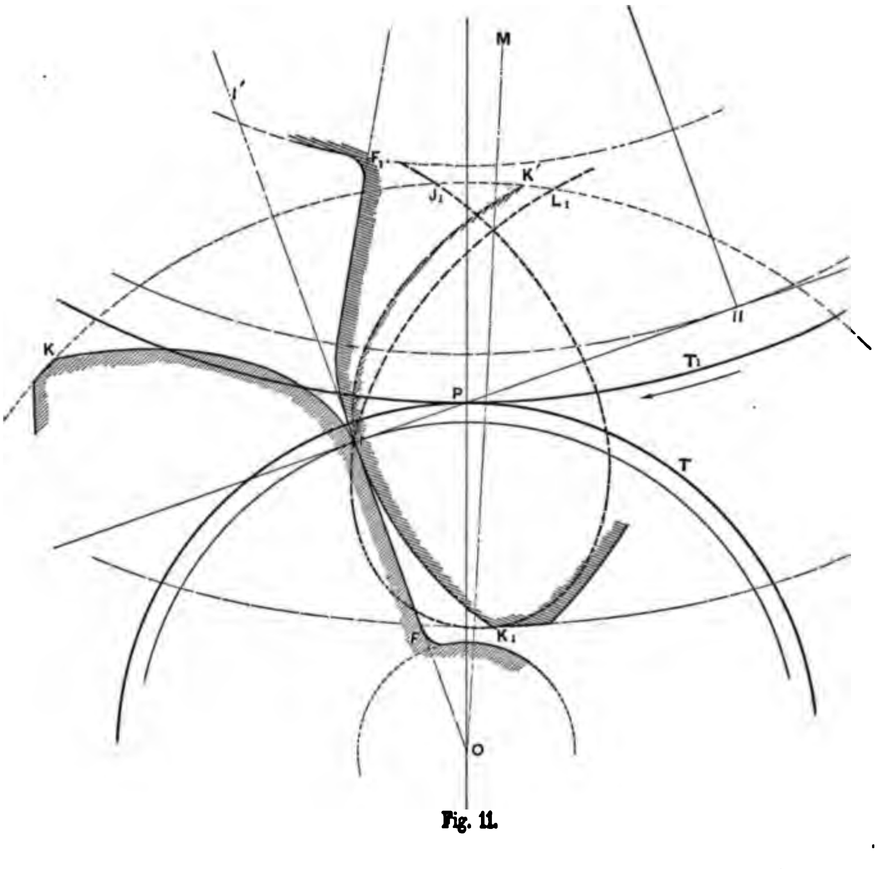

Fig. 11.

wheel T_1 is shown in Fig. 11; just in the position where the inner end I of the flank on wheel T has been reached by a point of the face IK_1. The gearing moving in the direction of the arrow, point K_1 will describe an elongated epicycloid curve, whose loop $J_1K_1L_1$ is seen in the figure. This curve enters the radial flank of tooth FIK, showing therefore that an interference will take place between the teeth. Gear-wheels made with such proportions must strike and wear each other at the points of interference. Practice sustains this deduction, for in all cases where pinions with a small number of involute teeth—say 12 to 15—are geared into large spur-wheels, a jog is soon worn into the teeth of the pinion just inside of the pitch circle (Fig. 12), and this wear is solely due to the undesired action of the profiles, even though they be cut with ever so much accuracy. The objectionable interference of the curves, as we observe it here, seems to be in exact contradiction to the geometrical correctness of the system, but really this is not so. It should not be forgotten that the involute consists of two symmetrical branches, one of which is the face IK; the second one IK' is left unexecuted; it lies symmetrically disposed to axis OII' opposite the branch IK. It is with this second branch of the involute that the part IK_1 of the face of tooth F_1K_1 would come into true geometrical contact; this, however, does not preclude that the lower portion of the first branch of the involute could at the same time be intersected by IK_1. In the case before us, the loop enters the flank so far that the evil cannot be remedied by hollowing out the face along the line of the loop curve, as this curve partly cuts away—so to say—that short but important portion of the involute between the pitch and the root circle, which cannot be dispensed with for the contact between points P and I. Every precaution should, therefore, be observed to avoid the interference in question.

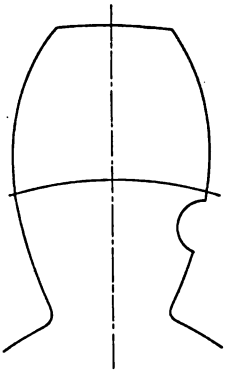

Fig. 12.

The number of teeth required in order to avoid this difficulty in the smallest involute gear-wheels—those having a root circle angle α of 75°—is *twenty* when both wheels have the same number of teeth; but it increases to *twenty-eight* when the engaging gear is a rack.[*] In order to prevent this failing case it would appear necessary to reduce the angle α of the root circle to less than 75°.[†] But other evils accrue from this reduction, and mainly that of greater axial pressure, so that it has not yet been adopted

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[*] See *Konstrukteur.*

[†] Brown & Sharpe, of Providence, R. I., make α slightly $> 75°$, namely $75\frac{1}{2}°$, apparently with the intention of having $\cos \alpha = \frac{1}{4}$ and so facilitating the designing of the root circle. On the other hand, a 75° angle is so easily drawn with the aid of a 30° and a 45° triangle that it seems preferable to maintain this value of α, which, moreover, was the one adopted by the late Prof. Willis.

in practice. The occurrence of this failing case is to be noted even with the use of Bilgram's excellent gear cutter. This tool is particularly well suited to cutting very accurate involute profiles, but, as explained above, the wheels should never have less than twenty teeth. Another device for guarding against the failing case consists in slightly rounding off the face profiles on wheel PK_1. The proper amount of this rounding off is readily determined by drawing the different positions which the flank of a tooth on wheel PK will assume when its pitch circle J is rolled upon T_1.* This curve can also be investigated theoretically—it is, for example, a cycloid when the tooth PK_1 belongs to a rack. But at best this remedy for the failing case does not appear to be really commendable, for its adoption implies that the principle of "sets of wheels" has been abandoned in such cases—the correction being in the first place, not constant as dependent on the number of teeth of wheel PK, and secondly, not necessary, and even positively incorrect, as soon as the number of teeth of PK is greater than *twenty-eight*.

LANTERN GEARING.†

For this case we have $\varepsilon'' = 0$, and $\varepsilon' = \varepsilon$, and hence $\dfrac{\varepsilon'^2 + \varepsilon''^2}{\varepsilon} = \varepsilon$, so that the expression for the lost work of friction, derived from formula (14), becomes

$$p = f\pi \left(\frac{1}{Z} \pm \frac{1}{Z_1}\right) \varepsilon,$$

which is twice as much for the same ε as in the cases above mentioned.

BEVEL GEARING.

In bevel gearing friction can be determined by the given formulæ, when introducing, instead of the real numbers of teeth, those

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* In the very useful *Practical Treatise on Gearing*, by Brown & Sharpe, already referred to, attention is directed to the failing case which is here considered, but its cause is ascribed to wide departure of circular arcs in these gears from the true involute curve, this departure being so great that points of teeth in the one wheel obtain no bearing on the flanks of teeth in the other. That such is not the true case nor the proper explanation has been shown above, and besides, in their own book (p. 21, Fig. 10), Messrs. Brown and Sharpe indicate clearly, and quite correctly, the intersection of the ends of the tooth faces of a rack with the tooth flanks of a pinion. The failing case in question is an unfortunate element in involute gearing, especially where wheels with only a small number of teeth are prescribed.

† See the author's *Konstrukteur*, 4th Ed., p. 534.

of what may be called the auxiliary wheels, which are wheels whose radii are the lines drawn from the point of contact of outside pitch circles perpendicular to the conical elements, and reaching the axes of the wheels respectively. The auxiliary wheels are always greater than the real wheels. For a crown wheel,* for instance, the radius of the auxiliary wheel becomes parallel to the axis, and therefore infinite, this auxiliary wheel corresponding to the rack.

WEAR OF TEETH.

The wear of tooth surfaces is intimately connected with the existence of friction, but not infrequently the question is treated in a misleading way. A common error regarding involute gear, and one which is firmly rooted in the minds of many engineers, consists in the belief that, because of the uniformity of the pressure Q, the wear at all points of the tooth surface will be the same, and that therefore the form of the profile will not be altered by abrasion. *This is altogether erroneous*, for the wear depends both upon the pressure per unit of surface and on the amount of sliding at each point. Owing to the curved profile of the tooth the problem of determining the pressure for each square inch becomes very complicated, and its solution can only be approximated by mean values. Setting aside, therefore, this part of the

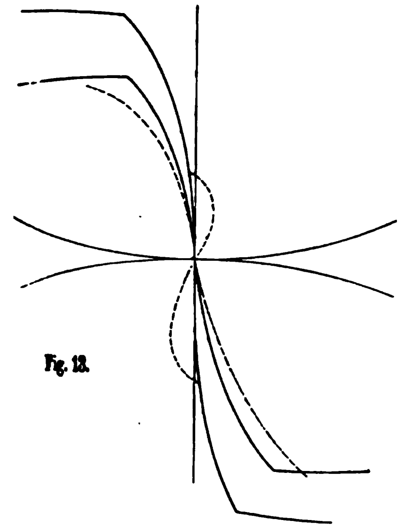
Fig. 13.

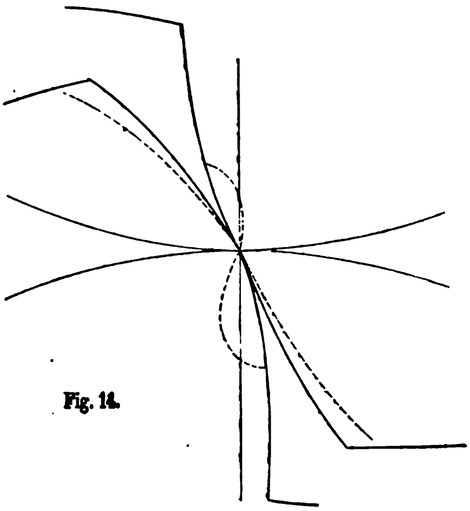
Fig. 14.

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* See *Konstrukteur*, 4th Ed., p. 546.

question, there remains the amount of sliding, which, as we found in (1) is

$$dg = ds - ds_1.$$

But in all kinds of gearing, without exception, ds and ds_1, are reduced to 0 at the pitch point,* so that theoretically the teeth would be subject to no wear at the pitch point. If they do, nevertheless, suffer a small amount of wear at that point, it is due to the abrasion of particles of metal very near the pitch point, and to a certain amount of shaking and springing of the axles, which always produces a slight displacement of the pitch circles.

The greatest amount of sliding corresponds with a maximum value of the angle θ—that is, it occurs at the beginning and end of contact (assuming the approach and recess of the teeth to take place on the two sides of the line of centers through the axes). Hence *the terminal points of the active profiles are subject to the greatest wear.* The actual amount of wear, *i. e.*, the thickness of the film of material abraded in a unit of time, depends geometrically upon the lengths of ds and ds_1, or taken for one pair of teeth, upon the lengths s' and s_1', s'' and s_1'' (Fig. 6), of the active profiles. A long face, s', slides upon the corresponding short flank s_1', and s_1'' upon s''. The short portion is subject to as much work of friction as the longer part, so that its mean wear must be greater, since the quantity of abraded particles, both teeth being of the same material, is the same in both parts. The effect of friction, therefore, in involute and epicycloidal gearing will be represented by the wear roughly indicated in dotted lines in Figures 13 and 14. The most unfavorable result is generally obtained in involute gear (Fig. 14), which is just the reverse of the erroneous impression above alluded to, because ε, and hence $\frac{ds_1}{ds}$, is generally larger, and $\frac{s'}{s'}$ and $\frac{s''}{s_1''}$ are smaller than in epicycloidal gear of the same number of teeth. This conclusion has always been sustained by my observations in practice, and I entertain no doubt but that others will confirm it.

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* In the case of certain kinds of gearing, which I have called shield gearing (*Konstrukteur*, 4th Ed., p. 586), the pitch point does not fall on the tooth profile.

DISCUSSION.

Mr. Wilfred Lewis.—The paper now under discussion treats of a subject to which I have recently given considerable study, and while, to some extent, it has been gratifying to find my researches substantially confirmed by such high authority, there are nevertheless a few important points wherein our views and conclusions differ, and to these I would ask your attention.

The first difficulty which I met with, in reading the paper, occurs in the formula immediately preceding equation (8),

$$p = p' \frac{\theta'}{\theta' + \theta''} + p'' \frac{\theta''}{\theta' + \theta''} \quad . \quad . \quad . \quad . \quad . \quad (a)$$

Equation (a) implies that the useful work done during the arcs of approach and recess is directly proportional to the length of those arcs respectively, and this, I think, cannot be granted upon the assumption that the normal pressure Q between the teeth is constant. It is, I admit, a close approximation, but it would seem to be more properly stated as such. Upon the assumption that the moment of resistance, $QR \sin \alpha$, is constant, the useful work must be proportional to the angle θ, but when Q is assumed to be constant, the useful work U' has just been demonstrated by equation (6) as proportional to $2r \cos \alpha'$, that is, to the chord of the describing circle, and not to its arc. The difference must be very slight indeed, but nevertheless, it is just the difference between an exact and an approximate solution of the problem. The "analytical investigation" of the same case does not pretend to be absolutely correct, when, for example, a small angle is taken for its tangent, and, although this is perfectly allowable as a close approximation, it could not be accepted if presented as literal truth.

Continuing the paper, I was somewhat surprised to find that the value of p, as deduced for cycloidal teeth and expressed by equation (14), was stated as identical with the value of p for involute teeth, as expressed by equation (22); and reflecting upon the peculiarities just mentioned, I was led to conclude that the identity of the two equations was due to the fact that one was derived by exact and the other by approximate methods. That a small difference does actually exist in favor of the involute form, has been shown by Mr. George B. Grant,* but for other purposes, which

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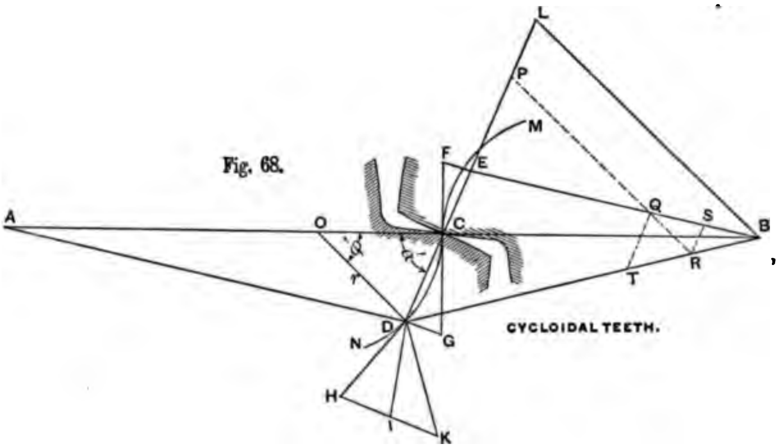
* *American Machinist*, for December 26, 1885.

will presently appear, I prefer to use the following demonstration of my own.

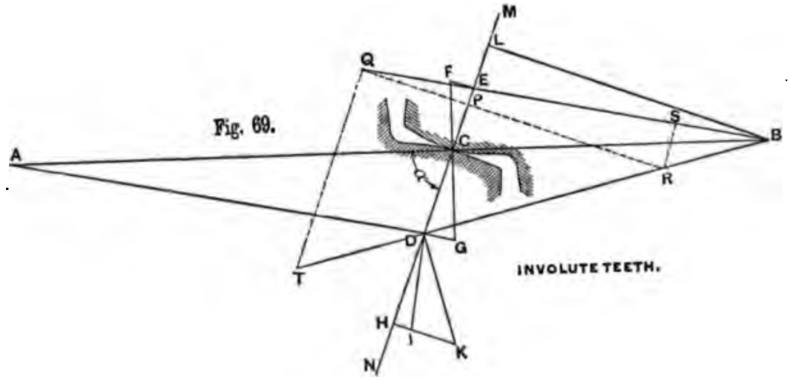
Referring to Fig. 68 for cycloidal teeth, and to Fig. 69 for involutes, let *A* and *B* be the centers of two wheels gearing together,

Fig. 68.



and *MCN* their path of contact. Let *D* be any point in the path of contact taken in each figure for the same arc of action from the pitch point *C*. Draw *AD* and *BD* and make *DI* and *DK* perpendicular to these lines respectively. Draw also, at a convenient distance from *D*, parallel to the surfaces in contact, and conse-

Fig. 69.



quently normal to the line of connection *DC*, the line *HK*, intersecting the perpendiculars to *AD* and *BD* in the points *I* and *K*. Then the instantaneous velocity of the point *D* may be represented in magnitude and direction by *DI* in the wheel *A*, and by *DK* in the wheel *B*, and *IK* will then be the velocity of sliding.

If, now, we prolong *DC* and draw *BE* parallel to *AD*, it is evi dent that the triangles DIK and BED will be similar, and that DE will represent the velocity of sliding in terms of the velocity at pitch line, represented by BC. We have now a construction for the velocity of sliding at any point in the path of contact, expressed in terms of the constant BC; and taking the product of sliding and pressure as proportional to the work lost in friction, we can readily find this loss upon the assumption that the moment of resistance is constant. This assumption is preferred to that of a constant pressure Q, taken by Professor Reuleaux, because it agrees more nearly with practical conditions, and therefore seems to give a more natural basis of comparison, although of course, for analytical purposes, either is correct. It is also thought that by this method of analysis the labor involved is very much reduced. When the moment of resistance is constant, the pressure at any point D becomes proportional to cosec α, and, if we draw FG perpendicular to the line of centers, and project DE upon it by normals to DE, we shall have FG proportional to the product of sliding and pressure. Now, by the assumed conditions, the arc of action measured by the curve CD, Fig. 68, must be equal to the arc of action measured by the straight line CG, Fig. 69, and therefore it appears that the loss in friction at the point D for cycloidal teeth is to the corresponding loss for involutes as CG, Fig. 68, is to the arc CD. Hence, it is evident that for any point D at a finite distance from C, the loss in friction for cycloidal teeth must be greater than the corresponding loss for involutes.

It further appears in Fig. 69 that CG, the arc of action, is always proportional to the product of sliding and pressure for any degree of obliquity; from which it follows that the loss in friction is independent of the obliquity, as implied in equation (22), and as found by Mr. Grant in the article mentioned.

Referring again to Fig. 68, we have

$$\text{arc } CD = \varphi' r, \qquad \text{and } CG = 2r \tan \frac{\varphi'}{2},$$

whence,

$$\frac{CG}{\text{arc } CD} = \frac{\tan \frac{\varphi'}{2}}{\frac{\varphi'}{2}};$$

or we may put

$$\frac{CG}{\text{arc } CD} = \frac{\cot \alpha'}{\frac{\pi}{2} - \alpha'}.$$

In order to represent graphically the actual loss in friction for cycloidal and involute teeth respectively, let us take *dce*, Fig. 70, to represent the arc of contact, *c* being the pitch point. Then, at the point *d*, erect the perpendiculars *da* and *db* equal to *FG* in Figs. 68 and 69 respectively, and similarly find the points *g* and *f* on the opposite side of the line of centers. Draw the straight line *bcg*, then will the area *cdb* + *ceg* represent the loss in friction for involute teeth. Draw also the curved line *acf* tangent to *bcg* at the point *c*, then will the area *cad* + *cef* represent the loss in friction for cycloidal teeth, and the difference will be represented by the area *abc* + *cgf*.

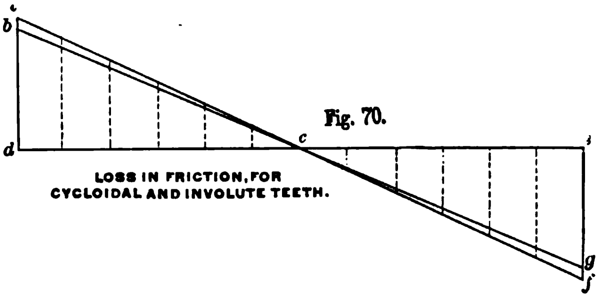


Fig. 70.

If we denote abscissas from the point *c* in terms of φ', the equation for the curved line *acf* becomes simply

$$y = a \tan \frac{\varphi'}{2}, \quad \ldots \ldots \ldots \quad (b)$$

in which *a* is a constant depending on the scale used.

It will be observed, in the analysis which I have briefly indicated, that the necessity for finding the face lengths in action has been overcome, and that the problem has thus been shorn of a needless and difficult feature.

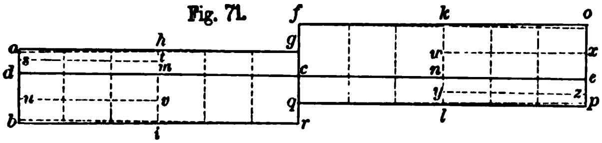
It must not be supposed, from Fig. 70, that the efficiency of involute gearing is always better than that of cycloidal, and the qualification, *for the same arc of action*, must not be overlooked.

The unfavorable comparison for involute teeth as against cycloidal is due, as stated in the paper, to differences in the length of the arc of action, and not to any peculiar merit in the cycloidal form of tooth.

In regard to the wear of teeth, I think there is a grave error in the assertion on the next to the last page, that in all kinds of gearing, without exception, the wear is theoretically reduced to zero at the pitch point; and I propose to show that the form of tooth has a great deal to do with the distribution of wear at the pitch point, as well as along the faces and flanks of the teeth.

In Fig. 68, let O be the center of the describing circle; draw OD the radius, and DH perpendicular to OD. Then, in the triangle DHK, we have IK proportional to the sliding, HK proportional to the surface upon which sliding takes place on the wheel B, and HI proportional to the surface upon which it takes place on the wheel A. If now we assume that the intensity of wear on any surface is proportional to the loss in friction divided by the surface sustaining such loss, we can readily construct a diagram upon the arc of contact for a base line, to represent the intensity of wear.

Fig. 71.



INTENSITY OF WEAR ON CYCLOIDAL TEETH, FIG. A.

Fig. 71 represents the intensity of wear on the teeth shown in Fig. 68. That portion above the base line *dce* refers to the wheel B, and the portion below to the wheel A. When the pressure Q is assumed to be constant, the wear will be uniform over the face and over the flank of each tooth, as shown by the parallel dotted lines *ag*, *fo*, *br*, and *qp*; but when the moment of resistance is assumed to be constant, the wear will increase slightly from the pitch point, as shown by the curved lines tangent thereto. To prove that the wear is uniform when the pressure Q is constant, prolong DE and draw BL parallel to OD, then the triangle BDL will be similar to the triangle DHK, and $\frac{DE}{DL}$ will represent the wear on the face of wheel B.

Now, by similar triangles, we have $DE = \frac{AB}{AC} \times CD$, and $DL = \frac{BO}{OC} \times CD$; whence $\frac{DE}{DL} = \frac{AB \times OC}{AC \times BO}$, which is constant.

Similarly, the wear on the flank of wheel A, represented by $\frac{DE}{EL}$ can also be shown to be constant.

For involute teeth the result is very different, for there the lines BL and CL are constant, and the intensity of wear may vary from infinity to zero.

To compare the wear on both forms of teeth by diagrams drawn to the same scale, we must assume some ordinate to represent the wear resulting from a unit of pressure in sliding a unit's distance over a unit of surface.

In Fig. 68, this ordinate should be laid off from the line *BL* to locate the parallel line *PQR*, and in Fig. 69 its oblique projection, equal to its original length times cosec α, should be laid off from *BL* for a similar purpose. At the points *Q* and *R*, where this line intersects *BE* and *BD*, draw the lines *QT* and *RS* parallel to the line of connection *CD*; then will these distances represent the intensity of wear at the point *D* on the wheels *A* and *B* respectively.

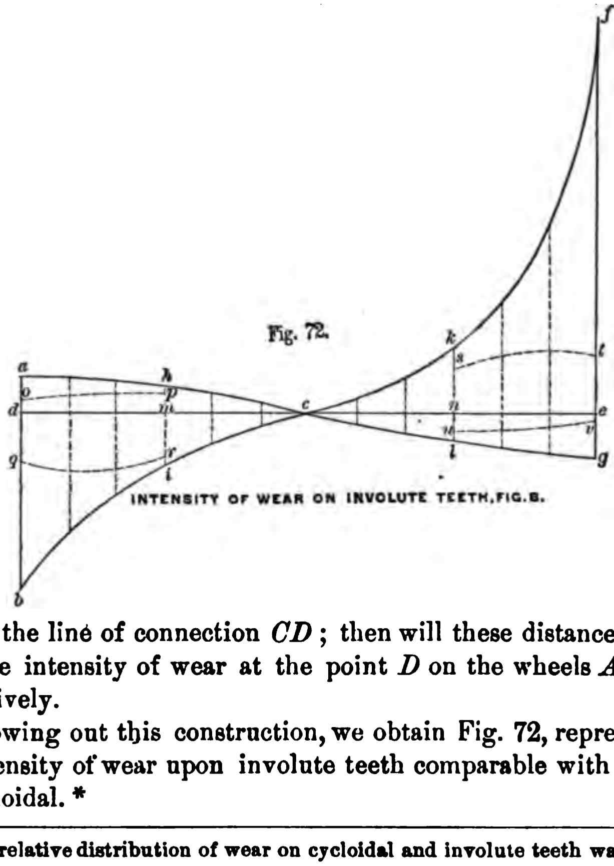


Fig. 72.

INTENSITY OF WEAR ON INVOLUTE TEETH, FIG. 8.

Following out this construction, we obtain Fig. 72, representing the intensity of wear upon involute teeth comparable with Fig. 71 for cycloidal. *

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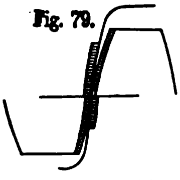
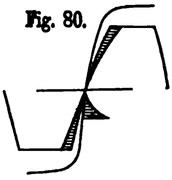
* The relative distribution of wear on cycloidal and involute teeth was pointed out by Mr. Bilgram in a lecture delivered at the Franklin Institute in November, 1881, and published in its Journal for January, 1882. The diagrams which he used at that time to illustrate his remarks are now of especial interest in connection with Figs. 13 and 14 of the paper under discussion, and they are here subjoined (Figs. 79 and 80), to indicate the wearing tendency on a single pair of engaging teeth.

Fig. 79.



CYCLOIDAL TEETH.

Fig. 80.



INVOLUTE TEETH.

We have not yet, however, arrived at the solution of the problem presented, upon the basis of actual working condition, and, in the present discussion, I can do but little more than indicate the direction in which the analysis may be pursued. It has been pointed out in a foot-note on the fifth page of the paper that in actual practice each pair of tooth-profiles is brought into engagement while the preceding pair is still in action, and the conclusion is drawn, rather hastily I think, that the rule laid down for finding the loss of work on the assumption that only one pair engage at a time, holds good in practice as well as in theory.

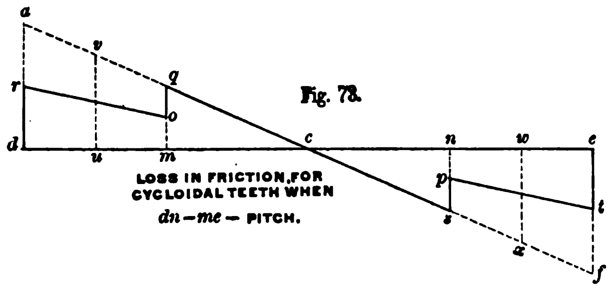


Fig. 73.

That this is an error of no small importance will appear by reference to Figs. 73 and 74, in which the arc of action dce has been taken equal to $1\frac{1}{2}$ pitch, and divided equally at c for convenience.

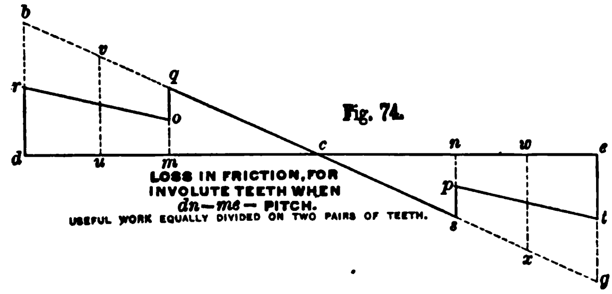


Fig. 74.

Now, if $dn = me =$ pitch, and the load is divided equally between two pairs of engaging teeth, we shall have for the loss in friction for each pair of teeth, the areas $droqcd + cetpsc = V$, instead of the larger areas $dac + cef$ or $dob + ceg = V'$. If the arc of contact be reduced to $uw =$ pitch, so that there shall never be more than one pair of teeth in action, the loss in friction for each pair of teeth will be represented by the area $wvc + cwx = V''$.

From similar triangles, Fig. 74, it will readily appear that the difference between these areas $V - V''$, is not measured by the difference in the arcs of contact, $dc - uw = \frac{1}{3}$, but the square of that difference, or $\frac{1}{9}$; and the same is approximately true for cycloidal teeth, Fig. 73. In other words, we have area $V = \frac{10}{9} V''$, instead of $\frac{4}{3} V''$.

From this consideration, it is evident that the general equation at the top of the seventeenth page,

$$p = f\pi\left(\frac{1}{Z} \pm \frac{1}{Z_1}\right)\frac{\varepsilon}{2}, \quad \ldots\ldots\ldots \quad (c)$$

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For involute teeth, the arc of action is so much more variable, that to obtain a close approximation of this kind would at present seem out of the question, but from other considerations which have not yet appeared, it is thought that the equation (*e*) may also be used to express the loss in friction for involute teeth as well.

The fallacy in the reasoning by which it has been inferred that the "assumed case holds good in practice," is very natural and easy. Suppose, for example, Fig. 73, there is a tooth, T_1, in action at *n*, when a new tooth, T_2, enters the path of contact at *d*, then as T_2 passes from *d* to *e*, it will lose in friction the work represented by the area called *A*, the tooth, T_1, will lose the area *ptsf*, and a new tooth, T_3, coming into gear will lose the area *raqo*, all of which, added together, completes the area *dac* + *cef* for the *assumed case* of one pair of teeth in gear at a time, and, if we stop here, there will be no reason to doubt that the inference referred to is correct. But, if we continue to move the wheels through successive arcs of action, the result will be quite different. Commencing with the tooth T_3 at the point m, the next area becomes $A + mqo$ and, continuing, the next becomes $A + cns$, when the teeth again stand in the positions originally assumed relative to the arc of contact. The error appears to have arisen from assuming an incomplete cycle of movements to represent the whole.

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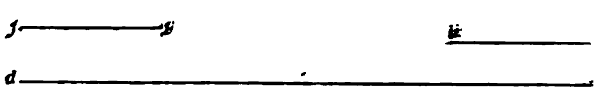

LOAD TRANSMITTED BY CYCLOIDAL TEETH.

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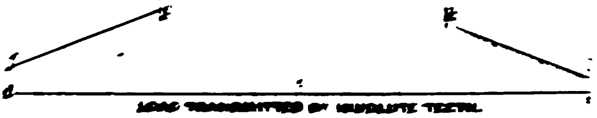

LOAD TRANSMITTED BY INVOLUTE TEETH.

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wear must soon be reached, in which the wear on simultaneous points of contact of both pairs of teeth is equal. But the wear can only be equal if the pressure is inversely as the velocity of friction, this is an assumption which may be more or less true. Therefore the total pressure will be no more distributed equally

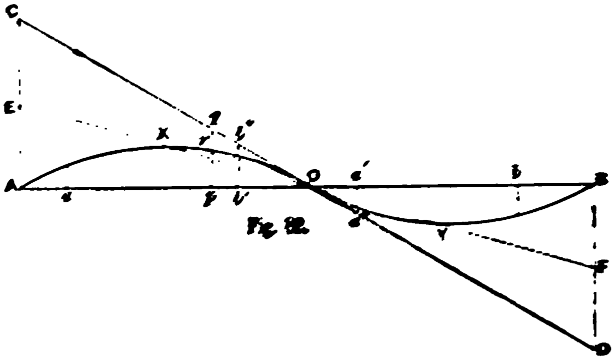

Fig. 82.

upon both teeth, but inversely as the velocity of friction. The diagram *MLNKM*, Fig. 81, therefore represents the variable pressure on each tooth-pair as their point of contact travels from *A* to *B*. For convenience, the triangle *KLNK* may be transported to *MPLM*.

If a pair of teeth is at *p*, the pressure will be equal to *mo*, while that on the other simultaneous contact is equal to *on*, the sum of both being equal to the total pressure, *MP* = *KL*.

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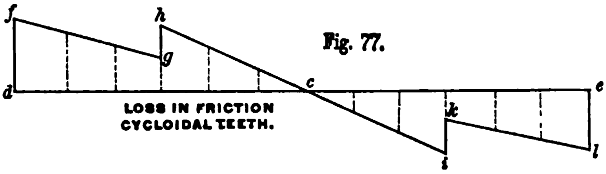

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These diagrams go to show that an extended arc of contact entails less friction for involute teeth than it does for cycloidal, and that, practically, formula (*e*) must come very near the truth for all cases.

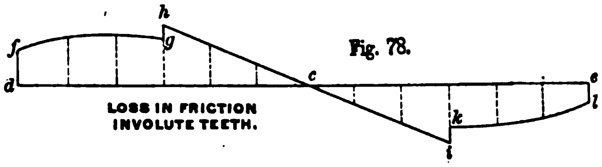

Fig. 78.

From Figs. 75 and 76, it appears that the load on involute teeth

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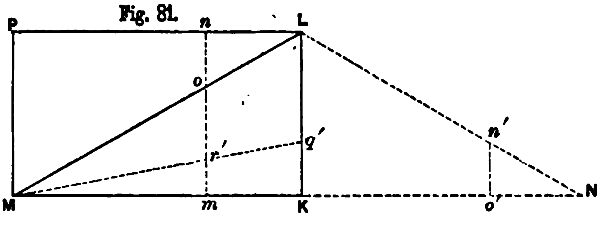
velocity *pq*. Since *AC* represents both the velocity of friction and the dynamic loss, the scale of the diagram is evidently such that the pressure on the teeth multiplied by the coefficient of friction can be considered as the unit. The multiplication of *mo* and *pq* can therefore be accomplished by making $Kq' = pq$, and drawing the line $q'M$, intersecting *mo* in r', when $pr = mr'$. When the state of persistency is once reached, the loss from friction is therefore no more represented by the surfaces *OAEO* and *OBFO*, but by the parabolic segments *OXAO* and *OYBO*.

Fig. 81.



Now, if the addenda are not so great as thus far assumed, if the path of contact extends only from *a* to *b*, it is plain that between *b'* and *a'* only one tooth is in action.

While passing from *a* to *b'* it will be assisted by the preceding tooth passing from *a'* to *b*, and only in this period will the curves *X* and *Y* show the loss from friction. The total loss is, therefore, shown by the surfaces *Ob''Xab'O* and *Oa''Yba'O*.

will eventually be applied and removed more gradually than it will on cycloidal, and that as a consequence the action should become smoother.

When the condition of "persistency" is established, the intensity of wear will be represented by the dotted lines *st*, *uv*, *wx* and *yz*, Fig. 71, and by *op*, *qr*, *st* and *uv*, Fig. 72, the middle portion in each remaining unaltered.

That there will actually be some wear at the pitch point *c*, as suggested on the next to the last page of the paper, there can be no doubt, but, in addition to the reasons mentioned, it should also be noted that wear must occur there, even with involute teeth, on account of the fact that every point in the tooth surface must remain in contact through a perceptible arc of action, owing to its compressibility—in other words, because the path of contact must have some thickness, as well as length and width. This consideration will affect the intensity of wear to some extent, but, it is thought, not seriously. The main point which remains unsettled, and the one which is always of the first practical importance to determine, is the coefficient of friction. Is it sufficiently constant throughout the arc of action to be taken as such, and if not, must the "Friction of Toothed Gearing" be considered a hopeless problem?

I am strongly convinced from the "Sellers Experiments on Gearing," which I have had the honor of reporting to this society, that the coefficient of friction must undergo very decided changes throughout the arc of action, so much so, indeed, as to impair, to a greater or less extent, the practical value of any formula yet devised for determining the loss in friction; and it is only for very slow motions, such as that of feed gearing, that I should think of applying formula (*e*) with any degree of assurance.

I am well aware of certain crudities in this hurried discussion, and fine points neglected that might have been considered, but the ground to be covered seems to expand as we proceed; and, to conclude, I think it will be admitted that the subject before us still offers a promising field for experimental research and analytical investigation.

Mr. Hugo Bilgram.—On the ninth page of his paper, Professor Reuleaux, by his analytical deduction, comes to the conclusion that, as regards the loss of power from friction, the most favorable case will be that in which the contact is equally divided on either side of the line of centers.

This appears to be in direct conflict with the assertion of Professor Willis, that the loss from friction during the approach, exceeds that which occurs during the receding contact, for which reason the addendum of the driven gear should exceed that of the driver, when a minimum of loss from friction at a given length of the arc of contact is desired.

This discrepancy can be traced to the difference in the premises of the two investigators. While Professor Willis compares the effective momentum given by the driver with that received by the driven gear, in the various stages of action, Professor Reuleaux computes the loss from friction for a *uniform* normal pressure between the faces of the teeth.

The actual pressure transmitted by the teeth, which is inclined to the common normal of the curves in contact by the angle of friction, may be resolved into two forces—the normal pressure, which in the paper under discussion has been considered a constant, and the tangential force of friction. On examination, it will be found that the force of friction has a much greater reacting leverage during the approaching than during the receding action. In order to overcome a uniform resistance in the driven gear, it is, therefore, necessary that the normal force between the teeth should be greater during the approach than during the receding action; and if this is an essential prerequisite in practice, the loss from friction during the approach, must exceed that of the other half of the action. The assumption of a uniform normal component is, therefore, not compatible with what actually occurs in practice.

The factor of friction is, however, as a rule, no sufficient reason for abandoning the practice of making the addenda of gears equal, except when special reasons intervene. When toothed gears are used intermittently or interchangeably as drivers and driven gears, the conclusion of Professor Reuleaux is unquestionable.

In the paper on a new Odontograph, mentioned by Mr. Wilfred Lewis in his discussion, I showed a simple graphical method by which the most advantageous relation between the addenda of the driving and driven gears may be found, the angle of friction being assumed as known.

There is, however, another statement in the paper just read which, I think, would be an injustice to myself, were I not permitted to correct the same. I refer to the mention of my bevel gear cutter in relation to the faulty action of the involute system of gearing, when the pinion has a low number of teeth. With the machine in question, pinions with as few as 12 teeth can be cut with entire freedom from this fault. When both gears are equal, the number can be reduced to 15 without any interference whatever, since 15 teeth mitre gears correspond in form and action with 21 teeth spur gears. With 15 teeth mitre gears there is, therefore, even theoretically, no fault. By experience, I have found that with a reduction of the number of teeth of mitre gears to 12, the fault which now exists in theory is so small that it escapes detection. The case is, however, different when a pinion gears with a larger wheel. But in this case, the fact that bevel gears are never used interchangeably, but are invariably made in pairs or triplets, permits the application of an expedient which obviates the difficulty. It consists in reducing the addendum of the wheel, and increasing that of the pinion correspondingly.

The difficulty only exists when blanks are sent for cutting which are turned by machinists who omit to take this precaution.

Mr. C. A. Smith.—In making a few remarks concerning this paper, I wish, in the first place to call attention to a few points of criticism directed to the *Practical Treatise on Gearing*, published by the Brown & Sharpe Mfg. Co. of Providence, R. I.

In the foot-note on the nineteenth page of his paper, Professor Reuleaux mentions the difference between the obliquity (or its complement) adopted by the Brown & Sharpe Mfg. Co. and that used by himself (75°), which he also says is the one used by Professor Willis. This must be a mistake, since by referring to Professor Willis' *Principles of Mechanism*, page 134, we find that he uses 75° for double curve teeth, but on the page just preceding this (133) he recommends 75½° for involute or single curve teeth, and gives his reasons for doing so. It is true that an angle of 75° is conveniently laid out on the drawing board by means of the ordinary triangles; but the fact is that the men in the workshop do not use triangles to lay out work; some of the most common tools being the scribe awl, straight edge and dividers. When the angle is assumed whose cosine is ¼ (75½° very nearly) the radius of the tooth curve may be obtained by simply bisecting, with a pair of dividers, the radius of the circle described on the pitch radius as a diameter, and as the formation of gear teeth must always be a work of the shop, rather than of the drawing table, the convenience of these proportions is at once appreciated by the "practical man."

It is sometimes said that the work of laying out gear teeth, templates, etc., should not be done by the machinist, but ought to be executed by the draughtsman. This is a wrong idea, because the machinist has more practice in doing accurate work (those, at least, who are employed on such a class of work), and he should therefore be the one to do it, under proper instructions, of course. As far as the drawings are concerned, it is not good economy to attempt to draw gear teeth accurately. In most cases they need not be drawn at all, but when they are drawn, an outline formed by the eye will answer the purpose of a working drawing as well as though they were drawn theoretically correct. On the other hand, the draughtsman does often understand as little of the correct principles of gearing as the machinist.

Referring to the foot-note on the last page but two, Professor Reuleaux again refers to the *Practical Treatise on Gearing*, by Brown & Sharpe Mfg. Co., and says that the *cause* of the interference of teeth on small gears "is ascribed to wide departure of circular arcs," etc., quoting almost the exact words given at the top of the twenty-second page, which reads thus:

"This rounding occurs because in these gears arcs of circles depart too far from the true involute curve, being so much that points of teeth get no bearing on flanks of teeth in other wheels." This refers to the circular arcs forming the tooth faces of the pinion.

The criticism here is evidently an oversight, since no attempt whatever has been made to explain the *cause* of interference. Fig. 10, to which Professor Reuleaux refers, simply calls attention to the *fact of interference.* The statement above quoted from refers to the tooth faces of the pinion, Fig. 10, and has no reference whatever to interference. The interference takes place at the flanks of the pinion teeth and faces of the gear teeth. To avoid this, the spaces between teeth on the pinion, below the base circle, are made as wide as is practically possible, as explained on the twentieth page. The remainder of the correction is made by rounding the faces of the gear teeth. An interchangeable set of involute gears cannot be made as low as 12 teeth without slightly altering the involute and making a combination tooth curve, unless the obliquity be increased, which again is objectionable on account of the increased friction which would necessarily accompany such a change.

I might even go further than stating that the above criticism is ill-founded, by saying that the explanation of interference, given by Professor Reuleaux, is not strictly correct, or that it is at least very indirectly explained and difficult to understand by any one but an expert. To state the case briefly, let C and D, Fig. 66, be the base circles of a pair of involute gears; E and F the pitch circles; GH the line of contact (not "the curve of contact"). AG and HB are perpendicular to the line of contact HG, and hence the latter is tangent to the base circles at G and H. The greatest possible line of contact obtainable with the involute tooth is the line GH, the contact commencing at G (when the wheels rotate as indicated by the arrows) and at the root of the involute K, and ending at H and at the root of the involute J. The points G and H, therefore limit the addenda, which cannot be greater than MH and GL respectively. If the addenda are made greater than this limit, then the teeth must be "corrected" to prevent them from interfering, or from being "undercut."

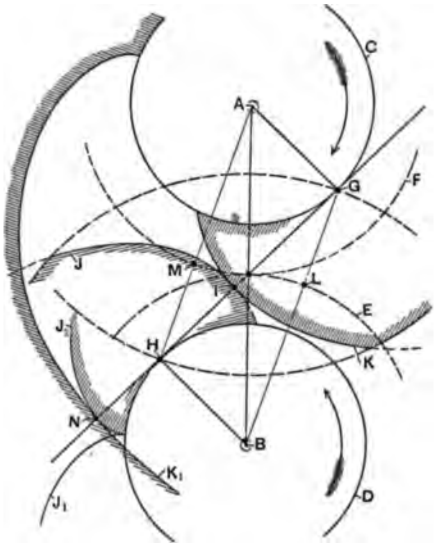


Fig. 66

Involute teeth will not even work "*geometrically* correctly" (as stated by the author of the paper under discussion) after the point of contact has passed the point H. While the point of contact I is between H and G, the involutes J and K are in external contact, so to speak, the wheel A driving the wheel B. After contact has passed H, as shown at N, the involutes are in internal contact, and the wheel A can no longer be the driver, so that to drive "geometrically correctly," the wheel B would have to become the driver, otherwise curve K_1 would simply move away from the curve J_2, the opposite branch of the involute J or J_1, with which it is now supposed to be in contact. It is easily seen that the second branch J_2 of the wheel B will always be in internal contact with the first branch K_1 of the wheel A, since the radius of curvature, NH, of the former curve is always less than the radius of curvature, NG, of the latter.

After explaining somewhat in detail the cause of interference, and drawing his conclusions, the author says, on his nineteenth page: "Practice sustains this deduction. . . . A jog is soon worn into the teeth of the pinion just inside of the pitch circle, and this wear is solely due to erroneous design of the profiles, even though they be cut with ever so much accuracy." On the last two pages he shows correctly that the wear on all gear teeth is greatest inside of the pitch line—that the natural tendency is to wear a jog in the teeth below the pitch line. This being true, the fact that the teeth of a gear have "jogs" worn into them is no evidence of interference or bad design, as above stated. If there is any interference, it will be discovered before the teeth have an opportunity to become worn, since they will not work until the interference is removed.

In regard to Professor Reuleaux's paper, as a whole, I should like to raise the question as to whether it is correct, in calculating the friction of gear wheels, to take into account only the friction at the teeth? In my opinion the friction at the journals should also be taken into account, since this, as well as the friction at the teeth, depends upon the obliquity of action, and the obliquity in turn depends upon the form of teeth; hence, to compare the friction of gearing having different forms of teeth, *all the friction* for which the teeth are responsible, either directly or indirectly, should be taken into account.

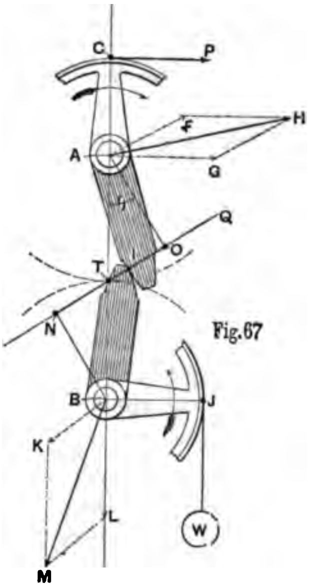


Fig.67

To illustrate more clearly what I mean, let us refer to the diagram Fig. 67. I have here represented a pair of teeth whose axes or shafts are at A and B. ON is the normal to the teeth at the point of contact, I. Let us suppose the gear A to be used to drive the gear B for the purpose of raising a weight, W. Now, it is impossible to apply a force to the gear B directly at I. The driving force must be applied at some point of the shaft A. We may suppose it to be a pulley driven by a belt, the working force being represented by CP. The diagram is so plain that I trust it needs no further explanation in detail. The only route for the power from the difference between these areas $V - V''$, is not measured by the difference in the arcs of contact, $de - uw = \frac{1}{3}$, but the square of that difference, or $\frac{1}{9}$; and the same is approximately true for cycloidal teeth, Fig. 73. In other words, we have area $V = \frac{10}{9} V''$, instead of $\frac{4}{3} V''$.

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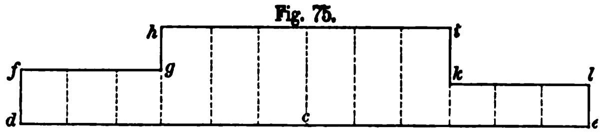
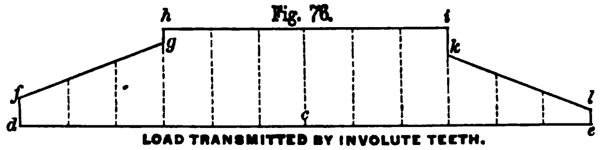


LOAD TRANSMITTED BY CYCLOIDAL TEETH.

Fig. 76.



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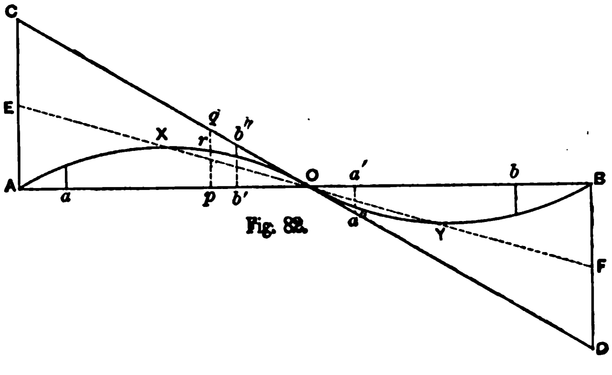

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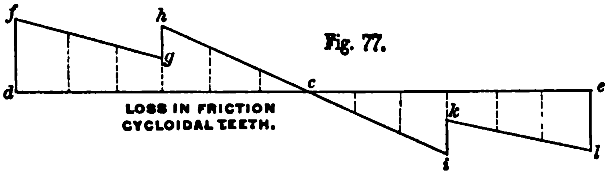

Fig. 77.

These diagrams go to show that an extended arc of contact entails less friction for involute teeth than it does for cycloidal, and that,

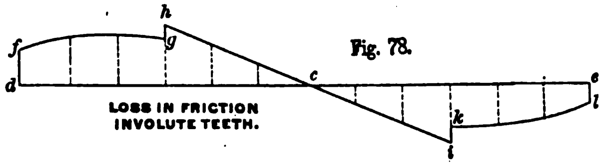

Fig. 78.

practically, formula (e) must come very near the truth for all cases.

From Figs. 75 and 76, it appears that the load on involute teeth

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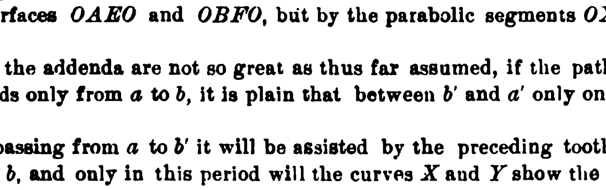
velocity pq. Since AC represents both the velocity of friction and the dynamic loss, the scale of the diagram is evidently such that the pressure on the teeth multiplied by the coefficient of friction can be considered as the unit. The multiplication of mo and pq can therefore be accomplished by making $Kq' = pq$, and drawing the line $q'M$, intersecting mo in r', when $pr = mr'$. When the state of

Fig. 81.



persistency is once reached, the loss from friction is therefore no more represented by the surfaces $OAEO$ and $OBFO$, but by the parabolic segments $OXAO$ and $OYBO$.

Now, if the addenda are not so great as thus far assumed, if the path of contact extends only from a to b, it is plain that between b' and a' only one tooth is in action.

While passing from a to b' it will be assisted by the preceding tooth passing from a' to b, and only in this period will the curves X and Y show the loss from friction. The total loss is, therefore, shown by the surfaces $Ob''Xab'O$ and $Oa''Yba'O$.

will eventually be applied and removed more gradually than it will on cycloidal, and that as a consequence the action should become smoother.

When the condition of "persistency" is established, the intensity of wear will be represented by the dotted lines *st*, *uv*, *wx* and *yz*, Fig. 71, and by *op*, *qr*, *st* and *uv*, Fig. 72, the middle portion in each remaining unaltered.

That there will actually be some wear at the pitch point *c*, as suggested on the next to the last page of the paper, there can be no doubt, but, in addition to the reasons mentioned, it should also be noted that wear must occur there, even with involute teeth, on account of the fact that every point in the tooth surface must remain in contact through a perceptible arc of action, owing to its compressibility—in other words, because the path of contact must have some thickness, as well as length and width. This consideration will affect the intensity of wear to some extent, but, it 'is thought, not seriously. The main point which remains unsettled, and the one which is always of the first practical importance to determine, is the coefficient of friction. Is it sufficiently constant throughout the arc of action to be taken as such, and if not, must the "Friction of Toothed Gearing" be considered a hopeless problem?

I am strongly convinced from the "Sellers Experiments on Gearing," which I have had the honor of reporting to this society, that the coefficient of friction must undergo very decided changes throughout the arc of action, so much so, indeed, as to impair, to a greater or less extent, the practical value of any formula yet devised for determining the loss in friction; and it is only for very slow motions, such as that of feed gearing, that I should think of applying formula (*e*) with any degree of assurance.

I am well aware of certain crudities in this hurried discussion, and fine points neglected that might have been considered, but the ground to be covered seems to expand as we proceed; and, to conclude, I think it will be admitted that the subject before us still offers a promising field for experimental research and analytical investigation.

Mr. Hugo Bilgram.—On the ninth page of his paper, Professor Reuleaux, by his analytical deduction, comes to the conclusion that, as regards the loss of power from friction, the most favorable case will be that in which the contact is equally divided on either side of the line of centers.

This appears to be in direct conflict with the assertion of Professor Willis, that the loss from friction during the approach, exceeds that which occurs during the receding contact, for which reason the addendum of the driven gear should exceed that of the driver, when a minimum of loss from friction at a given length of the arc of contact is desired.

This discrepancy can be traced to the difference in the premises of the two investigators. While Professor Willis compares the effective momentum given by the driver with that received by the driven gear, in the various stages of action, Professor Reuleaux computes the loss from friction for a *uniform* normal pressure between the faces of the teeth.

The actual pressure transmitted by the teeth, which is inclined to the common normal of the curves in contact by the angle of friction, may be resolved into two forces—the normal pressure, which in the paper under discussion has been considered a constant, and the tangential force of friction. On examination, it will be found that the force of friction has a much greater reacting leverage during the approaching than during the receding action. In order to overcome a uniform resistance in the driven gear, it is, therefore, necessary that the normal force between the teeth should be greater during the approach than during the receding action; and if this is an essential prerequisite in practice, the loss from friction during the approach, must exceed that of the other half of the action. The assumption of a uniform normal component is, therefore, not compatible with what actually occurs in practice.

The factor of friction is, however, as a rule, no sufficient reason for abandoning the practice of making the addenda of gears equal, except when special reasons intervene. When toothed gears are used intermittently or interchangeably as drivers and driven gears, the conclusion of Professor Reuleaux is unquestionable.

In the paper on a new Odontograph, mentioned by Mr. Wilfred Lewis in his discussion, I showed a simple graphical method by which the most advantageous relation between the addenda of the driving and driven gears may be found, the angle of friction being assumed as known.

There is, however, another statement in the paper just read which, I think, would be an injustice to myself, were I not permitted to correct the same. I refer to the mention of my bevel gear cutter in relation to the faulty action of the involute system of gearing, when the pinion has a low number of teeth. With the machine in question, pinions with as few as 12 teeth can be cut with entire freedom from this fault. When both gears are equal, the number can be reduced to 15 without any interference whatever, since 15 teeth mitre gears correspond in form and action with 21 teeth spur gears. With 15 teeth mitre gears there is, therefore, even theoretically, no fault. By experience, I have found that with a reduction of the number of teeth of mitre gears to 12, the fault which now exists in theory is so small that it escapes detection. The case is, however, different when a pinion gears with a larger wheel. But in this case, the fact that bevel gears are never used interchangeably, but are invariably made in pairs or triplets, permits the application of an expedient which obviates the difficulty. It consists in reducing the addendum of the wheel, and increasing that of the pinion correspondingly.

The difficulty only exists when blanks are sent for cutting which are turned by machinists who omit to take this precaution.

Mr. C. A. Smith.—In making a few remarks concerning this paper, I wish, in the first place to call attention to a few points of criticism directed to the *Practical Treatise on Gearing*, published by the Brown & Sharpe Mfg. Co. of Providence, R. I.

In the foot-note on the nineteenth page of his paper, Professor Reuleaux mentions the difference between the obliquity (or its complement) adopted by the Brown & Sharpe Mfg. Co. and that used by himself (75°), which he also says is the one used by Professor Willis. This must be a mistake, since by referring to Professor Willis' *Principles of Mechanism*, page 134, we find that he uses 75° for double curve teeth, but on the page just preceding this (133) he recommends 75½° for involute or single curve teeth, and gives his reasons for doing so. It is true that an angle of 75° is conveniently laid out on the drawing board by means of the ordinary triangles; but the fact is that the men in the workshop do not use triangles to lay out work; some of the most common tools being the scribe awl, straight edge and dividers. When the angle is assumed whose cosine is ¼ (75½° very nearly) the radius of the tooth curve may be obtained by simply bisecting, with a pair of dividers, the radius of the circle described on the pitch radius as a diameter, and as the formation of gear teeth must always be a work of the shop, rather than of the drawing table, the convenience of these proportions is at once appreciated by the "practical man."

It is sometimes said that the work of laying out gear teeth, templates, etc., should not be done by the machinist, but ought to be executed by the draughtsman. This is a wrong idea, because the machinist has more practice in doing accurate work (those, at least, who are employed on such a class of work), and he should therefore be the one to do it, under proper instructions, of course. As far as the drawings are concerned, it is not good economy to attempt to draw gear teeth accurately. In most cases they need not be drawn at all, but when they are drawn, an outline formed by the eye will answer the purpose of a working drawing as well as though they were drawn theoretically correct. On the other hand, the draughtsman does often understand as little of the correct principles of gearing as the machinist.

Referring to the foot-note on the last page, but two, Professor Reuleaux again refers to the *Practical Treatise on Gearing*, by Brown & Sharpe Mfg. Co., and says that the *cause* of the interference of teeth on small gears " is ascribed to wide departure of circular arcs," etc., quoting almost the exact words given at the top of the twenty-second page, which reads thus:

"This rounding occurs because in these gears arcs of circles depart too far from the true involute curve, being so much that points of teeth get no bearing on flanks of teeth in other wheels." This refers to the circular arcs forming the tooth faces of the pinion.

The criticism here is evidently an oversight, since no attempt whatever has been made to explain the *cause* of interference. Fig. 10, to which Professor Reuleaux refers, simply calls attention to the *fact of interference.* The statement above quoted from refers to the tooth faces of the pinion, Fig. 10, and has no reference whatever to interference. The interference takes place at the flanks of the pinion teeth and faces of the gear teeth. To avoid this, the spaces between teeth on the pinion, below the base circle, are made as wide as is practically possible, as explained on the twentieth page. The remainder of the correction is made by rounding the faces of the gear teeth. An interchangeable set of involute gears cannot be made as low as 12 teeth without slightly altering the involute and making a combination tooth curve, unless the obliquity be increased, which again is objectionable on account of the increased friction which would necessarily accompany such a change.

I might even go further than stating that the above criticism is ill-founded, by saying that the explanation of interference, given by Professor Reuleaux, is not strictly correct, or that it is at least very indirectly explained and difficult to understand by any one but an expert. To state the case briefly, let C and D, Fig. 66, be the base circles of a pair of involute gears; E and F the pitch circles; GH the line of contact (not "the curve of contact"). AG and HB are perpendicular to the line of contact HG, and hence the latter is tangent to the base circles at G and H. The greatest possible line of contact obtainable with the involute tooth is the line GH, the contact commencing at G (when the wheels rotate as indicated by the arrows) and at the root of the involute K, and ending at H and at the root of the involute J. The points G and H, therefore limit the addenda, which cannot be greater than MH and GL respectively. If the addenda are made greater than this limit, then the teeth must be "corrected" to prevent them from interfering, or from being "undercut."

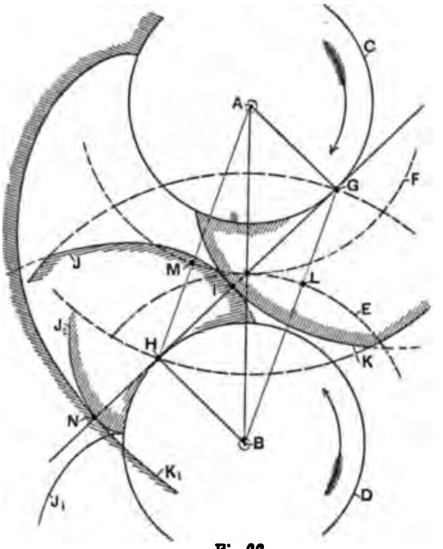


Fig. 66

Involute teeth will not even work "*geometrically* correctly" (as stated by the author of the paper under discussion) after the point of contact has passed the point H. While the point of contact I is between H and G, the involutes J and K are in external contact, so to speak, the wheel A driving the wheel B. After contact has passed H, as shown at N, the involutes are in internal contact, and the wheel A can no longer be the driver, so that to drive "geometrically correctly," the wheel B would have to become the driver, otherwise curve K_1 would simply move away from the curve J_2, the opposite branch of the involute J or J_1, with which it is now supposed to be in contact. It is easily seen that the second branch J_2 of the wheel B will always be in internal contact with the first branch K_1 of the wheel A, since the radius of curvature, NH, of the former curve is always less than the radius of curvature, NG, of the latter.

After explaining somewhat in detail the cause of interference, and drawing his conclusions, the author says, on his nineteenth page: "Practice sustains this deduction. . . . A jog is soon worn into the teeth of the pinion just inside of the pitch circle, and this wear is solely due to erroneous design of the profiles, even though they be cut with ever so much accuracy." On the last two pages he shows correctly that the wear on all gear teeth is greatest inside of the pitch line—that the natural tendency is to wear a jog in the teeth below the pitch line. This being true, the fact that the teeth of a gear have "jogs" worn into them is no evidence of interference or bad design, as above stated. If there is any interference, it will be discovered before the teeth have an opportunity to become worn, since they will not work until the interference is removed.

In regard to Professor Reuleaux's paper, as a whole, I should like to raise the question as to whether it is correct, in calculating the friction of gear wheels, to take into account only the friction at the teeth? In my opinion the friction at the journals should also be taken into account, since this, as well as the friction at the teeth, depends upon the obliquity of action, and the obliquity in turn depends upon the form of teeth; hence, to compare the friction of gearing having different forms of teeth, *all the friction* for which the teeth are responsible, either directly or indirectly, should be taken into account.

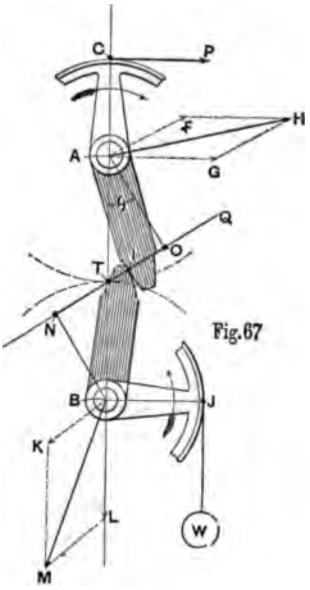


Fig.67

To illustrate more clearly what I mean, let us refer to the diagram Fig. 67. I have here represented a pair of teeth whose axes or shafts are at *A* and *B*. *ON* is the normal to the teeth at the point of contact, *I*. Let us suppose the gear *A* to be used to drive the gear *B* for the purpose of raising a weight, *W*. Now, it is impossible to apply a force to the gear *B* directly at *I*. The driving force must be applied at some point of the shaft *A*. We may suppose it to be a pulley driven by a belt, the working force being represented by *CP*. The diagram is so plain that I trust it needs no further explanation in detail. The only route for the power from the point *C*, where it is applied, to the point *J*, where the useful work is performed, is by way of the shaft *A*, the teeth at *I* and the shaft *B*, and in its transit, work is absorbed by friction at these three points—*A*, *I*, and *B*. It is evident that the result would be the same in principle, whether we consider the forces *P* and *W* to be applied in the plane of the gears or in different planes. First, then, the friction at *A* is proportional to the pressure *AH*, which is the resultant of the normal pressure at the teeth (which we may represent by *Q*), to which *AF* is made equal and parallel, and the applied force $CP = AG$. But the normal pressure, $Q = AF$, depends upon the obliquity, which is equal to $OAT = \theta$, *i. e.*:

$$Q = \frac{CP \times CA}{AO} = \frac{CP \times CA}{AT \times \cos\theta}.$$

This equation shows clearly that *Q* varies inversely as the cosine of the obliquity.

Second; the friction at *I*, of course, depends upon the pressure *Q*, and consequently upon θ.

Third; the friction at *B* depends upon the pressure *BM*, which is the resultant of the useful resistance, $W = BL$, and $BK = Q =$ a function of θ. Hence there are at least three points at which the work absorbed by friction depends directly upon the obliquity, and this, of course, depends upon the form of teeth adopted.

There are other minor points which have an influence slightly to modify the resultants *AH* and *BM*, one of these being the fact that the approaching friction is greater than the receding friction, a point just spoken of by Mr. Bilgram, and referred to by Professor MacCord in his *Kinematics*. I will, however, not discuss these minor points at this time, trusting that what I have said is sufficient to make clear my general idea on the subject.

There is a question incidentally connected with that of friction, which was suggested to the writer by a diagram * similar to Fig. 67, which may have an important influence on general theory of gear teeth. The fact observed was, that owing to the friction the force transmitted is necessarily variable, the immediate result of which would be to give an unsteady or fluctuating motion, and consequently produce more or less noise, however "nice" the teeth may have been formed. Such a conclusion seems warranted by ex-

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* *American Machinist* of May 1, 1886.

perience, as I have never seen a pair of gears which were free from noise at an ordinary speed of rotation.

Mr. Allan Stirling.—I would like to call attention to a practical difficulty in the way of spur gears for certain purposes. Those who have occasion to ride in elevator cages operated by spur gears, have noticed, of course, a peculiar trembling that there is to the cage. As far as my experience goes, this is inseparable from the use of spur gears for such purposes, and I can readily see, in the demonstrations that have been made before us now, the reason why this is so. If the friction at different points varies as has been shown, and when receding from the line of centers it is not so much as it is in approaching it, the different resistances due to friction are variable during the contact of two teeth; and as the teeth enter and leave, there must be an appreciable difference in the velocity, which would cause this trembling action. So far as I know, it is impossible to overcome this difficulty; and in order to get a smooth, gliding motion we are compelled to use a screw gear or some other motive power. I merely wish to make this observation in order to ask if any of the gentlemen can say that it is possible to make a smooth, gliding motion when using spur gears.

CCXXVIII.

ON THE FRICTION OF NON-CONDENSING ENGINES.

BY R. H. THURSTON, ITHACA, N. Y.

(Member of the Society.)

THE assumption of the distinguished engineer, De Pambour, that the wasteful resistance of a steam engine consists of a constant quantity, the friction of the unloaded engine, increased by some increasing function of the added load, has been accepted as correct by probably all recognized authorities since his time. Calling R_0 the resistance of the engine running free and under no other load than its own friction, and calling R_1 the resistance coming upon it as a useful factor of its work, and making f the co-efficient measuring the proportion of increased friction due to the load, the total resistance to be overcome by the engine piston is thus

$$R = (1 + f) R_1 + R_0 \quad \ldots\ldots\ldots\ldots \quad (1)$$

So far as the writer has observed, it has never been questioned whether the quantity f is constant or variable, and no recent attempts have been made to ascertain its value by experiment.

It has long been the intention of the writer to settle this question, which had for years existed in his own mind, and the opportunity has recently been offered to do so, at least as that question affects the modern forms of non-condensing high-speed engines now so generally in use, especially for electric lighting purposes. The first investigation was made, at the suggestion of the writer and under his general direction, in the winter of 1883-4, upon a "Straight Line Engine," exhibited that year, at the Annual Exhibition of the American Institute, by the Straight Line Engine Co., of Syracuse, N. Y., and built by them from the designs of Professor John E. Sweet, the inventor of its special features.* The results were sufficiently exact and satisfactory in

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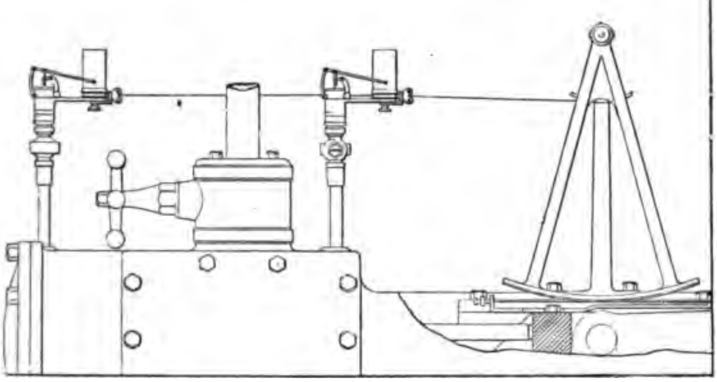
* The work was done with equal care and skill by Messrs. Mitchell and Aldrich graduates of Stevens Institute of Technology, of the class of 1884.

every respect to have been made the basis of the conclusions here to be stated; but it seemed to the writer desirable that they should be checked by similar work upon another engine, if possible of a different make, before attempting to state definite conclusions of any kind. The opportunity to secure such a repetition of the investigation was offered, during the past winter, at Cornell University, using a Straight Line engine, which could be fitted with a brake, and conveniently submitted to test. The engine is of the same make as the first described, but of a different size, and the results of the two sets of experiments are considered to accord so thoroughly as to justify publication. The following are the data and results of these two sets of determinations:

METHOD OF ATTACHING THE INDICATORS.

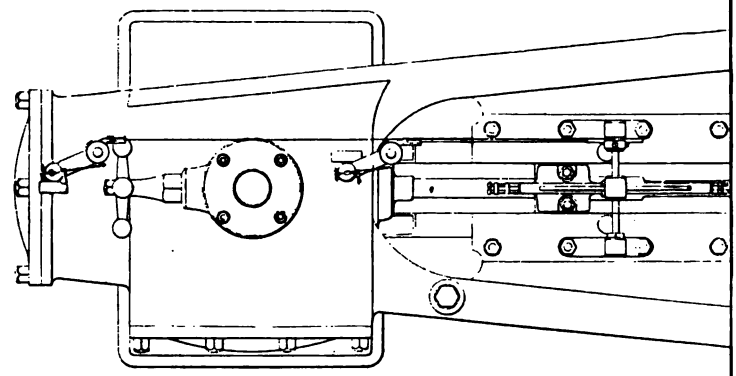
Fig. 15.



The first of these two engines was built from designs brought out in the year 1880, of which illustrations may be seen in the *Electrician* of December, 1883. As is well known, the engine derives its name from the fact that, in its design, the attempt has been made to take all stresses through straight members, the frame thus being made to consist of two straight compression and thrust members, connecting the cylinder heads directly with the main pillow blocks, and giving a characteristic appearance to the whole machine. The valve gear is of the "positive" type, the expansion made variable by the introduction of a governor on the main shaft actuating the eccentric, in the manner familiar to all who have seen the more common forms of high-speed engines. In the design of this governor, as throughout the whole engine, special care has been taken to provide against the impeding action of friction, the machine being intended to be as nearly frictionless as possible. The engine rests upon three points of support, and thus is not liable to be thrown out of line by any inequalities of foundation or bolting. When tested, the engine to be experimented with was simply set on blocking, and had no foundation; but so well was it balanced, and so perfectly was its alignment maintained, that it ran with absolute smoothness, and as steadily as if it had been given the heaviest foundation possible.

METHOD OF ATTACHING THE INDICATORS.

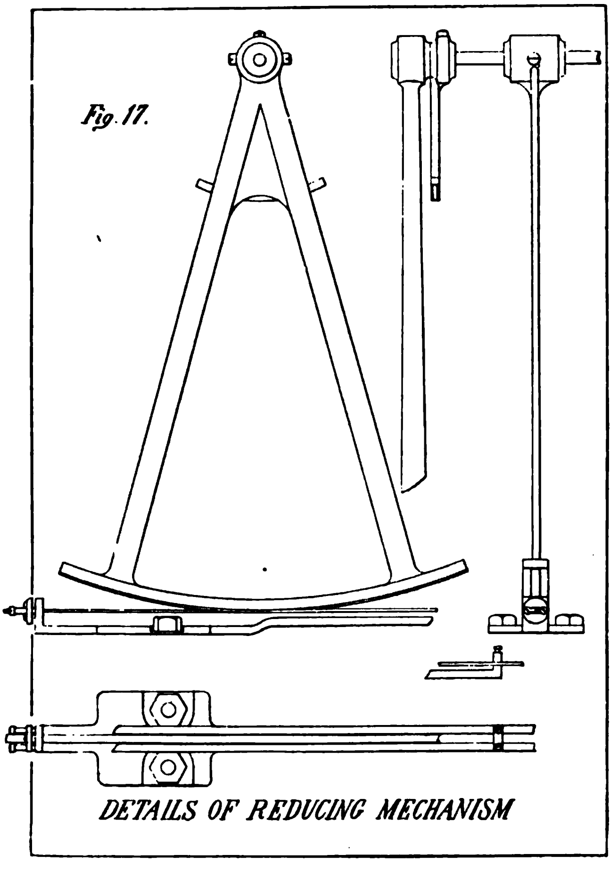
Fig 16



For the purposes of test, it was fitted with a pair of carefully standardized indicators and a Prony brake. Cards were taken simultaneously from both ends of the cylinder, and at the same instant readings from the brake were obtained. A comparison of the power indicated by the diagrams and that shown by the brake gave a difference which measured the friction of the engine. During the trial, the engine, when working at its rated power, consumed, according to the indications of the diagrams, 28.2 pounds of steam per horse-power per hour, or, probably, between

35 and 38 pounds, allowing for the loss by cylinder condensation, not accounted for on the indicator card, a very excellent performance for an engine of but 35 horse-power. The action of the governor was extraordinarily perfect. The engine was adjusted to make 230 revolutions per minute under 90 pounds steam pressure. The observers reported that it made the same number of turns whether loaded or unloaded, an evident impossibility with a governor of this class, in which only approximate isochronism can be attained. The writer, to settle the question, counted the revolutions, minute by minute, with a hand-speed counter, and made it 230 revolutions with the whole rated load on the engine (35 to 40 horse-power), and 231 when entirely unloaded, the brake-strap being loosened until it could be shaken about on the pulley, by the hand, with perfect ease. This was repeated until no

Fig. 17.



DETAILS OF REDUCING MECHANISM

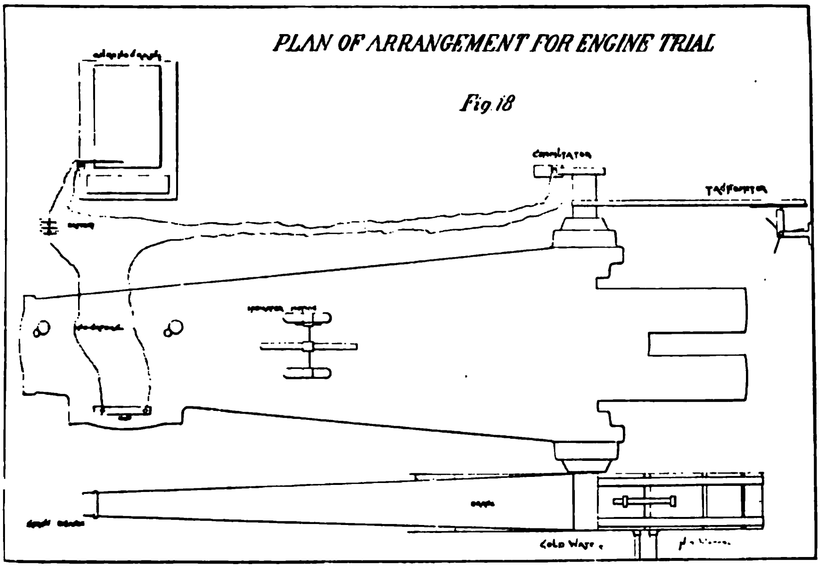
question could longer exist in regard to the matter. The variation with variable steam pressure was greater.

The following are the data obtained from the brake and indicator readings:

Number of Card.	Revolutions.	Steam Pressure.	Brake H. P.	Indicator H. P.	Diff.	Friction per cent.
1	232	50	4.06	7.41	3.35	45
2	229	65	4.98	7.58	2.60	34
3	230	63	6.00	10.00	4.00	40
4	230	69	7.00	10.27	3.29	32
5	230	73	8.10	11.75	3.65	32
6	230	77	9.00	12.70	3.70	29
7	230	75	10.00	14.02	4.02	28
8	230	80	11.00	14.78	5.78	25.5
9	230	80	12.00	15.17	3.17	21
10	230	85	13.00	15.96	2.96	18.5
11	230	75	14.00	16.86	2.86	17
12	230	70	15.00	17.80	2.80	15.75
13	231	72	20.1	22.07	2.06	9
14	230	75	25.00	28.31	3.36	11.75
15	229	60	29.55	33.04	3.16	9.5
16	229	58	34.86	37.20	2.34	6.3
17	229	70	39.85	43.04	3.19	7.4
18	230	85	45.00	47.79	2.75	5.8
19	230	90	50.00	52.60	2.60	4.9
20	230	85	55.00	57.54	2.54	4.4

This engine was 8 inches in diameter of cylinder, 14 inches stroke of piston, having a rod 44 inches long between centers, a balanced valve with stroke of 2 to 4 inches, according to position of governor and eccentric, a fly-wheel 50 inches in diameter, weighing 2,300 pounds, the steam and exhaust pipes having diameters of 2½ and 4 inches, respectively, and the whole machine weighing 2½ tons. The space occupied by the engine was 9 feet 4 inches in length, by 4 feet 8 inches in width, and 3 feet 10 inches in height.

Examining the above table of powers, it is seen that the difference between indicated and dynamometric power, the friction of the engine, varies somewhat with varying steam pressures and varying total power; but in such manner as to indicate the controlling cause to be irregular in action, and possibly to some extent due to errors of observation and to accident. The maximum is four horse-power, the minimum about two horse-power. The usual difference is about three and the variations are irregularly distributed throughout the whole range of experiments. It is evident at a glance that the law of De Pambour does not hold, and that it is as nearly correct to say that the friction of engine is constant as otherwise. The column of friction, as given in percentages of the total power, exhibits the same fact. There is continual, though somewhat irregular, reduction of the percentage of friction, throughout the range from the lowest to the highest power, and very nearly inversely as the power exerted. This is best shown by the curve given in the accompanying plate (Fig. 23), in which a smooth line has been drawn to represent as nearly as possible the mean of all observations. The power for which the engine is proportioned is 35 to 40 horse-power. At this power, the friction of engine is but about 6 per cent. of the total, or less than one-half that assumed by De Pambour, and accepted as correct by Rankine, for engines generally, and presumably for locomotives especially. The result is exceedingly gratifying, and seems to the writer extraordinary for so small an engine.



PLAN OF ARRANGEMENT FOR ENGINE TRIAL

Fig. 18

The repetition of the experiment upon an engine of another make, having a cylinder 9 inches in diameter and a stroke of piston of 12 inches, which would naturally give a somewhat increased percentage of friction, in consequence of the proportionally smaller stroke, at 20, 30, 50 and 65 horse-power, by brake, and running free, revolutions 300 per minute—a speed which may also have caused some increase in frictional resistance, not only in rubbing parts, but by increasing back pressure—gave a friction of engine measuring from 2.66 horse-power unloaded, to 4 horse-power at 20 to 30 horse-power, 4.8 horse-power at 50, and 5.3 at 65 horse-power, the total friction increasing perceptibly, as assumed by De Pambour, but decreasing in percentage of load, from 16 to 7.5, between 20 and 65 horse-power. It is very nearly constant throughout the whole range of power that the engine would be worked under ordinary circumstances, and may be so taken without serious error; while the adoption of the Pambour formula would give a value of f so small that its use would not be attended, ordinarily, with sufficient increased exactness to compensate for the additional trouble involved in its application. At their rated powers the two engines thus exhibit efficiencies of mechanism of about 94 and 90 per cent., respectively.

The second series of experiments were made* during the latter part of last college year, confirming the deductions already given, while some very interesting and original modifications were made in the details of method and trial. The engine taken for test was a machine recently built and sent to the Cornell University for purposes of experimental investigation in electrical measurement and other work of the college. It is an engine 7 inches in diameter of cylinder and 12 inches stroke, or, more exactly, 6⅞ inches in diameter; the cylinder having been bored slightly under size. The general plan of the engine is similar to the first of those already described, and, like that, is carefully designed with a view to reducing friction to a minimum, and giving a regulation of maximum efficiency. The brake was precisely like that used in the first described experiments, and was built for the engine constructed in the college workshops, under the direction of the inventor, and exhibited at the Centennial Exhibition in 1876. It was constructed by the Straight Line Engine Co., and adapted, with very little alteration, to the new engine. The indicators were carefully standardized and put in good order in every respect, by the makers, for the purposes of these investigations. The reducing mechanism used in connecting the indicator barrel to the cross-head of the engine was designed and built by the observers, and fitted with a very firm connecting arrangement, and with an ingenious detaching device. A sector was constructed which was pivoted above the cross-head, and hung in the vertical plane above the latter, the engine being horizontal. The arc of the sector carried a pair of steel ribbons, one attached to each end, each carried

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* By Messrs. W. A. Day and W. H. Riley, at Cornell University.

around the arc and secured, at its opposite end, to the end of a bar fastened on the cross-head, in such manner that, the two ends of the ribbons at the cross-head bar being well secured and tightly drawn up, by means of screws placed conveniently for the purpose, all back-lash was prevented, and an absolutely exact synchronism of movement of indicator line and cross-head was obtained. The engine was driven at 285 revolutions per minute, and it was therefore very important that this rigidity of connection should be secured. A smaller sector at the upper part of the larger one was the carrier of the cord, and the combination was thus a perfect means of reproducing the motion of the engine on the smaller scale required in working the paper barrel of the indicator. The "cord" was piano wire, a material much less liable to cause difficulty by stretching than any other that was available. Its free part was kept taut by a "spiral" (helical) spring, attached beyond the point of connection with the paper cylinder.

In the first of these experiments, as already described, Thompson indicators were used; in those about to be considered Crosby instruments. It was hoped that the new Tabor indicator could be used also, but none were received in time. The instruments used worked perfectly, and gave no trouble from beginning to end. The speed indicators were of several kinds. Hand instruments of two or three kinds were used to check the records of the automatic instruments. A "tachometer" was attached and belted to the engine shaft, and afforded a very convenient means of watching the momentary fluctuations due to variations of load, of steam pressure, and of accidental disturbances. A chronograph was also attached, connected with the standard clock in the physical laboratory, to beat seconds. A commutator was placed on the engine shaft, making contact at each revolution, and a key near the engine, for the purpose of breaking contact. A Brown mercury speed-indicator served excellently well for a constant speed-indicator. It exhibited instantly any variation of speed from the normal. The chronograph was set in operation when the indicator cards were taken, and thus gave the exact speed of the engine at that instant. Great care was taken to keep the instruments, and the engine as well, in good order and well lubricated throughout the series of experiments. Some stiffness of the governor, however, the cause of which was not discovered until after the work had been completed, caused it to work less perfectly than in the engine first used, and the speed varied more than in that series of determinations. When the governor was in its most perfect adjustment, the engine was capable of holding the standard speed within a fraction of one revolution throughout a wide range of work, and nearly down to the lowest power that such an engine is at all likely ever to be called upon to supply.

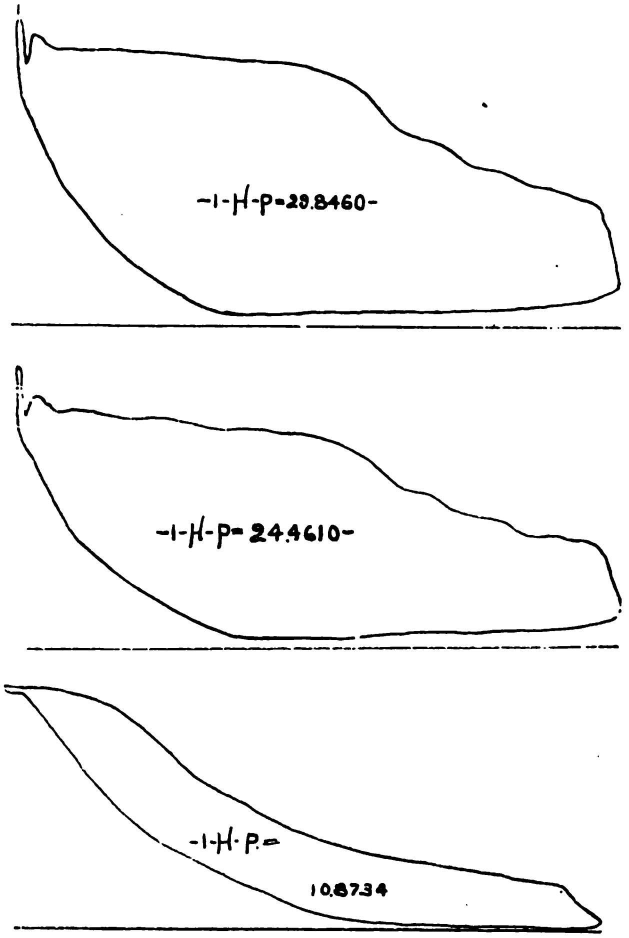

FIG. 19.

The mean effective pressure required to drive the engine alone, loaded and unloaded, throughout the whole range of the trials here made, was 4.55 pounds per inch of piston, and was nearly constant, as in the first investigation. The steam pressure usually ranged between 65 and 75 pounds per square inch at the steam chest, but, when it was desired to secure a card to be more easily worked up, the pressure was dropped to 20 pounds. A series of special experiments made to determine the question whether the friction of engine is variable with boiler pressure, although not in all

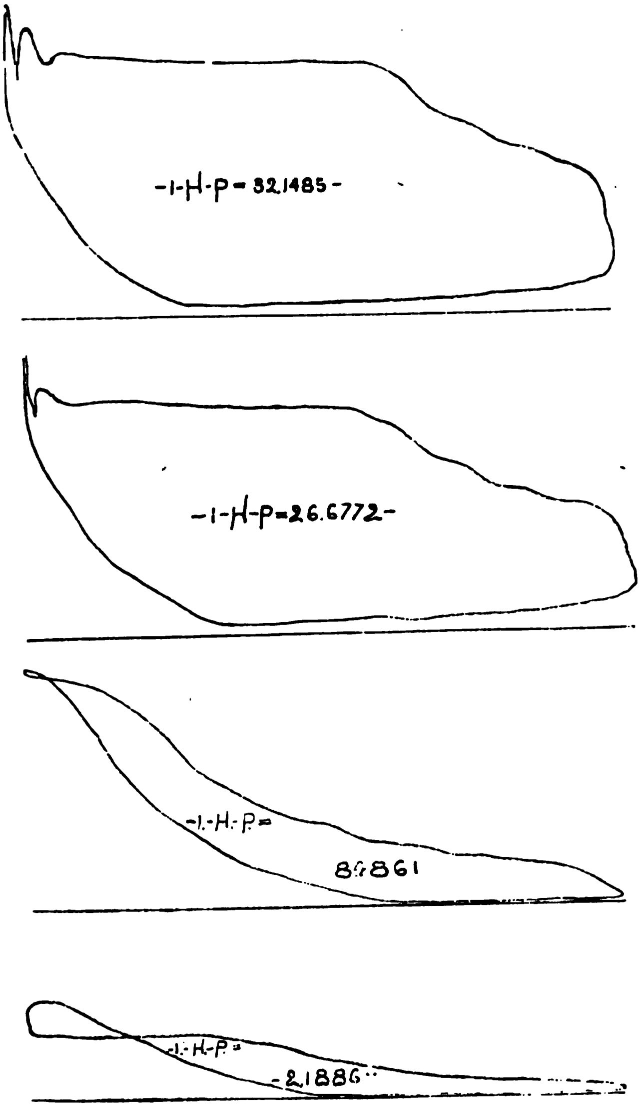

FIG. 20.

respects satisfactory, indicated a slight increase in engine friction as steam pressures rose. The conclusion already arrived at by the writer, as deduced from the work previously done, that the engine friction in this class of steam engine is constant, or sensibly so, under all loads is thus here again confirmed. The following are the data obtained, arranged as before, to exhibit the relations of the indicated to the dynamometric power:

1. No. of Card.	2. Rev. per Minute.	3. St. Press.	4. Brake Power. H. P.	5. Ind. H. PPI. per card.	6. Diff. Frict. H. P.	7. Mean F. Press.	8. Frict. per cent
1	282	19	0	2.26	2.26	3.70	100
2	288	65	4.87	8.43	3.56	5.56	42
3	286	66	7.61	10.95	3.33	5.25	30
4	284	65	10.80	12.98	2.89	4.18	20
5	285	71	13.10	15.99	2.61	4.25	18
6	284	76	15.80	18.79	2.99	4.71	16
7	284	74	18.55	20.73	2.65	4.18	12
8	280	67	21.00	23.73	2.73	4.37	11
9	279	65	23.61	25.95	2.33	3.78	9
10	280	75	26.39	29.95	2.36	5.38	11
11	280	72	29.03	32.22	3.19	5.15	10

The first glance at column 6 or at column 7 of the above table, in which the horse-power absorbed by the friction of the engine, and the mean effective pressure corresponding to that power are presented, shows that, as already concluded, the resistance of this class of engine at constant speed, is practically constant at all loads, and that the differences and irregularities observed are due to accidental causes. The variation of speed recorded here is in some cases due to differences of steam pressure, partly purposely produced, and partly coming of the fact that it was necessary to take steam as it could be obtained, and was impracticable to secure steady pressure, and in other instances was due to the fact, afterward discovered, that the governor had been adjusted in such manner as to be slightly cramped, and thus deprived of its wonderful sensitiveness and accuracy, as exhibited before this defect had been introduced, and after it had been remedied. Chronograph records, made later by Professor Anthony, exhibit the most extraordinary smoothness.

These variations of speed served the useful purpose of calling attention to the fact that the engine friction varied, at constant load and speed, with variation of steam pressure, and to a very noticeable amount, within the usual range of pressures met with in practice. It is seen that, in rising from 19 to 76 pounds steam pressure, the pressure demanded to give the engine its normal speed unloaded ranged from below 4 to above 5 pounds per square inch of area of piston, the pressure required in the cylinder rising, on the whole, though irregularly, as steam pressure rose. In order to determine whether this, which might prove to be a hitherto unobserved law, were true, the following data were obtained by a series of experiments made for the purpose of settling this new question.

No. of Card.	Rev.	St. Press.	I. H. P.	Mean Press.	Mean F. Press.	Per cent. Frict.	
1	250	25	6.01	10.84	1.95	18	Ten pounds on the brake.
2	271	39	6.52	10.85	2.71	27	
3	285	42	7.17	11.35	3.63	32	
4	280	46	7.08	11.60	3.59	31	
5	271	58	6.81	11.28	3.16	28	
6	289	63	7.85	12.25	4.65	38	
7	286	68	7.77	12.25	4.90	40	
8	283	77	7.88	12.47	3.74	38	
9	296	82	7.87	12.00	4.68	39	
10	275	71	2.10	3.46	3 46	100	No load on the brakes.
11	279	66½	1.995	3.22	3.22	"	
12	277	44	1.708	2.78	2.78	"	
13	275	35	1.71	2.80	2.80	"	
14	275	30	1.613	2.64	2.64	"	
15	272	25	1.876	3.11	3.11	"	
16	270	19	1.724	2.88	2.88	"	
17	270	15	1.712	2.86	2.86	"	

In the first set of experiments, here numbered 1 to 9, inclusive, the weight on the brake arm was kept constant at ten pounds; in the remaining experiments all weight was removed. In both sets, the same general effect is seen. As the steam pressure rises, the speed being the same and the resistance the same, the friction of the engine increases; from 2 pounds, at 25 pounds pressure in the steam chest, to nearly five pounds per square inch of piston at the maximum, 82 pounds steam in the valve-chest. As the steam pressure fell from this point to 15 pounds, in experiments 9 to 17, the load being thrown off entirely, and the speed being nearly constant, the mean pressure, measuring the friction, of engine, falls again below 3 pounds per square inch of piston. The difference is considerably less in the last series than in the first; which apparent discrepancy is accounted for by the fact that the variation of steam pressure in the first series was accompanied by a greater change of speed of engine than in the second. The resistance is seen to increase slowly, therefore, with increase in speed of rotation. The effect of change of pressure is, in these cases, more marked than that of alteration of velocity of the engine.

The accompanying illustrations show the apparatus and exhibit the facts revealed by the investigations which have now been described better than can the text. Fig. 15 shows the method of attaching the indicators, with an elevation of the engine cylinder and section at the cross-head; Fig. 16 exhibits the same arrangement in plan; Fig. 17 gives an enlarged view of the reducing mechanism and attachment to the cross-head; Fig. 18 is an outline plan of engine and surroundings, exhibiting the location of instruments; and Figs. 19 and 20 represent characteristic diagrams obtained by means of the indicator, showing the variations of steam distribution with variations of load on the brake. All these illustrations refer to the work of later date. Figs. 21 and 22 are given to exhibit the method of variation of mean friction pressures with variation of load, the variation of the percentage of friction resistance as a fraction of total resistance with varying loads, for the last investigation; and, for comparison, the same ratios, as obtained in the work done at the American Institute Exhibition, are given in Fig. 23. These last curves are seen to be approximately hyperbolic; while the first given is a straight line. The originals of these curves were carefully plotted by Messrs. Day and Riley, from the records of original observations, and beautifully represent the laws which it was the object of these investigations to reveal and establish.

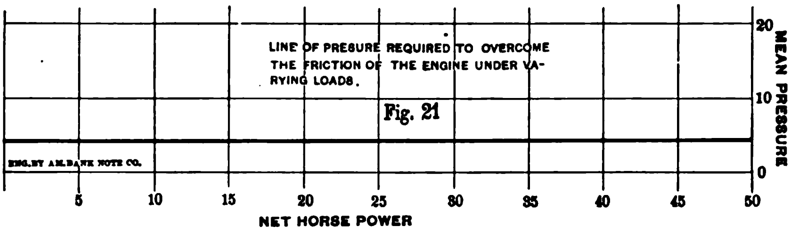

Fig. 21

After a survey of this work, it may be asked, How does it happen that rise in steam pressure produces evident increase of the frictional resistance of the engine? It was long ago shown by the writer, and is now well established by many independent investigations, that, with good lubrication, increase of pressure on a journal gives decreased co-efficients of friction, and this would seem to show that the friction of engines in which the resistance caused by friction is mainly due to journals and lubricated surfaces, should become less as pressures increase, the useful load and the speed of engine remaining constant. This query is a very natural one, and is based upon a correct statement of fact, however inconsistent it may seem to be with the results above derived. The cause of the apparent discrepancy is attributable, probably, to the variation produced by the action of the governor in the distribution of steam. It will be seen that

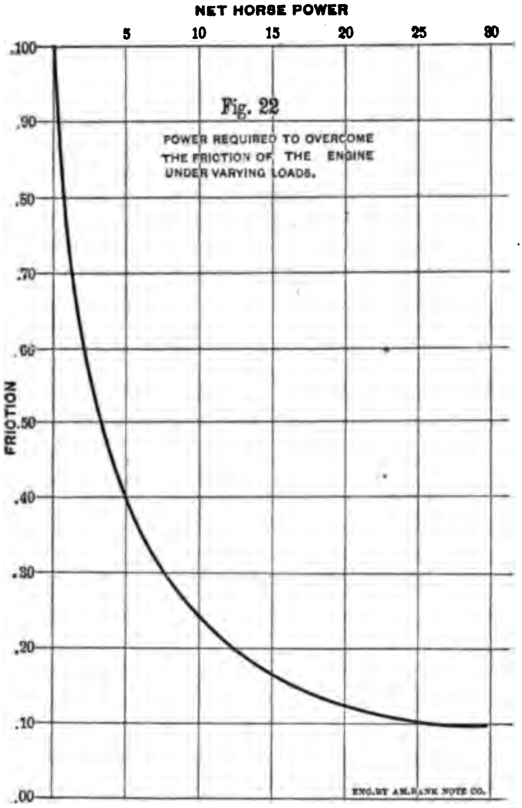
Fig. 22

POWER REQUIRED TO OVERCOME THE FRICTION OF THE ENGINE UNDER VARYING LOADS.

the effect of increase of steam pressure is to cause acceleration of speed of engine, a change essential to produce the action of the governor at all, and that it results in the readjustment of the set of the valve in such manner as to cause the greater proportion of the nearly constant amount of work performed to be done more nearly at the commencement of the stroke, at a point in the orbit of the crank-pin at which the work is mainly lost by friction, and to reduce the proportion of total work done at or near the "half-center," where it is principally useful. The proportion of useful to lost work is thus varied in such manner as to give a mean final result which is the less favorable as the steam pressure is higher, and the cut-off shorter, giving a higher ratio of expansion. It is also evident that, if this explanation is correct, the difference here noted will be less as the point of cut-off approaches and passes the half-stroke position of piston and cross-head. Could the valve be set with negative lead for all positions at the point of cut-off, as is considered right by some experienced engineers, the work would be more nearly performed at positions removed from the "dead points," and the variation here described would be thus reduced, while the efficiency of the engine would be increased.

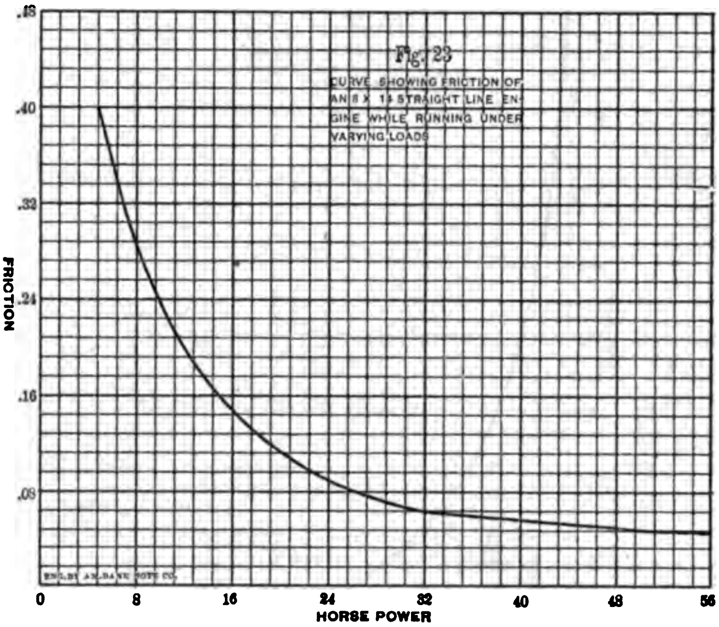

Fig. 23

CURVE SHOWING FRICTION OF AN 8 X 13 STRAIGHT LINE ENGINE WHILE RUNNING UNDER VARYING LOADS

Professor Rankine proposed the formula,

$$R = R_1 (1 - f) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

This formula is evidently inadmissible, at least for the class of engine which was made the subject of the experiments which have been here described. Since the friction of engine is, so far as can be here seen, sensibly independent of the magnitude of the load and of the resistance produced by it, the correct formula would seem to be:

$$R = R_1 + R_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

the total resistance met at the piston being the sum of the resistance of the engine itself and that of the load, both being determinable, both being independent, and being governed by entirely different laws.

The conclusions to be drawn from what has preceded are obviously the following:

(1.) The friction of the non-condensing engine, of the class here described, is sensibly constant at any given speed, at all loads, and is at different speeds entirely independent of the magnitude of the load.

(2.) The friction of engines, of the type described, is variable with variation of speed of engine, increasing as speed increases, in some ratio as yet undetermined, but probably different with every engine, and, for the same engine, with every change of conditions of operation.

(3.) The friction of engines increases with increase of steam pressure, in the case of the class here referred to, in a probably similarly variable manner with that observed with alteration of speed, neither method of variation being capable of representation by any convenient algebraic expression.

(4.) The total resistance, measured at the piston of the engine, is composed of two parts, the one sensibly constant at the working speed, the other variable with external load, and may be, for practical purposes, at least, represented by the expression,

$$R = R_1 - R_0,$$

in which R is the total resistance, as shown on the indicator diagram, R_1 the resistance due to the external load; *e. g.*, as measured by a Prony brake, and R_0 the resistance of the unloaded engine, as shown by a "friction card" taken with the steam-engine indicator.

It is sufficiently obvious that these conclusions are, at present at least, only certainly applicable to one class of engine. It is not improbable that the condensing engine may be subject to quite different laws. It is to be hoped that this question may be settled by direct experiment at an early day. The custom has obtained, hitherto, of allowing a certain pressure per square inch of piston as the equivalent of the friction resistance of the engine in marine practice—this pressure being taken at from 2½ pounds in the case of engines of moderate size, to 1½ with the largest engines. It has never yet been ascertained whether, or to what extent, the friction of engine is augmented by the imposition of load. The assumed figure represents from 5 to 10 per cent., usually, of the total indicated power of the engine. Isherwood has taken 7½ per cent. of the useful load as the amount of increase of friction of engine due to its action. This estimate is stated to be made on the basis of the data given by General Morin, whose coefficients for friction of lubricated surfaces are now known to be enormously larger than those customarily met with in practice in well lubricated journals of large size working under heavy pressures. In such cases, when the surfaces are in good order, the coefficient is known to fall to below 1 per cent., instead of being from 3 to 5, as given by Morin, as determined under the different conditions of his experiments. Where the journals are not well lubricated, and especially when they are rough or cut by abrasion, friction may increase enormously and may pass far beyond the figures given by Morin even; but such exceptional conditions cannot be taken into account to establish laws for application in design, or in good practice. For all cases in which the friction varies, as in the examples here above illustrated, the "friction card" sensibly represents the correct tare, whether the engine be loaded or unloaded.

A word in explanation of the fact here shown, that the increased load thrown upon the shaft, crank-pin, and cross-head journals does not noticeably increase the friction of engine, will be considered not out of place here. The friction of engine consists of the resistances due to the motion of the various piston, valve, and other rods through stuffing boxes and in guides, the friction of the piston rings on the cylinder surface, the friction of eccentrics, and often other parts, which are independent of the magnitude of the load thrown upon the engine by the useful resistance, in addition to the friction of the journals transmitting the effort of the steam to the exterior resisting work, and of the cross-head guides and other parts indirectly affected by its variation. It thus happens that the resistance due to the friction of the latter may be, and often is, but a small proportion of the whole friction of engine. The total friction of engine, as has been seen, in engines of the class here studied, and of the sizes described, amounts to about 10 per cent. of the total power developed when fully loaded; but the coefficient of friction of any one journal, if well lubricated, has been found by the writer, by hundreds of experiments, under such pressures as are usual on the main journals of the steam engine, to fall below 1 per cent., and the absorption of work and energy is thus a still lower proportion of the work of the steam in proportion as the speed of rubbing is less than that of the piston. The loss of power along the line of connection is thus exceedingly small. It should never exceed probably 2 per cent. of the work done, or between 10 and 20 per cent. of the total friction. Again: the coefficient of friction, within the usual range of pressures on these journals and the guides, with good lubrication, increases rapidly as pressures fall, and decreases as greatly when the pressures increase with variation of engine power and load, and this often occurs so rapidly that the total frictional resistance, on these parts, even, varies very slowly with variation of load; while the friction of the other portions of the engine, above mentioned, remains quite constant. The resultant effect is, as shown by the investigation here described, a practically constant friction of engine under all loads, the speed and steam pressure being constant. Whether this is true of condensing engines is doubtful, and it would be an important extension of this research could similar investigations be made of the friction of other forms, and especially the marine steam engine and pumping engines.

DISCUSSION.

Mr. George H. Barrus.—Prof. Thurston's paper touches a very interesting and important subject, and furnishes a valuable contribution to steam-engineering data.

I wish to offer some figures obtained on my tests of a 45 HP. Westinghouse engine running at a speed of 350 revolutions per minute, which gave results of a similar character.

The indicated horse-power developed by the engine, when running unloaded, was 6.1 HP. The difference between the brake HP. and indicated HP., when the latter was 44.5, was 6.1.; when the indicated HP. was 34.6 the difference was 5.6, and when the indicated HP. was 25.3 the difference was 6.3.

The following table presents similar results obtained on a Corliss engine in France, reported in the *Bulletin de la Société Industrielle de Mulhouse*, December, 1878:

	Indicated Horse Power.	Brake Horse Power.	Difference.
Unloaded	12.2	0.	12.2
Loaded	72.8	60.4	12.4
Do.	101.9	89.7	12.1
Do.	119.5	106.0	13.9
Do.	133.3	121.5	11.9
Do.	145.4	133.	12.4
Do.	158.4	144.8	13 6

Prof. R. H. Thurston.—Two or three points come up in the paper here which are of particular interest to me. One is the variation of the friction-resistance with speed of engine. In this instance it was shown very clearly that as the speed varied the resistance varied, and the variation of total resistance of engine was, in such cases, greater than the variation due to the imposition of the load; that is to say, speeding that engine up, under a constant load, from 275 to 300 revolutions, an appreciable increase of resistance would occur, of such importance as might modify the commercial economy of the engine to a very observable extent.

Then another thing came out: that the method of distribution of steam affects the friction of the engine vastly more than the magnitude of load. Assuming the two cases again for comparison: we take a friction card from the engine; then take a card with full load, and we do not find very much difference; we may even find the friction in the second case smaller than in the first. Now, if we drive the same load with low steam, and again drive the same load with high steam, we at once produce a difference in the method of distribution of steam in the engine. In the first case the mean pressure approximates more closely to the maximum pressure. We carry out the boiler pressure farther in the stroke; we cut off "longer" than in the other case, where, with high steam pressure, we cut off short, and expand very considerably, and get a greater difference between the maximum pressure in the cylinder and the mean pressure. In these two cases we find a marked difference in the amount of friction in the engine. With low steam, the friction of engine was very considerably less than with high steam, and I have presumed this difference to be accounted for by the fact that where we carry a high steam pressure and cut off short, the high pressure takes effect on the engine as it passes—or very soon after passing—the centers, and while the crank is sweeping through a large arc, and a considerable amount of work is done in friction, while a small amount is done in actually moving the load. Thus, by that method of variation we may change the method of distribution of friction of engine, and change its gross amount to such an extent that there we may find that, by carrying high steam, we may go beyond the point that represents maximum economy, by the simple fact of introducing this wide variation of steam pressure.

I would add to this the suggestion that members may find it of interest to note the point which it is desired to bring out with special clearness: that in every engine the friction of the engine does not consist solely, or even in large part, of the friction of rotating parts and that produced by pressure upon those parts; it consists of friction at several points varying in kind, and in method of variation, or in law, and it is the sum of these that we measure up when we take a friction card from an engine. Now, some of these are absolutely constant. The friction of the piston rings against the interior of the cylinder is often a very considerable proportion of the total friction, and that friction, where there is no leak past the rings, is probably constant under all loads, but perhaps not at all speeds. The friction that varies with a load is simply the friction of journals under pressure due to the load. For example, the main shaft journals not only carry the weight of the fly-wheel and the pull of the belt, but also a certain amount of pressure due to the transmission of work through them. The crank-pin takes a certain amount of friction due to the pressure of the crank-pin, due to the connecting rod operating on the journal, and that of course will change in magnitude with variations in load; and the same is true of the wrist-pin at the cross-head. But all the other journals of the engine, and all the other parts of the engine that move, are subject to other laws, simply because they do not transmit load. So that it is extremely possible that all these other frictions taken together may have a constant value under all conditions; while the other quantity which De Pambour mentions, is a quantity which, if measured, we find to be comparatively small; and in a well-proportioned engine, one with parts well lubricated, you will find that the friction of the main shaft journals, the friction of the crank, the wrist-pin and the slides, taken together, make a comparatively small fraction of the total friction of the engine. Consequently we might have—assuming that friction to vary as assumed by De Pambour, and by writers subsequent to him—we might have a variation under that law that nevertheless should not affect the total friction so greatly as to become practically important. Now, the fact is, that although this friction does exist, when we put more load on the engine, increasing the pressure on all these journals, the coefficient of friction goes down while the pressure goes up. The coefficient decreases continually and tends to reduce the total friction, and the result of these two variations in contrary directions is to produce such an effect, that it was supposed by one gentleman who has spoken to me about it, to result in a neutralization of the two opposing elements, and in constant frictional resistance under these variable conditions as to pressure and load. The fact is, usually, that an increase of total friction does occur to a slight extent: that is to say, the decrease of the coefficient of friction does not occur to so great an extent as the increase of pressure, and consequently there is a slight, and in these cases, a variable amount of added friction in consequence of the increase of load on the engine. It is easy to see that such cases may arise, and that the increased friction of engine, due to this varying load, may be overlooked entirely in our treatment of the engine, and the engine of a particular make may show that assumption to be a perfectly fair one; but it does not follow that this would be true of another engine that we may not have experimented with. I presume it is true for condensing engines, where we are handling a large amount of water subject to the laws of fluid resistance.

Mr. Wm. Kent.—It seems to me that experiments in the direction of reducing friction ought to be largely in the direction of trying to reduce the friction in the cylinder; as we have now determined that in these engines at least the friction is a constant quantity, the probability is that the largest portion of that friction is due to pushing the piston through the cylinder. I think experiments should be made in the direction of reducing that friction by determining the kind and size of rings and the method of pressing out these rings.

Mr. Geo. I. Alden.—I would simply say that this question was considered at the Free Institute at Worcester by one of the students in the last class, and the results, so far as we could judge of them from the limited experiments made, were in the same direction, namely, that the friction was substantially constant with the various loads.

Mr. A. R. Wolff.—It seems to me that the general practice of engineers in the measurement of power is of itself testimony to the general correctness of the fact that with the same initial pressure and the same rate of revolution, the friction of engine is almost independent of the load that is put upon it. Of course in the measurement of power it is a common practice to take the card with the engine loaded, and afterwards to take the "friction card" with the same initial pressure and the same rate of revolution. If there were any great variation in the friction of engine, if loaded or not, or if it had been considered of late years that any great variation existed, it seems to me that the course of power measurement ordinarily pursued would have been entirely unjustifiable. But on this very account I consider it of importance and value that we have had special experiments made by Prof. Thurston exactly in that line to corroborate the practice which has existed in the past few years.

Mr. H. R. Towne.—There is one point which Prof. Thurston's remarks on the difference in the distribution of steam suggests to me; there will probably be a very considerable difference in the lubrication of the main journals, in one case, under constant conditions of pressure, and, in the other case, subject to intermittent conditions of pressure. In the latter case there is a liability to partial or complete expulsion of the lubricant from between the two surfaces during the moment of high pressure, and a probable increase, therefore, of the sum total of friction beyond what there would be in the previous case.

Professor Thurston.—I presume, Mr. President, that the effect of a variable compression would be to modify somewhat the distribution of frictions in the engine, and the final result; but, from the fact that in this whole range of experiments we have not been able to produce any appreciable variation in the total friction of engine, I should say that that change, whatever it might be, would not be an important one. It is a matter that should be determined by direct experiment. I do not feel able or willing to say positively what would be the difference in the two cases. I should perhaps expect that a variable compression would produce a variable friction; it might be studied in comparison with another form of engine. It is a matter that cannot be settled positively, except by direct experiment made especially to determine that point.

In regard to another case, that where a balanced valve is used, of course it does to some extent make a difference in the character of the friction and its magnitude. With an unbalanced valve, or a partially balanced valve, such as we had here, the total friction of valve is less of course in the case of the balanced than of the unbalanced valve, but the nature of the friction is precisely the same; and so far as the law of variation of engine is concerned I imagine the fact is not important. If it has any importance, it is in the direction of the production of constancy of friction under all loads at a constant speed and still greater variations of friction under all variations of steam pressure. I have not much doubt that the friction of the common slide valve, as ordinarily operated, is an appreciable quantity in measuring up the total resistance of engine.

I said that the law of the condensing engine might be, and that I presumed it would be, different from that of the non-condensing engine, referring, of course, to the ordinary forms of condensing engine. Where there is an independent air, or circulating pump, I should expect that the engine itself would go on about its business without really being aware whether it had a condenser or not, and would be subject to the same laws as any non-condensing engine; but, actually, the friction of all these details should be taken in as a part of the friction of the engine. Those parts certainly will be subject to those laws which govern resistance due to motion of fluids and the introduction into the engine, whether by an independent system or by an attached system, of air pump or circulating pump, would introduce these new resistances—resistances following different laws from those found in the non-condensing engine; and I should expect, there, that the law of fluid friction, the resistance varying as the square of the velocities through the pipes and through the valves, would become so important a matter as to produce a difference that would be quite perceptible in the method of variation of total resistance. We know that the load upon an ordinary condensing engine, in the operation of its own air pump, and of its circulating pump, is a very severe tax; it is a quantity which must be variable with speed of engine. With constant speed of engine under variable load, as in a stationary engine driving a mill, and subject to the action of the regulator, you would have another variation due to the fact that with the same apparatus, the same set of valves, the same pumps, the engine is using different quantities of condensing water, and consequently the rate of flow through the pumps and pipes is continually varying. If one-half the power is used, one-half the amount of condensing water will pass through this system; and that means half the speed of flow through the pipes and pumps; and that means one-quarter as much resistance due to their friction. Thus, although the direct train of mechanism from piston to crank shaft would be subject to exactly the same laws as in the other case, the machine as a whole, in which we should include air pumps and circulating pumps, whether independent or not, would be subject, I presume, to a different law, and to a law which would, perhaps, be more exactly expressed by De Pambour's statement.

Mr. John T. Hawkins.—In the experiments given, it would be well, I think, if they were extended so as to cover some of the varying conditions under which such engines run, both in practice and under experimental tests, such as these, which would, I think, be somewhat likely to modify the results obtained.

It would appear probable, for instance, that the action of the Prony brake, when applied with the arm in the horizontal position and its effort vertically downward, would be to lift the main shaft in its bearings as the load is increased, and thus diminish the friction in its journals; and, in fact, this action of the brake might be such as to cause, not only an entire release of pressure of the shaft upon the lower half, at a certain load, but to cause pressure upward at a still greater load. With the brake-arm vertical, and its effort exerted horizontally at the end of the arm, the result of increased load would be to increase proportionally the horizontal pressure in these same journals without affecting the vertical pressure. Somewhat similar varying conditions might, I think, possibly result from main driving belts leading in different directions, vertically upward or downward, or in horizontal directions, or at certain angles. This, however, is not so clear.

Again, it is very certain that the friction of the cross-head guides is no very insignificant fraction of the total friction of a horizontal direct-acting engine; and equally palpable that, where an engine is running under—in which case the pressure upon the guides due to the angularity of the connecting rod is upward—the friction due to the weight of the parts must be obliterated before any pressure will be exerted upon the lower side of the guide; in other words, the weight of the parts compensates to that extent for what would otherwise be friction on the under side of the guides; while, if running over, increase of load would increase the friction at the guides proportionally to the pressure on their upper side.

It would, therefore, appear to be certain, for instance, that, in an engine running under, and in which the power was absorbed by a Prony brake with horizontal arm, the friction of the load would vary in a different way from one in which it was run over with the brake-arm vertical. The quantity by which it would be varied by these conditions can hardly be so insignificant as to warrant their being neglected in any investigation of this kind, but is probably of sufficient magnitude to render the first conclusion incorrect for one, if correct for the other set of conditions; although the figures in column 6, on the fifth page of this paper, varying, as they do, from 2.06 to 5.78 HP., do so sufficiently and irregularly enough, perhaps, to include as much or more than would come from the conditions above described. For the above reasons, therefore, I am inclined to believe that the conclusion No. 1 of the paper would not be substantiated under more exhaustive experiments, although qualified as it is by the word "sensible," and that we could hardly expect to set aside Pambour's long-accepted conclusions without more exhaustive experiments covering all such variations in the circumstances under which the engines run, as I have attempted to explain.

Prof. R. H. Thurston.—Referring to the remarks of Mr. Hawkins calling attention to the effect of the brake in adding to the pressure on the main journals, and thus influencing the results of the tests as reported, I would simply suggest that a quantitative statement be worked out, of the precise amount of this effect for the case reported. It will, I think, be found that the pressure to which he calls attention amounts to but one pound to the horse-power, and when, as is usual in all my work or work done under my instructions, the load is measured by the action of the end of the brake-lever upon platform scales, the effect is simply to counteract more or less completely the weight of the brake-strap on the pulley, and that the net effect is too utterly insignificant to be noticed. Even where the brake-lever pulls upward on a spring balance, as is often the case, the effect is too minute to affect such results as are here reported. Were the effect observable, it would produce a change in the direction indicated by De Pambour, and the absence of such observed effect is simply more strongly corroborative of the conclusions obtained from the trials described in the paper. With such proportions of brake as were here used, and with such as are customarily employed, no such action as is suggested by the last speaker would ever be in the remotest degree approximated. The position of the brake-arm and the weight acting at its end are of no importance whatever here. The action, or the results obtained, would not have been affected observably, I am sure, by any change in position of brake-lever.

I should expect the same to be true of the action of friction on the guides. The weight of parts, and the friction there, are too small in amount, too insignificant, to affect the final results appreciably, no matter whether the engine runs " over " or " under." It must be remembered that the friction there is never, in well-proportioned and well-lubricated engines, more than a small fraction of that supposed by De Pambour; instead of five per cent. or something like it, as has been assumed by those who have accepted Morin's coefficients, it is, or ought to be, in all cases of smooth-running and well-oiled surfaces, inside of one per cent.; while the pressure at that point is a very small fraction of the piston pressure, unless the length of rod be extraordinarily small. In the engine on which this work was done, the rod was of good length, and the surfaces as frictionless as possible. Again, about all the engines in use run "over," as did this engine, and, even were the results likely to be affected by such difference of adjustment, the conclusions here obtained would probably be applicable to ninety per cent. of all such engines in use—perhaps to ninety-five, or more, per cent.

While I agree fully with the speaker in thinking that we should derive great advantage, as I have myself suggested, from extended and repeated investigations of this kind, I doubt, I entirely disbelieve, that the conditions to which he calls attention will be found to affect the conclusions already reached, to any important extent.

Since writing* the paper which has been presented to the Society, my attention has been attracted to a paper by Professor Robinson, in the first volume of the Transactions of the Society,† on the "Efficiency of the Crank," where I find a discussion which bears directly upon the question of variation of engine friction with change of load at normal speeds, and confirms the suggestion, made by me in the paper referred to, that the reason of the variation of friction exhibited with change of steam pressures, while no such variation is observable with varying loads at constant pressure, may be partly the difference in method of distribution of work in the cylinder. It was suggested that the concentration

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* Contributed after adjournment, under the rules.

† Vol. I., p. 231, Trans. A S. M. E.

of the work at the beginning of the stroke, at the higher pressure, and its distribution more completely throughout the stroke at low pressures, may be one cause of the peculiar behavior noticed.

On consulting Professor Robinson's paper, page 9, a table will be found giving the efficiency of the crank, *i. e.*, the percentage of work passing it, and the proportion of work absorbed by it at various expansions. Taking the value of the coefficient of friction at one per cent. which the writer has stated in the original paper on friction of engine to be probably a fair maximum figure for machinery in good order, and for average pressures and lubrication, that table becomes the following:

EFFICIENCY OF CRANK.

Values of $\frac{A + a}{2r}$.

Cut-off.	0.1.	0.2	0.3.	0.4.	0.5.	1.0.
0.04	1 − 0.0047	1 − .0094	1 − .0141	1 − .0188	1 − .0235	1 − .0471
0.05	1 − .0044	1 − .0089	1 − .0133	1 − .0177	1 − .0222	1 − .0444
0.06	1 − .0042	1 − .0085	1 − .0127	1 − .0170	1 − .0212	1 − .0424
0.08	1 − .0040	1 − .0079	1 − .0119	1 − .0159	1 − .0198	1 − .0397
0.10	1 − .0038	1 − .0075	1 − .0113	1 − .0151	1 − .0188	1 − .0377
0.20	1 − .0033	1 − .0066	1 − .0099	1 − .0133	1 − .0165	1 − .0331
0.40	1 − .0031	1 − .0061	1 − .0092	1 − .0123	1 − .0153	1 − .0307
0.60	1 − .0031	1 − .0061	1 − .0092	1 − .0124	1 − .0154	1 − .0308
0.80	1 − .0034	1 − .0067	1 − .0111	1 − .0135	1 − .0168	1 − .0337
1.00	1 − .0032	1 − .0062	1 − .0094	1 − .0126	1 − .0157	1 − .0314

The above table, it should be noted, is prepared for the case of the *condensing* engine, and the variation in the distribution of work throughout the stroke, with varying expansion, is here very much less than in the case of the non-condensing engine. The variation of efficiency with change of steam pressures, in the case actually studied, is thus more marked than is here shown. Nevertheless, it is seen that, in this case, even, the proportion of work absorbed by the crank is very variable, amounting to about fifty per cent. more at the shortest cut-off than at full stroke. The relation $\frac{A + a}{2r}$ is the ratio of the sum of the radii of crank and main journals to the diameter of the circle described by the pin, or to the stroke of piston. In short-stroke engines, as screw engines, its value may be usually taken as about 0.5, and in long-stroke engines, as side-wheel marine engines, or mill engines, of slow speed of rotation, at about 0.2 on the average.

It is interesting to observe the difference in the proportions of work wasted on the crank in the extreme cases, as well as with variation of steam distribution. The percentage of power lost at the crank is small in all cases; but it is, even in condensing engines, ten times as great with the engine of shortest stroke as with that having the longest stroke in proportion to size of crank-pin and shaft. In the engine of longest stroke, it is but one-third of one per cent. at common ratios of expansion; while, in the machine of shortest stroke, it is three per cent. The maximum waste occurs at a cut-off taking place at about three-quarters stroke. For all engines of usual proportions, it may be assumed that the loss at the crank-pin should not exceed from two-thirds to one per cent., at ordinary ratios of expansion. Probably the waste at the several journals transmitting the pressure from piston to belt or driving-wheel may be there taken at about three times that on the crank, or not far from two or three, possibly five, per cent., and varying, as shown above, with variation of the distribution of steam. The whole variation is comparatively unimportant, however, and less than the writer would have anticipated. In non-condensing engines the loss is greater and vastly more variable.

is customary to gauge the water level and depth of cut to inches, and to set the buckets to cut six inches below the depth to which the channel is afterward tested. So accurately is this done with chain-bucket machines that vessels regularly pass with but a few inches between their keels and the bottom.

The digging action of the chain buckets takes place in a manner calculated to apply the power to the best advantage, the cutting edges entering horizontally and then curving upward on a short radius, so as to fill the buckets and retain the contents. They are also able to take off light cuts over large areas as efficiently as heavy cuts.

The entire apparatus is in equilibrium as to the weight of the working parts, power being required to elevate the dredged material only, and to overcome the digging and frictional resistances; whereas, in a dipper or grapple dredge the power expended in lifting the bucket and its attachments is lost at every stroke. This loss is quite considerable, and in a deep-water machine is frequently equal to raising a weight of several tons to a height of 35 or 40 feet at each lift. In regard to the expenditure of power in a chain-bucket machine, it appears from indicator cards taken by the writer from the engines of several machines, that about 226,000 to 353,000 foot-pounds or 6 to 10¾ horse-power is required per cubic yard of material actually discharged when in effective work. Looking at a single-bucket dredge in a similiar manner and working under similar circumstances, it is found to take 106 revolutions of an engine 14 × 16 with 75 to 90 pounds of boiler pressure, to deliver one bucketful of material of about 2½ cubic yards. This at 30 pounds mean pressure in cylinder is equal to 1,305,920 foot-pounds or about 15.8 horse-power per cubic yard. These are actual examples, both workin tolerably yielding material, and in about 32 feet of water. Different cases will vary considerably; but the writer thinks that, under ordinary circumstances, it may be assumed, that a dredge in which a single bucket is worked from a crane or boom, requires about double the power for the same work as a well-designed chain dredge.

It thus appears that the chain-bucket system is good in principle, and also good in practice, as its extensive use testifies, notwithstanding the mechanical difficulties to be overcome, in order to obtain simplicity and durability. These difficulties are not, however, confined to this class of machinery exclusively. Those experienced in the operation and maintenance of dredging machinery of whatever kind, do not need to be told of the almost incessant repairs made necessary by the severity of the duty, and by the wear and tear produced by conditions which are favorable to the rapid depreciation of the machinery.

Gradually, and by costly experience, are the weak points eliminated and the efficiency increased, until a machine is produced which can be counted on with tolerable certainty, to do the work required of it. By such a process have the structural details of the endless-chain machine crystallized into something like a system, varied of course to suit different requirements.

In considering these details, the construction of the chain of buckets claims first attention. It is of obvious importance to keep the weight of these buckets at a minimum while retaining the required strength. With many forms of chain, the tension caused by its own weight constitutes so large a proportion of the total working stress, that only a small balance is available for doing the work. The degree to which strength and lightness can be combined depends first, on the tensile strength of the metal employed, and then upon a skillful distribution of that metal. In the earlier forms, many examples of which are still in use, the endless chain consists of a series of stout wrought-iron links connected by joint-pins, the shell or body of a bucket being riveted to every alternate set of links. There are usually four links under and forming part of a bucket, and two intermediate connecting links, the eyes of which are bushed with steel. Such a bucket and one of the links are shown in Figs. 24, 25 and 26, which are drawn from actual measurement, and may be taken as a fair example of an extensively

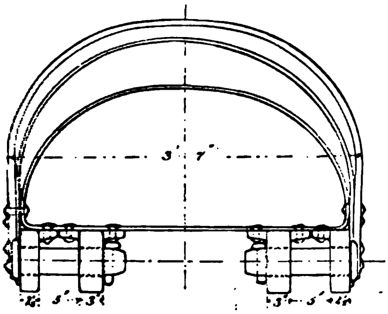


Fig. 24.

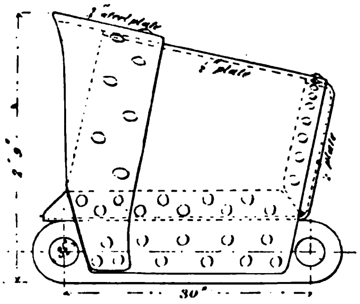


Fig. 25.

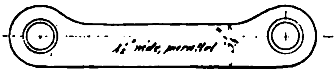


Fig. 26

used bucket. A chain of buckets constructed in this manner will do good and rapid work in moderately yielding material, while they can be kept at it; but the wear is excessive on account of the small and roughly fitted bearing surfaces of the joints, and the grinding action of sand and water. There being very little stiffness to this kind of bucket the springing of the plates and hard knocks tend to loosen the rivets, and about six months' work suffices to get the bucket ready for the repair shop.

Official returns of the performance of an extensive dredging plant in another locality shows, by a statement of repairs, the average life of a bucket of the kind described above to be 146 days, of a connecting link 77¼ days, and of a joint-pin 102 days, these being the average intervals of service before requiring removal for repairs. They will, of course, stand a limited number of such repairs before becoming finally useless.

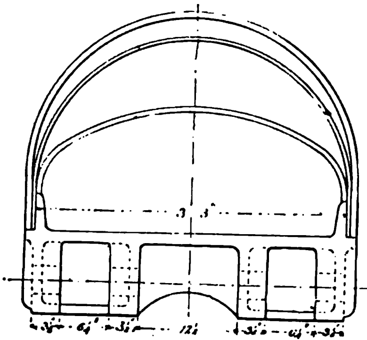


Fig. 27.

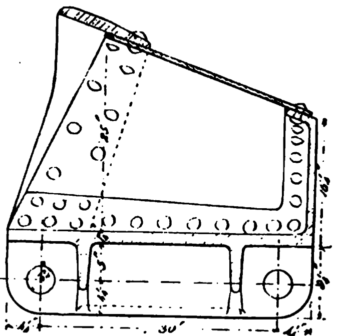


Fig. 28.

With the recent development of the art of making sound and tough steel castings, new possibilities were opened up, and the four links of a bucket are now made in a connected form of a single steel casting, to which the top or body is riveted. A bucket of this kind is shown in Figs. 27 and 28. The construction is thus greatly simplified, and much of the difficulty caused by the shaking loose of a number of riveted parts is obviated.

More recently still it has been shown that the entire bucket can be advantageously formed of a single steel casting. There still remained, however, the destructive and costly wear of the bushes, pins and links used in connecting the buckets. Increasing the bearing surfaces and improving the standard of workmanship does not remedy the evil, as the amount of metal worn away with a sand and water lubricant, is nearly the same; but being distributed over a larger surface, takes a little longer time, and it did not seem to pay to put good work on joints which would not stay good.

In Figs. 30 to 33 is shown a bucket* of somewhat different design, but which follows out still further the direction indicated by the previous example. In this bucket the endeavor is made to combine the elements of lightness, strength, capacity and durability to a degree not heretofore reached. Durability of the pin connections is obtained by providing large wearing surfaces, from which the sand and grit are excluded by a special form of self-expanding packing ring. These wearing surfaces are then lubricated by positive feed from a grease chamber formed in the bore of the tubular joint-pin, and which is arranged to be readily charged from the outside.

Fig. 30.

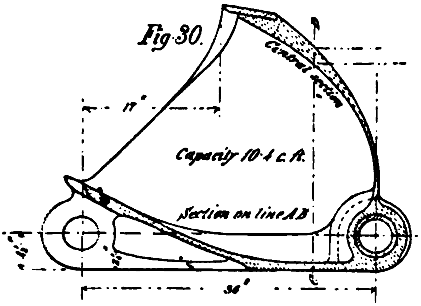
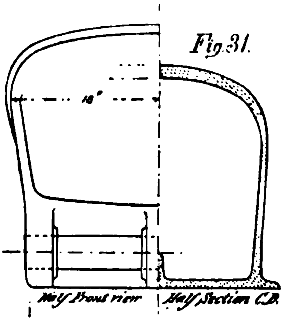


Fig. 31.



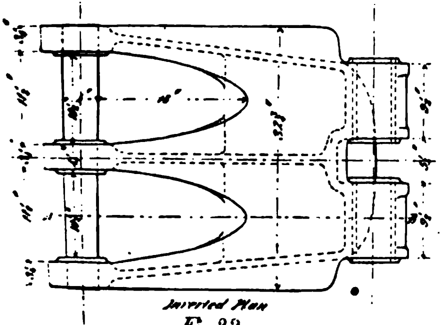


Fig. 32.

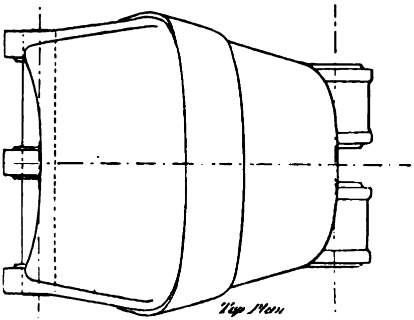


Fig. 33.

The bucket is composed of cast steel in one piece, with renewable steel cutting edges and wearing surfaces, and so proportioned that the safe working strength is in excess of the maximum tension

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* Patented.

that can be applied, so that the body of the bucket is practically indestructible.

It will be seen on reference to Fig. 29, in which several buckets embodying these features are shown in position upon the upper or driving tumbler, that they differ from all others, both in general form and in the fact that no intermediate links are employed to to connect them, as they are coupled directly to each other. They are adapted to work on an improved driving tumbler, in which the rear end of the bucket is supported by projections, forming an extension of the driving face of the tumbler. The links of the chain, in passing over the tumbler as commonly made in the form of a simple polygon, were subjected to severe bending strains, caused by the eyes of the links overhanging the corner of the tumbler. The links thus required to be made of greater weight and strength than was necessary to withstand the mere tension. Considerable wear also occurred between the tumbler faces and the links; the corners of the former becoming rounded and increasing the liability to breakage of the chain. With the improved tumbler, having an extended seat on which the bucket bears for its whole width and length, the bending stresses are reduced so that the principal stress is reduced to a simple tension of the chain. The form of the bucket also permits of an enormous tensile strain being provided

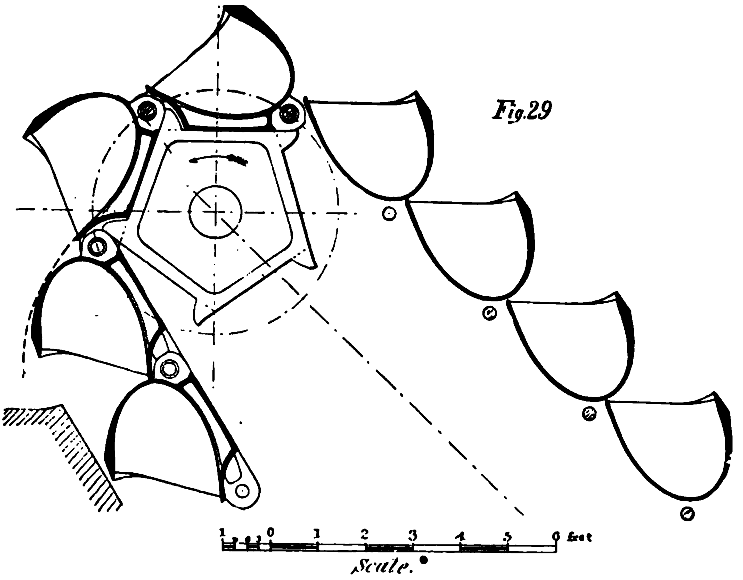
Fig.29

for without unduly increasing the weight; as the whole back, sides and bottom of the bucket, constitute a link of great strength, and excellent form to resist both tensile and bending stresses.

It is of the highest importance to reduce the number of parts as far as possible. There are few mechanical appliances which are exposed to as rough treatment as dredge buckets, and it is conceived that the best way to prevent them shaking to pieces, is to make them with no pieces to shake.

The elevator system has always been an attractive one to inventors, and the Patent Office contains the record of many curious devices applied to dredging and excavating, some of which are modeled after a flour-mill elevator, and many employing a pitch chain composed of a multitude of small jointed links, with other devices equally unsuccessful in practical work. Many of these have never been heard of since, and some, which have been heard of, have by reason of the flimsiness of their construction, served to extend the popular notion that an elevator machine may possibly be made to dig sand or mud; but for stones and hard material is entirely impracticable.

The writer believes this to be a misapprehension of the capabilities of the system, as it has been successfully demonstrated that "hard pan," slate, shale or other schistose rock can be economically dredged from its natural bed with a properly adapted chain-bucket machine. In dredging of this nature, the principal work of the buckets is to break up the material, and for this purpose they are made of great strength and fitted with steel teeth of special form. The action thus resembles a series of powerful pickaxes delivering rapidly repeated blows. It will be seen that in work of this kind, a special advantage is derived from the doubling of the buckets, that is, constructing the chain wholly of buckets, instead of half the number connected by links, the digging effect being thus doubled.

Buckets for heavy dredging purposes, made entirely of cast steel, were first used in this country, to the writer's knowledge, about four years ago, upon the works with which he was connected. These have since been doing the severest kind of work, tearing up shale rock from its natural bed in 30 to 36 feet of water, and in a current of 4 to 6 miles per hour; and, with the exception of some wear upon the renewable portions, are practically as good to-day as at first.

There must inevitably be deterioration of the wearing surfaces of the buckets and connections, but while this is enormous with the older forms, and appreciably reduced in the steel buckets referred to, by a judicious increase of bearing surface, it can only be minimized by keeping the abrading substance out of the joints, and supplying lubricant, as is done in the improved buckets shown in Figs. 30 to 33, designed by the writer.

For the purpose of comparing the three forms of bucket which have been illustrated, the following table of proportions and capacity is appended:

	No. 1.	No. 2.	No. 3.
Maximum depth to which buckets can work .... ft.	30	31	30
Number of buckets in chain.......................	40	34	46
Capacity of each bucket .. ..cu. ft.	12	15	10.4
Pitch of each bucket ....ins.	30	30	39
Weight of bucket-chain per foot run..................lbs.	480	623	440
Weight of bucket-chain per foot of capacity.......... lbs.	200	208	138
Total weight of chain..............................tons	43	47	29
Safe working tension of chain.......................tons	47	50	45
Wearing surface of pin-joints....................sq. ins.	31.5	42	75
Maximum pressure on pin-joints...........lbs. per sq. in.	3,340	2,660	1,340
Ordinary speed of buckets................ ...ft. per min.	60	70	180
Discharging capacity....................cu. ft. per min.	144	200	416
Discharging capacity cubic yards per day of 10 hours.....	3,200	4,400	9,200

Nos. 1 and 2 being examples from British practice, it may be of interest to give some further particulars respecting the vessels and their performance.

The vessel referred to as No. 1 is 161 feet long, 29 feet beam, and 10 feet depth of hold; and is fitted with the wrought-iron buckets shown in Figs. 24 to 26. In one season of 2,694 working hours, this machine dredged 323,760 cubic yards at a cost of $9,987, or $3\frac{8}{100}$ cents per cubic yard.

The second vessel is 130 feet long, 32 feet beam, and 10 feet 6 inches depth of hold. In the first six months' work this machine dredged 409,500 cubic yards in 139 working days, at a cost of $9,309, or $2\frac{27}{100}$ cents per cubic yard. The material was mud and clay with some gravel and limestone marl, with a considerable number of snags and roots, and the cost includes wages, fuel, stores, repairs and maintenance, but not towing or depositing the material dredged.

It will be seen from the table that the weight of the bucket-chain in column No. 3 (as illustrated in Figs. 30 to 33), while of equal strength is considerably less per foot than No. 1 or No. 2. The increase of capacity and larger wearing surfaces should also be noted, together with the fact that a reduced pressure per square inch is obtained in protected and lubricated joint connections, instead of a heavy pressure in a joint exposed to the grinding action of sand and water.

With the improved buckets and simplified construction, the chain-bucket machine is capable of still higher results than those stated above, as much of the difficulty labored under from excessive weight and wear and tear is done away with, and a machine produced which is cheaper in first cost, because the weight is less, and cheaper to maintain, because more durable.

Fig. 34 shows a type of machine adapted for channel work, and represents a dredge having a capacity of 6,000 cubic yards per day, and capable of dredging to a depth of 25 feet. The mode of working is shown in Fig. 36. The hull is moored in position by anchor chains controlled by a steam winch, and the buckets feed sideways over the bottom for the width of the channel to be made, advancing a short distance ahead at every cut by hauling in on the bow-chain. In some cases a spud anchor-post is used at the stern for mooring the hull, the buckets at the front end taking a radial cut about the spud as a center. A machine of this kind is represented in Fig. 35.

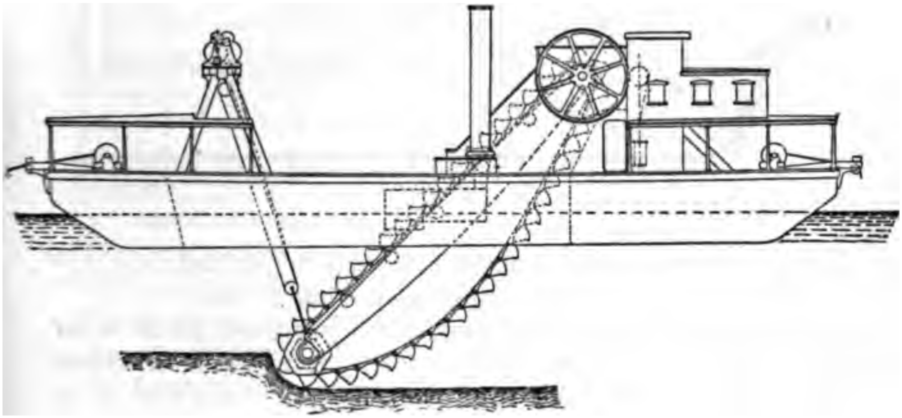

FIG. 34.

The range of application of the system extends to almost every kind of under-water excavation, and it is as well adapted for canal dredging as for channel dredging. A machine for the former class of work is arranged to cut in front, so as to excavate its own floating water, and to form the banks with the dredged material at one operation. In the case of a combination machine designed by the writer, the material is deposited on the bank by a continuous discharging apparatus, which allows the excess of water to drain off before depositing, thus making a more solid embankment, and avoiding the washing back of the material into the cut.

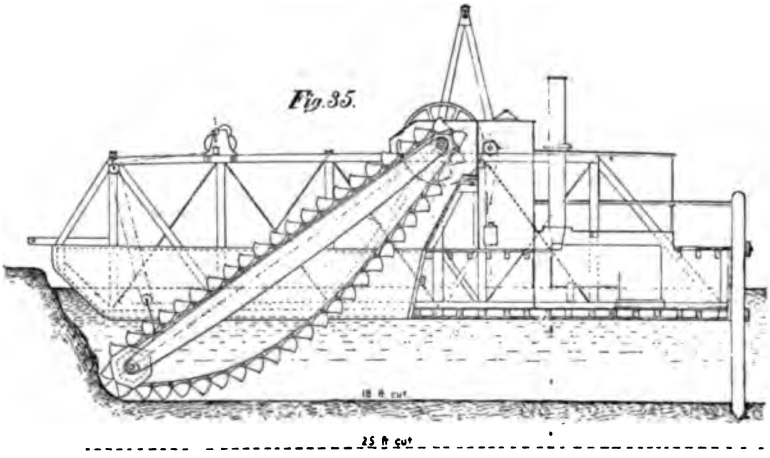
Fig. 35.

In considering the cost of these machines, regard should be had not only to the first cost, but to the cost of maintenance in relation to the capacity, and to the extent and duration of the work to be done. One elevator machine, with a capacity of 6,000 cubic yards per day, will do better and cheaper work than four other dredges with a capacity of 1,500 cubic yards each, if employed on the same work, although the former may cost twice as much to build and run as one of the latter. Where the work is such that the machines can be kept employed without great or frequent periods of idleness,

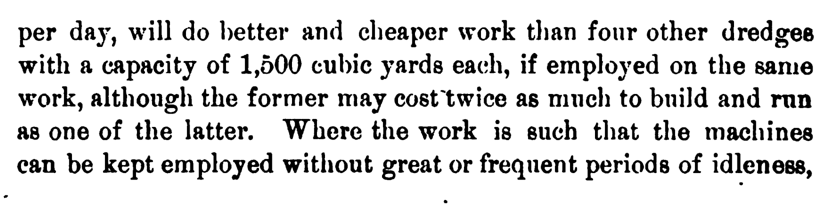
Fig. 36.

the first cost becomes a very small factor in the total cost of doing the work.

In many cases concentration of plant is desirable, as reducing the working and other expenses. This tendency is illustrated by the fact that a large proportion of British machines are now built as "Hopper dredgers," that is, a combined self-propelling dredge and scow, so that one vessel constitutes the complete plant. These vessels can dredge a load of 800 to 1,000 tons in a few hours, and then steam off at 8 or 9 miles per hour and dump it.

In conclusion, the writer has sought to present briefly the leading characteristics of the chain-bucket system of dredging and excavating, together with the defects and difficulties met with in making the system a success, both mechanically and commercially. He has also pointed out the direction in which improvements are being made in order to obviate these defects, and so far as he has gone he has endeavored to increase the general efficiency, and to construct a chain of buckets that will be durable.

In view of the national and public works, in which dredging and excavating form a considerable part, it is of importance that the subject should be more fully understood, and the fact appreciated, that the cost of such work is largely governed by the efficiency of the plant employed.

The writer has not attempted to go into the many details, which go to make up a successful endless-chain dredging machine; such as arrangement of hull, engines, and subsidiary gear, systems of framing, etc.; as they are capable of being adapted to a variety of conditions, and a description of which would exceed the limits of this paper.

DISCUSSION.

Mr. A. W. Robinson.—The greatest wear and tear of such a machine takes place, of course, in the hinge connections. This is one great reason why this form of machine has not been introduced into this country to the extent to which it has been used elsewhere. The fact that endless-chain machines are in use in other places, and wearing out so fast as they do, and are able to do satisfactory work, or at least economical work, in that way, shows, I think, that if we take away the obvious defects and deficiencies, we ought to have a pretty good machine.

The packing rings are made of moulded rubber; they are made open, so as to be removable, with the ends made to fold into one [illegible] ring which has a [illegible] in position, one [illegible] hammer [illegible] and retain it in position. The diameter of the pin on which these packings are used will vary with the size of the machine. For an ordinary heavy [illegible] the pin will be from 3½ to 4½ inches in diameter with a little extra [illegible]; the ring might be from 6 to [illegible] inches in outside diameter.

The details of the construction of the pin connections are not as clearly shown in the illustrations of the paper as I could wish. [illegible] a small piston fits into the bore, and [illegible] at the end of the pin. The piston can be made to force [illegible] lubricant as in an ordinary grease-cup.

The grooves in which the packing rings are situated are half round, and the packing rings are formed with tapering sides, expanding [illegible] so that when they occupy the groove, their sides are [illegible] hugging its sides, and any slight end-motion, which will [illegible] will not allow any dirt to enter the joint. This feature of protecting and lubricating the joints is one of the most important [illegible] of this kind.

Mr. [illegible] Hall.—Some years ago I had occasion, to some extent, to handle the problem of which this paper treats, out in the alluvial deposits of Idaho. With your permission I will illustrate. As I presume you are all aware, the country called the Snake River [illegible] of Idaho, is composed of an alluvial deposit extending for about 400 miles along, and varying from half a mile to 13 miles wide, and, as near as I was able to learn while engaged out there, [illegible] an even distribution of flour gold. It has been the dream of a great many, both smart and stupid men, to get that gold. I was engaged by a company who tried to get it by placing [illegible] out there, and in my attempts I started some of what are ordinarily called hydraulic machines. They were composed of [illegible] cabinets of copper plates with sluices and cradles, etc., [illegible] run-ways for the water to pass over. I represented two companies, distinct, though composed of the same capitalists, and one of them had for its object the solution of the problem by handling the dirt in a dry state, and adding the water afterward. The company possessed a bank twenty-six feet high from the water level, extending for four miles down the banks of the Snake River, and we had to pump from the Snake River the water we wanted to wash this bank off with. So I started as a man would start in a hay field to mow. I constructed a dredging machine twelve feet wide, carrying eight chains of buckets, such as the gentleman just referred to, composed of heavy chain made of cast-iron links. As I was dealing with dry material, I did not wish to use any lubricant, because I should not be able to exclude the fine dust which would be present in the bank. I therefore used wooden blocks in the joints of the chain and malleable iron buckets very much like a tea-cup, ten inches across the top. These buckets I armed with steel prods, one on each bucket. I never had two prods on one bucket, because the rocks would be caught between the prods and prevent penetration; but by putting them at different places on the different buckets I succeeded in digging the bank, which was so hard that it was difficult to pick in some parts. I was very much impressed with the idea that dredging could be very advantageously done on that principle. I did not in a practical way accomplish a large amount. I could dredge the material well enough; I could wash it well enough; but after I had it washed I found that I could scarcely get enough to pay for the trouble. The machine I had out there had a capacity of about fifteen hundred tons of material for ten hours, with a twelve horse power engine lifting it eight feet high. Of course, I cannot speak now of its efficiency as a regular dredge. I am simply speaking of the idea which I had and carried out of cutting a swath from the side of the bank, washing it on the copper plates and carrying it from the copper plates still higher, and throwing it upon the bank or into the river. The system was intended to go right on down the bank, cut that twenty-six feet of bank down and dump it into the river; and when we had dumped into the river what we could reach, then simply lifting it upon the bank. As I said before, I did not carry it out far enough to give me any data as to the desirability of that means for deep dredging; but I should say it would be a very satisfactory way of dealing with the question in many cases.

I would further say, that instead of using packing rings of any kind, or attempting in any way to guard against the entrance of dust, I used wooden bushings in all the boxes of my machinery, and I think there is a great deal less wear by using wood than by using any metal. Of course there will be a great deal of squealing and all that sort of thing that one must endure, but it does not cut the shaft as metal boxes would do.

Mr. H. R. Towne.—For more than twenty years I happen to have had a great deal of familiarity with dredging machinery, and to have had some connection with one of the larger dredging companies in this country; the subject is therefore of great interest to me. The endless bucket dredge has always been more or less used in England; never largely used here. There was one used on the Delaware River twenty-five years ago, but it never worked efficiently, and was abandoned. The statement Mr. Robinson has made as to the need of improvement of details in this matter, probably accounts, to a great extent, for the failure of the few machines of this kind that have been tried here. Obviously, with the endless bucket dredge, the working parts are carried clear down to the bottom, where they are exposed to mud, sand, and grit, and are liable to rapid deterioration accordingly; whereas, with the grapple dredge of ordinary construction, the parts that have to move upon themselves are in the upper part of the bucket, and their motions are slow as compared with those of the endless-chain dredge. But for all that there are reasons, in my opinion, why the dredge on this system of an endless chain of buckets has its limitations of use, and I question whether it will ever displace the dredges of other construction for much of the work that is to be done. For its best efficiency a machine of this kind requires large size. Much of the work that we have to do about our cities is small in each item, even if large in the aggregate; dredges are required to go into docks and slips, and to clean out the accumulation of mud there, and in my opinion, for that purpose, the grapple or bucket dredge has advantages over the endless-chain system.

I question somewhat the correctness of Mr. Robinson's assumption, that the cost of lifting the work is so widely different in the two systems as he states it to be. I think he is in error in this respect, although I admit the correctness of the theory stated in his paper, that the cost should differ because the parts are balanced here while they are unbalanced in the other machine. His figures of the actual horse-power required in this case, and my knowledge of the actual horse-power used in the other machines, are such as to show that the difference is not so great as theory would seem to indicate.

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ground included by the various other types of dredging apparatus, and that as a marine dredge it reaches its best efficiency when the volume of material is large, and the space not too confined. In its improved form it is, however, being used in many other ways for special purposes, such as embankment and levee building, dry excavation, drainage canals, irrigating ditches, and street trenching for gas and water pipe.

With regard to Mr. Towne's remarks as to the relative expenditure of power in the clam-shell or grapple and the chain-bucket dredge, it is freely admitted that there are examples of the former that in very favorable work will do better than the single-bucket machine instanced in the paper, and which he probably had in his mind. There are also better machines of the chain-bucket class, and the comparison was made between two actual examples, not under the most favorable conditions, but fairly representative of their average work.

In regard to the wooden bearings in the machine, referred to by Mr. Hill, it appears to be a special case. Soft bearings do not wear the pin much, but such bearings require frequent renewal. The mere cost of renewing the worn parts, even in steel, would be comparatively small were it not for the cost of the delay caused in replacing them. Consequently the case is only satisfactorily met by a construction which will be reasonably permanent and durable under the extremely heavy service which such machinery is called upon to perform.

CCXXX.

STRENGTH OF SHAFTING SUBJECTED TO BOTH TWISTING AND BENDING.

AN ACCOUNT OF THE WORK DONE UPON THIS SUBJECT IN THE LABORATORY OF APPLIED MECHANICS OF THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY.

BY GAETANO LANZA, BOSTON, MASS.

(Member of the Society.)

THE rules used by mechanical engineers to determine the strength of shafting are based upon experiments on simple torsion or on bending only, and no experimental work has been done, so far as the writer knows, upon a combination of the two. Nevertheless it is a fact that by far the greater number of shafts in use are subjected to a combination of twisting and bending, the former being due to the work transmitted, and the latter to the pull of the belts in the case of head or line shafting, or to the pressure on the crank-pin in the case of crank-shafts, or the shock of the water in the case of propeller shafts.

Under certain circumstances the bending may have the greatest influence, while the twisting may be predominant in others, or their influence may be equally divided. Which of these is the case will depend upon the location of the hangers and of the pulleys, the width of the belts, etc., etc.

As to the formulæ which take into account both twisting and bending, there are two, both of which are based upon the theory of elasticity. The first, which is the most correct from a theoretical point of view, is that given by Grashof and other writers on the theory of elasticity, and is

$$f = \frac{r}{I}\left\{ \frac{m-1}{2m} M_1 + \frac{m+1}{2m}\sqrt{M_1^2 + M_2^2} \right\},$$

where

M_1 = greatest bending moment;
M_2 = greatest twisting moment;

r = external radius of shaft;
I = moment of inertia of section about a diameter;
f = greatest allowable stress at outside fiber;
m = a constant depending on the nature of the material.

If, as is usually done for iron and steel, we make $m = 4$ we obtain

$$f = \frac{r}{I}\left\{\tfrac{3}{8} M_1 + \tfrac{5}{8}\sqrt{M_1^2 + M_2^2}\right\}.$$

The other formula, which is also based upon the theory of elasticity, but which is not as correct, is that given by Rankine, and is

$$f = \frac{r}{2I}\left\{M_1 + \sqrt{M_1^2 + M_2^2}\right\}.$$

While these formulæ are the only ones that take both twisting and bending into account, they are not very generally used, being recommended by some engineers for use in crank shafts and propeller shafts of marine engines, while they are seldom used elsewhere.

The formulæ in general use for line shafting and head shafting are most commonly those taking into account the twisting only, and the constants for these formulæ are those deduced from experiments upon pure torsion. Of this nature are the following formulæ given by Professor Thurston:

For head shafts well supported against springing,

Wrought iron, $d = \sqrt[3]{\frac{125\,HP}{N}}$. Cold rolled iron, $d = \sqrt[3]{\frac{75\,HP}{N}}$.

For line shafting, hangers 8 feet apart,

Wrought iron, $d = \sqrt[3]{\frac{90\,HP}{N}}$. Cold rolled iron, $d = \sqrt[3]{\frac{55\,HP}{N}}$.

For transmission simply, no pulleys,

Wrought iron, $d = \sqrt[3]{\frac{62.5\,HP}{N}}$. Cold rolled iron, $d = \sqrt[3]{\frac{35\,HP}{N}}$.

All such rules are based upon the torsion only, and the different constants are determined by using a factor of safety to make up for of the buckets and connections, but while this is enormous with the older forms, and appreciably reduced in the steel buckets referred to, by a judicious increase of bearing surface, it can only be minimized by keeping the abrading substance out of the joints, and supplying lubricant, as is done in the improved buckets shown in Figs. 30 to 33, designed by the writer.

For the purpose of comparing the three forms of bucket which have been illustrated, the following table of proportions and capacity is appended:

	No. 1.	No. 2.	No. 3.
Maximum depth to which buckets can work .... ft.	30	31	30
Number of buckets in chain.......................	40	34	46
Capacity of each bucket .. ..cu. ft.	12	15	10.4
Pitch of each bucket ....ins.	30	30	39
Weight of bucket-chain per foot run..................lbs.	480	628	440
Weight of bucket-chain per foot of capacity.......... lbs.	200	208	138
Total weight of chain ....tons	43	47	29
Safe working tension of chain.......................tons	47	50	45
Wearing surface of pin-joints.....................sq. ins.	31.5	42	75
Maximum pressure on pin-joints...........lbs. per sq. in.	3,340	2,660	1,340
Ordinary speed of buckets................. ...ft. per min.	60	70	180
Discharging capacity.....................cu. ft. per min.	144	200	416
Discharging capacity cubic yards per day of 10 hours.....	3,200	4,400	9,200

Nos. 1 and 2 being examples from British practice, it may be of interest to give some further particulars respecting the vessels and their performance.

The vessel referred to as No. 1 is 161 feet long, 29 feet beam, and 10 feet depth of hold; and is fitted with the wrought-iron buckets shown in Figs. 24 to 26. In one season of 2,694 working hours, this machine dredged 323,760 cubic yards at a cost of $9,987, or $3\frac{8}{100}$ cents per cubic yard.

The second vessel is 130 feet long, 32 feet beam, and 10 feet 6 inches depth of hold. In the first six months' work this machine dredged 409,500 cubic yards in 139 working days, at a cost of $9,309, or $2\frac{27}{100}$ cents per cubic yard. The material was mud and clay with some gravel and limestone marl, with a considerable number of snags and roots, and the cost includes wages, fuel, stores, repairs and maintenance, but not towing or depositing the material dredged.

It will be seen from the table that the weight of the bucket-chain in column No. 3 (as illustrated in Figs. 30 to 33), while of equal strength is considerably less per foot than No. 1 or No. 2. The increase of capacity and larger wearing surfaces should also be noted, together with the fact that a reduced pressure per square inch is obtained in protected and lubricated joint connections, instead of a heavy pressure in a joint exposed to the grinding action of sand and water.

With the improved buckets and simplified construction, the chain-bucket machine is capable of still higher results than those stated above, as much of the difficulty labored under from excessive weight and wear and tear is done away with, and a machine produced which is cheaper in first cost, because the weight is less, and cheaper to maintain, because more durable.

Fig. 34 shows a type of machine adapted for channel work, and represents a dredge having a capacity of 6,000 cubic yards per day, and capable of dredging to a depth of 25 feet. The mode of working is shown in Fig. 36. The hull is moored in position by anchor chains controlled by a steam winch, and the buckets feed sideways over the bottom for the width of the channel to be made, advancing a short distance ahead at every cut by hauling in on the bow-chain. In some cases a spud anchor-post is used at the stern for mooring the hull, the buckets at the front end taking a radial cut about the spud as a center. A machine of this kind is represented in Fig. 35.

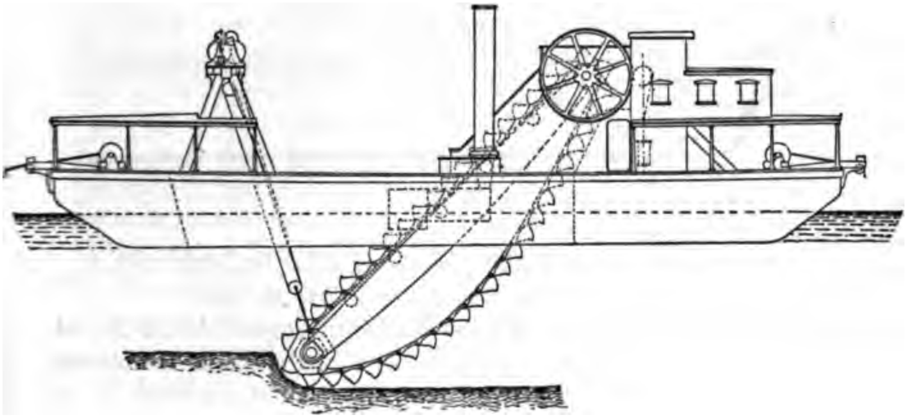


FIG. 34.

The range of application of the system extends to almost every kind of under-water excavation, and it is as well adapted for canal dredging as for channel dredging. A machine for the former class of work is arranged to cut in front, so as to excavate its own floating water, and to form the banks with the dredged material at one operation. In the case of a combination machine designed by the writer, the material is deposited on the bank by a continuous discharging apparatus, which allows the excess of water to drain off before depositing, thus making a more solid embankment, and avoiding the washing back of the material into the cut.

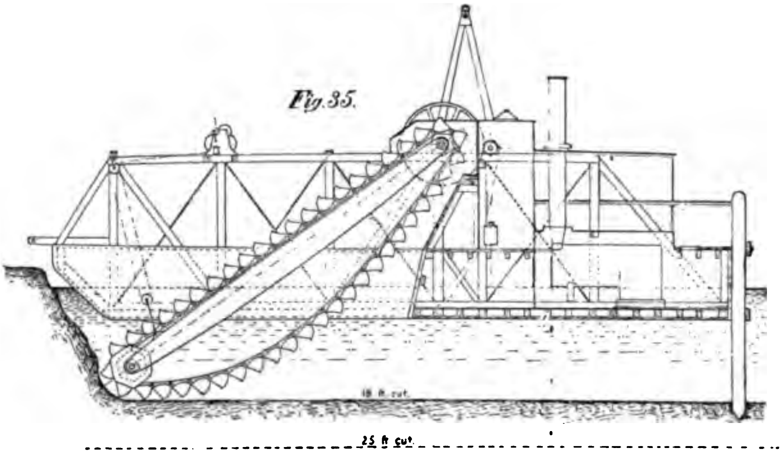
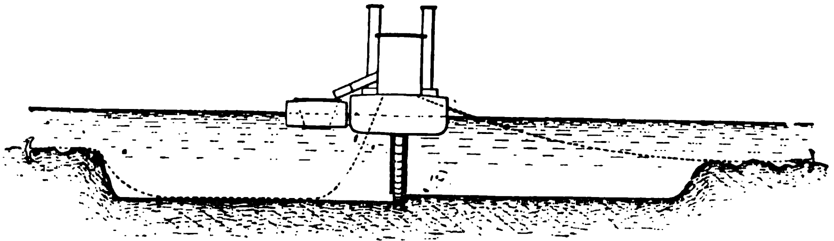


Fig. 35.

In considering the cost of these machines, regard should be had not only to the first cost, but to the cost of maintenance in relation to the capacity, and to the extent and duration of the work to be done. One elevator machine, with a capacity of 6,000 cubic yards per day, will do better and cheaper work than four other dredges with a capacity of 1,500 cubic yards each, if employed on the same work, although the former may cost twice as much to build and run as one of the latter. Where the work is such that the machines can be kept employed without great or frequent periods of idleness,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the first cost becomes a very small factor in the total cost of doing the work.

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DISCUSSION.

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The packing rings are made of moulded rubber; they are made open, so as to be removable, with the ends made to fold into one another, and each ring is held in place by a brass ring which has a hook and eye so formed in it that when it is placed in position, one stroke of a hammer will close the hook over and retain it in position. The diameter of the pin on which those packings are used will vary with the size of the machine. For an ordinary heavy machine for heavy dredging, this pin will be from 3½ to 4½ inches in diameter, with a little extra space; the ring might be from 6 to 7 inches in inside diameter.

The details of the lubrication of the pin connections are not as clearly shown in the illustrations in the paper as I could wish. Where a hollow pin is used, a small piston fits into the bore, and by a screw at the end of the pin, the piston can be made to force out a lubricant, as in an ordinary grease-cup.

The grooves in which the packing rings are situated are half round, but the packing rings are formed with flaring sides, expanding that shape, so that when they occupy the groove, their sides are always hugging its sides, and any slight end-motion, which will occur, will not allow any dirt to enter the joint. This feature of protecting and lubricating the joints is one of the most important in machines of this kind.

Mr. C. C. Hill.—Some years ago, I had occasion, to some extent, to handle the problem of which this paper treats, out in the alluvial deposits of Idaho. With your permission I will illustrate. As I presume you are all aware, the country called the Snake River Bottom of Idaho, is composed of an alluvial deposit extending for about 400 miles along, and varying from half a mile to 12 miles wide, and as near as I was able to learn while engaged out there, it contains about an even distribution of flour gold. It has been the dream of a great many, both smart and stupid men, to get that gold. I was engaged by a company who tried to get it by placing good money out there, and in my attempts I started some of what are ordinarily called hydraulic machines. They were composed of the ordinary cabinets of copper plates with sluices and cradles, etc., forming run-ways for the water to pass over. I represented two companies, distinct, though composed of the same capitalists, and one of them had for its object the solution of the problem by handling the dirt in a dry state, and adding the water afterward. The company possessed a bank twenty-six feet high from the water level, extending for four miles down the banks of the Snake River, and we had to pump from the Snake River the water we wanted to wash this bank off with. So I started as a man would start in a hay field to mow. I constructed a dredging machine twelve feet wide, carrying eight chains of buckets, such as the gentleman just referred to, composed of heavy chain made of cast-iron links. As I was dealing with dry material, I did not wish to use any lubricant, because I should not be able to exclude the fine dust which would be present in the bank. I therefore used wooden blocks in the joints of the chain and malleable iron buckets very much like a tea-cup, ten inches across the top. These buckets I armed with steel prods, one on each bucket. I never had two prods on one bucket, because the rocks would be caught between the prods and prevent penetration; but by putting them at different places on the different buckets I succeeded in digging the bank, which was so hard that it was difficult to pick in some parts. I was very much impressed with the idea that dredging could be very advantageously done on that principle. I did not in a practical way accomplish a large amount. I could dredge the material well enough; I could wash it well enough; but after I had it washed I found that I could scarcely get enough to pay for the trouble. The machine I had out there had a capacity of about fifteen hundred tons of material for ten hours, with a twelve horse power engine lifting it eight feet high. Of course, I cannot speak now of its efficiency as a regular dredge. I am simply speaking of the idea which I had and carried out of cutting a swath from the side of the bank, washing it on the copper plates and carrying it from the copper plates still higher, and throwing it upon the bank or into the river. The system was intended to go right on down the bank, cut that twenty-six feet of bank down and dump it into the river; and when we had dumped into the river what we could reach, then simply lifting it upon the bank. As I said before, I did not carry it out far enough to give me any data as to the desirability of that means for deep dredging; but I should say it would be a very satisfactory way of dealing with the question in many cases.

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If, as is usually done for iron and steel, we make $m = 4$ we obtain

$$f = \frac{r}{I}\left\{\tfrac{3}{8}M_1 + \tfrac{5}{8}\sqrt{M_1^2 + M_2^2}\right\}.$$

The other formula, which is also based upon the theory of elasticity, but which is not as correct, is that given by Rankine, and is

$$f = \frac{r}{2I}\left\{M_1 + \sqrt{M_1^2 + M_2^2}\right\}.$$

While these formulæ are the only ones that take both twisting and bending into account, they are not very generally used, being recommended by some engineers for use in crank shafts and propeller shafts of marine engines, while they are seldom used elsewhere.

The formulæ in general use for line shafting and head shafting are most commonly those taking into account the twisting only, and the constants for these formulæ are those deduced from experiments upon pure torsion. Of this nature are the following formulæ given by Professor Thurston:

For head shafts well supported against springing,

Wrought iron, $d = \sqrt[3]{\frac{125\,HP}{N}}$. Cold rolled iron, $d = \sqrt[3]{\frac{75\,HP}{N}}$.

For line shafting, hangers 8 feet apart,

Wrought iron, $d = \sqrt[3]{\frac{90\,HP}{N}}$. Cold rolled iron, $d = \sqrt[3]{\frac{55\,HP}{N}}$.

For transmission simply, no pulleys,

Wrought iron, $d = \sqrt[3]{\frac{62.5\,HP}{N}}$. Cold rolled iron, $d = \sqrt[3]{\frac{35\,HP}{N}}$.

All such rules are based upon the torsion only, and the different constants are determined by using a factor of safety to make up for the bending to which the shaft is liable to be subjected. On the other hand, some rules are given for the proper distance between hangers, etc., which are based upon transverse strength only. Thus Unwin gives the direction, 1. That the axle must be calculated as a beam to bear the weight of the pulley and the belt-pull that is to come upon it; 2. For shafting transmitting power and subject to torsion only, he gives

$$d = \beta \sqrt[3]{\frac{HP}{N}},$$

where $\beta = 3.294$ for wrought iron, and 2.877 for steel.

Rules are also given that the deflection under the belt-pulls should not exceed $\frac{1}{1200}$ of the distance between hangers, this deflection being calculated by the ordinary rules for transverse strength only.

Returning now to the formulæ for combined twisting and bending, given by Grashof and Rankine, it is to be observed that the constants used in them are such as have been deduced from pure tension experiments, and not from experiments on a combination of twisting and bending, which last is what should be done if they are to be used with confidence.

With a view to determine the behavior of shafting under such a combination of conditions by actual experiment, suitable machinery has been put up in my laboratory of applied mechanics, and the investigation has been commenced. The object of this paper is to give you an account of the method of procedure, of the machinery used, and of the few results thus far obtained, together with such few conclusions as these results will warrant, and to invite any comments or criticisms which you may be pleased to make in regard to the tests.

The principal points of the method of procedure are the following, viz.:

1st. The shaft under test is in motion, and is actually driving an amount of power which is accurately weighed on a Prony brake.

2d. A transverse load is applied which may be varied at the option of the experimenter, and which is weighed on a platform scale.

3d. It is intended to adjust the proportion between the torsional and transverse loads to correspond with the proportion between the power transmitted and the belt-pull sustained by a shaft in actual use.

4th. Tests are made not only of breaking strength but also the angle of twist and the deflection under moderate loads are measured.

5th. With our present machinery the largest shafts which we can break are those of 1.5 inch diameter, but the machinery can be easily enlarged so as to use larger shafts.

The machinery used will be made plain by the accompanying cut (Fig. 37), a part of it being some that was used by Mr. George Billings in some experiments which he made on the repeated bending of shafts under a transverse load.

In this illustration *B* is the shaft under test, the power is obtained from another shaft on the right, and is transmitted to this test shaft through the double Hooke's joint *C* on the right, and then the power is transmitted from *B* to the Prony brake *G*, through the double Hooke's joint *C* on the left.

The power transmitted, and hence the twisting moment, is determined by means of the weights at the end of the brake lever and the readings of the revolution counter *D*.

The boxes in which the experimental shaft runs are marked *E* in the figure, and rest upon the casting *H* which, in its turn, rests on the platform scale and is separate and distinct from the rigid frame *F*, which latter is firmly fastened to the floor of the laboratory.

It follows that any weight resting upon the shaft *B*, or the pulley *N*, which is on this shaft, is weighed directly on the scale; the flexibility furnished by the double Hooke's joints render it possible to determine the weight on the shaft. The transverse load is applied by screwing down the screws *S*, thus pressing down the boxes of the upper shaft, and causing the pulley *M* to press against the pulley *N*, and thus load the shaft transversely. In making an experiment upon breaking strength, the transverse and the torsional loads are adjusted at the option of the experimenter, and then the shaft is run until fracture occurs, which almost invariably takes place at the point where the pulley *N* is attached. If the loads applied are too small to cause fracture they are increased and the shaft run until fracture occurs. The apparatus for determining the angle of twist under moderate loads requires a more detailed explanation, and this will be deferred until after the account of the results of the breaking tests.

It has been made very plain by these tests, that in order to deduce any conclusions of value the time occupied in breaking

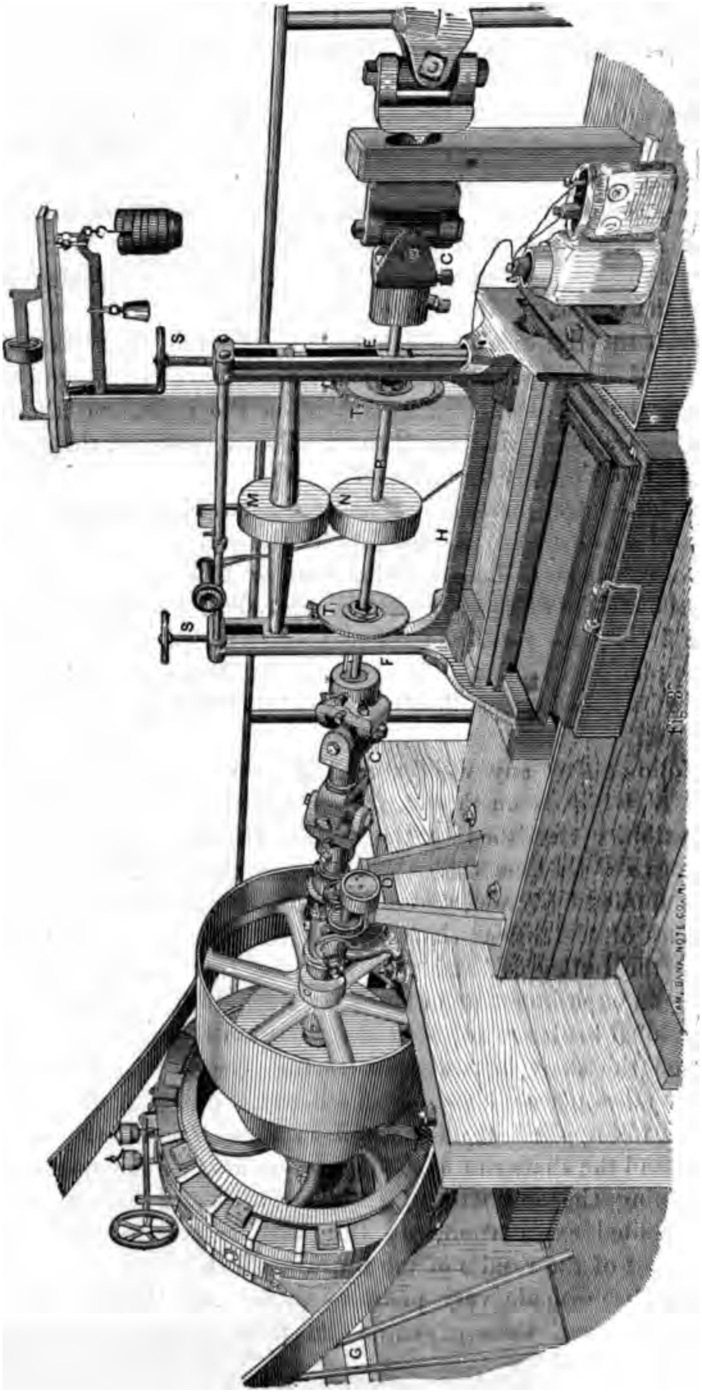

FIG. 37.

must be taken into account; *i. e.* the load required to break the shaft is very much smaller if sufficient time be allowed for it to operate, say 30 or 40 hours, than if we are required to break it in a short time, say one hour.

The mode of conducting an experiment is as follows:

1st. Decide upon the amount of horse-power to be transmitted, and the transverse load to be used, and adjust the brake and the screws *S* so as to realize them approximately.

2d. Set the shaft in motion, and let it run, keeping the boxes oiled, until it breaks, noting the time required to break it.

3d. If it finally breaks, then, in making the next experiment, diminish the loads and run it for a longer time, continuing this process until we ascertain what is the smallest load which will break the shaft by allowing sufficient time.

4th. The question next arises, What is absolutely the least load which will break the shaft? This, of course, requires a large number of experiments; and I cannot say that we have yet reached this determination.

The following table will give the results of the tests on iron shafts, and they will then be discussed:

No. of Test.	Time of running minutes.	Total revolutions.	$H. P.$ transmitted.	M_1, max. bend. moment.	M_2, max. twist. moment.	f_1, max. bend. fiber stress.	f_2, max. twist. fiber stress.	f, Grashof.	f, Rankine.	Diam. inches.	$T_1 + T_2$ 20″ pulley.	$T_1 + T_2$ 30″ pulley.	Bending moment 20″ pulley at middle 8′ span.	Bending moment 20″ pulley 2′ from end 8′ span.
8	37.5	7,040	11.717	11,514.1	3,926.4	60,024	10,234	62,168	61,755	1″.25	973	649	23,352	17,514
9	200	38,839	8.181	10,507.8	2,656.8	54,777	6,925	55,876	55,671	1″.25	679	453	16,296	12,222
10	162	31,641	5.291	9,891.0	1,714.6	51,562	4,469	52,062	51,976	1″.25	439	293	9,136	7,902
11	558	108,002	4.331	9,241.7	1,399.2	48,179	3,647	48,539	48,769	1″.25	359	239	8,616	6,462
12	408	80,694	6.276	9,241.7	2,027.6	48,179	5,287	48,911	48,769	1″.25	521	347	12,504	9,378
13	98	19,333	6.342	8,917.1	2,028.2	46,485	5,287	47,245	47,105	1″.25	526	351	12,624	9,468
14	423	82,741	6.283	8,917.1	2,029.7	46,485	5,290	47,246	47,106	1″.25	521	347	12,504	9,378
15	565	108,739	6.192	8,592.5	2,031.6	44,793	5,295	45,582	45,436	1″.25	514	343	12,336	9,252
16	443	88,208	6.338	8,267.8	2,026.8	43,100	5,283	43,914	43,763	1″.25	526	351	12,624	9,468
17	951	185,233	6.283	3,781.5	2,029.7	38,503	10,333	41,768	41,117	1″	521	347	12,504	9,378
19	....	....	14.834	8,268	4,744	84,185	24,152	91,928	90,368	1″	1,231	821	29,544	14,158
20	....	....	7.562	7,976	2,394	82,112	12,188	84,819	83,031	1″	628	419	15,072	11,304
21	....	....	9.972	8,917	3,232	90,793	16,454	94,484	93,716	1″	828	551	19,872	14,904
22	....	....	15.159	8,917	4,848	90,793	24,681	98,612	97,103	1″	1,258	839	30,192	22,644
23	....	....	2.955	7,652	945	77,913	4,911	78,814	78,239	1″	245	163	5,880	4,410

Two specimens of the 1″.25 shafting and two of the 1″ were tested for tension, the results being as follows:

		Breaking strength per square inch.
1″.25 diameter.	No. 1	46,800
	No. 2	49,865
	Average	48,333
1″ diameter.	No. 1	58,687
	No. 2	61,812
	Average	60,250

As to conclusions.

1st. It is plain from these results that a shaft whose size is determined by means of the results of a quick test would be too weak, and that our constants should be obtained from tests which last for a considerable length of time.

2d. A perusal of the tables will show that the results obtained apply more to the bending than to the twisting of a shaft, as the transverse load used in these tests was so large compared with the twist as to exert the controlling influence. This will be plain by a comparison of the values of f_1 f_2 and f.

3d. Nevertheless, a comparison of the fifth column with the last two will show that the bending moments actually used were generally less than such as might easily be realized in practice with the twisting moments used.

4th. It seems fair to conclude that, in the greater part of cases where shafting is used to transmit power, as in line shafting or in most cases of head shafting, the breaking is even more liable to occur from bending back and forth than from twisting, and hence, that in no case ought we to omit to make a computation for the bending of the shaft as well as the twist.

5th. As to the precise value of the greatest allowable outside fiber stress to be used in the Grashof formula, it is plain that it is not correct to use a value as great as the tensile strength of the iron, and while the tests show that this figure should not for common iron exceed 40,000 lbs. per square inch, it is probable that tests where a longer time is allowed for fracture will show a smaller result yet.

6th. Another matter of interest to note is, that we have a parallel to these tests in the experiments of Wöhler. His experiments were made by subjecting the shaft to bending back and forth without adding any twist. He found for good wrought iron a value for the breaking strength per square inch, under a continued repetition of stress, of about 32,000 lbs. per square inch.

Perhaps further experiments may lead to a value as low as this for greatest allowable fiber stress per square inch; at any rate, it would be more prudent to use this value than a value equal to the tensile strength.

TWIST UNDER MODERATE LOADS.

Next, as to twist under moderate loads, the results give us the modulus of elasticity for torsion. The results will be given first, and afterward the apparatus will be described. The values were obtained from experiments on 1″ shafts, and are as follows:

No. of Test.	
18	11,268,588
19	9,979,319
20	15,911,690
21	12,169,442
22	12,221,239
23	11,943,088
24	11,293,188
25	12,878,875
26	13,410,870
	9)111,076,299
	12,341,811

The tensile modulus of elasticity of this shafting, as determined by experiment, was 29,008,621 pounds per square inch.

The theory of elasticity gives the shearing modulus of elasticity as two-fifths the tensile or $\frac{2}{5}$ (29,008,621) = 11,603,448, which is not far from the average value found by experiment.

APPARATUS FOR MEASURING TWIST.

The apparatus for measuring the angle of twist under moderate loads was devised mainly by Mr. Theodore R. Foster, one of the graduates of the class of 1886, who took the subject of shafting for his graduating thesis. A brief description will now be given of it.

The disks T_1 and T_2 are made of brass, and each is in two pieces, so that they may be easily put on or removed from the shaft. They are placed on bushings which can be adapted to 1½ inch shafts or under. Each bushing has eight set screws for centering the disk on different sized shafts. Each disk has a boss at the center turned down to form a bearing; on the disk T_1 for two wrought-iron arms, and on the disk T_2 for one. One of the arms on T_1 carries a brass brush pointed with platinum, which presses against the circumference of the disk as it revolves, and so also the arm on T_2 carries another brush fixed in the same manner. The second arm on T_1 carries a tangent screw which presses against the arm with the brush. The arms are kept from revolving by suitable stops fixed in the frame while the disks revolve under the brushes. The two brushes are insulated from the arms, but connected with each other by a wire, a battery and a telephone being placed in the circuit. The circumferences of the disks are made of rubber, except that at one point in each circumference a thin piece of platinum is let into the rubber connected with the metallic portion of the disk, the circuit being completed through these pieces of platinum, the disks, and the shaft, so that if, in the revolution, the two brushes strike their respective pieces of platinum at the same instant, there is a current, and consequently a ticking in the telephone; whereas, if they do not strike at the same instant there is no current, and hence no sound in the telephone.

The manner of using the apparatus is as follows: Start up the shaft with a very small load on the brake, screw the tangent screw until a ticking occurs in the telephone, and measure (with a micrometer caliper) the distance apart of two metallic points which are fixed in the arms for that purpose at the same distance from the center. Next, put the desired load on the brake, and again screw the tangent screw until ticking occurs in the telephone, and again measure the distance apart of the same two points. The difference of the angles corresponding to these two chords gives the angle of twist corresponding to the difference of the two loads on the brake.

In closing this paper I must apologize for the meagerness of the results obtained thus far, hoping that what little has been done may prove of some interest.

DISCUSSION.

Mr. Wm. Kent.—I know of an experience in Pittsburg some years ago, which confirms Professor Lanza's statement, that a shaft will break under repeated strains where it may not break under one strain. In feeding tables for a rolling-mill, where the main shaft driving the table was subjected to twisting strains in reverse directions, that shaft would generally have a life of about six months, and when it broke, it broke like a broom. The fibers expanded out like a brush on both sides, and came apart in that way. It was wrought iron.

Mr. G. C. Henning.—I think the ground is very well covered by Mr. Baker's paper which we will hear soon. In it is taken up the case of Woehler's experiments, but he has not only duplicated them, but gone far beyond in comparing soft steel, mild steel, and wrought iron, and he there shows that it is not alone tensile strength or elastic limit which determines the life of shafts subject to torsional and bending strains, but it is the repetition of distortions which affects them most seriously. This case of a rolling-mill shaft is precisely the same thing; as a rule our designs for members used in that way have been entirely, not to say erroneous, but such as to indicate that we have designed them on the basis of ultimate strength, instead of on the capacity for doing work. The life of such members depends on something else, which we have not yet found out. It depends on the effect of superposition of strains, even if they are within the elastic limit.

Mr. J. T. Hawkins.—I would like to mention one or two instances which happened within the last three months in my experience, and which go to substantiate Professor Lanza's position as to the bendng strains on shafting tending to their rupture more than twisting strains. Within my knowledge, within the last three months a two-nch steel shaft has broken in the same place, and within the hub of the pulley, which would be very strong proof, I think, that it could not in that case have been caused to any considerable extent by twisting. It would certainly be due to the strain of the belt in producing transverse stress on the shaft. The fractures occurred in the same shaft in the same building, and very near the same place, twice within about three months—a two-inch steel line shaft.

Mr. T. S. Crane.—It seems to me important to have the attention of engineers called to the effect of this lateral strain and torsion. I had an experience once where I had to put the pulley to receive the entire power of the engine on a four-inch shaft, and I could not get a hanger near that pulley. I expected to have trouble with it, but it ran for two years, and then the shaft broke off just within the edge of the pulley. It broke with a brittle fracture, showing that the effect of the two strains had been to crystallize the iron. I did not think it would run as long as it did. The shaft was ready, and there was a great pressure to get the factory started, and there was no time to get a larger shaft. When it was replaced it was replaced with a larger shaft, and no further trouble arose.

Mr. C. C. Hill.—Some years ago I had experience in establishing a friction * plant for a large saw-mill, where the pressure on what we call the counter-shafts was large, greater than is usually put on belted shafts. I found the same difficulty there with shafts. The shafts were very much larger than for ordinary transmissions of a like amount of power, but we could not run them many months or years without their breaking right off close to the pulley.

Mr. H. R. Towne.—I wish to make a few comments on this paper, as the subject is one that interests me; and in the first place I cannot refrain from expressing the gratification which I think we all feel that our transactions are growing to include so much more of original investigation. Each of our recent volumes has contained more or less of it, and obviously the value of our transactions, to ourselves and to the world, will increase in proportion to the amount of this original work which is done. Professor Lanza tells us that he has just commenced this line of investigation, and it is certainly to be hoped that he will continue it in the direction in which he has started.

I wish to set forth a few points which can be properly considered in pursuing it. In the first place, the tests which have been made have been such as to result in the destruction of the shaft. In practice we do not intend, when designing a shaft, that it shall be destroyed, and I conceive that the conditions obtaining in a shaft which is subjected to such strains as to destroy it within a few hours or days may be different from those obtaining in a shaft subjected to ordinary conditions, where it is intended to endure permanently. It is to be hoped that the series of experiments will include tests of shafts under conditions approximating to those of ordinary practice. Now, in connection with such tests, one of the points of interest is to determine by experiment whether it is right, in calculating the stresses upon a shaft subjected to both bending and torsion, to assume that they are equal to the sum of the stresses resulting from each of the two strains taken independently, or whether the two coexisting stresses act upon one another to modify either; this is an unsettled question, experimentally at least, and an important one to know. That is, having a shaft which we know is expected to transmit a given amount of power by torsion, and knowing the strains existing in it by reason of the work thus done, are we to assume that subjecting that same shaft simultaneously to a bending strain augments the stresses in it exactly in proportion

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* Transmitting by friction gearing instead of belts.

to the bending, or does the fact that the two loads are co-existent serve to modify the stresses in any way?

One other question in connection with this is, I think, particularly important. A long shaft transmitting power becomes one of the best possible kinds of springs. Many of us know that a long rod of metal subjected to twisting or torsion gives us a beautiful spring action. We have had our attention called this morning to the fact that the transmission of power by gears is liable to result in intermittent impulses. We know that to be the case. I have recently had occasion to consider the case of a shaft transmitting considerable power, where I discovered that if a short length of the shaft was used, a perfectly steady and smooth action was obtained; whereas if the shaft had considerable length, the action became irregular and pulsating, and without taking time to tell you how I conducted the investigation, I satisfied myself that the cause of the irregularity was a pair of bevel gears, through which power was transmitted to the shaft. Now, let us take two springs each composed of a piece of flat steel, and first examine the one that is shortest, which is supposed to be clamped at one end into a solid block of metal and to project freely from it. If I apply a given power to the end of the spring, so as to bend it, and then release it, the spring will fly back and will vibrate several times, but will quickly come to rest. If I apply the same amount of power to the longer spring, it will give the same moment at the same point, but on releasing this spring it will vibrate very much longer than the other. The reason for this, if we think a moment, is the inertia of the parts, or more properly their momentum. This is still more the case in long shafting, because we have a spring consisting in that case of a shaft subjected to torsion, on which we have put more or less dead load in the shape of pullies and other weights, which frequently give us a very considerable amount of material in motion. Conceive of the shaft first as at rest, and of considerable length, and the power as applied to one end. The inertia of the parts tends to keep them at rest until an angle of torsion is developed sufficient to set the whole shaft in motion. Now our power is quite frequently more or less intermittent. When the inertia of the whole shaft, or of the parts at its far end, is overcome, they are set in motion; then a recovery takes place, and thus is established a vibration, which certainly must in some way involve, as one of its factors, the amount of matter in motion—in other words the momentum of the parts. So far as my reading extends, I have not noticed any discussion of this subject in connection with shafting, and I consider it a practical question. The ordinary formulas for determining the size of such shafts usually adopt some arbitrary limiting angle for the angle of torsion. D. K. Clark gives it in his manual (and I think it is taken by him from older authorities), as one degree for each twenty diameters of length of shaft. The correctness of that formula is based on the assumption that we shall not get a greater amount of this action which I have just described in one length than another, or at least that it will be directly proportionate. Practice, however, shows that this is not the case, but that, as we lengthen our shaft, after we have passed a certain point, with such conditions as these, vibration can be set up, and that the vibratory action is very objectionable in most cases. It is to be hoped, therefore, that the line of experiment which has been commenced may include considerations of these conditions, which exist in actual practice. Incidentally, I may remark in closing, that I had occasion recently to test for torsion a shaft two inches square, of cold rolled steel, which had been turned down at three points to form bearings, the bearings however being the full diameter of the sides of the shaft, that is to say, two inches in diameter, without reducing the diameter, the corners being merely taken off. The shaft was some twenty feet long, and the tests were made at two points, one of them involving a length of shaft of twenty feet subjected to torsion, the other of half that length. The result showed practical agreement with the ordinary formula, except some discrepancy as to the angle of torsion being directly as the length of the shaft, of which I am not yet quite satisfied, and intend to pursue further. As a matter of record it is perhaps worthy of note that, taking Clark's formula for the limiting conditions, $D = \frac{WRL}{16,600\,d^4}$, D being the angle of torsion in fractions of a circle, limited, as I said before to one degree in each 20 diameters of length, W being the weight in pounds, R the radius in feet, L the length of shaft in feet, and d the diameter in inches, he gives the above numerical factor for wrought iron, and in another place gives figures showing that the corresponding factor for steel would be about 33,000. Applying the formula to the actual record of the experiment which I made gives as this numerical factor 56,300 for cold rolled steel, as against about 33,000, which Clark gives for ordinary steel, showing very forcibly the value derived from cold rolling.

Professor Lanza.—I think the remarks of the president are very much to the point, and I am very glad to have his suggestions. We have not thus far been able to do much testing with the shaft under moderate loads. In regard to the way in which the twisting affects the ultimate strength of the shaft, for bending and *vice versa*, we are trying to determine how far we can depend upon Grashof's formula; and our method of procedure is as follows. Having broken one shaft by subjecting it to a certain bending moment, combined with a certain twisting moment, we compute by Grashof's formula the greatest fiber stress. Then if the formula represents correctly the facts, we ought to be able to break another of the same dimensions and material in the same length of time, by using a different amount of bending moment, and a different amount of twisting moment, so proportioned as to give by the formula the same greatest fiber stress. What the result will be remains to be determined.

CCXXXI.

A NEW CONICOGRAPH.

BY ANDREW C. CAMPBELL, BRIDGEPORT, CONN.
(Associate Member of the Society.)

It is with no small degree of timidity that the writer brings to the notice of this society a new device for describing the several curves known as the "conic sections." Instruments of this class, even though perfectly adapted to the purposes for which they are intended, have a very limited use in many branches of the profession, yet the problem has proven a fascinating one, and herein are presented the results of his investigation.

It is generally understood that the ellipse, parabola, and hyperbola may be drawn with such simple appliances as a string, straightedge, pencil, and a couple of pins. Ellipsographs by the score have been invented and patented in this country, to say nothing of the many devices for a like purpose which were originated abroad, so that it is not safe to rest very secure in the belief that the one herein described is entirely new, however original it may have been with the writer.

The instrument best known for describing the ellipse is the ellipse trammel, which consists of a fixed base plate with two grooves in its upper surface at right angles one with the other, and a trammel-bar having three adjustable points, two of which track in the grooves aforesaid, while the third traces the curve. This base plate is usually in the form of a cross having equal arms, and the smallest ellipse that can be drawn with such a device will have a major axis about twice the length of this cross, and a minor axis about one-half the major. Its application to larger ellipses is also dependent upon the size of this cross, for we cannot draw an ellipse with it whose major axis exceeds the minor by more than the length of the cross. However, this instrument has very many uses, and the principle of it is applied in almost all forms of ellipsographs. In searching the Patent-records Index very many "Ellipsographs" were found, but the search was vain for parabolagraphs and hyperbolagraphs, from which it was concluded that no great success had been achieved in these directions. For describing these curves the string and straight-edge are generally resorted to, and even recommended as the only "mechanical" means, though Professor Sylvester, who investigated the "Peaucellier cell," and invented many novel uses and forms of it, developed a system of linkages from it with which it is possible to describe the ellipse, parabola and hyperbola, as well as other complicated though definite curves. Many links are brought into action in his apparatus, but the result is obtained with mathematical exactness.

Having read the two instructive and suggestive articles in the *Scientific American Supplement*, by Professor MacCord, of Stevens Institute, in which he described two possible machines—one for drawing the hyperbola, and another for drawing the parabola—the writer became interested in the subject, and undertook to solve the same problems mechanically, and by cutting across lots the results aimed at were obtained in a comparatively simple manner, and there were embodied in a single instrument elements which admit of such transposition as to make it possible to delineate not only the parabola and hyperbola, but the ellipse as well; hence the name "*Conicograph.*"

I.—The Ellipse.

In order that the "reasons why" may be well understood it might be well to show how the "Gardener's Ellipse" is described with the string and pins, as this illustrates an important property of the ellipse, namely, that the sum of two lines drawn from the foci to meet at any point in the curve is equal to the major axis.

Having first determined upon the length of the major and minor axes, take a string (having a loop at each end) of a length equal to the major axis, and double it, using the half length thus obtained as a radius with which from one extremity of the minor axis as a center, describe an arc, cutting the major axis in two points equidistant from its center. Where said arc intersects the major axis will be the foci, and by securing the looped ends of the string aforesaid, one at each of these focal points by pins, we will find that when the string is drawn taut in any direction with a marking point, said point will lie on the curve.

Referring to the diagram, Fig. 38, let A B be the major axis and C D the minor axis of an ellipse. With A L (half of A B) as a radius and C or D as a center describe an arc cutting the line A B at the foci E and F, then C is a point in the curve, since C E + C F = A B. If the marking point be passed to the position G, and both parts of the string kept straight, the requirements are fulfilled, and G is a point in the curve, for E G + G F = A B.

Further, if the line E G be produced, and G H be made equal to G F, the whole line E H will be equal to A B. This is true of course wherever the point G is taken on the curve, and the point H will always lie on the circular arc H h, whose center is E, and whose radius is A B. (This line might be called the directrix of the ellipse, since its relation to the curve seems to be the same as the directrix of a parabola to the parabola.) If the angle F G H be bisected by the line S T, said line will be tangent to the ellipse at the point G and will always be at right angles to the line F H which it also bisects.

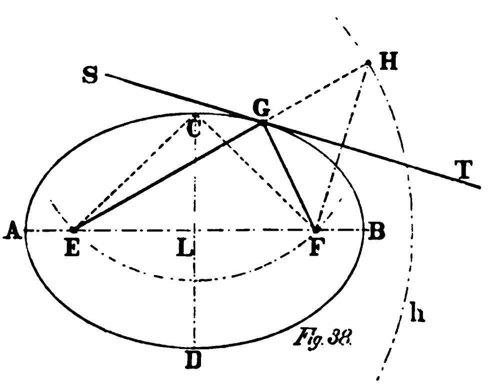

Fig. 38.

In designing a mechanism which will preserve these conditions in all positions, a parallelogram of equal links (see Fig. 39) 1, 2, 3 and 4, is employed. The links 1 and 4 are jointed at the focus F; 2 and 3 to one extremity H of the adjustable radius bar 6; 3 and 4 to one end S, of the tangent bar 5, while 1 and 2 are jointed at T and control the direction of the end T of said tangent bar 5. Now if the points E and F of this linkage be located at the foci of the required ellipse whose major axis is equal to E H, the intersection of the lines E H and S T may be shown to be a point in the ellipse. At this point then is arranged a compound slide carrying a marking tool so that by swinging this system of links in the plane of the paper, about the two controlling points E and F, the circumference of the required ellipse will be traced. As constructed, this instrument will not describe the entire ellipse at one setting (owing to the interference of some of the parts), so that after drawing one-half of the figure, the points E and F must be transposed before the remaining half can be drawn. The size of the parallelogram limits the maximum capacity of the instrument as an ellipsograph for describing closed curves, since the marking point G always lies *within* it. The only requisite to increase the maximum capacity is the substitution of longer links in the parallelogram. However, with the instrument shown, incomplete ellipse curves may be

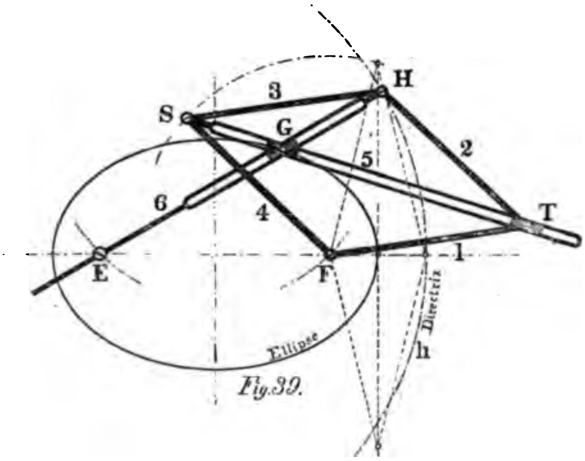

Fig. 39.

drawn with any required distance between the foci, the limit being the length of the adjustable bar (6) employed. The minimum capacity of the device is also limited, the consideration in this case being the least possible distance obtainable between the focus F and the marking point, when the latter is at that extremity of the major axis which is nearest the focus F aforesaid. (See dotted position in Fig. 39.)

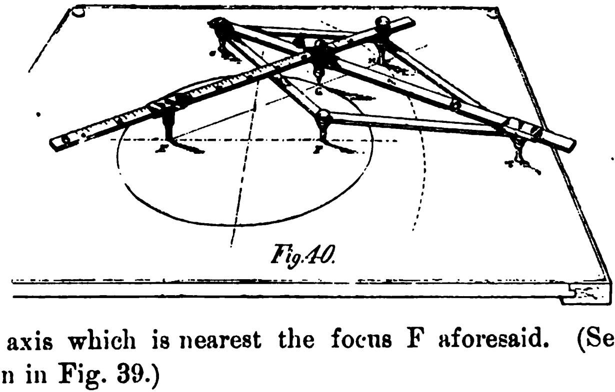

Fig. 40.

In constructing the instrument shown in perspective in Fig. 40, the links of the parallelogram should be of some light stiff material, preferably steel, and substantially jointed at their extremities to the upper end of four short supporting pillars, those at S H and T terminating at the base in the form of smooth-bottomed disks which may slide freely on the plane upon which the ellipse is to be traced, the fourth pillar at F having a shouldered needle-point in its lower end which punctures the drawing paper in one of the foci of the required ellipse, and forms one of the controlling points of the mechanism.

The tangent bar 5, is jointed at S to the upper end of one of these supporting columns, and extends toward the opposite angle of the parallelogram, where it passes freely through a slide at the top of the column T, so that whatever be the distance between the points F and H, the tangent bar exactly bisects it. The adjustable link 6, lies in a plane above the tangent bar 5, and one end thereof is jointed to the upper extremity of the column at H. Upon this bar is placed an adjustable slide which is secured in position by a clamping screw entering it from below, said screw forming the supporting column at E, which terminates at the base in the form of a shouldered needle-point. This slide must be so placed along the bar 6, that the distance between the centers of the columns E and H shall be equal to the major axis of the ellipse to be drawn. By arranging a scale on said bar, the only setting required can be very readily made.

The bars 5 and 6 are made of brass tubing in preference to other material, and the general stability of the apparatus might be increased by substituting a standard at E with a large flat base, and swinging the bar 6 from its upper end, thus preventing the twisting action which otherwise tends to deflect said bar.

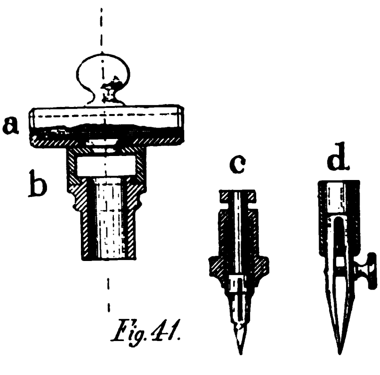

Fig. 41.

The compound slide at G, which carries the marking point, is shown enlarged in Fig. 41, and, as will be seen, consists of the two parts **a** and **b** of rectangular cross-section through the first of which slides the adjustable bar 6, while through the latter, slides the tangent bar 5; and these two parts are so jointed to one another that they may revolve in parallel planes about a common vertical axis. As the metal tubing from which these parts are made is quite thin, the construction shown was resorted to in order to get a swivel between them—using a disk counter sunk into the upper slide and riveted into the lower one. Extending downward from the under side of the lower, or tangent bar slide, is a tubular socket, into which may be placed the marking device, either a pencil **c** or a pen **d**, and since the former is subject to wear, it is arranged in a spring socket as shown, thus also insuring a uniform pressure over slightly irregular surfaces.

Your attention is particularly called to two important advantages possessed by this instrument. First, when the blades of the pen are placed in proper position to draw a line in the direction of the tangent bar, said pen is always guided thereby exactly in the direction of the curve, and thus gives a better line than can be drawn by any other instrument made for a like purpose. As far as the writer can discover, this is the first time this result has been obtained in an ellipsograph. The second feature is that it is possible to draw any number of curves parallel to the ellipse, either larger or smaller than the original figure, and this is often very desirable, as, for instance, in drawing elliptical gearing. A curve parallel to an ellipse is *not* an ellipse, and is therefore out of reach of all ellipsographs heretofore made. To do this with the instrument shown, it is only necessary to place the marking point the required distance from the center of the compound slide and at right angles to the direction of the tangent bar. The attachment shown in Fig. 42 is designed to be used in place of the regular marking device, and can be readily secured in proper position on the pendant sleeve of the compound slide, and easily adjusted to draw curves parallel to the ellipse.

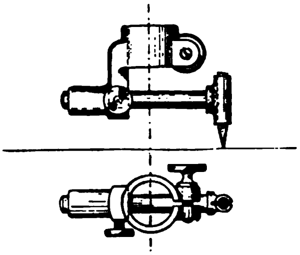

Fig. 42.

II.—THE PARABOLA.

In reference to the particular property of the parabola, of which advantage is taken, Davies' *Descriptive Geometry* may be quoted:

"If a right line, E D, and a point, F (Fig. 43), be taken in the plane of the paper, and a point, as G, be so moved in this plane that the distance from F be constantly equal to its distance from E D, that is, G F = G H, the point G will describe a curve called a *parabola*. The line E D is called the *directrix* of the parabola, and the point F the focus; the line A F, perpendicular to E D, the *axis*, and the point B, in which the axis intersects the curve, the *vertex of the parabola*. Points of the curve may be found thus: take any point in the directrix, as E, and draw, E F to the focus; draw also, E L perpendicular to the directrix, and at the point F make the angle E F L equal to the angle F E L, then the point L, at which the lines E L and F L intersect, is a point of the curve."

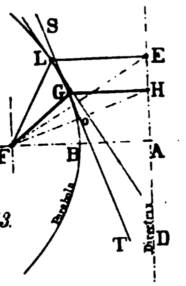

Fig. 43.

How nearly this Conicograph can be made to fulfill these conditions will be seen by the following: reference being made to the diagram, Fig. 44, and the perspective, Fig. 45, wherein the bar 6 is shown as controlled by its "Tee" head and the straight-edge V W, so that it lies constantly parallel to the axis of the parabola, and the point H thereon may be made to lie always on the directrix E D. Let F be located in the focus, and the angle of the parallelogram opposite to F be pivoted to the point H of the bar 6, then F H is exactly bisected at O by the tangent bar S T, since by construction S H = S F and H T = F T. If, from the point H, G H be drawn perpendicular to the directrix, it will intersect the tangent bar S T at G, a point in the curve for G H = G F and H O = O F. Since G O is common to the two triangles, O G H and O G F, the angles F H G and H F G are equal.

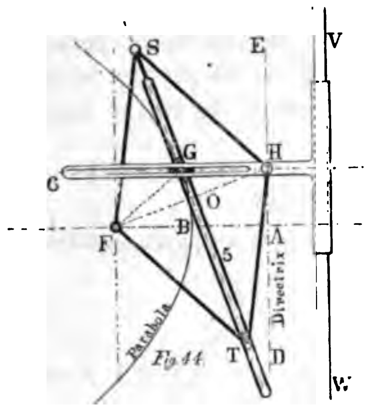

Fig. 44.

Fig. 45.

In order to adapt this instrument to these conditions, the adjustable slide E shown in Figs. 39 and 40 must be removed from the bar 6, and the attachment shown in Fig. 46 secured to said bar by the screw at H, which transforms the bar 6 into the " blade " of a Tee square, the attachment forming the "head." In drawing the parabola with this device a straight-edge must be placed on the paper, parallel to the directrix, and a little way from it. Along this straight-edge the head of the Tee square must be moved while describing the curve, thereby keeping G H at right angles, with the directrix E D and H always on it.

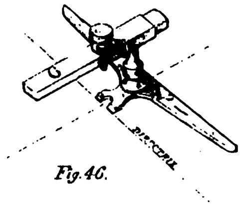

Fig. 46.

The attachment shown in Fig. 42 is applicable to the parabola as well as to the ellipse, and with it curves may be drawn parallel to the parabola. In inking these parallel curves the pen is always directed by the tangent bar, as in the case of the ellipse, and with equally perfect results.

III.—The Hyperbola.

In reference to the hyperbola, Davies may be again quoted:

"If two points, E and F (Fig. 47), be taken in the plane of the paper, and a point, G, be moved with the condition that it continue in the plane of the paper, and that the difference between the distances F G and E G be a constant quantity, the point G will describe a curve, P A G, called an hyperbola. . . . The points E and F are called the *foci*, the line B A is named the *transverse axis*, and the points B and A, in which it intersects the curve, are the vertices of the hyperbolas."

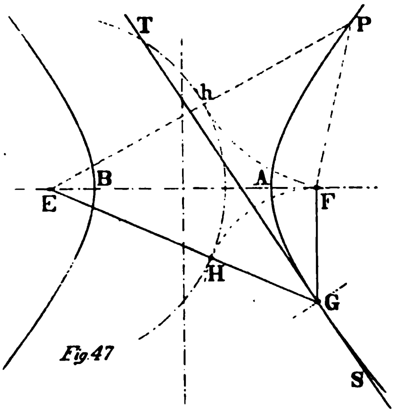

Fig. 47

Applying this Conicograph to describe an hyperbola, let the needle-point of F (See Fig. 48, and perspective, Fig. 49), be located at the focus of the hyperbola to be drawn, and the needle-point of E at the focus of the opposite hyperbola. In drawing an ellipse the slide G, carrying the marker, is always between the points E and H, while for drawing the hyperbola it must always be placed beyond H. In order to have it so located, the point H, in the adjustable bar 6, must be pivoted (by means of the removable screw at that point) to

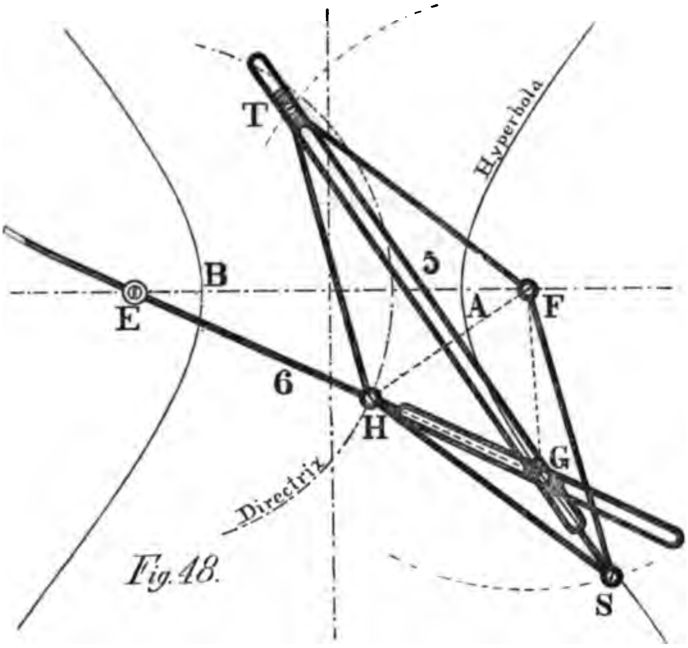

Fig. 48.

that angle of the parallelogram opposite the focus F, one end of said bar extending through the compound slide G, the other through the adjustable slide E. Taking G as a point on the curve,

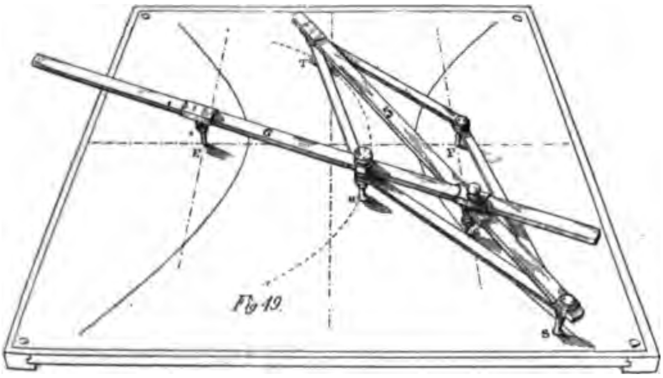

Fig. 49.

$GF = GH$ and $EG - GF = EH$, which latter is a constant, therefore, if E be taken as a center and E H as a radius, the point H will describe the curved directrix H h (Fig. 47). Bisecting F

H with a line at right angles thereto will locate the tangent S T. The angle G F H being equal to F H G, locates G as a point on the curve, and also as the point of tangency of the tangent bar 5 to the curve. Further, E P – F P = E h = E H, therefore P is another point on the curve of the hyperbola.

In drawing an hyperbola, then, with this Conicograph, the " settings" are as follows:

First, locate E on the adjustable bar 6, at a distance from H equal to the transverse axis B A of the hyperbola—then make A F = B E, and locate the needle-points of the instrument in the foci E and F.

By swinging the point H of the linkage as far as possible above and below the axial line, the marker attached to the compound slide at G will trace the required curve. To draw the curve of the opposite hyperbola the points E and F must be transposed, as in the case of the ellipse.

The attachment shown in Fig. 42 may be used for drawing curves parallel to a given hyperbola.

This drawing-instrument is presented for your consideration more as a novelty and curiosity than as a perfect machine, though even in its imperfect state it proves to be a very serviceable Conicograph, as the few specimens of drawings shown at the meeting in connection with this paper will attest.

DISCUSSION.

*Prof. McCord.**—I did not attempt in the reference which Mr. Campbell has made, to do what he has so admirably succeeded in doing, that is, to produce an apparatus which is capable of drawing *all* the curves. I expressed the opinion some time ago that I did not think, and I do not think now, that many gentlemen in our profession will be greatly benefited by an apparatus of this kind; that is to say in general; the drawing of hyperbolas and parabolas can be better effected without an instrument than with one; but for all that the ingenuity displayed in this is just as admirable. There will be special cases, however, where this instrument would be very useful. There is no doubt of that. But Mr. Campbell has done what I have not done. He has got something which is different from anything which I had, and something which is more general in its application than I had produced or hoped to produce.

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* Present by invitation.

Mr. A. B. Couch.—One of the special conveniences of this apparatus is its ability to draw curves parallel to the ellipse. In laying down the lines for elliptical gearing, it is usually necessary to substitute for the ellipse a series of circular arcs, for the purpose of constructing conveniently the addendum and root curves, which by this instrument can be drawn direct, and parallel to the pitch line, which is an ellipse.

Mr. Wm. Kent.—I have had recently occasion to want a glass cylindrical mirror made—not exactly cylindrical—but in the shape of an ellipse, and it strikes me that this apparatus may lead to the development of a machine which will grind the glass in that form. The apparatus might be constructed so as to generate an ellipse of the section needed; and instead of a pencil, it can have a rotating grinding or polishing wheel attached to the arm, which would grind or polish the required curve.

Mr. P. A. Sanguinetti.—I would like to make a remark which has occurred to me, following the train of Mr. Kent's observation. I have had occasion to require pieces of flat glass cut to an exact elliptic shape, and of different sizes. I found some difficulty in having the glass cutter make them correctly, even after furnishing him with a paper templet, over which he would lay the glass, and follow the outline with the diamond. Now if the diamond could be made to take the place of the pencil point in this instrument, I think it would afford a very easy and correct method of cutting the glass to any sized ellipse required within the limits of the instrument.

Mr. G. C. Henning.—I do not think that any one has mentioned one of the most common purposes to which this apparatus can be put. Several railroads are introducing parabolic curves on their systems. These companies, of course, must have the parabolic apparatus to describe the parabola in laying out the drawing, and as that is generally left to the younger draughtsmen, it is highly important that we have an apparatus which does represent the curve closely, and in that respect I think the apparatus will find considerable application.

Prof. De Volson Wood.—An engineer of eminence, who had examined parabolic curves and circular arcs, as he had constructed them, said that from practical observation in riding on the trains on those roads where they had put in parabolic arcs for the purpose named, and on other roads where they were constructed according to his idea, that he preferred the circular arc. I make this remark, so that if this society should be disposed to go into parabolic arcs for railroads, they may know that there is another side to the question.

Mr. Kent.—An engineer of the Pennsylvania Railroad Company told me some years ago that the proper form of curve is the elastic curve—not the parabolic or hyperbolic, but the elastic curve, which very gradually departs from a straight line and gradually increases its rate of curvature.

Mr. W. B. Le Van.—The Swedish Government uses elastic curves entirely for their railroads.

Mr. Henning.—I referred to the Pennsylvania Railroad. On that road they change their curves to parabolas, where they are now circular, especially on the mountain division, where the grades are heavy and the curves sharp.

Prof. J. B. Webb.—I have not heard the whole of this discussion, but there is one thing which may be said in favor of this instrument—it does not attempt to make the whole ellipse at one setting. It has been said against some ellipsographs that they make only semi-ellipses; I do not believe that any instrument sufficiently complicated to make the whole ellipse and guide the pen around the curve will ever be of practical use. As this instrument draws also the parabola and hyperbola, it may be added that it is *not possible* to make the whole of either of these curves at once! I have one other remark to make, suggested by Professor Wood's criticism—I suppose that, even if circular arcs are to be adhered to for railroad curves, this instrument need not be thrown out as, I believe, it will also construct a circle.

Mr. Campbell.—I might add in connection with the last statement that the instrument is capable of drawing a circle and also a straight line and a square!

CCXXXII.

SOME NOTES ON THE WORKING STRESS OF IRON AND STEEL.

BY BENJAMIN BAKER, FORTH BRIDGE RY., ENGLAND.
(Honorary Member of the Society.)

THE author has selected the above somewhat elastic title because, whilst anxious to accede to the request to bring certain results of his experience before the society, he has not had sufficient time, in the pressure of other duties, to analyze and arrange these results as he could wish.

Few engineers of experience who have had to deal both with machinery and with structural iron work, such as railway girders, can have failed to note inconsistencies in practice, and general vagueness as to the meaning and use of such terms as safe working stress or factor of safety. Rankine defines factor of safety as the ratio in which the ultimate strength exceeds the working stress, and assigns to it a value of 4 to 6 for ordinary steel and iron subject to a variable load. But if we consider for a moment how the proportions of almost all parts of a machine, from the axle of a country cart to the coupling rod of an express engine, have been arrived at, we shall see that it has been by the gradual strengthening of the parts which had proved by accumulated experience to be too weak, and not by calculating the dimensions on the basis of a factor of safety of 4 to 6.

To illustrate this fact we cannot, indeed, select a better example than that of the coupling rod of a locomotive engine. Thirty to forty years ago the coupling rods in general use were round rods, about 2⅝ inches in diameter at the center and a trifle smaller at the ends. They were next changed to flat rods about 3½ inches deep by 1½″ thick, and subsequently the dimensions were modified to 4 inches by 1¼ inches. Finally, we have now on many railways girder section rods 4½ inches deep, 2¼ inches wide over the flanges, and ⅜ inch thick in the web. Why were the successive modifications introduced? Obviously as the result of experience, and not of calculation, or of increased power in the engines. The sectional area at the center of the round rod was 5.4 inches; of the first mentioned flat rod 5.25 inches; of the second 5 inches, and of the last 6.4 inches. In no case would the direct stress on the rod, even assuming all the power of the engine were transmitted through it to the coupled wheels, exceed 6,000 lbs. per square inch; therefore direct stress had nothing to do with the alterations. What happened was this: Failures occurred with the round rods, and some shrewd, practical man instinctively concluded that the fracture occurred from transverse stress, and altered the section to a deep flat bar better able to resist bending. Theoretical considerations show the justice of this conclusion. At a speed of 50 miles an hour, the stress on the round coupling rods of an old type Great Western Railway engine, the writer finds, must have been 14,500 lbs. per square inch from centrifugal force alone, or say, 17,000 lbs., including the direct stress from the engine. This stress has since been gradually reduced until with the most modern girder section rods, the combined stress from centrifugal force and steam pressure does not exceed half of the above, or 8,500 lbs. per square inch.

Now, what is the factor of safety in the latter instance? Under direct pull the coupling rod would stand an ultimate stress of say 50,000 lbs. per square inch; under direct compression say 20,000 lbs.; and under transverse stress, which as we have seen is the one determining fracture, the calculated ultimate stress on the extreme fibers would be about 80,000 per square inch. Is the factor of safety here $\frac{80,000}{8,500} = 9.4$, or is it $\frac{17,000}{8,500} = 2$? Experience has shown that the coupling rods are sure to fracture ultimately if the stress reaches 17,000 lbs. per square inch; and the writer's experiments, to be hereafter referred to, point to the same conclusion. By making the working stress half the breaking stress as determined by actual practice, adequate security is found to be attained.

As it is with coupling rods so is it with other parts of machinery. A pin or some other member repeatedly fractures; it is made somewhat larger and stands.

Let us now consider for a moment another class of structure—railway bridges. The Conway tubular bridge, which has carried the heavy traffic of the London and Northwestern Railway for the past 36 years, is 412 feet in span, and under its own weight the tensile stress is 13,000 lbs. per square inch. With ordinary trains the stress is 17,000 lbs., and if covered with the heaviest engines in use on the line, 20,000 lbs. per square inch. The ultimate strength of the riveted structure is about 42,000 lbs. per square inch. No indications of weakness have developed during the 36 years' working, nor anything to suggest that the factor of safety, of say 2 to 2½, is unduly low.

On the other hand, railway experience has amply proved that with small span, and therefore light girders, such stresses as the above would quickly lead to destruction. For that reason, in structures such as the "Elevated Railway" of New York, the stresses are wisely limited to 8,000 lbs. per square inch in the flanges of the girders, 7,500 lbs. in the web bracing, and 4,500 lbs. where members are subject to alternate tension and compression. Although in Great Britain the Government regulations still authorize 11,200 lbs. per square inch on the net section, and in France 8,500 lbs. on the gross section, irrespective of the character of the load or the span of the girder, engineers of the present day do not act up to these regulations. In Great Britain a stress of 9,000 lbs. per square inch net section is seldom exceeded in light girders. In a recent German bridge over the Danube, the permissible stresses ranged from 6,900 lbs. to 13,000 lbs.; and in a recent Hungarian bridge, over the same river, from 8,700 lbs. to 11,000 lbs. per square inch, according to the character of the loading. In one of the latest French bridges the Government authorized a stress of 11,400 lbs. per square inch gross section, in lieu of the usual 8,500 lbs., on the grounds of the dead load constituting an exceptionally large proportion of the whole. Again, the Russian Government last year issued new regulations by which the limiting stresses were fixed at 8,500 lbs. per square inch for bridges under 50 feet span, and 10,300 lbs. for those over 100 feet span. Thus, whether we take the case of the coupling rod of a locomotive, or the bridge over which the locomotive runs, we find that engineers have learned by experience the important truth, that the strength of a structure or piece of mechanism cannot be determined by the simple process of breaking a piece of the material in a testing machine, but must be ascertained either by the gradual accumulation of the results of actual working, or by testing the material under conditions as far as possible analogous to those obtaining in the case under consideration.

Wöhler's experiments on the so-termed "fatigue" of metals are well known. The writer, wishing to satisfy himself as to the behavior of modern structural steel under different stresses, has, during the past few years, carried out experiments in some respects similar to, and in others differing from, those of Wöhler's; and has also made analogous tests of hard steel and of iron. The experiments may be roughly classified under four heads: (1) Rotating spindles with a weight at the free end, causing alternate tension and compression on the fibers as the spindle revolves. (2) Flat bars bent in some cases one way only, and in other cases both ways. (3) Specimens so designed as to give alternate direct tension and compression on small pieces of metal; and (4) Full-sized riveted girders.

SERIES No. 1.

No.	Revolutions.	Stress per sq. inch.	Factor *a*.	Factor *b*.
		Soft Steel.		
1.	40,510	36,000	1.75	2.45
2.	60,200	36,000	"	"
3.	68,400	34,000	1.84	2.56
4.	92,070	"	"	"
5.	107,415	"	"	"
6.	128,650	"	"	"
7.	155,295	"	"	"
8.	14,876,432	26,000	2.42	3.4
		Hard Steel.		
9.	5,760	67,000	1.88	2.82
10.	7,560	65,000	1.93	2.90
11.	14,600	53,500	2.36	3.45
12.	16,300	"	"	"
13.	26,100	46,500	2.72	4.10
14.	32,445	51,000	2.40	3.60
15.	157,815	40,500	3.08	4.55
16.	472,500	34,000	3.70	5.55
		Best Bar Iron.		
17.	108,160	34,000	1.70	2.38
18.	110,000	35,000	1.66	2.32
19.	141,750	34,000	1.70	2.38
20.	389,050	32,000	1.90	2.65
21.	408,000	30,200	2.00	2.80
22.	421,470	32,000	1.90	2.67
23.	480,810	31,000	1.95	2.75

The above series includes a representative number of the writer's experiments with rotating spindles. As a rule, the spindles were 1 inch diameter, and projected about 10 inches from the end of the revolving shaft in which they were fixed. A speed of between 50 and 60 revolutions per minute was maintained day and night. The "soft steel" was fine rivet steel, having a tensile strength of from 60,000 lbs. to 64,000 lbs. per square inch, and an elongation of 28 per cent. in 8 inches. The "hard steel" was a high-class "drift" steel, having a tensile strength double the above, and an elongation of one-half the extent. The "iron" was the best rivet iron, having a tensile strength of from 58,000 to 61,000 lbs., and an elongation of 20 per cent. "Factor *a*" is the ultimate tensile strength per square inch of the specimen divided by the calculated stress upon the outside fibers, due to the load on the end of the projecting bar. "Factor *b*" is the ratio of the static load required to bend the bar a moderate amount beyond the elastic limit, to the load actually imposed upon the revolving bar. These definitions will be made more clear in further references to the table.

SERIES No. 2.

No. of Bends.		Stress per sq. inch.	Factor *a*.
		Soft Steel.	
24.	12,240	44,000	1.59
25.	12,325	"	"
26.	12,410	"	"
27.	18,100	42,000	1 67
28	18,140	"	"
29.	72,420	36,000	1.94
30.	147,390	34,500	2.03
31.	262,680	34,000	2.05
32.	1,183,200	27,500	2.55
33.	3,145,020	34,500	2.03
		Best Bar Iron.	
34.	184,875	34,000	1.68
35.	250,513	"	"
36.	3,145,020	"	"

The above series is a selection from the writer's experiments with flat bars bent laterally. Generally the bars were 1 inch wide by ½ inch thick, and 32 inches long between the bearings. The steel specimens were cut from the tension member plates of the Forth Bridge, and had a tensile strength of about 70,000 lbs. per square inch, and an elongation of 20 per cent. in 8 inches. The iron specimens were rolled bars.

A careful consideration of the results of the preceding experiments will, the writer thinks, illustrate many points of interest to practical engineers. Experience has shown that screw shafts, and axles generally, made of the finest quality of high tension steel are not practically as strong as when made of soft steel, having theoretically perhaps little more than half the strength of the former. Referring to Series No. 1, we find, comparing experiments 8 and 14, that under working stresses in each case equal to about 40 per cent. of the ultimate strength, the hard steel failed with only 32,445 revolutions, while the soft steel stood 14,876,432. Again, comparing experiments 16 and 23, it will be seen that with practically the same number of revolutions the hard steel, though more than double the tensile strength of the iron, broke under a working stress only 10 per cent. greater. It is impossible, in the face of results such as these, to contend that the ordinary laboratory tests of a metal give any adequate measure of its value as a material of construction.

Iron of high quality holds its own, as compared with mild steel, in these experiments, and this is consistent with the general experience as to the driving axles of locomotives, which are subject to repeated bendings of considerable severity. Certain of the soft steel specimens would have given higher results had they not been turned down, as fracture occasionally appeared to have been accelerated by the slight tool marking. On the other hand, No. 8 stood exceptionally well, although it was a turned-down specimen. All of the hard steel bars were put in with the skin on.

An illusion entertained by some engineers that alternating stresses are destructive only if the stress exceeds the elastic limit, is effectually disposed of by these experiments, because none of the stresses in question exceeded the said limit, and some of them were very far below it. Thus, in experiment 16, the working stress was but one-half of the stress at the elastic limit under direct tension, and only one-third of the stress at the elastic limit of the material when under transverse stress; which was really the condition of the specimen in the experiment. "Factor *b*," in the case of experiment 16, has a value of 5.55, which means that less than one-fifth of the static weight required to bend a hard steel pin a small amount, will suffice to fracture the pin if the stress be alternating, as in the case of the pins of connecting rods, for example. If we take what is usually termed the breaking load, or, say in a ductile material like steel, the stress which would deflect the bar as a beam an amount equal to half the span, then the load which ultimately broke the bar in experiment 16 was only one-seventh of the original static breaking load—a sufficiently remarkable result.

Other points of interest may be referred to in connection with Series 2. In general the bars were tested in pairs, so that when one bar broke its companion could be otherwise tested and examined. For example, the companion to No. 28, after being subjected to 18,140 bendings, was tested for tension, and failed with 48,000 lbs. per square inch, and 2.6 per cent. elongation; the original strength of the steel being 70,000 lbs. and 20 per cent. elongation. Again, the companion to No. 32 was, on close examination, found to have a flaw like those found in crank shafts. Nos. 33 and 36 were companion bars bent one way only, so that the stresses were not alternating, hence the largely increased endurance. They were both taken out before actual fracture, but with deep set flaws, clearly illustrating that the cause of failure under repeated stresses is very frequently not so much a gradual deterioration or crystallization of the metal, as the establishment of small but growing flaws. This, of course, is well known to locomotive and marine engineers; and on some railways it is the custom to run crank axles until the incipient flaw is detectable, and then to hoop the webs of the cranks; whilst marine surveyors do not condemn a shaft necessarily on the first appearance of a flaw, but license it to be run for a further definite period.

Another noteworthy fact illustrated by these experiments was, that a structure or piece of mechanism may be subject to a repeated stress equal to 90 per cent. of that which would break it, and yet specimens cut from the metal may exhibit no signs whatsoever of deterioration. The broken half of nearly every specimen in Series No. 2 was tested by the writer with that result. Thus, as the stress was applied at the center of the bars, it followed that at a point 90 per cent. of the half span from the bearings, the stress would be 90 per cent. of that which broke the bar. Although the bars broke short off at the center, at the point referred to, they could invariably be bent double without fracture. Having reference to this fact, and to the fact that the tensile strength was also little affected, it is clearly hopeless to expect to learn much from testing specimens of metal from structures or machines which have been long in use. Unless the experimenter happens to hit off the right moment immediately preceding the commencement of failure, he need not expect to learn much from the behavior of the metal in the testing machine. Professor Kennedy, in an interesting and instructive lecture recently delivered before the Royal Engineers at Chatham, has given the results of tests of 47 pieces of iron and steel, which had either been in constant use for many years until they were so much worn as to require renewal, or, which had broken in actual use; but in no case did he find anything distinctly pointing to a weakening effect due to actual fatigue. This is exactly the result which the writer's experiments would have led him to anticipate; but it by no means follows that the very piece of metal tested by Professor Kennedy and found uninjured would not have broken a few days after in actual working. A man of 70 years of age may be as sound as he was at 20; but the 50 years have told on him nevertheless, and the breakdown is certainly near and may be sudden. Having referred to Professor Kennedy's lecture, it is necessary perhaps for the writer to say that he does not agree in some of the conclusions set forth therein. Thus, Professor Kennedy expresses his belief that the failure of coupling rods occurs as much by the gradual disintegration of the dirt between the laminations, and the oxidation of the iron, as by vibration and repetition of load; and that a homogeneous material like steel remains comparatively uninjured by repetition. This of course is negatived by the results of the experiments cited in the present paper. Professor Kennedy further says: "If a load exceeding the limit of elasticity be applied a considerable number of times the bar will be actually broken, but at the same time we know that if any load exceeding the limit of elasticity be but once applied, the structure to which the bar belongs is distorted and rendered useless." This the writer thinks is a dangerous fallacy, tending to delay the application of truly scientific principles to the design of structures and mechanism.

Both the bridge engineer and the mechanician must reckon with the fact that, owing to the contingencies of manufacture, parts of the metal in every structure are subject to initial stresses exceeding the elastic limit; but fortunately it is not true that the structure is thus "rendered useless." Almost every plate and angle bar of a bridge is cold-straightened before going into the work, which means, necessarily, an initial stress exceeding the elastic limit. Boiler shells are bent cold, and heavy initial stresses close up to the elastic limit remain permanently in operation. The writer has calculated the intensity of these stresses, and verified his results by cold straightening bars, planing away the outside skins on each side and thus relieving the bars of one couple of initial stresses, and leaving the other couple near the neutral axis to operate. The result has been that the bars have ceased to retain their straightness, and have become curved to the extent of ¼ inch, or other amount indicated by calculation. Similarly, lathesmen well know that a long shaft often wobbles when the outer skin is turned off, and many other instances will occur to practical men, proving beyond contest that, in almost every metallic structure, some parts of the metal are stressed beyond the elastic limit. The effects of repetitions of stress cannot therefore be ignored, even where the nominal, or average, stress on the metal is small, because the possible heavy initial stress near the neutral axis may lead to the establishment of a growing flaw, commencing at the center of the cross-section of the bar, and therefore undetectable.

As regards riveted structures, it is of course the aim of both engineer and manufacturer to insure every rivet griping the plates with a stress fully up to the elastic limit. M. Considère, who has recently completed a most important and scientific series of experiments on riveted joints, sums up thus: "In all constructions in which the riveted portions have not already commenced sliding, the rivets work solely by longitudinal tension, and the adherence which this tension gives rise to between the plates in contact constitutes the sole resistance." And further: "Any alternating stress in excess of that adherence rapidly dislocates the work." The writer's experiments fully confirm these conclusions. At the Forth Bridge the rivets in the heavy bedplates are 1$\frac{3}{16}$ inch diameter, and pass through 8 inches thickness of plates. Closed with hydraulic riveters of 40 tons pressure, the rivets as a rule fill the holes fairly; but care is taken that the stress is kept within the frictional adherence arising from the elastic grip of the rivets. With rivets of the same length as those in the bedplates, the average resistance to sliding was found to be equivalent to a shearing stress of 14,500 lbs. per square inch of rivet area; whilst the maximum shearing stress on any rivet in the bridge is limited to 11,200 lbs. per square inch. M. Considère found that with ordinary riveting he could reckon upon an adherence of from 11,300 lbs. to 14,400 lbs. per square inch, which closely agrees with the former results. The writer, by testing plates in ordinary condition, secured with bolts screwed up to a known tension, found the coefficient of friction averaged $\frac{1}{3}$; and he also found the soft steel rivet, after being closed up under hydraulic pressure, had an increased ultimate strength of 10 to 15 per cent., and an elastic limit of about 45,000 lbs. per square inch. Now 45,000 lbs. × $\frac{1}{3} = 15,000$, which was the ascertained resistance to sliding of the plates; and it is clearly demonstrated, therefore, that in sound work the rivets are permanently strained up to the elastic limit. This result was further confirmed by temperature experiments, measurements of contraction of rivets, and other means which need not be detailed here.

In order to ascertain whether alternating stresses were as prejudicial to members, such as piston rods, subject to direct pull and thrust, as to shafts subject to transverse bending, the writer carried out a series of experiments on specimens so designed as to give alternate direct tension and compression on small pieces of metal. These specimens were of three types illustrated (not to scale), by Figs. 50, 51, and 52. In the first, the pieces of metal tested were sometimes of round and sometimes of flat cross-section, and were bolted to a couple of spring bars, as shown on the sketch; the stress being applied by opening and closing the legs of the tongs, and thus putting the metal into alternate tension and compression. In the second group, the spring bars and specimens were all sawn and slotted out of one piece of steel, and the necessity of constantly tightening up the nuts was thus avoided. In the third the specimens were shaped as shown by Fig. 52, and a bending stress applied at the center of the bars.

Fig. 50.

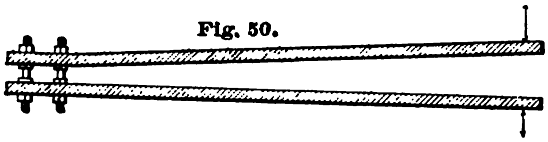


Fig. 51.

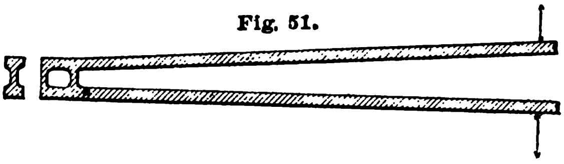
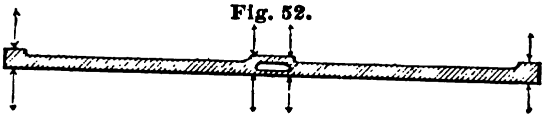


Fig. 52.



Series No. 3.

Soft Steel.

	No. of bends.	Stress per sq. inch.	Factor *a*.
Fig. 50.	28,008	37,000	1.90
	49,320	38,000	1.84
Fig. 51.	11,880	28,000	2.50
	29,568 (hard steel).	16,700	4.90
Fig. 52.	230,518	35,000.	2.00
	294,735	25,000	2.80

Other experiments were made, but the above are sufficient to prove that alternating stresses are at least as prejudicial when the stresses are direct as when they are indirect.

Series No. 4.

The writer has availed himself of the opportunity afforded by the large use of special plant and machinery at the Forth Bridge Works, to note the influence of varying stresses on full-sized riveted steel girders. These observations are still in progress, and can be but very briefly referred to herein. In one instance the lever of a large plate-bending press is of box girder section, built up of eight 4" × 4" × ⅝" angle bars, two 13" × ¾" web plates and two 17" × ½ × ⅝ flanges. The span is 15 feet 8 inches, and the ordinary daily working stress on the metal is 43,000 lbs., and occasionally 57,000 lbs. per square inch. Many thousand applications of this stress have been made, and the beam has taken a permanent set of ⅞", but so far is otherwise intact. Observations are also being made of the behavior of sixty riveted steel box girders of 18 feet span, built up of two 12" × 3" channels and two flange plates; which girders are subject to very many thousand repetitions of stress ranging from zero to 13 tons per square inch.

As regards the important question of the proper working stress on iron and steel, the writer's experience leads him to believe that both the old-fashioned government regulations, giving the same limiting stress for all kinds of loading, and the modern formulæ, based chiefly on Wöhler's experiments, fail to meet the just requirements of the practical engineer. It is in many cases a great economical advantage and convenience, to have reference not merely to the variation of stress, but also to the probable number of applications. For example, the writer knows the bending press box girder lever, previously referred to, will last its time, although the working stress is about ⅔ of the ultimate strength of the material; and it would have been a mere waste of money to make it four times as strong, and so give it the factor of safety of 6—usual and proper enough for a structure such as the "Elevated Railway" of New York, where a practically infinite number of repetitions of stress have to be provided for. Again, in many instances it is preferable to face the fact of occasional breakages than to attempt to give the strength required to insure absolute durability. Thus, in case of railway springs, a fractured leaf is a common and unimportant incident, little to be wondered at, as the working stress often ranges from 60,000 lbs. to 80,000 lbs. per square inch, on steel having in its tempered condition an ultimate tensile strength of about 160,000 lbs. per square inch. If it were as essential to guard against the failure of a spring leaf as it is to guarantee the safety of a railway bridge, it would be necessary to more than double the number of leaves in most springs.

In all works, temporary or permanent, where the stress alternates from tension to compression, large rivet area must be provided, as a very few repetitions suffice to loosen rivets. Where metal has been subject only to compressive stresses of varying intensity, the writer has not so far been able to detect any deterioration, and he takes account of this fact in settling the proper working stress on the struts of bridge trusses and similar works.

Twenty years ago, being uncontrolled by government regulations, the writer adopted a working stress of 16,000 lbs. per square inch on many large iron girders carrying a heavy dead load; although at that time a departure from the universal 11,200 lbs. per square inch was regarded with suspicion. The results of modern research have, however, now given the engineer a free hand, and the British 5 tons per square inch and the Continental 6 kilos. per square millimeter have ceased to be regarded with superstitious reverence. A machine or a bridge can only be well proportioned by carefully considering the special conditions of the case, in the light of experimental data and past experience. A string of formulæ will not make an engineer.

In concluding this necessarily very hurried and imperfect paper, the writer would like to bear testimony to the admirable behavior of a very good friend of his—mild steel. During the past three years he has had to deal with about 24,000 tons of that material, and to submit it in many cases to very harsh treatment. He has had more cases of so termed "mysterious fractures" with the few tons of wrought iron, used for certain temporary purposes, than with the whole 24,000 tons of steel. This result of his experience may be of interest to brother members of this society who now are, or will doubtless be, large users of mild steel; and the testimony is perhaps of the greater value as the work at the Forth is pressed on day and night, and no precautions are taken which would not equally be necessary, were the material the highest class of Lowmoor iron, costing double or treble the price of the steel.

DISCUSSION.

Prof. De Volson Wood. *—There were two points in this paper which struck me, and to which I am pleased to call attention. The first is on the eighth page.

"This, of course, is negatived by the results of the experiments cited in the present paper. Prof. Kennedy further says: 'If a load exceeding the limit of elasticity be applied a considerable number of times, the bar will be actually broken, but at the same time we know that if any load exceeding the limit of elasticity be but once applied, the structure to which the bar belongs is distorted and rendered useless.'"

It is the last part of this quotation to which I wish to call attention—that if a bar is strained beyond the elastic limit it is made useless. Some years ago, when preparing a book upon this subject, I came across some experiments made by Hodgkinson, given in my *Treatise on the Resistance of Materials*, on page 44. A bar of iron was broken; then one of the pieces was broken again, and then thirdly a piece of that one which was broken the second time was broken again. The section of the piece was thus reduced, but it took more to break the second piece than it did the first, and more to break the third than it did the second. The section of the third piece was ten per cent. less than the first, but it required a stress twenty per cent. greater to break it. I wrote something like this: "The repeated fracture of iron makes the pieces stronger." When I revised it I thought that was pretty strong; I was not certain of the demonstration; so I put in the word "apparently." I, however, had not completed the book before we received an experiment, made at the Navy Yard at Washington, and a record of that experiment is given on page 250, and it was that experiment which is so full of interest. The identical pieces which were used in making those experiments are on exhibition in my lecture room, at the Stevens Institute of Technology. The piece had two eyes, Fig. 91, the ends being greatly enlarged, so that there would, apparently, be no danger of fracture through those. Now this par-

FIG. 91.

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* By invitation.

ticular specimen was being pulled apart at *a*. As Commodore Beardslee said, the piece that was being pulled apart evidently had failed. It was being reduced at this point, when, for some unexplained reason, one or the other of the eyes broke. He said, "Weld the eye, and we will break it to-morrow." They welded the eye, put it into the machine, expected to see it fail at this point *a*, but instead of that it began to reduce at *b*. Now, you notice that this section was perceptibly reduced from what it was originally, and very perceptibly smaller than at *b*, and yet it did not break at *a*; it broke at *b*; from which we inferred that the part at *a* was made stronger, not by breaking, but by resting. It required twenty-five per cent. greater stress to break the piece at *b* than was pulling it apart at *a* the day before. It was out of the machine several hours, and we can only conclude that the particles had so arranged themselves that they could resist better at *a* than at *b*. I have used this illustration sometimes—that the particles at *a* got their backs up and so resisted harder than before.

Experiments were afterwards made in this lecture room to confirm that experiment, and the conclusion reached is this—that overstraining iron once, if you allow it to rest completely afterwards, does not injure its resistance; and experiments were carried further—that you might overstrain it two or three times and not only apparently make it stronger, but increase its elastic limit. There is an end to that just as there is to the increase of the strength of a person by overdoing and getting rested, and then doing it again. After a time he feels the effect, and ultimately, if it is carried too far, it will injure him, ruin him; and so it may ruin the piece. The latter part of that conclusion of Kennedy was not correct, that a bar cannot be overstrained once without destroying its usefulness.

Another point was on the twelfth page of the paper:

"A machine or a bridge can only be well proportioned by carefully considering the special conditions of the case, in the light of experimental data and past experience."

The idea that struck me was this—who of us, after reading the conclusions and the criticisms of experimentalists, would feel that we were warranted in proportioning a structure accurately, according to the light of past experience? It resolves itself into this: at first we make a few experiments, and establish a law. We try that law a while. Somebody experiments again, and finds that that law is not quite correct, and proposes a modification; and after going on in this way with experimental matters we virtually become empiricists. We cannot make laws which will work nicely and accurately under all the circumstances. Formulated experience is exceedingly valuable. Without it, what could the man do who could not experiment? With it, the experimentalist is not tied down and confined to a formula. I have sometimes illustrated this matter thus: a man experiments in hydraulics and lays down a law in regard to the flow of water. Another man experiments and says that is all wrong, for it does not agree with the facts which he has found. A third experiments, and says that is all wrong, and he gives us his facts; and a fourth one experiments, and says they are all wrong and here are the facts, and by and by we come to the conclusion that the conditions are so varied that they may all be right for the conditions under which they established their rules, and none of them correct for the others. Now, the properties of materials, as manufactured, vary, and it is necessary to make experiments in order to gain a knowledge of the modification of these properties. I have heard, sometimes feared, that the so-called coefficients or factors of safety would be worked too close, because we may not take into account all the conditions. I think it was Holley who said: "Talk about coefficients of safety, or factors of safety, why, they are factors of ignorance!" I admit it. There is a certain degree of ignorance bordering along the line of exactness which you cannot settle one side or the other, and in my instruction I always advise being on the safe side, in case of doubt.

Mr. G. M. Bond.—In regard to the statement on the second page, as to the girder section of side-rods on a locomotive being considered the best, it would seem that some railroads in this country differ from that opinion. I noticed the other day one instance especially—the Old Colony Railroad Co. They consider that a side-rod is made too stiff laterally by making it web section, and they find in their experience that the ordinary flat parallel bar is the best. Other roads, however, seem to consider the web section the best.

Mr. Wm. Kent.—Mr. Baker's paper is a valuable contribution to the growing literature of the subject upon which it treats, in giving new records of experiments which may, some day, be collated with other similar records, and lead to generalizations and deductions which can scarcely be made at present, on account of the insufficiency of reliable data. That the data now available are not sufficient to enable us to frame practical formulæ for the working stress of iron and steel, is shown by Mr. Baker's refusal to accept "the modern formulæ based chiefly on Wöhler's experiments," and his disagreement with some of the conclusions drawn by Prof. Kennedy from other experiments. We have no higher authorities on the working stress of iron and steel than Wöhler, Kennedy, and Baker, and if they differ among themselves in their generalizations, we must accept their opinions only tentatively, and wait for more extensive experiments before forming too positive conclusions.

The deduction drawn by Mr. Baker from his Series No. 1 appears to the writer insufficient and misleading. He states that axles "made of the finest quality of high tension steel are not practically as strong as when made of soft steel," and "comparing experiments 8 and 14, under working stresses in each case equal to about 40 per cent. of the ultimate strength, the hard steel failed with only 32,445 revolutions, while the soft steel stood 14,876,432." No other meaning can be drawn from this language than that soft steel is superior to hard steel under repeated stresses, and such meaning was, no doubt, intended, yet neither the statement in reference to the axles nor the figures of series No. 1, necessarily lead to such a conclusion. If it is true that axles of the "finest quality of high tension steel" are not practically as strong as those of soft steel, it by no means follows that there is not an intermediate grade of hard steel, say 40 to 60 carbon, which is not practically stronger than either "the finest quality of high tension steel" or "soft steel." So, also, while it may be true that when a piece of soft steel, and a piece of hard steel of the same size, both strained to 40 per cent. of their ultimate strength, are tested by repeated strains, the soft steel stands 14,000,000 revolutions, while the hard steel stands only 32,000, it by no means follows that the hard steel is not actually superior to the soft steel when both are strained to the same amount. In fact, this is clearly shown by Mr. Baker's table. Experiments No. 3, 4, 5, 6, and 7, all soft steel, all strained to 34,000 pounds per square inch, stood from 68,000 to 155,000 revolutions, while No. 16, hard steel, also strained to 34,000 pounds per square inch, stood 472,500 revolutions, or more than three times as many as the best of the soft steel specimens, and about seven times as many as the worst of them, while No. 15, strained to 40,500 pounds per square inch (or nearly 20 per cent. more than the 34,000 pounds), stood 157,800 revolutions, or more than the best of the soft steel specimens. Again, comparing No. 15 with Nos. 1 and 2, the hard steel, strained to 40,500 lbs. per square inch, stood from 2½ to 4 times as many revolutions as the soft steel strained to only 36,000 pounds per square inch. The deduction I would draw from Series No. 1 is, that the hard steel is superior to the soft steel.

In practice such a question as this might arise: A certain tension member is to be dimensioned on the basis of 10,000 lbs. allowed static strain to the square inch. Shall soft steel, or a medium hard steel be used to give the greatest probable life under repeated strains? Here the dimension of the member is fixed; it will weigh no more and cost no more if made of the hard steel than of the soft. I would say, use the steel which stood 472,000 revolutions under a strain of 34,000 lbs. to the square inch, rather than the one which stood only 155,000 revolutions—in other words, use the hard steel rather than the soft. This is the deduction to be drawn from the experiments of Series No. 1, although there may be other reasons why a softer steel might be preferred, such as the greater liability of the hard steel to damage during the processes of manufacture into its finished shape.

Some nine years ago a number of experiments were published by Mr. William Metcalf (*Metallurgical Review*, Vol. I., p. 399), which tend to show that a medium hard steel resists repeated shocks better than soft steel. His paper is well worthy of study by those interested in the subject, but the following may be quoted here:

"The next case was that of steel for small pitmans, where the test required was that a machine should run 4½ hours, at a rate of 1,200 revolutions per minute, unloaded before the pitman broke. . . .

"The first trial was with .53 carbon steel, mean time of six trials, 2 hours, 9⅛ minutes. Second trial, .65 carbon steel, mean time of six trials, 2 hours, 57½ minutes. Third trial, .85 carbon steel, mean time of three trials, 9 hours, 45 minutes. . . .

"This led to a trial of a set of twelve pitmans of a finer quality of steel than the above.

The .30 carbon ran 1 h. 21 m., heated and bent before breaking.
.49 " " 1 h. 28 m. " " "
.53 " " 4 h. 57 m., broke without heating.
.65 " " 3 h. 50 m., broke at weld, where imperfect.
.80 " " 5 h. 40 m.
.84 " " 18 h.
.87 " broke in weld, near the end.
.96 " ran 4 h. 55 m., and the machine broke down."

Another statement of Mr. Baker seems open to criticism: that "a structure or piece of mechanism may be subject to a repeated stress equal to 90 per cent. of that which would break it, and yet specimens cut from the metal may exhibit no signs whatever of deterioration." It is not at all certain, as he says, that in a bar tested by transverse strain the stress at a "point 90 per cent. of the half span from the bearings, the stress would be 90 per cent. of that which broke the bar." In steel which bends greatly before breaking, it is very far from true. Before making such a broad generalization from transverse tests, a crucial test should be made by direct tension, and I venture to say such a test would utterly destroy the generalization. Take a bar of any soft or medium steel, cut it in half, and break one-half by tensile strain. Then test the other half to 90 per cent. of this strain, not repeatedly, but only once, take it out of the testing machine, and lay it aside for a day or two, and then test it again, plotting carefully its extensions under successive increments of load up to the breaking point. It will show two changes at least, first, greatly increased elastic limit, second, greatly decreased ductility (measuring the elongations anew, and not from the original marks), and most probably a third change, a considerable increase of tensile strength. This class of phenomena ought now to be well known to engineers. They were published about ten years ago by Prof. Thurston, in the Transactions of the American Society of Civil Engineers, and were discussed at considerable length by Commander Beardslee in the reports of the U. S. Iron and Steel Testing Board, besides having found their way into some American text-books. They are entirely inconsistent with the conclusion that a structure "may be subject to repeated stress equal to 90 per cent. of that which would break it, and yet specimens cut from the metal may exhibit no signs whatsoever of deterioration," if such structure is made of the qualities of iron and steel generally used in construction.

Mr. G. C. Henning.—I would like to say a few words in regard to Professor Wood's remarks on Professor Kennedy's assertion that a rod becomes useless when it is once strained beyond the elastic limit. From tests we have found out that material becomes stronger after being once strained beyond the elastic limit, but we have also found out that it invariably decreases in ductility. Materials are similarly affected when strained below elastic limit, but to a much less degree. If we take two bars, one of which has been stretched beyond the elastic limit, that bar will not stretch as much for any given load put upon it as the other bar which has not been strained. The result will be this: if we have two members side by side or close together, one of which has been strained beyond the elastic limit, and which are loaded equally per square inch of section, these two members will not work in harmony. The direct result will be that the member which has not been strained beyond the elastic limit will receive and carry less of the load than the other member. This will throw unequal strains into the structure, and the result will be that the structure itself will not act uniformly throughout. Whenever a load is put on a structure some members will take more stress than others, and our principal object in making substantial structures and distributing the stress in proportion to the section of the material will be destroyed. The result is that we can no longer have much confidence in such a structure, and especially if a long one where there are many parts duplicated, if the members are so irregular in character in regard to strain, which affects the shape and consequently the distribution of stress, which is of vital importance to the life of the structure. For that reason I think Professor Kennedy is correct, that such a bar must not be used in a structure in connection with other bars which have not been so treated. To strain only one member is all wrong. Let me take the case of a bridge. If we should stretch the members in only one panel that panel will be stiffer than the rest. The result is that when a train comes on the bridge the part which is deflected must regularly receive an extra impulse from the load, and that side which has not been stretched in that way will always get a greater load and the bridge will never act properly under it, and it is then only a question of quantity of stress which may produce serious consequences. Our object in fabricating structures is to get all the parts as nearly uniform as possible, and therefore we must select such members as have been treated in a way that is likely to affect, not their ultimate strength—we do not care much about that—but their elasticity, that is, their relative expansion under given loads, and pair them off or reject them. All our structures are machines, whether we can see the motion or it be so slight that it can only be observed by careful measurement. If you have, of course, only a single member, like the connecting rod of an engine, that may be stretched beyond the elastic limit and not affect anything else, but generally we have many members to make up one structure.

In regard to Professor Baker's paper directly, I think it is one of the notable papers of treatment of material as used by the engineer. We have done too little of this work. We have confined ourselves to the work of simply finding out what material will stand ultimately in one test. Materials are not used that way at all. Materials are used time and time again, and thousands and millions of times, and we must not build for present use but for future use, because to have any confidence in the life of a structure we must know that after long years of use the structure will be as good as when originally constructed. Professor Lanza made experiments on the bending of shafts, and he found similar results. He had not strained the shaft beyond the elastic limit, but the shaft broke. We know too little about the exact experiments that Mr. Baker made to allow us to criticise the results closely. He tells us that these results are simply a few—he gives simply a generalization of the experiments he has made, which were quite numerous, and oft repeated. Many accidents happen in every-day life, especially to machinery which is subject to largely varying strains, which show us that our usual custom of proportioning parts according to the actual tensile or compressive strength of material is not correct, and that has been one of the reasons why the scientific engineer's deductions have been looked down upon by the practical man, who says if you proportion this according to the strength of that little piece, you are all wrong, because it will break in a little while. That is what we found out in the connecting rod of the engine; that we were not proportioning it according to what the rod was doing; we were proportioning it by testing it in a way in which it was not at all used in the structure; and I believe that such results as were given by Mr. Kent, quoting Mr. Metcalf, were deduced from a very small number of tests, and they should not have that weight which these results given in the paper carry with them, made by an engineer who has made this a specialty, particularly with the object in view of determining the life of material, rather than by one who has made only a few to determine a particular case. I think, as a general thing, the results of this paper are far ahead of our present practice in engineering, especially in large structures, and we cannot do enough to find out more in that same direction, and I only hope that we will get more of this information from Mr. Baker. (Applause.)

I would like to add something that I had forgotten, if no one else wishes to speak.

Mr. Baker has made a little remark here at the end of the paper, which I think is too important to be overlooked, and I am sorry that no one of the members here has called attention to it; it is where he says that: "In my experience of using 24,000 tons of steel I have not had as many failures as in using a similar quantity of pounds of wrought iron." I think Mr. Baker is an engineer who takes an advanced standing in the use of steel, and all our old fogy engineers who stick to wrought iron will be surprised at that remark. They forget that this steel which Mr. Baker speaks of is a material more like wrought iron than anything else we have, only much superior to it. It has no fiber and no slag between the fibers. I think all of our best experience leads us to believe more strongly that steel of that character, when made uniformly, is an article so far superior to wrought iron that it is only a question of time when the latter will be replaced by the former.

Mr. Kent.—I wish to make a remark on the subject of which Prof. Wood and Mr. Henning both spoke, in regard to Prof. Kennedy's observation as to making useless a bar once strained beyond the elastic limit. Mr. Baker differs from Prof. Kennedy. Prof. Wood, I believe, differs from Prof. Kennedy and agrees with Mr. Baker. Mr. Henning disagrees with Mr. Baker and Prof. Wood, and agrees with Mr. Kennedy, as I understood it. My preference is to side with Mr. Baker and Prof. Wood in this case. Suppose we have four tension members in a very long bridge panel, each of which would break with say fifteen or twenty per cent. extension. What would happen if we should take one of these members which happened to be a trifle short and stretch it one per cent. or two per cent. in order to lengthen it to fit the connecting pin? The result would be, as far as we know from the experience on record, that if we stretch that bar one per cent. or two per cent. we would increase its initial elastic limit a little; we would increase its tensile strength somewhat; we would change its condition with reference to the other parts; and, therefore, according to Mr. Henning's statement, we would make that structure not to act in harmony. But I think we would not change, as far as I know, the modulus of elasticity; that is, I think, a thing that can scarcely be definitely asserted yet, but it requires experiment.

Mr. Henning.—I would like to correct Mr. Kent. I am fully in harmony with Prof. Wood and Mr. Baker. I was speaking with respect to the modulus of elasticity. I said the relative strain due to given loads; that has been deduced from hundreds of experiments I have made on full-sized bridge bars. The results of these experiments have shown me that the relative strain of bars decreases as you increase the stress on the bars beyond the elastic limit, upon repetition of stress. Finally, the bar will break without any strain and at a higher load than the bar originally stood. The result in those four bars instanced by Mr. Kent I will explain in this way. The one which has been strained will not stretch as much as the others, but will pull unequally on the pins. The pins will stand at an angle with one another. They will begin to twist the structure. Then that bar will stand more strain than the other bars until those pins begin to stand parallel again, and that bar will continue to be stretched, and it will again be strained beyond the elastic limit before it will be as long as the other bars. Now, this bar is to some extent out of harmony with the others, and it does not come into play. The result is that the other bars are carrying more load than they are intended for, and the total stretch of the bar will not be the same, and it will cause unsymmetrical deflection of the bridge.

I am in harmony with Mr. Baker in what he asserted; only I want to point out that Prof. Kennedy and Mr. Baker were speaking in a different way. They were not looking at the same problem from the same point of view. Each was right in his way.

Mr. J. T. Hawkins.—Referring to the experiments of Prof. Thurston with his torsion machine, and others alluded to by Prof. Wood, my recollection of the former is, that specimens having been strained to the yielding point or slightly beyond the limit of elasticity, they were allowed to rest for a certain time under the stress which caused them to yield, or nearly that, and that after a given time it was found that they had so far recuperated, under these conditions, that the elastic limit became higher than before.

I would like to ask Prof. Wood if, in connection with these tests, any experiments were made in which, after reaching or slightly exceeding the elastic limit, the specimens were allowed to recuperate not under stress—that is, immediately upon the commencement of yielding—removing all stress and allowing them to remain in that condition for an equal time with those specimens which recuperated under stress?

If such tests were had, I would also ask him if any tabulated or plotted results have been made, so that they might be compared in the two cases? I can see that such data as might be gotten at in this way would be very valuable: for instance, it would be exceedingly well to know the results of such experiments as applied to the case given by Mr. Kent. Perhaps, however, this has been thoroughly gone through with, but I do not remember seeing anything of the kind.

Professor Wood.—Experiments were made in the Institute under the direction of Professor Thurston, with his automatic torsion machine, in which pieces, after being twisted considerably beyond their elastic limit, were entirely relieved of stress, and after resting for a shorter or longer time, were again twisted. It was found that good fibrous iron thus treated exhibited increased elasticity when again twisted. These facts are shown in the large cut in Part II. of *Thurston's Materials of Engineering;* or, in my *Treatise on the Resistance of Materials.* Possibly Mr. Hawkins' question will be more specifically answered by the table on page 602 of Thurston's work just referred to.

Mr. H. R. Towne.—Mr. Henning has called attention to the fact that the experiments mentioned in the paper are quite few in number, and the inference is given that they are examples of a very large number. That may be so, and if it is so it will qualify what I have to say. But there is one indication which they give which makes it regrettable that the experiments were not carried farther; for the reason that it appears that they go up to the point of indicating the best results—or near to it—and stop short. Take for instance Series No. 1, on page 4. If we were to plot the results there, showing in the first place the number of revolutions causing rupture, as indicated in the first column (by the way, unless the paper is read quite carefully, I think it will not be clear at first that the figures indicating the number of revolutions are those giving the number of revolutions which the shaft or piece stood before breaking; this is apparent on careful reading, but it is not stated directly)—if we take these as representing the ordinates and the stress per square inch the abscissæ, we may trace the curve so produced. We start with only 40,000 revolutions, with a stress per square inch of 36,000 pounds, and continue until with only 26,000 pounds stress per square inch we get 14,000,000 revolutions. So again, with hard steel, starting with less than 6,000 revolutions and 67,000 pounds of stress per square inch, increasing the number of revolutions to 472,000, the stress is reduced to 34,000, giving us another diagram. In bar iron, the third set of figures, the diagram would be somewhat different again, and so on all through. Taking the Series No. 2, on the fifth page, where the number of bends at a stress of 44,000 pounds is only 12,000; if the stress is reduced to 34,000 pounds, the number of revolutions rises to over three millions.

Now it appears from this that after reducing the stress to a certain point the increase in the endurance of the specimen becomes very rapid, and that the series was not carried to the point where that rapidity of gain ceases; whereas it would be much more interesting and probably more valuable to us if the series was continued still further to show the ultimate development of the cause, for the reason that the stresses at which the test series terminate—34,000 pounds in the one case and 26,000 pounds in the other—are still above the working limits of practice.

The figures cited from the London and Northwestern Bridge, the Conway Bridge—are particularly interesting—as showing that here is a structure which has endured for thirty-six years, apparently with a factor of only two and a half or two; but as offsetting that it should be remembered that that bridge was built under the most skillful engineering supervision, with extreme care in the proportioning of parts, in the testing of material and in the inspection of the work as done, and that the ratio of the static load to the live load is unusually large by reason of the nature of the structure. This latter fact is one, I think, which comes into most bridge-work, and does not apply to machine designs; namely, that where the static load is very large in proportion to the live load a much smaller factor or margin can be adopted than in structures where the entire stress, or nearly the entire stress, is due to the live load.

Several of the speakers this morning have alluded to the effect of the straining of material beyond its elastic limit, and I may emphasize what I have previously said as to the fact of this being beneficial by citing the case of chains, in which for a long time it has been the practice in the concern with which I am connected to stretch the material purposely beyond its elastic limit, both to increase its strength, and also to decrease and define its stretching point. Chain made of soft iron of considerable ductility stretches inevitably as soon as it is loaded even to less than its intended limit, and that stretching tends to alter the pitch of the chain. Where it is important that the pitch shall be constant, in order to overcome that tendency the chain is stretched initially, when first made, to a point beyond the intended load that it is to work under, and a permanent set thereby given it which brings it to its final and intended pitch. After that, so long as the loads are kept within the intended limit, which is considerably below the stretching load put upon it for this purpose, no further change will take place. The elastic strength of the iron has undoubtedly been increased by giving it this initial set. The same beneficial result can be obtained in any material by treating it in like manner. It is important to know the exact amount of load which is put on in this way; otherwise the material may be injured by unduly stretching it.

CCXXXIII.

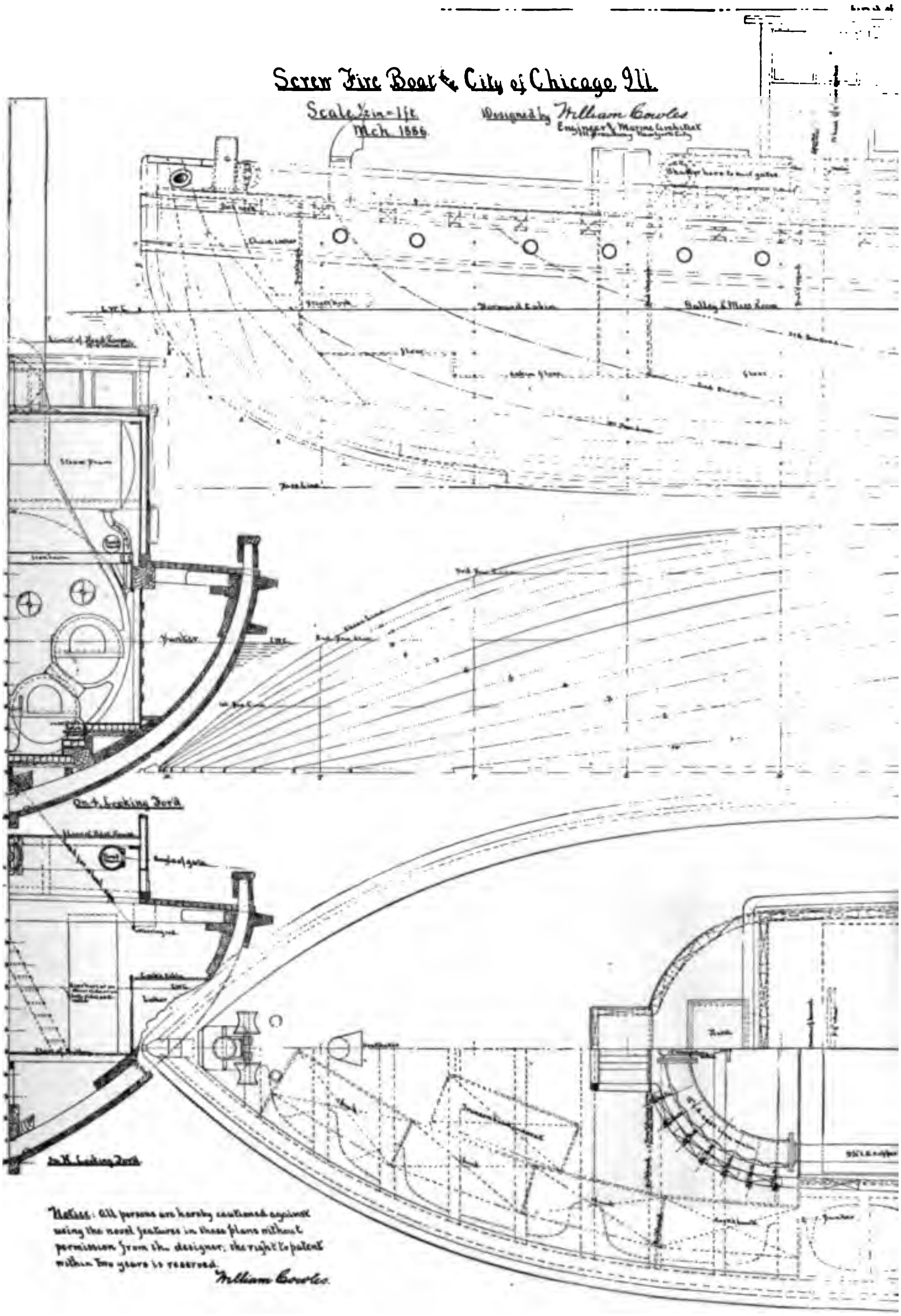
FIRE BOATS.

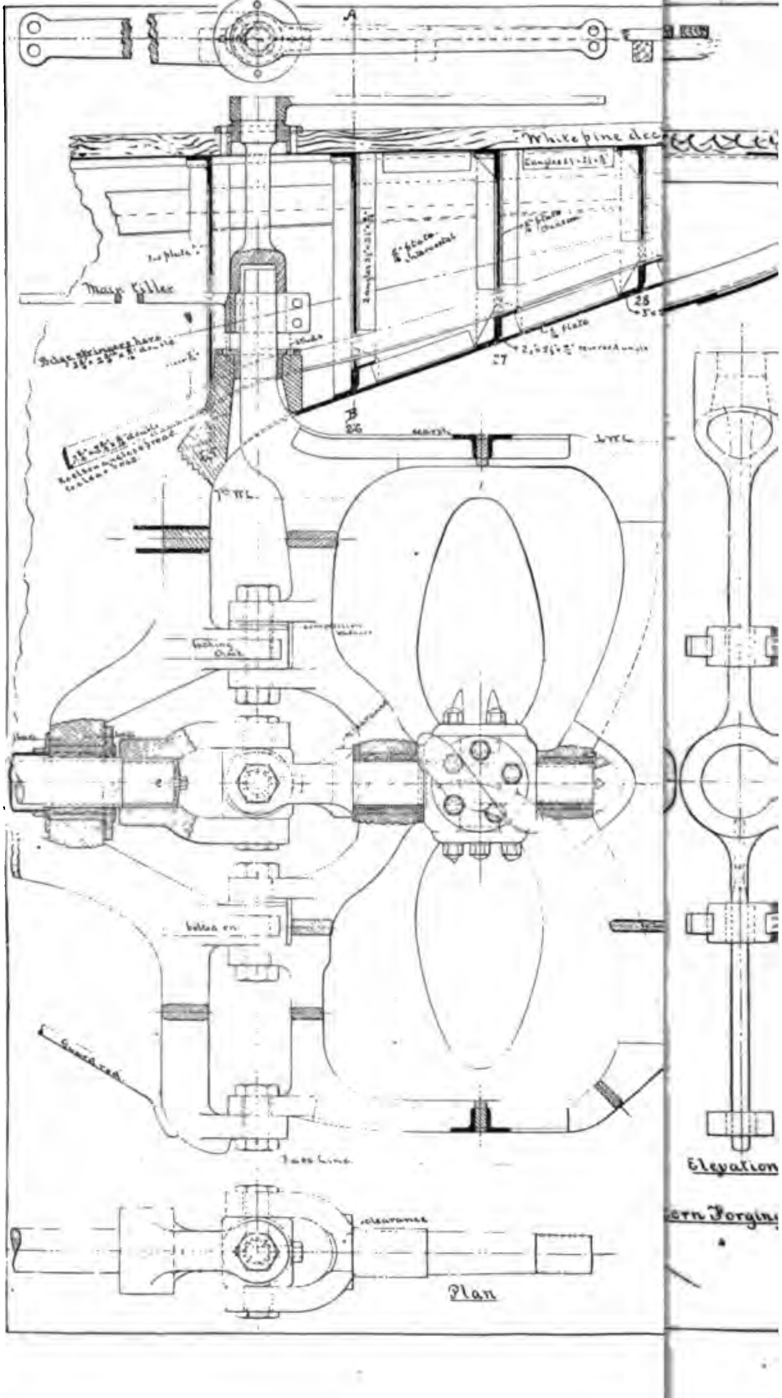
BY WILLIAM COWLES, NEW YORK CITY.
(Member of the Society.)

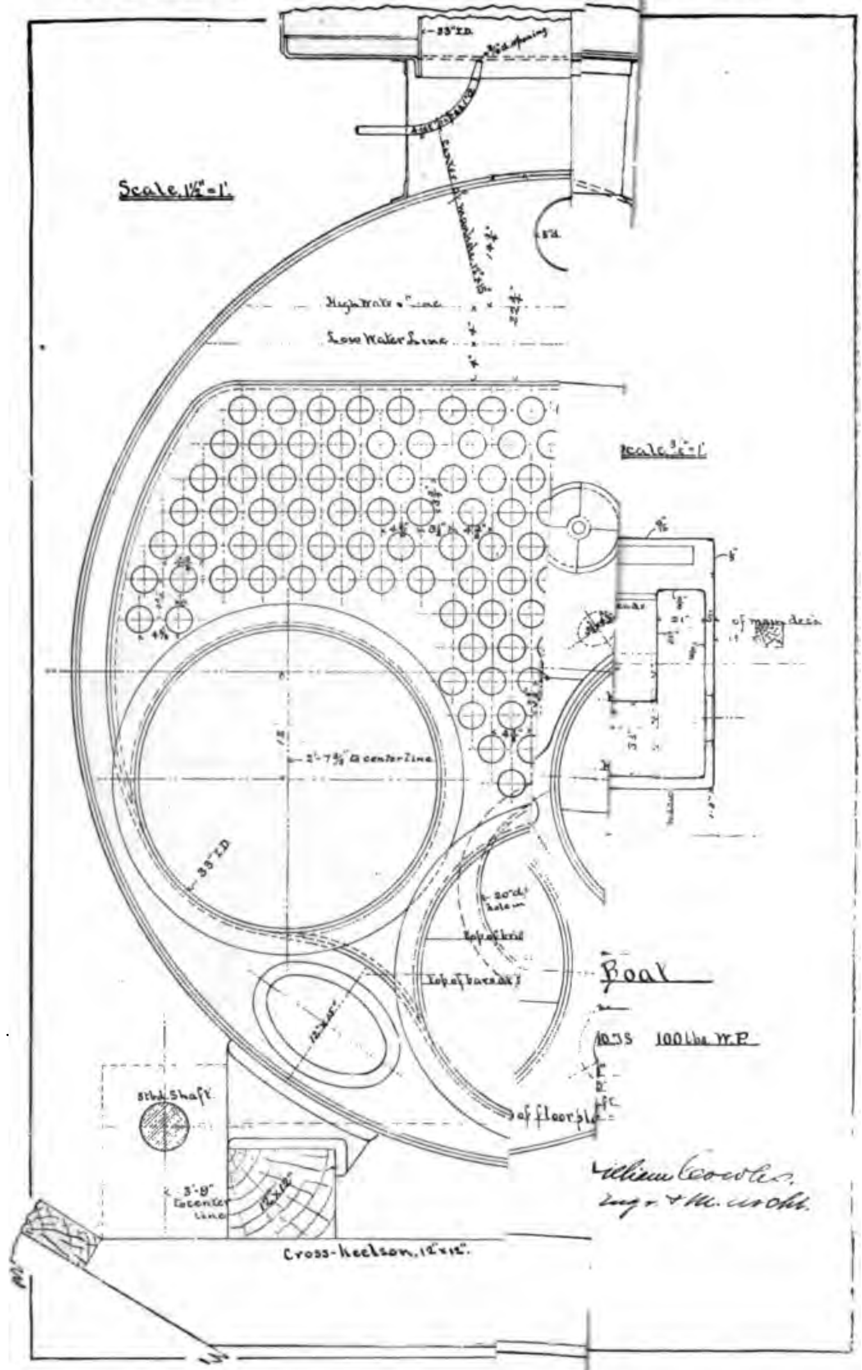
THE United States Government regulations concerning the matter provide that all steamers shall be equipped with a steam pump or pumps so connected as to be used in case of fire, together with a small supply of 2½-inch hose, pipes, axes, etc.; but this has reference only to self-protection. Heretofore it has been a common custom among corporations having much property on water fronts or afloat to equip their tugs with a steam pump larger than required by law, and with a little extra hose to be used for the fire protection of their property on shore or on other vessels. Several years ago, a number of city governments began to take up and enlarge on this idea, and in 1884 there were a number of *fire tugs*, regularly owned or chartered as such, being used and managed in connection with city fire departments. But these boats are essentially tugs equipped with comparatively small fire pumps, and, with the exception of the *Zophar Mills* of New York City, have very little pumping capacity.

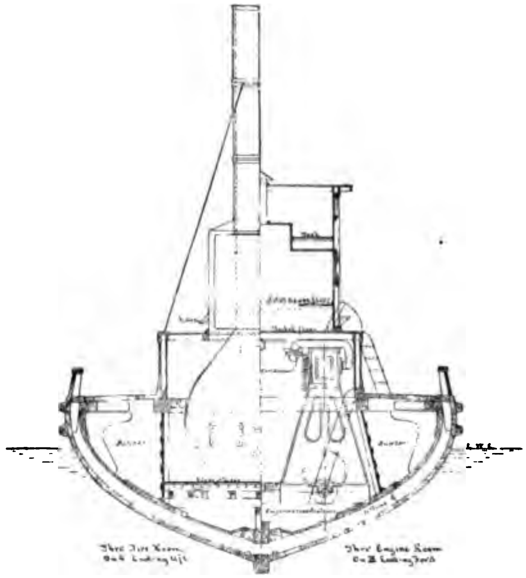
As far as known to the writer, it was in the year 1885 that the *first floating fire engine*, self-propelling and of high power, was designed and built; this was the *Seth Low* of the Brooklyn Fire Department. (See Figs. 53 and 55.) Since the *Seth Low*, two others somewhat similar, but with improvements, have been designed and are now built and in service, one in Cleveland (see Figs. 55 to 59), the other, the *Geyser*, in Chicago (see Figs. 60 and 61). Two designs, one wood the other iron, were made for the Cleveland boat, and after receiving bids on both, the wooden boat was decided upon and built.

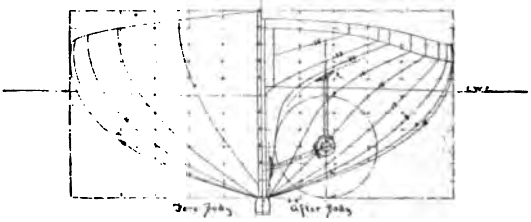
A Comparative Table has been prepared showing these three boats in contrast with the *Zophar Mills*, the largest and best of their American predecessors, and the *Merryweather*, an English contemporary built in 1886, which seems to have been the first of its kind across the

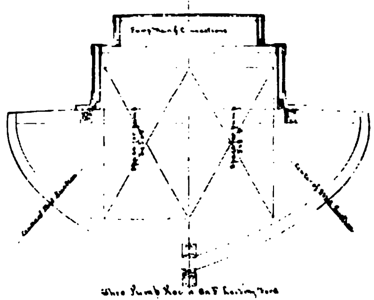

Screw Fire Boat for City of Chicago Ill.
Scale ½ in = 1 ft
Mch. 1886.
Designed by William Cowles
Engineer & Marine Architect
Forward Cabin
Galley & Mess Room
L.W.L.
Bunker
On 4 Looking Forw'd
Keel Line
Notice: All persons are hereby cautioned against using the novel features in these plans without permission from the designer; the right to patent within two years is reserved.
William Cowles.


A
White pine dec
Main Tiller
B
26
27
28
L.W.L.
bolted on
Guard rod
Base Line
clearance
Plan
Elevation
ern Forgin


Scale 1½" = 1'.
33" I.D.
High Water Line
Low Water Line
2'-7⅝" to center Line
33" I.D.
Scale ¾" = 1'
of main deck
20" dia hole
Top of bridge
Top of bars
Boat
100 lbs W.P.
of floor pl
Stbd. Shaft
3'-9" to center Line
Cross-Keelson, 12"×12".
William Cowles,
Engr. & M. Archt.


Thro' Fire Room
Thro' Engine Room


L.W.L.
Fore Body
After Body



water, and of which the chairman of the London Fire Brigade Committee is reported to have said, "It is unquestionably what the committee requires on the Thames." * The *Merryweather* was built by Messrs. Merryweather & Sons, Engineers, London, to the order of the Egyptian Government, for use at Alexandria, and is undoubtedly an excellent fire engine of great capacity, considering her size, and may be assumed to be very well suited to a port like Alexandria, but her suitability is not so obvious for use on the water fronts of London or any other very large port. Her reported maximum fire capacity is a single 1¾-inch to 2-inch stream thrown 200 feet; about equal to one first-class shore fire engine of the largest size used in American cities. That is, it would take from eight to ten *Merryweathers* to equal one such boat as Chicago has in the *Geyser*, and the latter is none too large for efficient use in a large city. The table indicates also a marked difference between the modern boats and the *Zophar Mills* in dimensions, cost and capacity. The latter boat is simply an enlargement of the tug idea, an enlargement which makes her very cumbersome and expensive. One of the unparalleled advantages in having fire boats is the capacity thus obtainable for throwing large streams at a large fire. Little streams from ⅞-inch to 1¾-inch, such as the ordinary single shore engine can throw, are almost useless at a large fire except to keep it from spreading; they are vaporized before they can penetrate, thus even adding to the fire, whereas a strong 2½-inch to 3½-inch stream will tear its way through walls, partitions, goods, etc., into the heart of a big fire and break it up so that the small streams can operate on it effectively. There are very few, if any, firemen, even in large cities, who know much about streams above two inches in diameter; we must go to these modern, high-power fire boats to find out the power required and the effect with larger streams. So far as known, there are almost no data published in regard to the velocity, nozzle-pressure, volume of water and power required in fire streams above 1¾ inches diameter. When these fire boats have been thoroughly experimented with, this knowledge can be obtained for streams up to 4 inches diameter. It is to be regretted that this has not yet been done. The certain increase in the efficiency of larger fire pumps, with their larger areas and passages, over that obtained in the smaller pumps of shore fire engines is an important item.

The main features in the design of a high-power fire boat are the boiler and pumps, of course. The boiler must have all the

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* See *Engineering*, June 18th, 1886.

capacity which can be got on a certain prescribed displacement, which means two, three and even four times the capacity of that in an ordinary tug of similar dimensions. The hull and motive machinery must be secondary matters, as in a dredge or floating elevator. But maneuvering power and speed, in the order mentioned, are also very important items. Then, too, the boats for each port or locality must have an individuality of their own. A boat designed to suit the conditions and hydrographic features of one port might be entirely unsuitable in another. It would make far too long a paper to go into details in these matters. The plans and table will give much information as to detail to those who feel interested enough to go into the matter. Your attention will be called, however, to a few points.

The ordinary " Scotch " boiler is much lighter per square foot of heating surface than the "water leg" boilers, and for that and several other important reasons it was adopted by the writer as the best type which could be used under the circumstances. By careful designing, and by specifying the best material obtainable in the market and the best methods of workmanship, it was proved that the weight of this type of boiler, per square foot of heating surface, could be reduced from 50 lbs. and over, as in transatlantic steamships and United States naval vessels, to 35.26 lbs. in the *Seth Low*, 35. lbs. in the Cleveland fire boat and 31.9 lbs. in the boiler of Chicago's *Geyser*. This calculated weight includes water to 4 inches above highest heating surface, felting and asbestos, breeching, grates, saddles, stack and attached valves.

These boats are not intended for sea-service, and therefore it was permissible to carry the water line higher and use a dry pipe. A ratio of 38 to 1 was also used between the heating and grate surface on account of having a forced draught. These two features explain part of the reduction in weight; the rest of it comes by using the highest grade of material and workmanship, and in cutting out the useless and " dead " water spaces below the furnaces by putting in extra fire tubes and a " regenerating " flue for admitting heated air into the back connection.

Besides the great advantage of throwing large streams and having special and rapid accessibility to all water-front or harbor fires, a properly designed fire boat need not have her field of operations confined to the water front. She may be given such a capacity as to make her a large movable pumping station capable of carrying her equipment, crew, coal, etc., to the nearest practicable point, and operating from that point with unlimited water supply upon a fire 1,200 to 1,500 feet distant, through several lines of hose of large size. And even beyond this 1,500-foot belt, fire boats can be used to reinforce shore engines, the latter either connecting their suctions direct to the delivery from the fire boat, or the boat can deliver into tanks from which the engines can draw. Scarcity of water for large fires is a thing of which many large cities have to complain. Again, special pipes or mains can be laid connecting the nearest convenient point on the water front with any desired building or district in a city, the fire boat connecting with the main and operating somewhat on the Holly system.

These fire boats are designed to keep up steam constantly. A banked fire in one of the furnaces, with the others primed ready to start, is found to work well. The boats are put on the regular fire alarm and telephone circuits by special slip cable connecting with the instruments permanently located in the pilot-house, and the boats can make much better time in going to a fire than the shore engines can, because they are able to start as quickly, have generally less obstruction, and always very much more speed. When we consider the cost of real estate and building in a city of the first or second class, it is plain that a first-class fire boat, fully equipped and with a capacity for throwing six to eight strong two-inch streams, can be had at about the same cost as a shore engine throwing one two-inch stream, when to the cost of the latter is added the outlay for hose and carriage, equipment, house and lot, horses and other matters necessary to maintain the engine and crew. It is safe to say that an improved form of floating fire engine is the cheapest and by far the most efficient method for the fire protection of shipping and water-front districts in large or small ports, and that such machines are destined to come into more general favor. From the studies and experience of the writer in the matter he has concluded that it is quite possible to build and equip complete ready for service a small floating fire engine with a capacity of two to three first-class shore engines (two to three 2-inch streams), suitable for a small port, for $20,000, and one of half this capacity for $14,000. This includes the cost of hose, pipes, nozzles, etc., which is always quite a large item.

COMPARATIVE TABLE OF FIRE BOATS.

DATA.	*Geyser* of Chicago.	CLEVELAND BOAT.		*Seth Low* of Brooklyn.	*Zophar Mills* of New York.	*Merryweather* of Alexandria, Egypt.
		Built, (wood).	Not built, (iron).			
Boiler.						
Type, description, and number	"Scotch"—4 furnaces, horizontal steam drum athwartships—forced draught—one.	"Scotch"—3 furnaces, vert. steam drum and superheater — forced draught—one.	Same as in wood boat.	"Scotch"—3 furnaces, vert. steam drum and superheater — forced draught—one.	Flue and return tubular "water leg." Two furnaces, steam chimney—natural draught—two.	Vertical cylinder "drop tube" fire engine type—forced draught—one.
Dimensions of shell over all	11 ft. 4½ ins. d. by 16 ft. long.	10 ft. 1 in. d. by 14 ft. long.		9 ft. 3 ins. d. by 14 ft. long.	8 ft. face and d. by 14 ft. long.	About 3 ft. 6 ins. d. by about 6 ft. high.
Dimensions of furnaces.	36 ins. d. by 13 ft. 10 ins. long. Grates 7 ft. long.	36 ins. d. by 11 ft. 10 ins. long. Grates 7 ft. long.		33 ins. d. by 11 ft. 10 ins. long. Grates 6¼ ft. long.	39 ins. wide by 6¼ ft. long.	About 3 ft. 3 ins. diam.
Working pressure......	100 lbs.	100 lbs.		100 lbs.	60 lbs.	120 lbs.
Grate surface	84 sq. ft.	63 sq. ft.		53.6 sq. ft.	About 86.5 sq. ft.	About 8.3 sq. ft.
Heating surface........	3,091 sq. ft.	2,269 sq. ft.		1,810.5 sq. ft.	About 2,040 sq. ft.	275 sq. ft.
Ratio of G. S. to H. S...	1 to 38.	1 to 36.		1 to 34.1.	1 to 23.6	About 1 to 33.
Weight complete, steam up	44 tons.	35 tons.		28.65 tons.	Over 50 tons.	About 2 to 2¼ tons.
Weight per sq. ft. H. S.	31.9 lbs.	34.55 lbs.		35.26 lbs.	About 55 to 60 lbs.	About 16 to 19 lbs.
Material................	Otis steel plates, 65,000 to 70,000 T. S.; 47 to 50% "contraction of area," 25% elongation. Stays and rivets of iron, 52,000 T. S., 32% elongation.	Otis steel plates, same as *Geyser*.		Shoenberger & Co's steel plates, 60,000 to 65,000 T. S.. otherwise same as *Geyser*.	Iron plates, 50,000 T. S.	
Builders	John Mohr & Son, engineers, Chicago.	Cleveland Steam Boiler Works, Cleveland.		McWilliams and Brown, engineers, Jersey City.	Pusey & Jones, Engineers, Wilmington, Del.	Merryweather & Sons, Engineers, London.

Pumps.						
Type and Builders	Vertical duplex cranks and fly-wheels, steam valves ordinary slide, water valves special form, rubber, with extra large area and straight ports. Clapp and Jones Mfg. Co., Hudson, N. Y.	Same type as in *Geyser*, but different design—valves as in *Amoskeag* fire engines. Thos. Manning & Co., Engineers, Cleveland.	Same specifications as for wood boat.	Same type and design as in *Geyser*, with different water-valves. Clapp & Jones Mfg. Co., Hudson, N. Y.	Same type and similar design to pumps in *Seth Low*, same valves. Clapp & Jones Mfg. Co., Hudson, N. Y.	**Horizontal direct-acting "double cylinder," known as "Admiralty" pattern. Merryweather & Sons.**
Number of pumps......	Two.	Two.		Two.	Two.	One.
Total number of water cyls. and their dimensions.................	Four—9 ins. d. by 10 ins. str.	Four—8 ins. d. by 10 ins. str.		Four—8 ins. d. by 10 ins. str.	Four—7½ ins. d. by 9 ins. str.	Two—7 ins. d. by 24 ins. str.
Total number of steam cyls. and their dimensions............... ..	Four—17 ins. d. by 10 ins. str.	Four—16 ins. d. by 10 ins. str.		Four—16 ins. d. by 10 ins. str.	Four—16 ins. d. by 9 ins. str.	Two—9 ins. d. by 24 ins. str.
Hull.						
Material...............	Wood.	Wood.	Iron.	Wood.	Iron.	Iron.
Net register tonnage ...	71.60.	33.08.		41.36.	121.05.	
Displacement to L.W.L.	201 tons.*	136 tons.*	125 tons.*	157.5 tons.†	About 380.	
Length over all.........	105 ft. 0 ins.	79 ft. 0 ins.‡	78 ft. 0 ins.‡	99 ft. 3 ins.	About 122 ft. 0 ins.	51 ft. 0 ins.
Length on L. W. L	95 ft. 5 ins.	70 ft. 0 ins.	71 ft. 6 ins.	90 ft. 0 ins.		
Beam over all..........	24 ft. 8½ ins.	23 ft. 4½ ins.	22 ft. 11¼ ins.	23 ft. 9 ins.		
Beam over plank or plating	23 ft. 0 ins.	22 ft. 8 ins.	22 ft. 2¼ ins.	23 ft. 0 ins.	25 ft. 0 ins.	10 ft. 6 ins.
Beam on L. W. L.......	21 ft. 8 ins.	22 ft. 0 ins.	21 ft. 7 ins.	22 ft. 3 ins.		
Depth, moulded........	11 ft. 0 ins.	10 ft. 0 ins.	10 ft. 0 ins.	8 ft. 6 ins.	About 12 ft. 0 ins.	
Draught, extreme......	8 ft. 10 ins.	8 ft. 4 ins.	7 ft. 7 ins.	7 ft. 6 ins.	9 ft. 9 ins.	4 ft. 0 ins.

* In fresh water. † In salt water. ‡ The designer was limited to an over all length of 80 ft.

COMPARATIVE TABLE OF FIRE BOATS—*Continued.*

DATA.	*Geyser* of Chicago.	CLEVELAND BOAT.		*Seth Low* of Brooklyn.	*Zophar Mills* of New York.	*Merryweather* of Alexandria, Egypt.
		Built, (wood).	Not built, (iron).			
Hull—(continued).						
Coal capac'y in bunkers. (Tons of 2,000 lbs. each.)	27 tons.	12 tons.	15 tons.	18 tons.	20 tons.	Very small.
Number of men that can be quartered on board.	16	10	14	14	About 16 to 20	Very few.
Builders...............	Miller Bros., Chicago.	Murphy & Root, Cleveland.		Trundy & Murphy, Brooklyn.	Pusey & Jones, Engineers, Wilmington.	
Motive Power.						
Type of engine and no. of cyls...............	Simple, vertical direct-acting. Two cyls. on wrought-iron col'mns.	Simple, vertical, direct-acting. One cyl. on cast-iron frame.	Same specification as for wood boat.	*Two* separate engines for *twin screws*—simple, vert., direct-act'g. One cyl. each—on cast-iron frames.	Simple, vertical, direct-acting. One cyl. on cast-iron frame.	Simple, vertical, direct-acting. Two cyls. on cast-iron frames.
Diam. of cyls...........	18 ins.	18 ins.		16 ins.	30 ins.	9 ins.
Stroke of pistons.......	20 ins.	20 ins.		18 ins.	30 ins.	10 ins.
Remarks...............	Links, no cut-off valves, steam reverse gear, exhaust to atmosphere or through heater, and stack for blast.	Same as *Geyser*—hand reverse-gear.		Links, Mayer cut-off valves, exhaust to "high pressure" sur. condenser or to stack for blast.	Link, steam reverse gear, extremely heavy parts all through—exhaust to sur. conden'r, no st'm blast in stack.	Ordinary double engine, exhaust to stack—no condenser.
Builders....	C. F. Elmes, Engineer, Chicago.	Excelsior Iron Works, Cleveland.		McWilliams & Brown, Jersey City.	Pusey & Jones, Engineers, Wilmington.	Merryweather & Sons.
Propeller wheels—number and type.........	One—4 blades, removable, of cast-steel. Blade surface half way between towing and speed-wheel.	One—Kunstadter patent, swivel joint for manœuvering in Cuyahoga River—very crooked and narrow. 4 blades, removable, cast-steel—blades 22 ins. wide at center of length.		Two—4 blades, solid, cast-iron—blade surface half way between towing and speed-wheel.	One—4 blades, cast-iron, heavy towing-wheel.	One—3 blades, ordinary form.

Diam. of wheel........	7 ft. 10 ins.	6 ft. 0 ins.		6 ft. 0 ins.	8 ft. 6 ins.	About 8 ft. 6 ins.
Pitch of wheel—avg.....	*12 ft. 0 ins.*	*9 ft. 0 ins.*		9 ft. 0 ins.	13 ft. 0 ins.	
Approximate total cost of boat complete, *excepting* fire apparatus.	$36,870.	$25,200.		$33,000.	About $60,000.	
Tests of Pumps and Boilers.						
Largest Streams and distances *	1—3½″—431′; or, 1—4″—386′.	1—3½″—275′, not a maximum.		1—3½″—360′.	1—3″—300′.	1—2″—200′.
Capacity in 2″ diameter streams and distances.	With one pump, 4—2″—249′. †	With one pump, 3—2″—240′.		4—2″—250′.	2—2″—240′.	1—2″—200′.
Capacity in small streams and distances.	14—1¼″—204′.	4—1¼″—170′, and 6—1″—220′.		12—1″—about 185′.		

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* All distances measured horizontally to end of solid water; no spray included. † With both pumps at once, 1—3″—234′, and 4—2″—220′.

The *Geyser*, in rough sea (Lake Michigan), made 4 knots in 15 minutes by taffrail log—at the rate of 18¾ miles per hour.

The Cleveland boat, in a rough sea and strong wind (half the time on starboard quarter and half the time on port bow)—Lake Erie—made 10½ miles per hour for over one hour, by bearings and chart. Contract speed, 9 miles per hour in smooth water. Same boat turned complete circles about 135 feet in diameter, in 1 min. 33 sec., at three-quarters speed.

The *Seth Low*, in smooth sea and moderate breeze, made 12.8 miles in one hour by bearings and chart. One-half the run with tide and other half against tide over same course.

DISCUSSION.

Mr. W. B. Le Van.—I would only state that the value of fire boats is so important a matter that all the police boats of the city of Philadelphia are now made fire boats.

Mr. J. W. Cole.—I would suggest, in speaking of these tugs, that in the year 1868 the tug *John Fuller* threw, I think, 8 two-inch streams, and was chartered as a fire boat by the city of New York, and by Philadelphia, I think, in 1869, to assist them, when they had low water in the Schuylkill, in pumping into Fairmount Reservoir. As a wrecking tug, she would throw a solid twenty-inch stream over her side. She had a Niagara pump built in Brooklyn.

CCXXXIV.

FORMULÆ AND TABLES FOR CALCULATING THE EFFECT OF THE RECIPROCATING PARTS OF HIGH SPEED ENGINES.

BY GEO. I. ALDEN, WORCESTER, MASS.

(Member of the Society.)

THE effect of the reciprocating parts of an engine upon the pressures transmitted to the crank pin at different points of its path has been fully treated by Mr. Chas. T. Porter, and subsequently much has been written upon the same subject by other authors. Some recent articles upon High Speed Engines have attracted the attention of the writer, and led him to review and to put into condensed form his own researches upon the subject, and to compute tables by means of which the results of analytical research may be made more easily available for practical use.

To permit verification of the formulæ and methods employed, the complete analysis is briefly given, followed by the tables, with an explanation of the manner in which they may be used. There is also given a graphical method of determining the amount of cushioning or lead required to absorb the surplus energy of the reciprocating parts as they are brought to rest at the end of the stroke.

ANALYSIS.

Notation.

Let A = piston area in inches.
" P = effective pressure per square inch.
" $2r$ = stroke (r = crank).
" l = length of connecting rod.
" α = crank angle.
" θ = angle of connecting rod with line of centers.
" n = ratio of crank to connecting rod.

Let M = mass of reciprocating parts.
" w = weight of reciprocating parts.
" v = velocity of reciprocating parts.
" φ = acceleration of reciprocating parts.
" v' = velocity of crank pin, considered uniform.
" N = number of revolutions per second.
" H = horizontal pressure transmitted to connecting rod.
" R = thrust (or pull) on connecting rod.
" T = component of R at right angles to crank on forward stroke.
" T' = same component on return stroke.
" T_0 = resistance of load at crank pin.
" dt = an infinitesimal increment of time.

The total effective pressure upon the piston being at any instant PA, the amount of this force transmitted horizontally to the connecting rod is $H = PA - M\varphi$.*

$$\text{But } R = H \sec\theta = (PA - M\varphi)\sec\theta.$$

$$T = R\sin(\alpha + \theta) = (PA - M\varphi)\sec\theta\sin(\alpha + \theta)$$

Eliminating θ by the relation $r\sin\alpha = l\sin\theta$, we have

$$T = (PA - M\varphi)\left(1 + \frac{n\cos\alpha}{\sqrt{1 - n^2\sin^2\alpha}}\right)\sin\alpha. \quad . \quad (1.)$$

The formula for the velocity of the piston is:

$$v = v'\sin\alpha\left(\frac{n\cos\alpha}{\sqrt{1 - n^2\sin\alpha}} + 1\right). \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

$\varphi = \dfrac{dv}{dt}$; and since $r\,d\alpha = v'dt$ we have:

---

* It is assumed that the velocity of rotation is uniform; and also that the acceleration of all the reciprocating parts is the same as that of the piston.

$\varphi = \frac{v' \cdot dv}{r \cdot d\alpha}$. Performing the differentiation, substituting the resulting value of φ in (1), and putting $M = \frac{w}{g}$ and $v' = 2\pi r N$, we have:

$$T = \left[PA - \frac{w}{g} \cdot 4\pi^2 r N^2 \left(\frac{n \cos(2\alpha)}{\sqrt{1 - n^2 \sin^2 \alpha}} + \frac{n^3 \sin^3(2\alpha)}{(1 - n^2 \sin^2 \alpha)^{\frac{3}{2}}} + \cos \alpha\right)\right]\left(1 + \frac{n \cos \alpha}{\sqrt{1 - n^2 \sin^2 \alpha}}\right) \sin \alpha. \quad . \quad (3.)$$

This may be written as follows:

$$T = \left(PA - \frac{w}{g} \cdot 4\pi^2 r N^2 C\right) D\text{; or better,}$$

$$T = PAD - 1.226 w r N^2 CD. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

Denote the value of T for the return stroke by T', and substitute $(180^\circ + \alpha')$ for α in formula (3), remembering that PA becomes negative when it changes direction, and we have

$$T' = \left[P'A + \frac{w}{g} \cdot 4\pi^2 r N^2 \left(\frac{n \cos(2\alpha')}{\sqrt{(1 - n^2 \sin^2 \alpha')}} + \frac{n^3 \sin^3(2\alpha')}{(1 - n^2 \sin^2 \alpha')^{\frac{3}{2}}} - \cos \alpha'\right)\right]\left(1 - \frac{n \cos \alpha'}{\sqrt{1 - n^2 \sin^2 \alpha'}}\right) \sin \alpha'. \quad . \quad (4.)$$

Or in abbreviated form,

$$T' = P'AD' + 1.226 w r N^2 C'D'. \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

The tables which follow give values of C, D, C', and D', for every ten degrees of the crank circle in both forward and return stroke.

General Remarks.

Each table has a column giving the piston position in fractions of the stroke, corresponding to the crank angles in the left hand column. The tables give the common logarithms of C and D, with ten (10) added to the characteristic whenever it is negative. Angles and distances in the forward stroke are reckoned from the initial position of crank and piston respectively. For the return stroke, angles and distances are reckoned from the 180° position of the crank and the corresponding piston position. The indicator cards in Figs. 62 and 63 are taken from an illustrated catalogue of

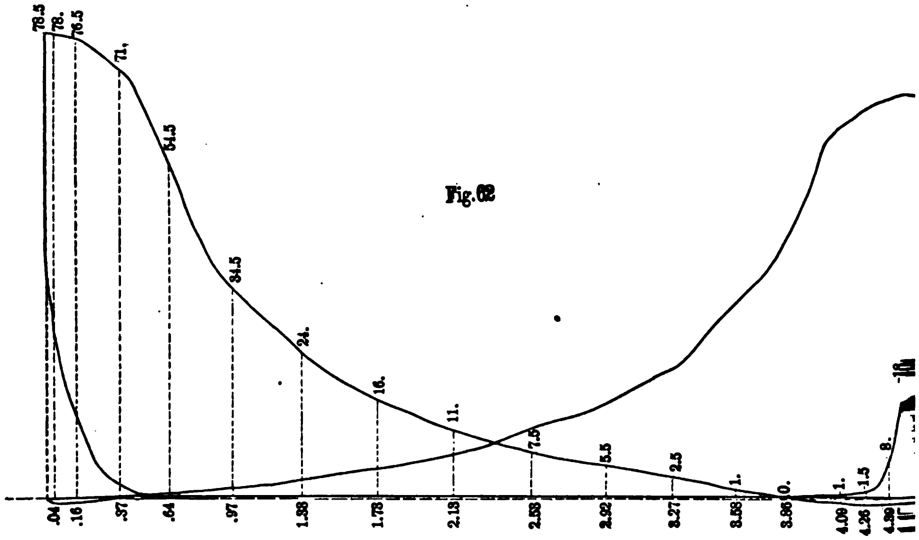


Fig.62

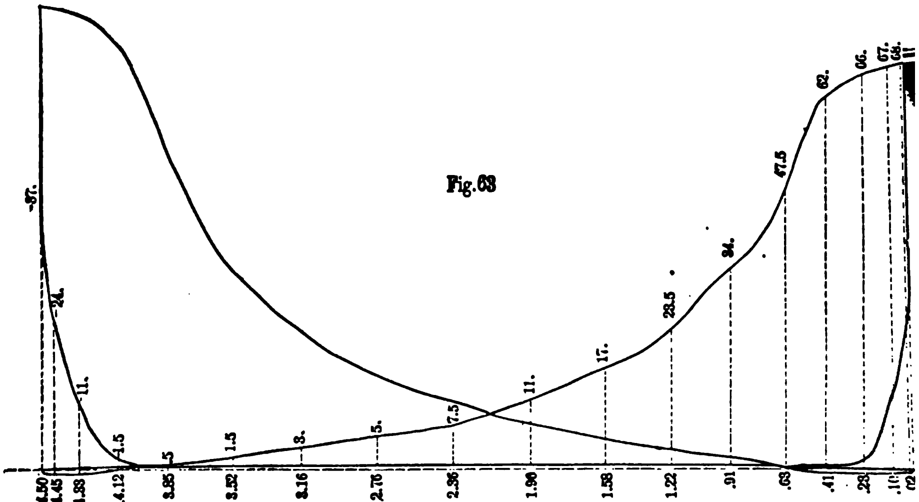


Fig.63

SCALE.
32 Pounds to the Inch.

the Porter-Allen steam engine, and are there given as cards actually taken from one of their 16″ × 30″ engines. In all the calculations, energy is reckoned in foot-pounds.

Fig. 65 is merely a sketch for illustration of the graphical method explained in the text referring to it.

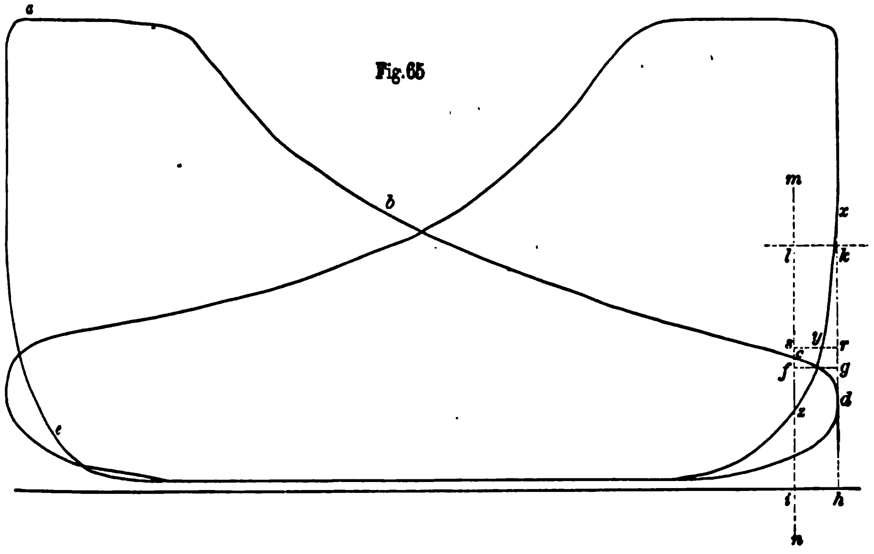


Fig. 65

TABLE I.

RATIO OF CRANK TO CONNECTING ROD, $\frac{1}{4}$.

Forward Stroke.

CRANK ANGLES OR VALUES OF a.	PISTON POSITIONS.		LOG C.	LOG D.
0°	.0000		0.096910	
10°	.0095		0.086998	9 335355
20°	.0374		0.056308	9.626000
30°	.0826		0.001612	9.784518
40°	.1429		9.916864	9.885082
50°	.2156		9.788586	9.950106
60°	.2974		9.584952	9.989855
70°	.3849		9.189855	0.013676
80°	.4747	Negative terms.	8.824386	0.012380
90°	.5635		9.397940	0.000000
100°	.6484		9.617042	9.973450
110°	.7270		9.723628	9.933647
120°	.7974		9.789198	9.878028
130°	.8584		9.826717	9.806600
140°	.9090		9.848989	9.714385
150°	.9487		9.862930	9.592333
160°	.9772		9.869795	9.417259
170°	.9948		9.873808	9.116782
180°	1.0000		9.875061	

TABLE II.

RATIO OF CRANK TO CONNECTING ROD, $\frac{1}{4}$.

Return Stroke.

CRANK ANGLES OR VALUES OF a'.	PISTON POSITIONS.		LOG C′.	LOG D′.
0°	.0000	Negative terms.	9.875061	
10°	.0057		9.873808	9.116782
20°	.0228		9.869795	9.417259
30°	.0513		9.861370	9.592333
40°	.0919		9.848989	9.714385
50°	.1415		9.826717	9.806600
60°	.2025		9.789192	9.878028
70°	.2730		9.723628	9.938647
80°	.3516		9.617042	9.973450
90°	.4364			0.000000
100°	.5252		8.824386	0.012380
110°	.6150		9.189855	0.013676
120°	.7025		9.584952	9.989855
130°	.7844		9.788586	9.950106
140°	.8570		9.916[illegible]64	9.885082
150°	.9173		0.001612	9.784518
160°	.9625		0.056303	9.626000
170°	.9905		0.086993	9.335355
180°	1.0000		0.096910	

TABLE III.

RATIO OF CRANK TO CONNECTING ROD, $\frac{1}{5}$.

Forward Stroke.

CRANK ANGLES OR VALUES OF a.	PISTON POSITIONS.		LOG C.	LOG D.
0°	.0000		0.079181	
10°	.0091		0.069594	9.317793
20°	.0360		0.040045	9.609014
30°	.0795		9.987943	9.768675
40°	.1377		9.907960	9.870461
50°	.2081		9.789363	9.937370
60°	.2877		9.607187	9.979526
70°	.3735		9.277678	0.002225
80°	.4621	Negative terms.	8.231979	0.008491
90°	.5506		9.309895	0.000000
100°	.6358		9.561530	9.977689
110°	.7156		9.694175	9.941637
120°	.7877		9.774700	9.891035
130°	.8509		9.826003	9.823723
140°	.9037		9.859174	9.735181
150°	.9455		9.880494	9.615897
160°	.9757		9.893645	9.443394
170°	.9939		9.900815	9.144342
180°	1.0000		9.903090	

TABLE IV.

RATIO OF CRANK TO CONNECTING ROD, $\frac{1}{5}$.

Return Stroke.

CRANK ANGLES OR VALUES OF a'.	PISTON POSITIONS.		LOG C′.	LOG D′.
0°	.0000		9.903090	
10°	.0061		9.900815	9.144342
20°	.0243		9.893645	9.443394
30°	.0544		9.880498	9.615897
40°	0962		9.859174	9.735181
50°	.1491	Negative terms.	9.826004	9.823723
60°	.2122		9.774700	9.891035
70°	.2844		9.694175	9.941637
80°	.3642		9.561530	9.977689
90°	.4494		9.309895	0.000000
100°	.5373		8.231979	0.008491
110°	.6264		9.277678	0.002225
120°	.7122		9.607187	9.979526
130°	.7918		9.789363	9.937370
140°	.8623		9.907960	9.870461
150°	.9205		9.987943	9.768675
160°	.9640		0.038973	9.609014
170°	.9909		0.069594	9.317793
180°	1.0000		0.079181	

TABLE V.

RATIO OF CRANK TO CONNECTING ROD, $\frac{1}{5}$.

Forward Stroke.

CRANK ANGLES OR VALUES OF a.	PISTON POSITIONS.		LOG C.	LOG D.
0°	.0000		0.066923	
10°	.0088		0.057646	9.305698
20°	.0350		0.029184	9.597337
30°	.0774		9.979156	9.757710
40°	.1342		9.902927	9.860538
50°	.2031		9.791136	9.928818
60°	.2814		9.622587	9.972640
70°	.3610		9.331913	9.997346
80°	.4538		8.188647	0.005908
90°	.5580		9.227964	0.000000
100°	.6275		9.520955	9.980418
110°	.7880		9 671451	9.947166
120°	.7814		9.763907	9.899326
130°	.8459	Negative terms.	9.808069	9.834590
140°	.9003		9.864736	9.748374
150°	.9435		9.891493	9.671023
160°	.9747		9.908116	9.459946
170°	.9936		9.917856	9.164772
180°	1.0000		9.920822	

TABLE VI.

RATIO OF CRANK TO CONNECTING ROD, $\frac{1}{6}$.

Return Stroke.

CRANK ANGLES OR VALUES OF a'.	PISTON POSITIONS.		LOG C′.	LOG D′.
0°	.0000	Negative terms.	9.920822	
10°	.0064		9.917856	9 164772
20°	.0258		9.908416	9.459946
30°	.0565		9.891498	9.671028
40°	.0997		9.864736	9.748874
50°	.1541		9.808069	9 834590
60°	.2185		9.763907	9.899326
70°	.2919		9.671451	9.947166
80°	.3725		9.520955	9.980418
90°	.4419		9.227964	0.000000
100°	.5461		9.188647	0.005908
110°	.6389		9.331918	9.997346
120°	.7185		9 622587	9.972640
130°	.7968		9.791186	9.928818
140°	.8657		9.902927	9.860538
150°	.9225		9.979156	9.757710
160°	.9649		0.029184	9.597337
170°	.9911		0.057646	9.305698
180°	1.0000		0.066928	

To illustrate the manner of using the tables, let us solve the following problem:

Given—	Diameter of cylinder	16″
	Stroke (= $2r$)	30″
	Length of connecting rod	60″
	Weight of reciprocating parts	500 lbs.
	No. of revolutions per second	2.5
	Area of piston (average of 2 sides)	200 sq. in.

Also given the indicator cards, either actual or theoretical.

Required, 1st. The diagram showing the actual distribution of energy to the crank pin.

2d. The diagram showing how the energy would be distributed if not affected by the reciprocating parts.

3d. The effect of reciprocating parts.

4th. The value of the term ΔE in Rankine's formula for fly-wheel.

To construct the required diagram, first compute the values of T for the forward stroke from the formula

$$T = PAD - 1.226wrN^2CD.$$

The values of P for piston positions, corresponding to each ten degrees of the forward stroke, are obtained from Fig. 62, and are represented by the lengths of the ordinates included between the admission and expansion line of the forward stroke, and the exhaust and compression line of the opposite card. [A convenient way of drawing these ordinates is to multiply the fraction of the stroke as given in the second column of the appropriate table by the length of the card in inches. The result will be the distance of the ordinate (in inches) from the end of the card.]

Having taken these values of P, we have all the data for the PAD term of our equation, for we know the area of the piston, and we have the value of D in the fourth column of the appropriate table, which for our problem is Table I. The term $1.226wrN^2CD$ is also easily calculated for each ten degrees of the crank position, the process being simply multiplication. Care should be taken to give this term its proper algebraic sign.

Applying the process as above outlined to our problem, we find the values as given in the following columns:

TABLE VII.

Forward Stroke.

α	PAD	$1.226wrN^2CD.$	$T.$
0°	0	0,000	
10°	3,377	1,266	1,111
20°	6,472	2,304	4,168
30°	8,646	2,927	5,719
40°	8,366	3,034	5,332
50°	6,151	2,624	3,527
60°	4,689	1,799	2,890
70°	3,303	765	2,538
80°	2,264	− 329	2,593
90°	1,520	− 1,197	2,717
100°	1,085	− 1,865	2,900
110°	429	− 2,175	2,604
120°	151	− 2,226	2,377
130°	000	− 2,058	2,058
140°	− 151	− 1,702	1,551
150°	− 171	− 1,366	1,195
160°	− 418	− 927	509
170°	− 471	− 318	− 153
180°			

Using the formula

$$T = P'AD' + 1.226wrN^2C'D',$$

and taking values of P' from the ordinates on Fig. 63, we have the corresponding solution for the return stroke as follows:

TABLE VIII.

a	$P'AD'$.	$1.226wrN^2C'D'$	T.
0°	0,000	000	
10°	1,780	– 818	1,462
20°	3,502	– 927	2,575
30°	5,163	– 1,366	3,797
40°	6,424	– 1,702	4,722
50°	6,086	– 2,058	4.028
60°	5,135	– 2,226	2,909
70°	4,034	– 2,175	1,859
80°	3,199	– 1,865	1,334
90°	2,200	– 1,197	1.003
100°	1,543	– 329	1,214
110°	1,032	765	1,797
120°	586	1,799	2,385
130°	267	2,624	2,891
140°	77	3,034	3,111
150°	– 183	2,927	2,744
160°	– 930	2,304	1,374
170°	– 1,039	1,266	227
180°	0,000	0,000	000

We have now the means of constructing the required diagram, which is shown in Fig. 64.

AB, Fig. 64, is the distance traveled by the crank pin during one stroke. This is divided into eighteen equal parts, to correspond to the ten-degree crank positions. Through these divisions, ordinates are drawn, upon which are laid off the values of T from the foregoing Table VII., for the forward stroke above the line from A to B; and the values of T' taken from Table VIII., are for the stroke below the line from B to A.

AcdeB is the required curve for forward stroke, and *BfghA* is the same for return stroke. The whole area represents the work done on the crank pin in one revolution.

By laying off on the ordinates already drawn in Fig. 64, the values of PAD in Table VII. above AB, and those of $P'A'D'$ in Table VIII. below BA, we have the diagram *AklmBnopA*, which shows how the pressure on the crank pin would be distributed if the reciprocating parts had no weight.

The lengths of the ordinates included between these lines, give the effect of the reciprocating parts, this effect being to diminish the pressure on the crank pin where the curve *Acde*, etc., lies inside the curve *Aklm*, etc., and *vice versâ*.

To find the value of △ *E*, first draw the rectangle *vwxy*, whose area is equal to that of the curve *AcdeBfghA*.

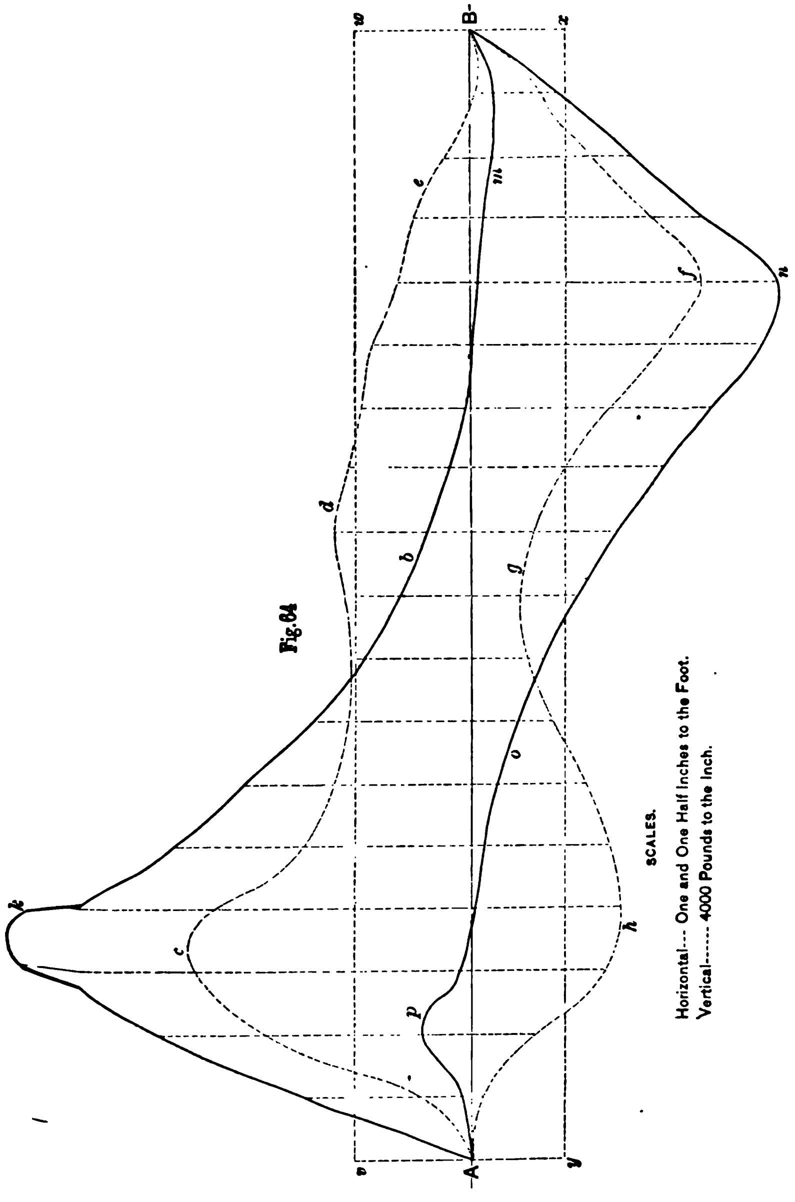


Fig. 64

There are two equal parts of the enclosed area not common to the rectangle and the diagram *AcdeBfghA*, one part within the rectangle and one outside. Either of these equal areas represents the $\triangle E$ of Rankine's formula for fly-wheels above referred to, and one of these areas divided by the area of the rectangle *AvwBxyA*, is the $\frac{\triangle E}{fp\, ds}$, given in Morin's tables.

By the aid of the tables above given, correct diagrams can be produced in a short time by any engineer, or by an assistant who simply knows how to use logarithms for multiplication alone.

The reciprocal diagram of stress for trusses and girders is a means of rapid comparison of designs as well as of direct calculation of stresses. May not the tables for constructing accurate diagrams, representing distribution of energy to the crank pin bring these diagrams into more general use in the designing of high speed engines?

Method of finding the compression line which will just take up the actual energy of the reciprocating parts at the end of the stroke.

The actual energy of the reciprocating parts at any point of the forward stroke is:

$$\frac{Mv^2}{2} = E = .613wr^2N^2D^2,$$

in which D is given for every ten degrees of the crank position in tables I., III., and V. For the return stroke, $E = .613wr^2N^2D'^2$, in which D' is given in tables II., IV., and VI.

The horizontal component of the load on the crank pin is, $T_0 \sin \alpha$. Near the end of the stroke, sin α diminishes in nearly the same proportion as α increases, and is zero (0) at the end of the stroke; consequently the work of this horizontal component is approximately $\mu = \frac{T_0 \sin \alpha}{2}$, multiplied by the distance of the piston from the end of the stroke.

We have then a ready means of computing the values of E and μ.

Let *abcdea*, Fig. 65, be an indicator card for the forward stroke of an engine. Draw a vertical line *mn* near the end of the stroke. Draw a horizontal line *fg*, so that the area *fghi* will represent approximately the energy exerted by the steam on the steam side of the piston during the remainder of the stroke.

Upon the line *fg* construct a rectangle *fgkl*, whose area is equal to the actual energy of the reciprocating parts at *mn* divided by the piston area $= \frac{E}{A}$.

The whole area *ihkl* is the total energy to be overcome in bringing the piston to rest. Upon the line *lk* construct a rectangle *lkrs*, whose area is equal to the work of the horizontal component of the load, per square inch of piston area from *mn* to the end of the stroke $= \frac{\mu}{A}$.

The area *srhi* represents the energy to be absorbed by compression, therefore the compression line *xyz* of the opposite card must be such as to make the area *xyr* equal to *zys*.

If we apply this construction to the illustrative problem previously made use of, drawing *mn* at $\frac{95}{100}$ of the stroke, which gives $\alpha = 150°$, we find approximately

$$gk = \frac{E}{200fg} = 20.42 \text{ lbs.},$$

$$kr = \frac{\mu}{200fg} = 15.24 \text{ lbs.};$$

therefore

$$gr = 5.18 \text{ lbs.}$$

The principle upon which the above construction is based, may be stated as follows: the actual energy of the reciprocating parts at any given point of the stroke, plus the energy exerted by the steam upon the piston in the direction of its motion, from the given point to the end of the stroke, should equal the work of the horizontal component of the load, plus the work of back pressure and compression, from the given point to the end of the stroke.

DISCUSSION.

Prof. Gaetano Lanza.—The introduction of the high-speed engine has called attention to the importance of studying the action of the reciprocating parts and the distribution of the pressure on the crank pin.

Quite a number of people have written upon the subject, but all (so far as the writer knows), who have published anything in regard to it, have been content to use approximate methods or approximate formulæ, claiming that the exact methods and formulæ would lead to too great complexity. The approximations adopted are of two kinds:

1. Some inexact value is used for the acceleration of the piston.

2. The fact is not properly taken account of, that the accelerations of the different parts of the connecting-rod are different, the cross-head end moving with the cross head and sharing its motion, while the crank end shares the motion of the crank pin; and all other points have a motion different from either.

In Mr. Charles T. Porter's book, both of these approximations are used, while Prof. Alden employs the correct value of the acceleration of the piston, but considers all the reciprocating parts as having the same motion as the piston.

I am in the habit of giving instruction to my own classes upon this subject, and they are not allowed to use any of these approximations, but are required to use exact values throughout, inasmuch as, while the calculation of the tables involves a little more labor, their application is just as easy, if they are correct as if they are only approximate, and we are fully paid for our trouble in calculating the tables by the fact that the results are accurate.

My formulæ and tables will now be given with a brief explanation of the mode of deducing them, my own notation being used, inasmuch as it differs somewhat from that of Prof. Alden.

A table will then be given referring to the Porter-Allen 10″ × 20″ engine, of the Massachusetts Institute of Technology, and the corresponding table will be computed by Prof. Alden's method, thus showing how much difference arises by adopting the approximate values which he has used instead of the correct ones.

Inasmuch as Prof. Alden has confined himself to the horizontal throw, that will be treated first, but a treatment of the vertical throw will be given subsequently.

Horizontal Throw.

As far as the velocity and acceleration of the piston are concerned, both of us follow exactly the same method, and we obtain, of course, equivalent formulæ. They will be given here in my own notation:

Let α = angular velocity of crank expressed in circular measure.
t = time employed by crank in passing from line of dead points to any given position (variable), so that
αt = crank angle in circular measure.
l = length of connecting rod in feet.
r = length of crank in feet.

s_1 = distance (in feet) passed over by the piston while the crank describes the angle αt.
v_1 = velocity of piston at the end of the time t.
f_1 = acceleration of the piston at the same time.
n = number of revolutions per minute, so that
$\alpha = \frac{2\pi n}{60} = \frac{\pi n}{30}$.

Also let

$$A = \cos \alpha t \; ; \; B = \frac{\cos 2\alpha t}{\sqrt{\left(\frac{l}{r}\right)^2 - \sin^2 \alpha t}} \; ; \; C = \frac{\sin^2 2\alpha t}{4 \left\{ \left(\frac{l}{r}\right)^2 - \sin^2 \alpha t \right\}^{\frac{3}{2}}}$$

Then we shall have,

$$s_1 = r \left\{ 1 + \left(\frac{l}{r}\right) - \cos \alpha t - \sqrt{\left(\frac{l}{r}\right)^2 - \sin^2 \alpha t} \right\} \quad . \quad . \quad (1)$$

$$v_1 = \frac{ds_1}{dt} = \alpha r \sin \alpha t \left\{ 1 + \frac{\cos \alpha t}{\sqrt{\left(\frac{l}{r}\right)^2 - \sin^2 \alpha t}} \right\} \quad . \quad . \quad . \quad . \quad (2)$$

$$f_1 = n^2 r \left[\frac{\pi^2}{900} \left\{ A + B + C \right\} \right] \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Let now s_2 v_2 and f_2 represent respectively the space passed over by, the velocity of, and the acceleration of the crank pin, in a horizontal direction, then we have

$$s_2 = r\,(1 - \cos \alpha t) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$$v_2 = \alpha r \sin \alpha t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

$$f_2 = n^2 r \left\{ \frac{\pi^2}{900} \cos \alpha t \right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Now proceed to compute tables giving the values of $\frac{f_1}{n^2 r}$ and $\frac{f_2}{n^2 r}$ for every 10° of arc, these values depending only on the ratio $\frac{l}{r}$, and being applicable to any engine having the same ratio of connecting rod to crank, whatever be its crank length or its speed.

Then from these tables can readily be deduced the values of f_1 and f_2 for every 10° of crank angle, as soon as the crank length and speed are known. These tables, as worked out for values of $\frac{l}{r}$ equal to $5\frac{1}{3}$, $5\frac{1}{2}$, and 6 respectively, are given below, the values of $\frac{f_2}{n^2r}$ being the same for all values of $\frac{l}{r}$.

CRANK ANGLE.	$\frac{f_1}{n^2r}$			$\frac{f_2}{n^2r}$
	$\frac{l}{r} = 5\frac{1}{3}$	$\frac{l}{r} = 5\frac{1}{2}$	$\frac{l}{r} = 6$	
0°	.01302	.01296	.01279	.01097
10	.01274	.01268	.01252	.01080
20	.01189	.01184	.01171	.01031
30	.01054	.01051	.01042	.00950
40	.00878	.00877	.00873	.00840
50	.00671	.00672	.00674	.00705
60	.00446	.00449	.00457	.00548
70	.00216	.00221	.00234	.00375
80	− .00006	.00000	.00016	.00190
90	− .00209	− .00203	− .00185	.00000
100	− .00387	− .00381	− .00364	− .00190
110	− .00534	− .00529	− .00516	− .00375
120	− .00651	− .00648	− .00640	− .00548
130	− .00739	− .00738	− .00736	− .00705
140	− .00802	− .00804	− .00807	− .00840
150	− .00845	− .00848	− .00857	− .00950
160	− .00872	− .00877	− .00890	− .01031
170	− .00886	− .00892	− .00908	− .01080
180	− .00891	− .00897	− .00914	− .01097

Now, let

W_1 = weight of piston, piston rod, and cross head combined,

F_1 = total force required to produce the acceleration f_1 in these parts, then will

$$F_1 = \frac{W_1}{g} f_1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

and the (negative) rotative effect due to this force is to be found in the same manner as explained by Professor Alden.

With the connecting rod, however, the case is quite different, although Professor Alden counts it in with the other reciprocating parts, and treats them all alike. The acceleration of the cross-head end is f_1, while that of the crank-pin end is f, and that of any intermediate point is intermediate between f_1 and f_2, and can be very easily expressed in terms of f_1, f_2 and the distance of the point from the cross-head end of the rod. Now, imagine the rod divided into a very large number of very small pieces, and the mass of each of these pieces multiplied by the acceleration of that piece, the sum of the products will be the total force required to impart the required acceleration to the rod. The same result may be obtained by multiplying the weight of the rod by the acceleration of its center of gravity, so that, if we let

W_2 = weight of connecting rod,
F_2 = force required to impart to it the required acceleration,
f_0 = acceleration of its center of gravity,
x_0 = distance of center of gravity from cross-head end, we shall have:

$$f_0 = f_1 - (f_1 - f_2)\frac{x_0}{l}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

$$F_2 = \frac{W_2}{g} f_0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

In order to find x_0 we must weigh the two ends of the rod, and if we let

W_A = weight of cross-head end,
W_B = weight of crank-pin end, then

$$x_0 = \frac{W_B}{W_2} l. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

Next determine the point of application of this force F_2, and after this has been ascertained, resolve it into two parallel components, F_A and F_B, where

F_A = component of F_2 at cross-head end.
F_B = component of F_2 at crank end.

Then add F_A to F_1 and their combined (negative) rotative effect is found in the manner explained by Professor Alden, *i. e.*, by multiplying $F_1 + F_A$ by D, where

$$D = \sin at \left\{ 1 + \frac{\cos at}{\sqrt{\left(\frac{l}{r}\right)^2 - \sin^2 at}} \right\}. \quad . \quad . \quad (11)$$

On the other hand, the rotative effect of F_B is

$$F_B \sin \alpha t,$$

which, added to the other, gives the entire rotative effect.

The determination of the point of application of F_2, and hence of the values of F_A and F_B involves a determination of the moment of inertia of the rod about the axis of the cross-head pin, and this would be a very long and tedious piece of work, if it had to be done by measurement and computation. A very easy and elegant method of making the determination was pointed out to the writer by Mr. Wilfred Lewis, who had previously made an independent and complete analysis of the whole problem, and it consists in hanging up the rod on a knife edge, allowing it to oscillate by gravity, and counting the number of oscillations in a given time, thus determining the length of the equivalent simple circular pendulum, from which we can readily determine the moment of inertia, or, more conveniently, the distance from the center of the cross-head pin to the center of percussion of the rod about the axis of the cross-head pin, which distance will be called ρ.

If we denote by I the moment of inertia of the rod about that axis, and observe that

$$I = \rho x_0 W_2, \quad . \; . \; . \; . \; . \; . \; . \; . \qquad (12)$$

we can readily deduce the following equations:

$$F_2 = f_1 \frac{\Sigma_0^l w}{g} - \frac{f_1 - f_2}{l} \frac{\Sigma_0^l wx}{g} = \frac{W_2}{g} \left\{ f_1 - \frac{f_1 - f_2}{l} x_0 \right\} . . \qquad (13)$$

$$x_1 F_2 = \frac{f_1}{g} \Sigma_0^l wx - \frac{f_1 - f_2}{gl} \Sigma^l wx^2 = \frac{W_2}{g} \left\{ f_1 - \frac{f_1 - f_2}{l} \rho \right\} x_0. \qquad (14)$$

where x_1 = distance from center of cross-head pin to point of application of F_2; also

$$x_1 = \frac{f_1 (l - \rho) + f_2 \rho}{f_1 (l - x_0) + f_2 x_0} x_0. \quad . \; . \; . \; . \; . \; . \; . \; . \qquad (15)$$

$$F_A = \frac{W_2 n^2 r}{g l^2} \left\{ (l^2 - 2l x_0 + x_0 \rho) \left(\frac{f_1}{n^2 r} \right) + x_0 (l - \rho) \left(\frac{f_2}{n^2 r} \right) \right\}. \qquad (16)$$

$$F_B = \frac{W_2 n^2 r x_0}{g l^2} \left\{ (l - \rho) \left(\frac{f_1}{n^2 r} \right) + \rho \left(\frac{f_2}{n^2 r} \right) \right\} . \; . \; . \; . \; . \; . \qquad (17)$$

Hence the entire (negative) rotative effect of the forces producing the acceleration of the reciprocating parts is

$$(F_1+F_A)D+F_B\sin at=\left\{F_1+F_A+\frac{F_B}{1+\dfrac{\cos at}{\sqrt{\left(\dfrac{l}{r}\right)^2-\sin^2 at}}}\right\}D; \quad (18)$$

and if F denotes the equivalent force at the piston required to produce the acceleration of the reciprocating parts, we shall have

$$F=F_1+F_A+\frac{F_B}{1+\dfrac{\cos at}{\sqrt{\left(\dfrac{l}{r}\right)^2-\sin^2 at}}}, \quad . \; . \; . \quad (19)$$

and the quantity to be deducted from the rotative effect diagram is

$$FD. \quad . \; . \; . \; . \; . \; . \; . \quad (20)$$

A table of the values of D, for $\frac{l}{r} = 5\frac{1}{3}$, $5\frac{1}{2}$, and 6 respectively follows:

CRANK ANGLE.	$D=\sin at\left\{1+\dfrac{\cos at}{\sqrt{\left(\dfrac{l}{r}\right)^2-\sin^2 at}}\right\}$		
	$\frac{l}{r}=5\frac{1}{3}$	$\frac{l}{r}=5\frac{1}{3}$	$\frac{l}{r}=6.$
0	.00000	.00000	.00000
10	.20573	.20476	.20216
20	.40241	.40057	.39567
30	.58155	.57905	.57241
40	.73579	.73293	.72583
50	.85934	.85645	.84878
60	.94831	.94575	.93896
70	1.00091	.99900	.99393
80	1.01473	1.01641	1.01370
90	1.00000	1.00000	1.00000
100	.95218	.95321	.95591
110	.87847	.88039	.88545
120	.78374	.78630	.79309
130	.67275	.67564	.68330
140	.54978	.55264	.56025
150	.41845	.42095	.42759
160	.28163	.28347	.28836
170	.14157	.14254	.14513
180	.00000	.00000	.00000

Denoting by a the area of the piston-head end, in square inches, there will next be given the values of $\frac{F}{a}$ for three special engines, whose dimensions are as follows:

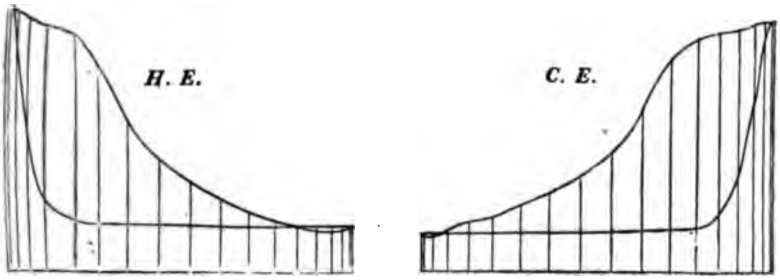
	Brown.	Harris-Corliss.	Porter-Allen.
Diameter, piston	11″	8″	10″
Diameter, piston rod	1″.87	1″.5	1″.75
Stroke	30″	24″	20″
Revolutions per minute	60	64	204
$\frac{l}{r}$	5⅓	5½	6
W' in lbs	193	87.25	130.90
W_A in lbs	78.5	33.25	55.66
W_B in lbs	84.5	33.50	67.14
ρ	4′.847	4′.064	4′.066

Crank angle.	$\frac{F}{a}$		
	Brown.	Harris-Corliss.	Porter-Allen.
0	6.842	4.704	41.378
10	6.215	4.609	40.566
20	5.834	4.326	38.161
30	5.223	3.874	34.308
40	4.422	3.278	29.243
50	3.465	2.589	23.175
60	2.408	1.786	16.372
70	1.001	0.969	9.272
80	0.207	0.533	2 119
90	−0.835	0.615	− 4.737
100	−1.798	−1.322	−11.133
110	−2.637	−1.943	−16.895
120	−3.859	−2.475	−21.917
130	−3.746	−2.912	−26.136
140	−4.425	−3.222	−29.537
150	−4.782	−3.517	−32.154
160	−4.973	−3.700	−35.013
170	−5.174	−3.804	−35.100
180	−5.224	−3.839	−35.472

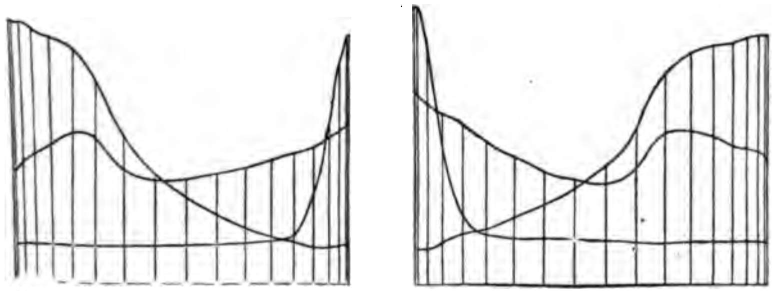
Inasmuch as my method of working up a pair of simultaneous cards differs somewhat from Prof. Alden's, a set of diagrams (Figs. 83, 84, 85, 88, 89, 90) will be added for the Porter-Allen engine.

The diagrams speak for themselves, and need no further explanation.

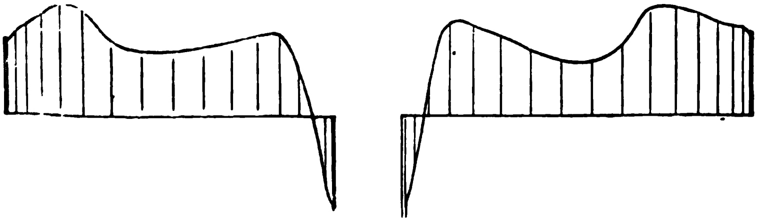
I.—PORTER-ALLEN ENGINE.



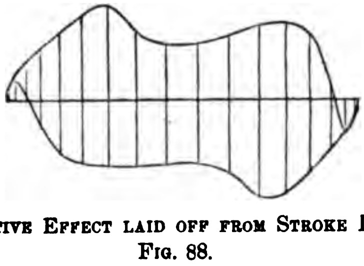
INDICATOR CARDS.
FIG. 83.



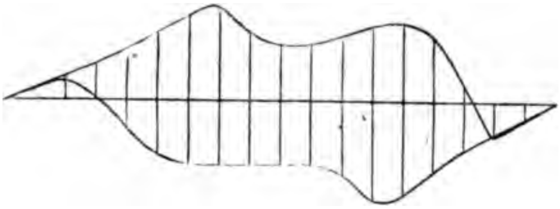
TRUE STROKE CARDS, WITH EFFECT OF RECIPROCATING PARTS INDICATED.
FIG. 84.



CARDS SHOWING EFFECTIVE PRESSURE ON PISTON.
FIG. 85.



ROTATIVE EFFECT LAID OFF FROM STROKE LINE.
FIG. 88.



ROTATIVE EFFECT LAID OFF FROM DEVELOPMENT OF CRANK-PIN CIRCLE.
FIG. 89.

In order to show how great an error is made by using Prof. Alden's approximations, the values of $\frac{FD}{a}$, for the Porter-Allen engine, will be computed, and compared with the values of Prof. Alden's corresponding quantity, viz., $\frac{1.226wrN^2CD}{a}$.

The results are given in the following table :

Crank Angle.	$\frac{FD}{a}$	$\frac{1.266wrN^2CD}{a}$	Difference.
0	0.00	0.00	0.00
10	8.20	7.18	1.02
20	15.10	13.17	1.93
30	19.64	16.98	2.66
40	21.21	18.05	3.16
50	19.67	16.33	3.34
60	15.37	12.25	3.12
70	9.22	6.64	2.58
80	2.15	0.49	1.66
90	− 4.74	− 5.26	0.52
100	−10.64	− 9.87	−0.77
110	−14.96	−12.93	−2.05
120	−17.38	−14.33	−3.05
130	−17.86	−13.67	−4.19
140	−16.55	−12.77	−3.78
150	−13.75	−11.37	−2.38
160	− 9.81	− 7.27	−1.54
170	− 5.09	− 3.76	−1.33
180	0.00	0.00	0.00

A glance at this table will show that the difference amounts in some cases to nearly than 25 per cent of the true values.

Vertical Throw.

If we let s_3, v_3, and f_3 denote respectively the space passed over by, the velocity of, and the acceleration of the crank pin in a vertical direction, we shall have

$$s_3 = r \sin at\,; \quad v_3 = \frac{ds_3}{dt} = ar \cos at\,; \quad f_3 = -\,a^2 r \sin at.$$

Moreover, the vertical acceleration of the cross-head end of the rod is zero, and hence that of a point at a distance x from the cross-head end is $f = f_3 \frac{x}{l}$.

Hence, if F_3 denote the total vertical throw of the rod, we shall have

$$F_3 = \frac{f_3}{gl}\, \Sigma_0^l wx = \frac{W_B}{g}\, a^2 r \sin at.$$

Its point of application being at a distance ρ from the cross-head end, the equivalent vertical force at the crank pin is

$$F' = \frac{\rho}{l} \left\{ \frac{W_B}{g}\, a^2 r \sin at \right\},$$

and the rotative effect due to F' is

$$R' = F' \cos at = \frac{\rho}{l} \left\{ \frac{W_B}{g}\, a^2 r \sin\, at \cos at \right\}.$$

If we assume the crank pin to lie above the line of dead points while moving from the head end towards the crank end, and thus calling the head end upper quadrant the first quadrant, it will follow that this rotative effect is to be added in the first quadrant, to be subtracted in the second, to be added in the third, and subtracted in the fourth.

Tables of values of $\frac{R'}{a}$, for the three engines already mentioned, will now be given, and also the diagram of rotative effect, taking account of the vertical throw.

CRANK.	$\frac{R'}{a}$		
	Brown.	Harris-Corliss.	Porter-Allen.
0	0.000	.000	0.000
10	.063	.116	1.400
20	.117	.219	2.632
30	.158	.289	3.623
40	.179	.336	4.120
50	.179	.336	4.120
60	.158	.289	3.623
70	.117	.219	2.632
80	.063	.116	1.400
90	.000	.000	0.000
100	–.063	–.116	–1.400
110	–.117	–.219	–2.632
120	–.158	–.289	–3.623
130	–.179	–.336	–4.120
140	–.179	–.336	–4.120
150	–.158	–.289	–3.623
160	–.117	–.219	–2.632
170	–.063	–.116	–1.400
180	–.000	–.000	–0.000

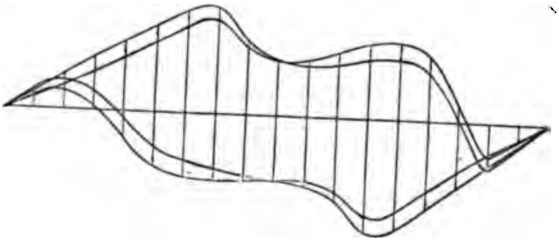


FIG. 90.

Mr. Wilfred Lewis.—The criticism which I had intended to make on this paper is substantially the same as the one just given by Prof. Lanza, namely, that the connecting rod has not received the consideration which it requires. This ground, I think, has been well taken and maintained in the discussion we have heard, and I am sure the analysis presented is much fuller and better than anything I can attempt to give orally at this time. My own researches on the subject are not very fresh in mind, having been made some two years ago for the purpose of determining the inertia of the reciprocating parts in our shop engine, and I have not had the leisure to review them carefully in preparation for this meeting. At that time I studied the question very thoroughly, and developed my results by means of formulæ and tables similar to those given in the paper. Further than that, however, I made the connecting rod the subject of special investigation, and constructed diagrams in which the center of oscillation, as well as the center of gravity, was duly considered. Both of these points were determined by experiment, the center of gravity, by weighing the rod at each end, and the center of oscillation, by suspending it at a convenient point and counting the vibrations in a given time. My method of determining the effects of inertia was, if I remember rightly, somewhat different from that developed by Prof. Lanza in his more recent analysis, and I cannot say at present whether we agree entirely or not.

Since the New York meeting adjourned * I have compared my original method of determining the inertia of the reciprocating parts with the one given by Prof. Lanza, and found that although we differ somewhat in our modes of treatment, the results obtained in either way are identical.

Having found the centers of gravity and oscillation by experiment, I considered the weight of the connecting rod to be distributed at the cross-head pin and the center of oscillation relative thereto so as to maintain the same center of gravity, and proceeded to find the inertia of the masses moving at these two points respectively. With the mass of the connecting rod so distributed I simply added to that part at cross-head pin, the mass of the piston, piston rod and cross head, as partaking of the same movement, and thus disposed the whole mass of the reciprocating parts at the two points mentioned.

The work of determining the inertia at these points by means of general formulæ is, however, very laborious, and even by the aid of tables the greatest care and attention must be exercised to avoid mistakes. The result of the numerous calculations, when finally plotted, may be taken, to some extent, as a measure of the integrity of the various steps in the process, but the only positive safeguard at present lies in a repetition of the whole work. In view of this point and the fact that graphical results are the ends to be attained, it has occurred to me that they should be reached, as far as possible, by graphical methods, so as to reduce the necessary calculations to a minimum, and I now propose to show how this may be done with comparative ease.

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* Contributed under the rules.

Fig. 93 is a construction for finding the horizontal velocity of any point in the connecting rod for any part of the stroke. Fig. 94, a construction for finding the vertical velocity and acceleration of any point in the connecting rod for any part of the stroke; and Fig. 95, a construction for finding the horizontal acceleration of any point in the connecting rod for any part of the stroke.

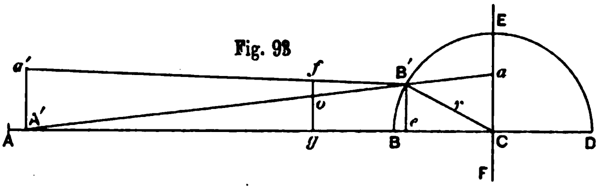


Fig. 93

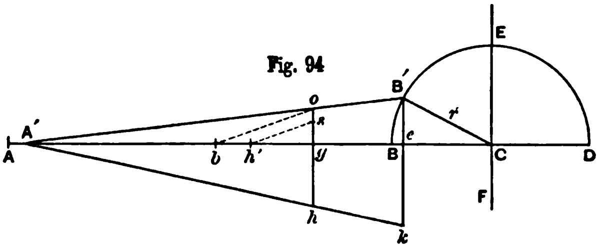


Fig. 94

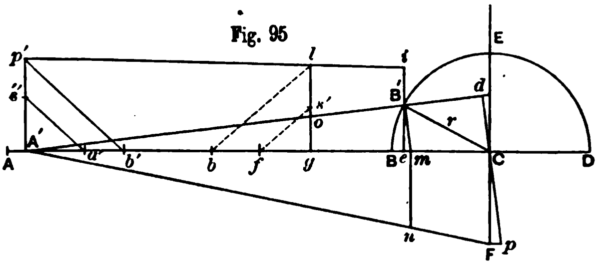


Fig. 95

In these figures ABC is the line of centers, AB the connecting rod, and BC the crank, and the distance represented by the radius r is taken to be the measure of crank velocity and its radial acceleration as well. In Fig. 93, let B' be any point in the crank circle, and $B'A' = BA$, the connecting rod; draw ECF perpendicular to AD, and, if necessary, produce $A'B'$ to the intersection a, then will Ca be the measure of the velocity of the point A', as shown by Rankine and others. The horizontal velocity of the point B' is measured by the ordinate $B'e$, and if we lay off $A'a' = Ca$, and draw $a'B'$, we may adopt a construction used by Prof. Lanza, and find the velocity of any point o in the connecting rod by the ordinate fg passing through it.

The vertical velocity at A' is of course zero, while that at B' is measured by the distance eC, consequently in Fig. 94 ek is laid off equal to eC, and ordinates to the line $A'k$ measure the vertical velocity of the points in the connecting rod through which they pass.

The construction for finding the vertical acceleration of any point in the connecting rod is very simple indeed, for at any point o, the ordinate og is at once the measure of it.

The construction for finding the horizontal acceleration shown in Fig. 95 is as follows: draw Cd perpendicular to $A'B'$, meeting it, or the line produced, in the point d, and make $B'm$ parallel to Cd. At the point m, erect, perpendicular to AD, mn equal in length to $B'd$ and draw $A'nF$. Produce dC and draw Fp parallel to AD, then will Cp represent the horizonal acceleration of the point A'. If now we lay off the ordinates $A'p' = Cp$, $ei = eC$, and draw the line $p'i'$, any ordinate gl will measure the horizontal acceleration of the point o through which it passes.

To demonstrate that Cp is the measure of horizontal acceleration for the point A', let the velocity of the point B' be resolved into its components Cd along $A'B'$ and $B'd$ perpendicular thereto. The component along $A'B'$ produces no acceleration while the component at right angles produces at B' in the direction of $A'B'$ an acceleration represented by $B'd$, and at A' in the same direction an additional amount which is inversely proportional to the distances $A'B'$ and $B'd$. Therefore, by the construction, CF represents the acceleration of A' in the direction of $A'B'$, and consequently Cp is the acceleration in the direction of AD.

If, now, we assume o for the center of oscillation of the connecting rod about the cross-head pin, we can proceed to construct diagrams for the horizontal accelerations of A and o and also for the vertical acceleration of o. Such diagrams are shown in Fig. 96, the ordinates being taken for every 30° of crank angle.

The actual forces of inertia may be found by multiplying these accelerations by the masses concentrated at A and o respectively. This is a matter of simple proportion, de-

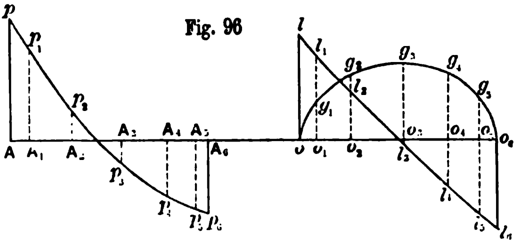


Fig. 96

pending upon the scale desired, and constructively it requires no special explanation. Thus far the velocity of the crank pin in feet per second has been assumed as measured by the radius r of the crank in feet, and consequently, for any velocity, v, of the crank, the distance r on the diagrams will measure an acceleration of $\left(\frac{v}{r}\right)^2$. For a unit of mass, or about 32.2 lbs. at crank pin, the inertia in a radial direction will also be expressed by $\left(\frac{v}{r}\right)^2$ and whatever the masses at A and o or the speed of the engine may be, the scale of the diagram Fig. 96 can readily be adjusted to correspond with the scale of the indicator cards by constructing a simple proportion.

The three diagrams shown in Fig. 96 can best be combined by finding the rotative effect of each, and this has been done in Figs. 97, 98, and 99, for every 15° of crank angle. Fig. 97 represents the rotative effect of a unit of mass at cross-head pin; Fig. 98, the same for the horizontal throw at center of oscillation; and Fig. 99, the same for the vertical throw at that point. To determine the rotative effect, the inertia at each point A and o must be multiplied by the effective radius at which it acts. But the velocity at any point in a given direction is also the measure of the effective radius at which a force in that direction acts, and we have for the rotative effect at A', $A'p'$ times $A'a'$. This multiplication is readily accomplished by laying off in Fig. 95, $A'b' = r$ and $A'a'$ the same as in Fig. 93, then by drawing $a's''$ parallel to $p'b'$, we have $A's''$ for the rotative effect due to a unit of mass at the point A'. Similarly, in Fig. 94, the rotative effect due to

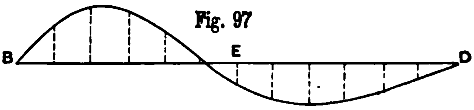


Fig. 97

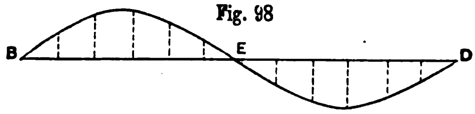


Fig. 98

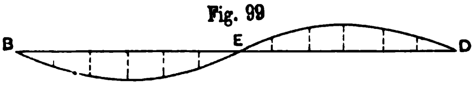


Fig. 99

vertical throw is measured by the product of *og* and *gh* or by *gs*, and in Fig. 95 the rotative effect due to the horizontal throw of *o* is equal to *gl* times *gf* or to *gs'*. Combining Figs. 98 and 99,

Fig. 100

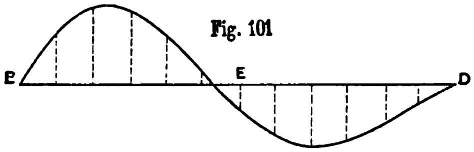


Fig. 101

we have Fig. 100 for the rotative effect of a unit mass at *o*; and, combining Figs. 97 and 100, we have Fig. 101 for the rotative effect of a unit of mass at *A* and *o* respectively.

By the aid of Fig. 101 the three diagrams shown in Fig. 96 may readily be combined into one, representing the equivalent force of inertia at cross-head pin, in order to be directly comparable with an indicator card. At the dead centers Fig. 101 is useless, for at these points the effect of inertia can be found directly from Fig. 96.

In actual practice it will, of course, be unnecessary to construct so many figures, and to illustrate the use of this graphical method, we will take the following practical example:

R = length of crank = 1 ft.,
L = length of connecting rod = 4 ft.,
N = No. of revs. per sec. = 3,
w_a = weight of connecting rod at cross-head end = 90 lbs.,
w_b = weight of connecting rod at crank end = 150 lbs.,
t = time of oscillation of connecting rod when suspended at cross-head pin = .959 sec.,
W_p = weight of piston, piston rod and cross-head = 560 lbs.

From the above figures it can readily be found that the center of gravity of the connecting rod is two and a half feet from cross-head pin, and its center of oscillation three feet from the same point. Therefore, to maintain the same center of gravity, we must assume forty pounds at cross-head pin and two hundred pounds at the center of oscillation. The total weight of the reciprocating parts may now be distributed at these two points.

W_a = total weight concentrated at cross-head pin = 600 lbs.
W_0 = weight concentrated at center of oscillation = 200 lbs.

Let the piston area A = 200 square inches, and suppose that we wish to construct an inertia diagram directly comparable to an indicator card four inches long, taken by an indicator having a forty pound spring.

To fix the scale of ordinates, we must determine the radius r to be used in our constructions. and this can readily be found by the formula

$$r = \frac{1.226\ W_a R N^2}{40\,A}$$

from which, by substitution, we derive

$$r = \frac{1.226 \times 600 \times 1 \times 9}{40 \times 200} = .823 \text{ in.}$$

For the scale of abscissas, we simply let the length of the card, 4″, determine the stroke, and find the points $A_1 A_2$, etc., Fig. 103, corresponding to the crank angles assumed, as in Fig. 96. The construction for finding the ordinates in Fig. 103 is shown in Fig. 102, r being laid off equal to .823″ and the distance $AB = 4r$. The inertia of the mass at cross-head pin is found as in the previous construction to be $A'p'$. The

Fig. 102

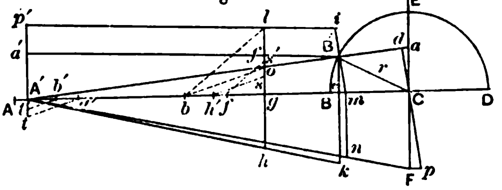
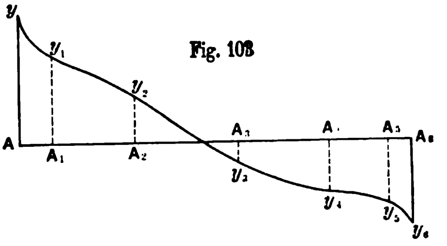


Fig. 103



rotative effects of the horizontal and vertical throws at o are also found, as already described, to be gs' and gs respectively, and, as they act in opposite directions, their sum is ss'. Now, the rotative effect of a force at A has been found by taking the product of the force and its velocity Ca, and, therefore, the force at A' necessary to produce a given rotative effect can be found by dividing the effect produced by the velocity. ss' is the effect produced by a weight at o equal to the weight at A, and consequently it must be multiplied by the ratio $\frac{W_o}{W_a}$ to find the true effect. This is done in the next construction for finding $A't'$, which is the inertia at A' equivalent to the inertia at o. If we lay off $A't = ss'$ and $A'a' = Ca$, we may find $A't'$ by laying off $A'b' = \frac{W_o}{W_a}r$ and drawing $b't'$ parallel to a', then $p't'$ will be the ordinate in Fig. 103 for the point A'. Similarly, ordinates may be found for any other points, except the dead points, where the construction is somewhat simpler, as already pointed out. By comparing App_6A_6 Fig. 96, which may be recognized as the usual form of inertia diagrams, with Fig. 103, the disturbing effect of the connecting rod is plainly to be seen.

Other diagrams might be constructed to show the resulting pressures on guides and bearings, but enough has been done to suggest the extended application of graphical methods to problems which have heretofore resisted or yielded only with difficulty to the most searching analyses.

It is thought, in conclusion, that the graphical method here given presents fewer difficulties, is more direct and certain in its results, and that if used in connection with the analytical method it will act as a convenient and efficient check.

It should be noted that the center of oscillation o, has been located with reference to the cross-head pin for convenience only, and that the crank pin, for instance, might have been taken as one of the centers equally well, and the weight of the connecting rod disposed accordingly, without affecting the results obtained.

Prof. G. I. Alden.—I would like to thank Prof. Lanza for two things. One is for verifying some of the tables, which I calculated while I was full of other duties. He has, I understand, made calculations from them, and comparing them with his work has found them substantially correct. And I also wish to thank him for supplementing the paper with his able discussion in regard t the effect of the connecting rod, which for the sake of greate simplicity in the use of the tables I did not take account of in m paper. I thought something would be gained if tables could b obtained which were simple enough so that any engineer woul be able to construct substantially accurate diagrams. In mos cases this is not attempted with any approach to the accurac obtained by the use of my tables. Anything we can do to brin these tables, or the more exact results which Prof. Lanza has obtained, more readily into use will I think be valuable. I hop that at some time some of the gentlemen will look over the las paragraph of my paper in regard to the method of finding th compression line of the engine. I do not know whether it woul be of special value to discuss it now.

My purpose* in presenting the paper under discussion was a very simple one, viz., to furnish tables by means of which the construction of diagrams, much more accurate than those usuall employed, might become common among engineers. I aimed t eliminate the labor due to mathematical intricacy, and still secur valuable practical results. That only approximate results ar aimed at does not necessarily detract from the value of the table given.

It is, however, important to know the degree of accuracy obtained by an approximate method, and the manner in which the error affects results. In the present case the discussion has given us the means of determining these points. Methods of computing and representing the error due to the approximation, have been given. They are instructive and beautiful examples of critical analysis and original investigation, but from their complexity are of no practical value to the engineer as methods of constructing the required diagrams.

There is one point in the discussion to which I desire to call attention, viz., the comparison made by Prof. Lanza between the results—for a special problem—of his own methods and those given in my paper. Assuming the correctness of his figures, he has, inadvertently, no doubt, made unwarranted use of them in the comparison referred to (page 212). Having given the (negative) rotative effect due to "horizontal throw," he compares, in a table, these results with the total (negative) rotative effect as cal-

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* Added since the adjournment of the meeting under the rules.

culated by my approximate method, and remarks, that "a glance at the table shows that the difference amounts in some cases to more than 25%." He then proceeds to give, in a table, the rotative effect of "vertical throw" for the same problem, but does not combine his two partial solutions. It is necessary to do this, in order to obtain, by his method, the total rotative effect. I therefore give the following table, using Prof. Lanza's figures:

Crank Angle.	Rotative Effect due to "Horizontal Throw." (Negative.)	Rotative Effect due to "Vertical Throw." (Positive.)	Total Rotative Effect, by exact Method. (Negative.)	Total Rotative Effect, by Approximate Method. (Negative.)	Difference.
0°	0.00	0.000	0.000	0.00	0.000
10°	8.20	1.400	6.800	7.18	0.38
20°	15.10	2.632	12.468	13.17	0.702
30°	19.64	3.628	16.017	16.98	0.963
40°	21.21	4.120	17.090	18.05	0.960
50°	19.67	4.120	15.550	16.33	0.780
60°	15.37	3 623	11.747	12.25	0.503
70°	9.22	2.632	6.588	6.64	0.052
80°	2.15	1.400	0.750	0.49	– 0.260
90°	– 4.74	0.000	– 4.740	– 5.26	– 0.520
100°	– 10.64	– 1.400	– 9.240	– 9.87	– 0.630
110°	– 14.96	– 2.632	– 12.328	– 12.93	– 0.602
120°	– 17.38	– 3.623	– 13.757	– 14.33	– 0.573
130°	– 17.86	– 4.120	– 13.740	– 13 67	+ 0.070
140°	– 16.55	– 4.120	– 12.430	– 12.77	– 0.340
150°	– 13.75	– 3.623	– 10.127	– 11.37	– 1.243
160°	– 9.81	– 2.632	– 7.178	– 7.27	– 1.092
170°	– 5.09	– 1.400	– 3.690	– 3.76	– 0.070
180°	– 0.00	– 0.000		0.00	0.000

This comparison shows the close approximation obtained by the use of the tables given in my paper. The inconsistency of Professor Lanza's comparison of his partial solution with the results of my approximate method is quite apparent from the fact that by his own figures, his partial solution is much farther from the truth than the approximation with which he has compared it. Moreover, the error of the approximate method is of the opposite kind from that shown by his comparison. It may also be noted that in the problem solved by Prof. Lanza the weight of the connecting rod is nearly equal to that of all the other reciprocating parts $\frac{122.8}{137}$. It would seem that the percentage of error would be considerably reduced for a case in which the ratio of weight of connecting rod to that of the other reciprocating parts is smaller and more nearly that found in modern practice. Mr. Lewis, in illustrating his graphical method, has used a ratio of $\frac{3}{7}$.

In view of all the facts brought out in the discussion, it appears that the tables and method of using them, as given in my paper, may be recommended as the best practical means available for ordinary use in making computations for rotative effect of reciprocating parts in high speed engines.

CCXXXV.

IS WATER GAS AN ECONOMICAL FUEL?

BY WILLIAM KENT, NEW YORK.

(Member of the Society.)

In the discussion of Mr. F. W. Taylor's paper on water gas, at the Chicago meeting, the writer stated that the conclusion to which Mr. Taylor had been led by his experiments, viz.: that water gas is not as economical as Siemens gas, when used as a fuel for steel-melting furnaces, would probably be confirmed by a theoretical consideration of the amount of heat carried away in the chimney gases. A further study of the subject shows that the opinion then given was correct.

Let C represent the carbon in a given quantity of fuel.
O the oxygen needed to make carbonic oxide (CO) with this C.
2O the oxygen needed to make carbonic acid (CO_2) with the same C.
H_2O the water needed to make water gas of the formula (CO + 2H) with the same C.
N - the nitrogen in the air from which the O is taken to burn C to CO.
2N the nitrogen in the air from which the 2O is taken to burn C to CO_2.
Ex. air the excess of air used in final combustion in the furnace over that required to make complete combustion.

The fuel may be used in three ways:

1st, by direct and complete combustion in the furnace.
2d, by partial combustion in the Siemens gas producer, and final combustion of the Siemens gas in the furnace.
3d, by partial combustion in the water gas producer, and final combustion of the water gas.

The results of the three ways of burning the fuel are as follows

	Materials used.	Products.
1. Direct.	$C + 2O + 2N$ + Ex. air	$CO_2 + 2N$ + Ex. ai
2. Siemens.	In producer, $C + O + N$	$CO + N$ (Siemens gas
	In furnace, $CO + N + O + N$ + Ex. air	$CO_2 + 2N$ + Ex. ai
3. Water gas.	In producer, $C + H_2O$	$CO + 2H$ (Water gas
	In furnace, $CO + 2H + 2O + 2N$ + Ex. air	$CO_2 + 2N$ + Ex. ai $+ H_2O$.

The final product of (1) and (2) is the same. The final produc of (3) differs from that of (1) and (2) only in containing H_2O, whic is the same H_2O (water) which was added in the producer in mak ing the gas. This is true whatever may be the actual formula o the water gas. All the water originally used in the manufactur of the gas reappears in the products of combustion in the chimne The only difference is, that in the chimney it appears in the for of steam, superheated to the temperature of the chimney gase while it originally enters the system as cold water. All the hea contained in the water thus escaping in the chimney, above th amount it contained when it was originally introduced into th system, is entirely wasted.

The amount of heat thus wasted can be calculated if we kno the amount of water used, and its temperature on entering and o leaving the system.

Suppose a pure water gas to be of the formula

$$\begin{array}{lcc} & CO + H \text{ made from} & C + H_2O \\ \text{or, pounds,} & 12 + 16 + 2 & 12 + 18 \end{array}$$

The escaping gases would contain, in addition to nitrogen and e cess of air,

$$\begin{array}{c} CO_2 + H_2O \\ \text{pounds } 44 + 18. \end{array}$$

The 18 pounds of water being the same from which the water ga was made with 12 pounds carbon. The carbon, if completel burned from C to CO_2, would generate 12 × 14,500 = 174,000 hea units (Fahrenheit). Suppose that the chimney gases are cooled b regenerators or other means down to 212° F., that the water escape in the form of steam of that temperature—its total heat, includin latent heat of evaporation, being 1,178° F.—and that the water en tered the system at 78° F. The heat carried away by this water escaping in the shape of steam, in excess of that which it introduced into the system, would be

$$1{,}100 \times 18 = 19{,}800 \text{ heat units,}$$

equal to about 11.4 per cent. of the whole amount of heat produced by burning the 12 lbs. carbon from C to CO_2.

This is the most favorable case, for it is in practice never possible to cool the chimney gases of heating furnaces to a temperature as low as 212° F.

Theoretical considerations therefore indicate a loss of economy in the use of water gas as fuel, equal to at least 11 per cent. of the total fuel value of the carbon used, and this, independent of any loss which may arise from imperfections of the producer or its method of operation.

Such a loss is not met with in the Siemens system, nor in the ordinary system of direct burning of the fuel upon a grate, except in so far as moisture in the shape of cold water may be present in the fuel used.

The fact that there is a loss of economy in the use of water gas as a fuel, in comparison with the direct use of the carbon from which it is made, is clearly revealed in other ways, however, even by some of those who have written in favor of the water-gas processes. For instance, Dr. Henry Wurtz, in Vol. VIII. Trans. Amer. Inst. Mining Engineers, pp. 295, 296, shows that 37.5 pounds of anthracite coal are required to generate 1,000 cubic feet of water gas, and that water gas has a heating power of 311° F. per cubic foot. This equals 311,000 heat units per 1,000 cubic feet. Assuming anthracite to produce 13,000 heat units per pound (a moderate figure), the 37.5 pounds if burned directly would produce 13,000 × 37.5 = 487,500 heat units, showing that the water gas has a thermic value of only 63.8 per cent. of the coal from which it is produced.

Better results are deduced theoretically by Mr. Wm. A. Goodyear, Vol. XI. Trans. Amer. Inst. Mining Engineers, p. 311. He shows that water gas derived from anthracite has about 81 per cent. of the heat-producing power of the carbon contained in the anthracite. Neither of the two writers named, however, seems to have considered the cause of the loss of heat to which I have referred, viz.: the carrying away of heat in the superheated steam in the chimney gases.

DISCUSSION.

Mr. Fred. W. Taylor.—The writer agrees with Mr. Kent in hi main conclusion, that "water gas is not as economical as Siemen gas, when used as a fuel for steel melting furnaces;" but he can not agree that the above conclusion could be arrived at by " theoretical consideration of the amount of heat carried away i the chimney gases" alone. On the contrary, if we were to cor sider merely the relative amounts of heat carried away by th chimney gases, water gas would be shown to be more economic than Siemens gas.

In the calculations as a result of which Mr. Kent arrives at th following conclusion, "Theoretical considerations, therefore, ir dicate a loss of economy in the use of water gas as fuel, equal t at least eleven per cent. of the total fuel value of the carbon use and this independent of any loss which may arise from imperfe tions of the producer or its method of operation. Such a loss i not met with in the Siemens system, etc., etc," he has entirel overlooked the fact that the water which passes off in the chin ney, in the form of steam, is produced by the combustion of th hydrogen (H) in water gas which, in burning from H to $H_2($ develops a larger amount of heat than any other constituent water gas.

Mr. Kent very truly states that "the carbon, if complete burned from C to CO_2, would generate 174,000 heat units (Fahrer heit);" but he makes no mention of the amount of heat which th hydrogen develops in burning from H to H_2O.

Mr. Kent has also overlooked the fact that the waste produc of enough Siemens gas to generate the same amount of heat would be generated by a given amount of water gas contain abo four-fifths as much superheated steam as the waste products water gas. Any conclusions, therefore, which are based on ca culations which do not consider all of the elements of the probler are likely to be misleading.

It would seem to the writer that the following were a bett method of dealing with this problem.

Let us first select a sample of Siemens gas and one of wat gas, whose analyses show them to be of about average qualit Such samples, I think, would be the following:

For water gas, a sample taken from Lowe's gas producers, aft passing through purifier, at Novelties Exhibition, Philadelphia, October 16, 1885:

CO	44.5 vols.	
H	50.9	
O	.7	Air.
N	2.8	
	1.1 undetermined.	

For Siemens gas, an average of a large number of samples taken from the Siemens producers at Midvale Steel Co., Nicetown, Philadelphia, 1883:

CO_2	1.5 vols.
CO	23.6
H	6.0
CH_4	3.0
N	65.9

One cubic foot of water gas of the above composition will develop in burning 327 heat units (lb. Fahrenheit), including the latent heat of evaporation of the superheated steam which escapes in the stack. 2.42 cubic feet of Siemens gas will also develop 327 B. t. u. But one cubic foot of water gas, after having been burned, will produce only 2.87 cubic feet of waste or chimney gases, while its equivalent, 2.42 cubic feet of Siemens gas, will produce 4.59 cubic feet of chimney gases.

Supposing that the chimney gases enter the stack at a temperature of 400°, which is a low chimney temperature for furnaces, and that the water and air used in making and burning the gases entered the furnaces at a temperature of 78° F., the 2.87 cubic feet constituting the chimney gases resulting from the combustion of one cubic foot of water gas will carry to waste 44.305 B. t. u. (including the latent heat of evaporation of the steam contained in them), while the 4.59 cubic feet constituting the chimney gases resulting from the combustion of 2.42 cubic feet of Siemens gas will carry to waste 51.51 B. t. u. (including the latent heat of evaporation of the steam contained in them).

From the above figures we see that in the case of Siemens gas $\frac{51.51}{327} = 15.75$ per cent. of the total heat generated is lost in the stack, while in the case of water gas only $\frac{44.305}{327} = 13.55$ per cent. of the total heat generated is carried away by the chimney gases.

If the relative values of the two gases were to be judged by the relative amounts of heat wasted in the chimney, water gas would be shown to be 2.2 per cent. more economical than Siemens gas. In point of fact, the two principal causes which render water gas less economical than Siemens gas have relation to the process of manufacture of the two gases. They are as follows:

First. The fuel available for making water gas is more expensive than that used for Siemens gas.

Second. There is a great waste of heat in making water gas, during that part of the process in which air is blown through the coals to heat them to incandescence. Just how much heat is here wasted no calculations can show. Experience, however, shows it to be large.

Mr. Kent.—I have not stated in my paper at all that this loss of heat in the chimney was the principal loss in the water-gas process. I state distinctly that the loss of eleven per cent. is independent of any loss which might occur from the defects of the producer.

Mr. Taylor's criticism consists chiefly in saying that I have overlooked several things. I have overlooked them purposely, and that is the proper way to get at this problem, by overlooking the intermediate stages of the process, and considering only the beginning and the end of the process, the fuel used to generate the heat, and the amount of heat wastefully discharged. He begins his calculation by taking a pound of water gas and comparing it with an equivalent amount of Siemens gas. I begin with a ton of coal, and burn that coal in three ways: first on a grate, second in the Siemens system, and third in the water-gas system. My calculation here shows conclusively, I think, that in burning in the Siemens system, or on a grate, the final product in the chimney will be the same. In burning in the water-gas system there will be an amount of superheated steam going off, which carries away heat equal, at the very lowest calculation, to eleven per cent. of the heating value of the carbon in the coal.

Mr. A. P. Trautwein.—The concern with which I am connected has been engaged for some ten years or more in developing the methods of manufacture and use of water gas, principally, however, for the purposes of illumination, rather than for metallurgical or general fuel purposes. Of course, the applicability of water gas for these latter uses has received more than mere incidental consideration, because schemes to introduce fuel gas into cities have repeatedly been proposed, and, from our close connection with the gas-lighting interests, have naturally come under our notice. The conclusion, however, was definitely reached, in every instance, that whatever the advantages of water gas in other respects, it cannot be regarded as an economical means of utilizing to the best extent the calorific value of coal. There are, however, instances in which water gas must be applied for fuel purposes, quite independent of these economic considerations, and in the operations of our own establishment it now finds a daily and very successful application.

Regarding the point about which Messrs. Taylor and Kent are in controversy, there is but this to say: It is quite immaterial, for the purpose of arriving at a solution of the problem, whether the primary combustion of the coal is carried on in an atmosphere of steam (as is done in the water-gas system), and the secondary combustion concluded in the presence of air, or whether (as in the Siemens producer and its regenerative furnace) the combustion, both primary and secondary, occurs in air; because, granting that the laws of the conservation of energy hold true, it is not necessary to consider any of the intermediate steps; and we have but to look to the final products of combustion, and note wherein they differ, and how much heat they carry to waste, to arrive at a knowledge of how much more heat energy is lost in one process than in the other. This is the method suggested by Mr. Kent, whose way of looking at the whole problem is, in my opinion, unquestionably correct. The final products of combustion are the same in both cases, except that in addition to the carbonic oxide and nitrogen, we also find, in the water-gas system, a certain amount of water vapor, which entered it at the boiler in the form of comparatively cold feed water, and here reappears as superheated steam. It is this steam, which, aside of course from all questions of radiation and similar losses, represents the waste of energy in the manufacture of fuel gas by the water-gas process, as compared with the Siemens producer and furnace.

Mr. F. W. Taylor.—I would ask what Mr. Kent has to say to the fact that the waste chimney gases of Siemens gas contain four-fifths as much superheated steam as the waste chimney gases of water gas. He has entirely overlooked the fact that the waste products of Siemens gas contain superheated steam.

If he will take the pains to study the actual composition of the waste chimney gases of the Siemens gas, he will find that they contain four-fifths as much weight of superheated steam as is contained in the waste products of water gas.

How he can say, in view of that fact, that there is a waste of eleven per cent. of the total fuel value of the water gas, which does not take place in the Siemens gas, I cannot see. That is his main conclusion.

Mr. Kent.—My answer to that is that the coal used in making that Siemens gas must have been very wet.

Mr. Taylor.—The coal used in making that Siemens gas was the ordinary bituminous coal, from which most of the Siemens and illuminating gas in the country is made, and contained no more moisture than such coal ordinarily contains.

In the process of generating Siemens gas, the light hydrocarbon and hydrogen gases are driven off from the coal first. They constitute the most valuable and richest part of the Siemens gas; and the steam contained in the waste products of Siemens gas results from the combustion of this hydrogen.

I have never seen an analysis of good Siemens gas which did not contain a considerable amount of free hydrogen and light hydrocarbon gases.

I would inform Mr. Kent that when the bituminous coal used for making Siemens gas does not contain a sufficient amount of moisture to generate the desired quantity of hydrogen, it is customary to add water to the coal, either in the form of steam introduced into the producer, or water in the ash pit.

I agree with Mr. Kent entirely that water gas is less economical as a fuel than Siemens gas. But I disagree with him that any such conclusion can be arrived at by the examination of the chimney gases alone.

Mr. Geo. Schuhmann.—We are not using any water gas, but I have read all the literature on this subject that I could find, and have no doubt Mr. Kent's view is correct theoretically, and assuming perfect combustion, we can get more heat units out of a ton of coal by burning it direct, than we can by transforming it first into water gas; but this does not answer the question at the head of this paper, for we may also prove that we cannot get as many heat units out of a certain amount of steam as we could get by direct combustion of the coal which was necessary for generating said amount of steam, and still a great many factories, etc., are heated more economically with steam than by burning coal in stoves; so it is with water gas, although at a disadvantage theoretically, I think it can be used as fuel where Siemens gas will not work satisfactorily, and where coal can only be used with a large waste; for instance, in small furnaces, or where a large amount of heat is to be concentrated at a certain spot without heating too much of the surrounding material. I have been looking around for a gas producer that will make gas for small furnaces, but the manufacturers of the various modifications of Siemens producers would not guarantee economy in small furnaces; one manufacturer told me that Siemens gas could not give much economy without regenerative chambers, which, we know, are very expensive affairs. I have not given up the search yet, but am inclined to believe that for small furnaces water gas is a more economical fuel than Siemens gas, especially if the latter can hardly be used at all. Therefore, speaking from a practical point of view, I should answer Mr. Kent's question: "Is water gas an economical fuel?" by saying: "That depends on what it is to be used for." I simply make these remarks, so that those who are experimenting with water gas will not get discouraged by reading Mr. Kent's paper.

Mr. Taylor spoke about the large amount of heat developed by burning hydrogen, but that does not alter Mr. Kent's argument, because it requires just as many heat units to dissociate the hydrogen from the oxygen when blowing steam over hot coal.

Mr. Kent.—I entirely agree with Mr. Schuhmann on that point, and I only referred to heating on a large scale, as in a steel melting furnace. The other day some one asked me about small furnaces, and I said I would either use water gas for it, or take common illuminating gas out of the street mains. The choice then would be between building a water-gas producer or buying the gas from a gas company.

The Chairman.—While not pretending to any expert knowledge on the subject of Siemens and water gases, it has seemed to me, as I have listened to this discussion, that there is, after all, an explanation of the apparently radical difference between Mr. Taylor and Mr. Kent, which may be compared to the case of a steam engine and boiler. One method of testing a steam engine and boiler plant, as a whole, would be to weigh the amount of fuel put in at one end and measure the amount of heat which disappeared in the chimney, and to consider the useful effect as the difference between them. But if the purpose was to apply the steam generated to producing power, it would be necessary to take into account one of the intermediate steps of the process, namely, the steam engine, and the efficiency derived from that would thus become a factor. Mr. Taylor's position, as I understand it, brings in these intermediate questions; while Mr. Kent's ignores those and takes account only of the two extremes of the process. Both points of view may be correct. It may be necessary, under different conditions, to consider the one or the other, but I do not see that there is necessarily any conflict between them. I understand that that is also Mr. Schuhmann's explanation of the matter.

Mr. Schuhmann.—Yes, sir; it depends a great deal on the use to which the gas is to be put.

CCXXXVI.

THE NEW CALORIMETER.

BY GEO. H. BARRUS, BOSTON, MASS.

(Member of the Society.)

IN a paper submitted at the Boston meeting in November, 1885, the writer described an apparatus * for determining the amount of moisture in steam, which operated on an entirely different plan from any hitherto used. It was pointed out that the new method consisted in evaporating the moisture held by the steam, and measuring its quantity by determining the amount of heat required for that purpose.

At the time of submitting the paper, the new instrument had been in use sufficiently long to establish its practicability, and to show that it was substantially reliable; but it had been used only in an experimental way. During the past year, it has been employed for several cases of practical tests of boilers, and it is proposed now to give the results of these tests and thus furnish a supplementary notice relating to its practical operation.

To enable the subject to be properly understood, the cut, Fig. 86, showing the principal features of the new calorimeter, given in the former paper, is here reproduced.

In all cases thus far tried, the pipe conveying the steam to be tested has been a half-inch iron pipe. A long thread is cut on this pipe, and it is screwed into the main steam supply pipe of the boiler in such a manner as to extend diametrically across to the opposite side. The inclosed part is perforated with from 40 to 50 one-eighth inch holes, and the open end of the pipe sealed. If the pipe is screwed into the under side of the main, the perforations begin at a distance of one inch from the bottom. The connection between the main and the calorimeter is made as short as possible, and covered with hair felting. Where the calorimeter can be attached

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* A New Form of Steam Calorimeter. Trans. A. S. M. E., Vol. VII., p. 178.

to the under side of the main, the distance to the stop valve H (see cut) need not exceed six inches. In this position the apparatus is self-supporting. The steam for the superheater is also supplied by a half-inch iron pipe, but this may be attached to the main at any convenient point, and no special form of connection is required.

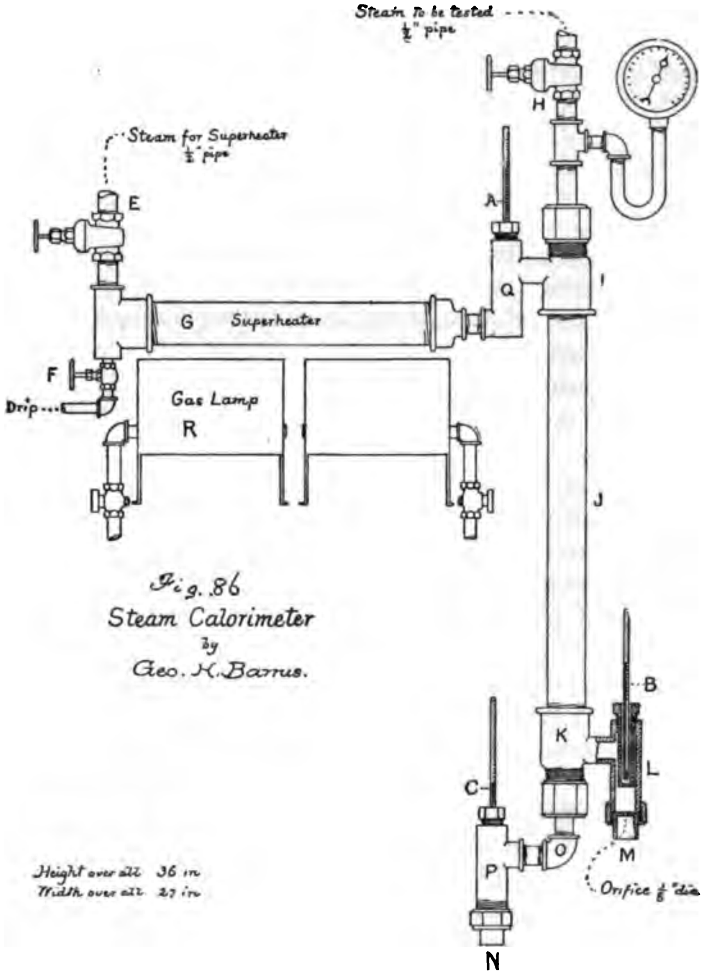

Fig. 86
Steam Calorimeter
by
Geo. H. Barrus.

CASE NO. 1.—TESTS AT THE ARLINGTON MILLS, LAWRENCE, MASS.

Two kinds of boilers were tried; one, termed the "Hawkins," a horizontal *direct* tubular boiler, and one, termed the "Old Boilers," a set of three horizontal *return* tubular boilers. The Hawkins boiler is 8 ft. in diameter, 20 ft. long, and contains 206 3-inch tubes. The area of the heating surface amounts to 3,242 sq. ft. The steam pipe is 8 in. in diameter and is provided with straight-way valves. It is attached to a point near the front end of the boiler. There is no dry pipe or steam dome. The Old Boilers are 60 in. in diameter, 17 ft. long between heads, and each contains 77 3-inch tubes. The collective area of the heating surface amounts to 3,306 sq. feet. The steam pipes are 4 inches in diameter leading into a 10-inch main, and are provided with globe valves. They are attached to the front ends of the shells, and there are no dry pipes or domes.

The calorimeter was attached, at a distance of 6 inches, to the under side of a horizontal pipe which could be connected to either boiler, this being a pipe supplying an engine which consumed about 4,000 lbs. of steam per hour. When the boilers produced more than this quantity, the surplus passed off into an independent pipe.

The results of the tests of Case No. 1 are given in Tables Nos. 1 and 2.

TABLE No. 1.—Results of Tests.

Case No. 1.

Which Boiler.	Hawkins.	Old Boilers, Nos. 2, 3, and 4.			Hawkins.		
Date, 1886.	Apr. 13.	Apr. 14.	Apr. 15.	Apr. 16.	Apr. 30.	May 4.	May 5.
Equivalent evaporation per hour with feed 100° and pressure 70 lbs....lbs.	4,168.1	4,240.4	7,106.4	6,860.0	6,769.7	6,144.3	6,487.1
Equivalent evaporation per hour with feed 100° and pressure 70 lbs. per sq. ft. heating surface..............lbs.	1.29	1.29	2.15	2.07	2.09	1.89	1.99
Horse-power developed according to A. S. M. E. Standard..........H. P.	138.9	141.3	236.9	228.7	225.6	204.8	214.6
Percentage of moisture in steam given by calorimetric test.........%	1.02	0.86	0.54	0.49	0.82	0.49	0.43

TABLE No. 2.

DATA AND RESULTS IN FULL OF CALORIMETER TESTS.

Case No. 1.

Number for reference.	Date.	Which Boilers.	Gange Pressure.	Number of degrees inlet steam was superheated.	Number of degrees outlet steam was superheated.	Number of degrees wet steam was superheated.	Number of degrees lost by superheated steam due to radiation from calorimeter.	Number of degrees representing radiation from supply pipe.	Amount of moisture in the wet steam.	
									Expressed in degrees of superheat.	Expressed in percentage.*
1	Apr. 13.	Hawkins.	89.	99.	54.5	8.	8.	9.5	19.	1.02
2	" 14	Old Boilers.	89.	75.	37.	5.5	8.	9.5	16.	0.86
3	" 15.	"	86.	74.	37.	7.	10.5	9.5	10.	0.54
4	" 16.	"	86.	74.	39.	9.5	7.	9.5	9.	0.49
5	" 30.	Hawkins.	85.	72.	38.	10.5	8.	9.5	6.	0.32
6	May 4.	"	80.	77.5	41.5	9.5	8.	9.5	9.	0.49
7	" 5.	"	84.	68.	36.5	6.5	7.5	9.5	8.	0.43

NOTE.—The duration of each of these tests was about one hour.

CASE NO. 2.—TESTS AT THE COCHECO MILLS, DOVER, N. H.

Two kinds of boilers were also tried here; one, a set of two 82 H. P. Babcock and Wilcox boilers which have a collective area of heating surface amounting to 1,886 sq. ft., and one, a set of two 60-in. horizontal return tubular boilers having a collective heating surface of 2,100 sq. ft., each containing 80 3-inch tubes. The steam is delivered at the back ends of the B. and W. boilers, and at the front ends of the tubular boilers, in both cases through 4-inch nozzles.

The calorimeter was attached during the tests numbered from 8 to 12 to the under side of the 6-inch horizontal main pipe of the B. and W. boilers, and during test No. 15 to the same pipe nearer the boilers at a point where it runs in a *vertical* direction. The supply pipe in the first-named position was 6 inches long; in the second, 10 feet long. During tests 13 and 14, the calorimeter was attached to the vertical nozzle of one of the horizontal tubular boilers, and here the same form and length of supply pipe were used as in test No. 15.

The results of these tests are given in Tables Nos. 3 and 4. Table No. 5 gives the full record of the observations taken during test No. 10.

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* Obtained by dividing the preceding column by 18.6, the number of degrees corresponding to 1 per cent. of moisture.

TABLE NO. 4.—DATA AND RESULTS IN FULL OF CALORIMETRIC TESTS.

Case No. 2.

Number for reference.	Date.	Which Boiler.	Distance of calorimeter location from boiler measured along pipe.	Duration of Test.	Gauge Pressure.	Number of degrees inlet steam was superheated.	Number of degrees outlet steam was superheated.	Number of degrees wet steam was superheated.	Number of degrees lost by superheated steam due to radiation from calorimeter.	Number of degrees representing radiation from supply pipe.*	Amount of moisture in the wet steam. Expressed in degrees of superheat.	Expressed in percentage.
				Hrs.	Lbs.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Per cent.
8	June 16, A.M.	B. and W.	43 ft.	¾	70.4	67.4	33.9	6.8	7	10	9.7	0.52
9	June 16, P.M.	"	"	2½	70.0	72.1	38.1	7.9	7	10	9.1	0.49
10	June 17.	"	"	3	70.6	88.6	47.3	11.2	8	10	12.1	0.64
11	June 22.	"	"	2½	69.8	89.3	49.0	12.5	8	9	10.8	0.58
12	June 23.	"	"	3½	70.3	85.9	46.0	12.5	8	7	12.4	0.66
13	June 25, A.M.	Hor. Ret. Tub.	2 ft.	3½	59.0	120.5	49.8	7.9	10	14	38.8	2.06
14	June 25, P.M.	"	"	2½	58.1	130.3	55.0	7.4	10	14	43.9	2.32
15	June 30.	B. and W.	5 ft.	3½	71.2	82.2	41.3	10.7	8	14	8.2	0.44

* These quantities are obtained by measuring the condensation in the supply pipe due to radiation, with the calorimeter disconnected.

TABLE No. 3.

RESULTS OF TESTS ON THE BABCOCK AND WILCOX BOILERS.

Case No. 2.

Date, 1886.	June 16.	June 17.	June 23.	June 23.	June 30.
Equivalent evaporation per hour from 100° at 70 lbs. per sq. ft. of heating surface. .lbs.	2.55	3.67	3.17	3.25	3.16
Horse-power developed, A. S. M. E. rating. H. P.	156.9	230.5	199.6	204.4	198.5
Percentage of moisture in the steam as determined by the calorimeter................%	0.49	0.64	0.58	0.66	0.44

NOTE.—The rate of evaporation on the horizontal tubular boilers was not measured at the time of these tests. From data obtained at another time, it is judged that about 2.5 lbs. of water were evaporated per sq. ft. of heating surface per hour.

TABLE No. 5.—FULL RECORD OF OBSERVATIONS, TAKEN DURING TEST No. 10.

Case No. 2.

Time.	Gauge attached to wet steam pipe.	TEMPERATURES.			REMARKS.
		Super-heated steam entering jacket.	Super-heated steam leaving jacket.	Steam issuing from wet steam pipe.	
		Thermometer A.	Thermometer B.	Thermometer C.	
7.50	66.5	315.5	310.	313.	Wet steam blowing through jacket at 7.50.
8.15	69.	381.	344.	321.	
30	69.	392.	350.	320.5	Light the superheater lamp at 7.53.
37½	66.5	398.	352.	319.	
45	67.	412.	360.	326.	
52½	69.	404.	359.	327.	
9.00	72.5	411.	363.	327.	
07½	71.	403.	359.	325.	
15	71.5	411.	364.	329.	
24	69.5	410.	363.	325.5	
33	73.	406.	361.	324.	
40	73.	410.	362.	328.	
47	72.	409.	363.	328.	
53	69.	407.	362.	327.	
10.03	73.	401.	359.	326.	
15	69.	407.	361.	327.	
22½	70.	406.5	362.	328.5	
35	74.	399.	360.	329.5	
45	68.5	406.	362.	327.5	

TABLE No. 5—*continued.*

Time.	Gauge attached to wet steam pipe.	TEMPERATURES.			REMARKS.
		Super-heated steam entering jacket.	Super-heated steam leaving jacket.	Steam issuing from wet steam pipe.	
		Thermometer A.	Thermometer B.	Thermometer C.	
10.52½	70.5	409.	362.	328.	
11.00	67.5	409.	362.	327.	
.07½	72.0	401.	360.	327.	
.15	70.5	405.	359.5	326.	
.23	75.	405.	362.	330.	
.32	71.	406.	361.	328.	
.40	65.	409.	361.5	327.	Shut off superheater lamp at 11.41.
.51	73.5	320.5	316.	318.	
12.03	68.5	316.	312.	314.	
	70.6	406.6	361.3	327.2	Average of observations from 8.45—11.40 incl.
		318.0	314.0	316.0	Indication of thermometers in saturated steam of average pressure.
		88.6	47.3	11.2	Number of degrees of superheating.

Still another set of horizontal return tubular boilers was tried at the Trenton Electric Light Co.'s plant. These boilers are 5 feet in diameter and 15 feet long between heads. The steam space is divided horizontally into two compartments by a perforated diaphragm. The steam is supplied to two Westinghouse Automatic Engines 12-inch diameter, 11-inch stroke, making 338 revolutions per minute. The evaporation amounted to about four pounds of water per square foot of heating surface per hour. The results of the calorimeter tests showed that the steam contained 0.37 per cent. of moisture.

No comment is needed for the purposes in view, in regard to the nature of the various results obtained. The object is solely to show to the interested student of the subject how the new calorimeter operates in the cases of actual practice where it has been applied. It is hoped that enough ground is covered by the tests, to do this in a way which will be understood. In the course of many days' work which the writer has devoted to its use, he has had an opportunity critically to observe its performance. No defect has yet appeared. On the contrary, several advantages over the instrument commonly used have come prominently to notice The manipulation of the apparatus is exceedingly simple. The action is continuous, and nothing but the gas valve requires adjustment. The readings are confined to the three thermometers and the steam gauge, and the temperatures do not require to be taken with that accuracy which characterizes the employment of the familiar forms of water calorimeters.

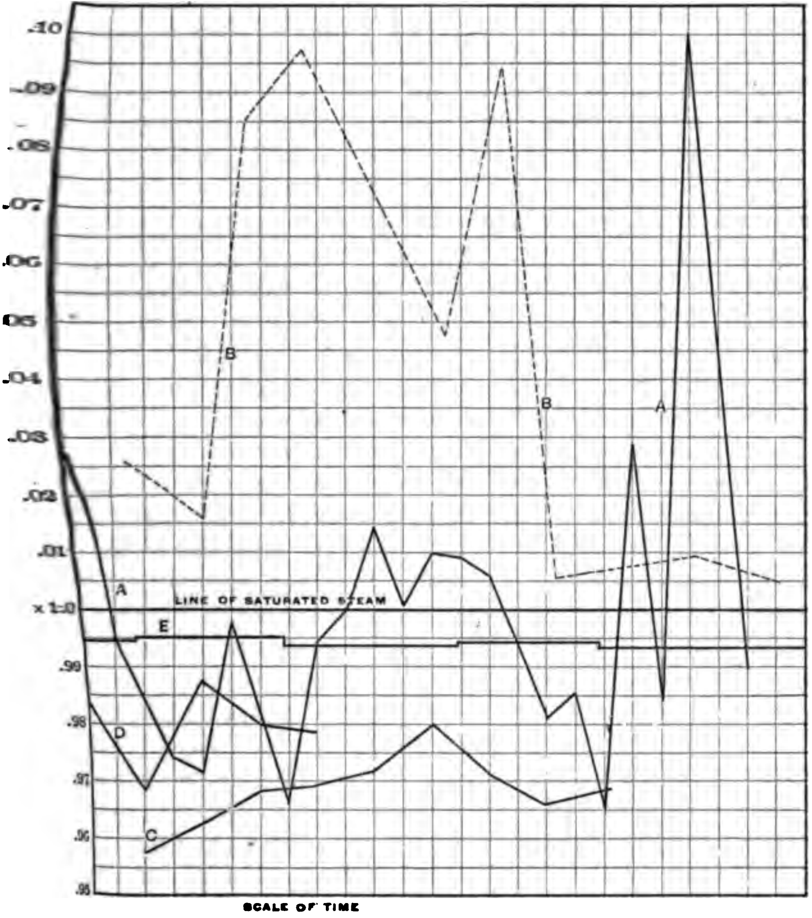
The objection which Mr. Babcock raised in the discussion on the former paper, that there might be a difference in the size of the orifices, does not appear to be a serious one, even if a considerable difference should exist. If any doubt arises as to the equality o the two currents, it can easily be removed by condensing the issuing steam in two tubs of water, and measuring the rate of discharge by actual test. Knowing the exact measure of the two currents, the effect of an inequality can be allowed for. In the case, for example, of test No. 13, a difference of five per cent would affect the result expressed in degrees of superheat, three degrees, and expressed in percentage of moisture, 0.16 per cent.

The writer is indebted to Mr. Hartshorne, Superintendent of the Arlington Mills, Mr. Holland, Agent of the Cocheco Mills, The Babcock and Wilcox Co., and Messrs. Westinghouse, Church, Kerr & Co., for whom the various tests were severally made, for permission to use the figures here given.

DISCUSSION.

Mr. G. H. Babcock.—I suppose in mechanics, as in agriculture and ethics, a tree is to be known by its fruit. It may be well therefore, to apply this test to the instrument which Mr. Barrus presented to us last year, and of which he has now given us data by which we may judge of its working as compared to other calorimeters. It may be difficult, perhaps, to establish an exact basis of comparison, as it is evident that in no case have we anything more definite by which to judge of the qualities of the steam than the indications of the instruments themselves, which may be correct and may not. Nevertheless, that instrument which will give us the most uniform results under any given circumstances is doubtless to be preferred. The calorimeters heretofore used have given very irregular results, and it has only been by the averaging of a large number of such tests that anything approximating accuracy could be attained. I have plotted a number of different calorimeter experiments made by different engineers

Fig. 87.



with different calorimeters, in order to illustrate this point, and as a means of judging whether the results obtained by the Barrus calorimeter are any more uniform than those by others. In the diagram, Fig. 87, the line *A* shows the results of a test of the Galloway boiler at the Centennial Exhibition. The horizontal scale is time, the vertical scale is quality of steam—one being saturated, more than one, superheated, while less than one denotes percentage of water. The tests were made every twenty minutes, and vary in their indications, very erratically, from 3½% moisture to 10% superheating. The average of all the experiments shows some superheating, although the Galloway boiler has no superheating surface, and it is not possible that any such condition of the steam could exist. These extremely variable results are the more remarkable because they were obtained from a boiler in which the quality of the steam could not vary to any appreciable extent, when run as it was at this test. The instruments used were a barrel calorimeter with ordinary platform scales and thermometers graduated to degrees. It is interesting to note the increased variation toward the close of the test, when the experimenters were somewhat fatigued.

Line *B* (dotted) is plotted from a series of tests made on a Babcock & Wilcox boiler by Mr. Chas. L. Clarke, a member of this society, with a barrel calorimeter, the scale and thermometers being very closely graduated, to avoid, as far as possible, irregular results. This also shows a small average superheating, although there was no superheating surface in the boiler.

Lines *C* and *D* are plotted from experiments made by our late member, J. C. Hoadley, with his calorimeter which is described in the Transactions of this society, Vol. VI., page 715. These results, it will be seen, are nearer uniform than those from the barrel calorimeter, and it is probable that they err in an opposite direction by showing too great a proportionate moisture. The heavier line *E* is plotted from the experiments given in Mr. Barrus' paper, just read, and shows a very close approximation to uniform results—perhaps as nearly so as it would be possible to attain with any apparatus for this purpose. This instrument giving average results for the time of the test, varying from three quarters of an hour to three and one-half hours, the line is drawn horizontal for the time of each test, and represents the average for that time. Judged by the standard of uniformity of results, therefore, the calorimeter of Mr. Barrus is a decided advance as an instrument of precision, but whether its records are above or below the actual facts in the case will depend very much upon circumstances, and is a difficult question to determine. The liability to error is, I think, much less than in other instruments for the same purpose.

CCXXXVII.

CASTING ALUMINIUM BRONZE AND OTHER STRONG METALS.

BY THOMAS D. WEST, CLEVELAND, O.

(Member of the Society.)

THE ability to cast in molds a material which possesses strength equal to the best forgings of wrought iron and steel is an achievement which mechanical engineers may well be interested in. While it is true that the strongest metals are susceptible of being cast, yet there are peculiarities about them which cause greater inconvenience and expense in the effort to procure good, solid, clean castings than occur with cast iron. These come mainly from the difficulties in overcoming the evils due to *oxidation, shrinkage, and contraction.* These elements, of course, exist in the founding of cast iron; but the degree to which they make labor and trouble is very slight as compared with stronger metals. The list of these latter is now enlarged by that comparatively new metal "aluminium bronze," and the experience of the writer is here to be cited in his endeavors to establish principles and methods for its successful molding and casting in the foundry of the firm manufacturing this bronze and other alloys of aluminium, at Lockport, N. Y. It is hoped, also, that ideas may be presented which may, in many cases, prove of value for the making of castings from other metals.

The difficulties which beset the casting of aluminium bronze are, in some respects, similar to those which were encountered in perfecting methods for casting steel. There is much small work which can be successfully cast by methods used in the ordinary molding of cast iron; but in peculiarly proportioned and in large bronze castings other means and extra display of skill and judgment will be generally required. In strong metals there appears to be a "red shortness," or degree of temperature, after it becomes solidified, at which it may be torn apart if it meets a very little resistance to its contraction, and the separation may be such as cannot be detected by the eye, but will be made known only when pressure is put upon the casting. To overcome this evil and to make allowances for sufficient freedom in contraction, much judgment wil often be required, and different modes must be adopted to sui varying conditions. One factor often met with is that of the incompressibility of cores or parts forming the interior portion of castings; while another is the resistance which flanges, etc. upon an exterior surface, oppose to freedom of contraction of the mass. An illustrative case, which the writer had to deal with in the foundry referred to above, was the casting of some blast-furnace tuyeres, the dimensions of which were about twelve inches diameter at the small end, and fourteen inches diameter at the large and seventeen inches long. These tuyeres were made hollow for the passage of water to keep them cool when the furnace was in blast. The core to form this hollow space was one which foundry men would term a "mean core" to make and to use, and the stronger it could be made by means of the mixture of the sand the easier would it be to make and to handle. But it was proved at the hydraulic testing of the first casting that these conditions were the worst for causing leakage. It was found that in order to procure a casting which would not leak, the core must be "rotten" and of a yielding character. This was obtained by using rosin in coarse sand and filling the core as full of cinders and large vent-holes as possible, and by not using any core-rods or irons. The rosin would cause the core, when heated, to become soft, and would make it very nearly as compressible as a "green sand" core, when the pressure of the contraction of the metal would come upon it. It might be well to state that the size of this core was such as would leave one inch thickness at each end, and one-quarter inch at the inner and outer sides of the casting, and was all surrounded with metal, excepting four one-inch round openings through which the core delivered its vent or gas. By means of dried rosin or green-sand cores we were able to meet almost any difficulties which might arise in ordinary work from the evils of contraction, so far as cores were concerned. For large cylinders or castings which might require large round cores which could be "swept," a hay-rope wound around a core barrel would often prove an excellent yielding backing, and allow freedom for contraction sufficient to insure no rents or invisible strain in the body of the casting. To provide means for freedom in the contraction of exterior portions of castings *which may be supposed to offer resistance* sufficient to cause an injury, different methods will have to be employed in almost every new form of such patterns. It may be that conditions will permit the mold to be of a sufficient yielding character, and again it may be necessary to dig away portions of the mold, or loosen bolts, etc., as soon as the liquid metal is thought to have solidified. In any metal there may be invisible rents or strains left in a casting through tension when cooling sufficient to make it fragile or to crack of its own accord, and it is an element which from its very deceptive nature should command the closest attention of all interested in the construction of castings.

Like *contraction*, the element of *shrinkage* is one often found seriously to impede the attaining of perfect castings from strong metals. In steel castings much labor has to be expended in providing risers sufficient to "feed solid" or prevent "draw-holes" from being formed, and in casting aluminium bronze a similar necessity is found. The only way to insure against the evils of shrinkage in this metal with work requiring "risers" or "feeding heads" *is to have the "risers" larger than the body or part of the casting which they are intended to "feed."* The "feeder" or "riser" being the largest body, it will, of course, remain fluid longer than the casting, and, as in cast iron, that part which solidifies first will draw from the nearest uppermost fluid body, and thus leave holes in the part which remains longest fluid. The above principle will be seen to be effective in obtaining the end sought. It is to be remembered that it is not practical to "churn" this bronze, as is done with cast iron. A long cast-iron roll, one foot in diameter, can, by means of a "feeder" five inches in diameter and a half-inch wrought-iron rod, be made perfectly sound for its full length. To cast such solid in bronze, the feeding head should be at least as large as the diameter of the roll, and in length about one-quarter longer than the length of roll desired. The extra length would contain the shrinkage hole, and when cut off, a solid casting would be left. This is a plan often practiced in the making of guns, etc., in cast iron, and is done partly to insure against the inability of many molders to "feed" solid, and to save that labor. There is nothing in the way to prevent making large guns of aluminium bronze as solid as those procured of cast iron. It is simply a question of having a good "riser" or "feeding head," and such guns would surpass in strength any other me used for their manufacture.

A method which the writer found to work well in assi avoid shrinkage in ordinary castings in aluminium bronz "gate" a mold so that it could be *filled or poured as q possible, and to have the metal as dull as it would flow, to a full run clean casting.* By this plan very disproportion ings were made without "feeders" on the heavier parts, a which "draw" or shrinkage holes would surely have a had the metal been poured "hot."

The plan or principle adopted in pouring this bronze i to that employed for casting steel, which, as is well kno sists in pouring through a spout controlled by a valve, w the metal flow from the bottom of a ladle instead of th lip, as practiced in pouring cast iron. The exact plan w writer used for castings weighing over fifty pounds was to pouring basin sufficiently large to contain all the metal n to fill the mold, and give any surplus which might be req a "flow off," or to fill up "feeding heads," etc., and so any metal from flowing into the mold until it was all in t ing basin. The entrance of the gate would be stopped b of an iron plug, and the moment all was ready, it would b and the metal would almost instantly fill the mold, would its gates be made. By such a plan it will of c readily seen that there was no danger of any *air*, "s oxide entering the mold through the "pouring ga element which would seriously mar the appearan casting. Besides the difficulties from difference in t which resembles gold, such oxide would be very liable a disunion of the particles of the body, or make an u able surface when the casting was finished up. Th itself is one that works well in our ordinary molding sa "peels" extra well. As a general thing, *disproportionat* weighing over one hundred pounds are best made in instead of "green" sand molds, as such will permit of work and a duller pouring of the metal, for in this meth is not that dampness which is given off from green sa and which is so liable to cause "cold shuts." When the of casting work will permit, many forms which *are tionate in thickness* can be well made in green sand b the surface of the molds and gates with silver lead or pl

Information upon manufacturing, strength and melting of aluminium bronze can be found in an article from the writer published elsewhere.*

From "blow holes," which are another characteristic element likely to exist in strong metals, it can be said that aluminium bronze is free. Should any exist, it is the fault of the molder or his mold, as the metal itself runs in *iron molds* as sound and close as gold. Sand molds, to procure good work, must be *well vented*, and if of "dry sand," thoroughly open sand mixture should be used and *well dried*. The sand for "green sand" work is best fine, similar to what will work well for brass castings. For "dry sand" work, the mixture should be as open in nature as possible, and for blacking the mold, use the same mixtures as are found to work well with cast iron.

The different problems of making *strong metal castings* all present peculiarities which call for special treatment in the manner of molding, etc., but, as said in the beginning of this paper, the difficulties to be overcome mainly hinge more or less upon one or other of the three elements, *oxidation*, *shrinkage*, or *contraction*. These are being controlled better every day, and progress being made by which almost any kind of casting will be procured as readily as of cast iron. With aluminium bronze, "Mitis," and steel castings to be had, the engineer should not want for strong metals to meet almost any conditions which he may desire.

DISCUSSION.

Mr. W. B. Le Van.—Speaking of molding, I would like to call the attention of the Society, especially of those members who visited the Watts Campbell Company yesterday, to that old cylinder which was cast in England, 1753. This casting made in 1753 is more perfect than the majority of those which we now make in our foundries to-day. It seems to me that engineers should blush to think that in one hundred and thirty years we have made but little improvement in the foundry business. I calipered the cylinder with a stick, and found very few imperfections in it at all, although it had never been bored. This cylinder had been in use as a steam engine for twenty odd years. It was made by Thomas Newcomen, iron-monger, Dartmouth, England.

Mr. T. R. Morgan, Sr.—I do not quite agree with Mr. Le Van.

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* In the *American Machinist*, Oct. 26, 1886.

I have seen some castings, just as he says, made some forty year ago, which were excellent; but to say that we have not made advances in the molding art, I believe is a mistake.

Castings of a much more complicated kind are made all ove the world to-day, to meet modern wants, than were required then to-day, castings of a complicated kind are made in one piece, whic would have been made in a number of pieces then, to simplify th molding; hence, the general run of molders of to-day have t be men superior to those of the past, although, no doubt, owin to the lack of facilities in the past, it was the fact that in exceptional cases there were molders who did turn out work the which would be highly creditable to the best of men to-day; bu such were exceptions, and not the rule.

There are now problems and practice for the molder of to-da which he has had to work out, which were not required in th past; and, in addition, they are now of a far more difficult character than then, when they had to make molds for cast iron onl I refer to the demand for molds for steel castings, steel shrinkin two to one of cast iron, and the molds for steel having to be mad dry and hard, while the molds for cast iron are made of gree sand. These new features and material present new problems t the molder in the making of the complicated castings which ar now in demand, and the future will tax his knowledge and judgment as never before. The higher temperature of molten steel, it quicker cooling and greater shrinking qualities in comparison wit cast iron, demand that the successful molder of to-day shoul be a better man than the molder of the past, to do similar wor in molds for cast iron. There are many complicated casting that are easily made in cast iron, which would be very difficul to make of steel, but will be successfully met, I have no doub as facilities and demands increase for such. To do justice t molds for steel castings, owing to their quick cooling and grea shrinking qualities, it is necessary that the molds in certai places should give way in the proper time, under the proper pressure, to prevent undue straining of the casting while in the molte condition.

It must be evident that to meet such demands successfully eac complicated steel casting must become a problem calling for th best reasoning and calculating faculties, and much more so tha a similar cast iron casting, whose cooling and shrinking qualitie are much less than those of steel; hence the molder of to-da requires additional care and knowledge to succeed in making steel castings of the same kind as those of cast iron.

Mr. Oberlin Smith.—I would like to ask Mr. West to state his experience, in making these castings, as to the question of blow-holes—what methods he took to avoid blow-holes—how numerous they were in spite of his best efforts, if there were any.

Mr. W. H. Weightman.—I should like to ask Mr. West the comparative cost of this aluminium bronze. It is a matter of interest to me, and I would like to know.

Mr. J. M. Dodge.—I would like to speak in a general way about the subject of molding and casting. The physical characteristics of the metals now in practical use are not thoroughly understood. I find, when I go to inquire of parties about the use of metals, that a tensile strength, for instance, of 100,000 pounds may mean that you cannot bend the casting at all; and malleable iron, which I have had a good deal of experience with, is better than wrought iron in this one regard, that before it bends, it will exceed the limit of wrought iron, and after it bends to a great extent, it is useless, and it might just as well break. There is no machinery I know of (not excepting the American agricultural machinery, which is undoubtedly the finest machinery in the world, considering that it goes into the hands of mechanically ignorant men and does wonderful things) in which the bending of a piece is not practically its destruction. The principal question I would like to ask about this bronze is, How strong is it and how much can you bend it? How much good is it practically, not on a testing machine, but will it stand 80,000 pounds and bend at 30 degrees or more? A three years' test of malleable iron showed it to average better than 44,000 pounds to the square inch, tensile strength. Furthermore, we had tests running for three years—we made six tests from three furnaces each day—and on only two occasions did we ever find a blow-hole in our test pieces. Furthermore, during all that time, we never found it was necessary to destroy a piece to tell whether it was good or not. Malleable cast iron, well made, has characteristics which merit the careful attention of the engineer. It is looked upon as a substitute for wrought iron, but not quite as good. It is practically better than merchant bar iron.

Mr. Oberlin Smith.—If Mr. Dodge means to say that the malleable iron which we get in the market is as good as wrought iron in general, I do not agree with him. Perhaps he does not mean to state it as strongly as that. I have found that steel castings are better than malleable iron. They cost about the same, and take about as long to get. [Laughter and applause.] The great trouble with both of them (and I don't know but that we should use a stronger adjective than great) is the outrageously long time you have to wait. You take an order for doing a thing in a hurry, and want your castings. The makers will say that they take three or four days to anneal. You say to them, "Why can't you mold this thing to-day or to-morrow, and have it annealed, and give it to me in a week or ten days? Finally they acknowledge that they can do it in two weeks, but keep me waiting four. It is all an abominable nuisance, and we want a substitute for these castings which can be made as quickly as forgings.

There is another new material mentioned near the close of Mr. West's paper in which I feel considerable interest. I refer to "Mitis" castings. This looks to me like the coming metal. The accounts of it seemed so favorable that I went to Worcester to investigate it some months ago. I saw them make the molds, start the fire, bring it up to melting point, and melt and pour the metal; and they weren't long about it, either. Then I took some pieces into the blacksmith shop and forged them myself. It is, as near as I can judge, pure wrought iron of the best quality—that is, when you make it so. I felt so much pleased with it that I ordered a lot of rings for a certain purpose, to take the place of forgings, knowing they would come much cheaper than forgings on account of their shape. I was very much disappointed to find numerous blow-holes in these castings. I made an allowance for the new state of the art, and, accepting the fact that it was but in an experimental stage, did not lose my faith. I am assured now that the measures being taken for improving this process will get rid of the blow-hole trouble to a great degree. I would like to ask Mr. West to tell us what experience he has had with this metal, and what he thinks are the special causes for so many blow-holes in it. We would naturally expect fewer holes than in other metals made from iron, because it is so very fluid when it is poured. And will he tell us whether he sees any practical difficulty in reducing the evil to almost nothing by proper methods of molding, etc. If that trouble can be gotten rid of, I am inclined to believe it will be the coming metal, to take the place of wrought-iron forgings.

Mr. Dodge.—About malleable iron being stronger than wrought iron, I would say that our English manufacturer manufactures chain for England and the Continent. He takes our patterns, and duplicates them in wrought iron. In every case of testing, the malleable iron began to straighten the wrought iron out. Englishmen will pay for wrought iron drive chain, and Americans won't. If a chain here stretches, it is thrown away. If it stretches in England, it is sent back to be reset. Take malleable and wrought iron of equal size exactly, and the malleable iron will stretch the wrought iron a little before yielding itself.

Mr. Smith.—Can you reset wrought iron?

Mr. Dodge.—Yes, if it is sent to us. Here they let it wear right out, and throw it away. Both cast and wrought iron can be reset, in spite of the fact that prices have been cut so that some concerns slight their work; but two or three concerns in this country make malleable iron of which they need not be ashamed.

Mr. Smith.—I understood you to say that malleable iron is stronger than wrought iron. How is that—it can be stretched and still be good, and then set back to its original pitch?

Mr. Dodge.—Well, it will do it. I never was inside of it to see how. [Laughter.]

Mr. C. C. Hill.—On the subject of shrinkage in molds when used for these strong metals, there is one thing which I have not seen mentioned. That is, to put into a mold certain pieces of solid matter (metal, for instance) which can be withdrawn about the time when the casting is becoming solid, so that the shrinking can take place freely, and crush the mold. I have employed something nearly like that in using complicated chills. I make the chills in several parts, and then put a stratum of sand between the parts, sufficiently porous to be crushed by the casting. I have made very nice articles of chilled work in this way.

Mr. West.—In answer to the gentleman who spoke with regard to that old cylinder which was so true, there is a point there which it might be well to speak upon a little further. It is a fact which is not appreciated, that to get a *true, large* casting of anything cylindrical, it is best to be "swept"—the plan, no doubt, by which the above cylinder was molded. In casting large fly-wheels, pulleys, etc., foundries have generally to go to work and make "full" wooden patterns; and when they are molded, and the castings are tried, they are found to be in all sorts of shapes. The blame is put on the molder, and in such cases he is generally unjustly censured. There is no process which can be employed for obtaining *true molded* cylindrica castings better than that of "sweeping;" for if a moment's though was given to realize how easily wood can become distorted fro the effects of dampness and heat, it would readily be seen why i is impracticable to expect *true molded* castings for such wor as *large* fly-wheels, pulleys, etc., from "full" wooden patterns.

Most of the members here will recollect that at the Chicag meeting last spring we saw eight blowing engines, which stood i a row at the South Chicago blast-furnaces. You might stand o the end, and as you sighted along the line of those fly-wheels, yo would say that they were turned, for they run so true that yo cannot see any perceptible "out" on any of them. Now, thos wheels were made in the foundry of which I am the foreman, an we made them by "sweeping" the rim, using a pattern for forming the arms and hub only. On account of the wheels bein about sixteen feet in diameter, and weighing over fifteen ton each, they were cast in halves, for convenience in shipping. Th centers of them are merely planed off so as to make them com together true at the joint, and when they are put together, the run as true as those observed in Chicago.

With reference to the cylinder which Mr. Le Van mentioned, do not see anything remarkable in that. It merely involves th principle of sweeping to the circle.

I don't know that I could touch on the other question of blow holes in a better way than by relating my experience with th Cowles Company in casting aluminium bronze. Several attempt were made at a cylinder casting of this metal by different foundr men, and in all cases the results were condemned on account o the blow-holes in the castings. In fact, the firm was commencin to believe that their metal could not be got solid in large casting They came to the Cuyahoga Works, and asked that the castin might be tried in the shop of which I am foreman. We did no have facilities there for melting the metal, but we made a mol and carted it over a stone pavement for about a quarter o a mile to a brass shop where we could melt the metal. I was poured, and the casting turned up as sound as need be.* The cylinder measured 24″ long, 14″ diameter on the inside, an 16″ diameter on the outside. All that I had to guide me in m

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* This casting was a duplicate of the one exhibited at the meeting of the Sc ciety, which was one molded by my own hands in the foundry of the Cowle Electric Smelting and Aluminium Co., Lockport, N. Y.

first attempt to cast this metal was the position of "blow-holes" in castings shown to me, which had been previously made, and my own confidence that the metal could be poured into an iron mold, and become solid. When I saw that, then I felt sure that the trouble was not in the metal itself, but was in some way connected with the mold or the man that made it, and all there is required to obtain sound castings is what I have stated in my paper—condensed, it was simply to get the mold made porous and firm, and to fill it as quick as possible and to pour through what we would call a "secondary basin or pot." This will be an item which may escape the attention of a great many. When you pour a stream of metal into a gate which is smaller than the hole, its friction must necessarily carry through some air with it. That air goes down and mingles more or less with the metal in the mold, and if the metal does not stay hot long enough to allow the air to escape, it imprisons the air there, and then gives us holes. This theory for the cause of some "blow-holes" in castings, I have never heard or seen advanced, and it will no doubt meet with some opposition, but nevertheless I believe such action can be proved to be the cause of many "blow-holes" in castings, especially those of strong metal.

In making these castings, I adopted the plan of making all connections with the pouring basin air-tight, and using the plug principle in filling the basin with metal, and letting it stay there long enough so that all dirt which might be there would float to the top, and as soon as I was assured of this fact, and I saw that the metal was about of the right temperature, then the plug would be pulled, and the metal would go down, and instantly fill the mold. Such work as that is, of course, pretty rough on a mold; but with that mode of working and manner of molding described in the paper, I got over the difficulties of blow-holes in this metal, and I think that the difficulties which Mr. Smith mentioned with "Mitis" metal, could partially, if not wholly, be overcome by the adoption of the principles set forth by my paper.

With reference to Mr. Morgan's remarks on steel, all I can say is, that I do not know that I can suggest anything in that line other than the ideas which the paper and this discussion will present. The main idea to which I would call attention is that of providing *air-tight pouring gates*, so as to prevent the flowing metal from carrying air into the mold to mingle with the metal. I never had any experience directly with any other of the strong metals. My experience with this has been very pleasant, and led me to believe that there was this one element in all strong metals alike—the tendency to blow-holes.

Bronze is made in different grades. The more aluminium which is alloyed with the copper up to eleven per cent., the stronge the metal becomes. Over eleven per cent. it becomes brittle; and it is said to become more brittle up to twenty-five per cent., and after that to increase again in strength. That I have never seen tested, but it is said to be the case. One of the samples here exhibited is marked 49.30, which means that it stood a tensile strain of 49,000 pounds to the square inch with an elongation of thirty per cent. in one inch. The strongest metal that I ever saw produced there had a tensile strength of 126,000 pounds per square inch.

Mr. A. H. Emery—Was it forged in any way, or is it simply a it was cast?

Mr. West.—Just as it was cast—taken from the mold. In making these bars, they thought to save labor by casting in an iron mold; but those little projections where the section is reduced would show weakness when it was put in the machine so they finally learned that it had to be made in sand—something of a yielding character, which would allow the metal to give. Another sample, marked 100,000 pounds, has about ten and a half per cent. of aluminium. Here is a piece of the metal as i comes from the electrical furnace. You will see that it change its color when being re-melted. That piece has probably in i about sixteen per cent. of aluminium, and is what they would call very rich metal. It is then standardized; which means that it i put in an ordinary crucible such as they use for melting brass, and by re-melting it is brought down to any standard which they want

Mr. Oberlin Smith.—Will Mr. West tell us whether that meta can be soldered or welded?

Mr. West.—I have had no experience in that matter, and cannot say.

With regard to this 100,000 pound metal, if they obtain fou per cent. of elongation in its test, it is remarkable. Metal a strong as that is, as a general thing, has but from one to two per cent. of elongation—it contains but two and one-half per cent of aluminium. They say that a screw made of aluminium is equa to the best steel screw made, and will drive into the hardest kind of wood without distorting.

Some gentleman has asked me how about aluminium bronze for bearings. All I can say about that is that I do not know, having had no experience with the metal for this purpose, but I think that, if the conditions were carefully studied, some one grade of the metal could be selected which would be found to give excellent satisfaction. There is quite a series to select from.

Mr. Morgan.—At what price can aluminium bronze be made?

Mr. West.—This metal with the more aluminium is, of course, dearer metal. Their special metal, having a tensile strength of from 100,000 to 110,000 pounds, will probably sell for about fifty cents a pound in the casting. What they call their C metal, that is, metal that will stand a tensile strength of about 60,000 pounds, will sell for about thirty-five cents. It goes down to metal which has pretty much the color of copper itself. This sample is marked 28,000, nineteen elongation, and has a percentage of two and a half aluminium.

CCXXXVIII.

INTRINSIC VALUE OF SPECIAL TOOLS.

BY OBERLIN SMITH, BRIDGETON, N. J.
(Member of the Society.)

In a paper read before this Society last May, entitled "Inventory Valuation of Machinery Plant,"* the writer referred to "special tools" as the most difficult things things to deal with in all inventory-taking. This is on account of their peculiar liability to become obsolete, or partly so, over and above all other kinds of manufacturing property. Assuming that we are here dealing with such tools as are used in the various branches of manufacture with which the mechanical engineer comes in contact, we may briefly define the term "special tool" as a tool which is individually adapted for performing some particular operation so well that it is unfit for general work—in other words, it is a specialist. Used in its general sense, the term in question covèrs both special machines and special tools, if such a distinction can be made. In the former category are many machines which can be profitably modified, in case the commercial demand ceases for the goods upon which they are specially made to operate, so as to use them for other purposes. Among such machines are special lathes, drill presses, milling machines, punching presses, etc., which retain enough of the general character of the type of machine after which they are named, to enable them to be altered over for other kinds of work, either general or special. An example of this has been seen in the case of gun-factories adapting such plant to the production of sewing-machines or type writers, at comparatively small expense, when the demand for fire-arms lessened.

Among special tools proper, are usually counted jigs, templets, and gauges. To this list should be added working drawings and foundry patterns, as explained in the paper referred to. These drawings and patterns, though used in every machine-shop, are essentially of exactly the same nature as are the jigs, etc., used in

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* Trans. A. S. M. E., Vol. VII., p. 433.

the large manufactories, where work is so cheaply made on the "duplicate system." The general principles for the valuation of such tools are clearly laid down by the writer in the former paper, and are to the effect that each article should be rated at what it would cost to reproduce it to-day, less a depreciation for its wear-and-tear, and another depreciation for its percentage of obsoleteness. As stated, the wear-and-tear does not amount to much on this class of tools, because they are not ornamented for marketable purposes (so that they grow shabby with age); and because they have to be kept up to their original standard of working condition in order that they may do their work properly. The only question, therefore, necessary to consider in this paper, is their *amount* of obsoleteness; and this is a very important consideration to the manufacturer having a large amount of money invested in such plant, when he is considering its intrinsic value.

Concerning this subject, there are two principal schools of belief, with a practice, in individual cases, ranging all the way between the extreme views held on either side. The first of these views is the one expressed in the former paper, namely, that a special tool should not be reckoned as partly obsolete while in the full career of its usefulness, but that very careful watch should be kept for any indications of a lessening demand for the merchandise which the tool is made to produce. The moment such indications appear, depreciations should commence and should be made rapid enough to be surely on the safe side, and to bring the value of the tool down to nothing but what it is worth for old material, before said marketable demand has entirely ceased. Of course, good judgment is as necessary in this matter as in managing all other kinds of property. A catastrophe, causing a demand for goods wholly and suddenly to cease, although possible, is not likely to happen. If such does happen; for instance, by earthquakes occurring all over a country hitherto unaffected by seismic disturbances, and suddenly annihilating the demand for some article which the tool-owner manufactured for non-earthquake-proof buildings, but which is useless for the modified buildings replacing them; or when some original and brilliant invention makes a new article suddenly popular, and entirely throws out all its predecessors; or when some fashion in dress is rapidly changed by the verdict of a foreign fashion-monger, as was shown in the decadence of hoop-skirts some years ago. It is very rarely however, that such occurrences amount to catastrophes. In such instances as the last two named, the change is not so sudden as to prevent a shrewd manufacturer from foreseeing the prospective state of things, and ceasing his production gradually. Of course, if his whole capital is invested in tools for one particular article, catastrophes or semi-catastrophes may occur regarding it, just as in the case of the merchant who has put all his property into the cargo of one wrecked ship, or into a mine where a promising vein of ore stops at a sudden fault. Such risks must be taken in all businesses, and can only be guarded against by a proper system of insurance. The wise course in this, as in the other cases mentioned, is to divide up one's investments in such a way as to run the risk of loss in only one direction at a time.

The opposite view regarding valuation of special tools, and one which is held by many shrewd and careful manufacturers, is that they should be depreciated by a large percentage every year, until their value stands *nil* upon the inventory. One popular belief is that a special tool hasn't any value at all until it has paid for itself in extra profits. Carrying this latter principle to a logical conclusion, it should be rated first at nothing, and gradually *appreciated* up to full price. All this is, of course, working upon the safe side, as regards the danger of over-valuation, but it is difficult to see how a corporation, for instance, could account to its stockholders for the money which they paid in, if all the plant happened to be of this special nature. An inventory upon such a basis would also be rather an awkward document to show to would-be purchasers of the business who were to be assured of its prosperity.

An esteemed friend of mine, who has been a very successful manufacturer of cotton machinery, is in the habit of depreciating many of his special tools to almost nothing upon his books, fearing they may pass out of use and cannot be made available for other purposes. I recently called his attention to the logical discrepancies in such a method, putting the case something like this: "Regarding valuation of special tools, I rate them at the bare cost of reproduction, and this only when they are in frequent and regular use. The moment such frequency lessens, or there is any doubt about their continued usefulness, they are heavily depreciated. Thus, at any time, all the plant of a shop is worth fully its recorded value, to any purchaser who might buy out and run the business, providing the business is successful. Of course, if it is unsuccessful, and is to be sold at auction, it, or any other kind of business, could not realize cash for its assets to an amount anywhere approaching their valuation. If your principle were carried out, a mile of rail —

way embankment, on a prosperous and paying route, would not be worth as much as the real estate upon which it stood, because somebody might want to buy it for a canal, and the grading would all have to be turned inside out, so to speak. A successful mine would not be worth nearly its cost, because it might be sold to be used for farming land; and any machine-shop might attach but little value to any of its plant, except perhaps the buildings, boiler and engine, because somebody might want to buy it for a laundry. According to your system, would it not be the case, that a new manufacturing company, having just invested a large part of its capital in the most approved plant for its purposes, would be obliged to say to its stockholders: 'Gentlemen, our assets last week when you paid in your money, were $100,000, but they are now only $50,000, as much of this plant could not be sold for other purposes than those intended by its makers, for nearly the sum which we yesterday paid.'"

The most pertinent point in his reply was, that mining property is always considered as incurring extra risk, and is expected to pay a much higher profit (say four to one) than other property, in order that said risk may be covered. This is true, but if the same principle is applied to special tools, and their intrinsic value is considered to be only, say, one-fourth of what it would cost to reproduce them, then occurs the insurmountable difficulty above referred to: How shall the managers of a property account to its owners for the full amount of the funds placed in their hands, the day or week after having purchased their tools, if a considerable share of such purchase has been depreciated 75 per cent. when first entered in the books? If the plan is adopted of entering them at full price in the beginning, and then depreciating them very rapidly, although in as full, or fuller, use than at first, what becomes of logic and common sense? If a tool is in perfect order five years after it is put into operation, and is used even more frequently than it was at first, in making goods the demand for which has increased, without an apparent probability of a decline, why is that tool not worth as much to its owners as it was at the start?

In practical dealings with this question, it seems to me that the best course is to give all special tools an inventory rating at their apparent value, according to the principles enunciated in the former paper and the first part of this, and then to lay aside a portion of the extra profits which these tools have earned by their special usefulness, in the general reserve fund or "surplus" of the concern.

They may thus be drawn upon, should any too sudden collapse in values takes place. This surplus-keeping is a safe course (just as it is in the general conducting of the business in reference to strictly commercial risks), and a wiser one than to pay out all the profits which these tools earn to the partners or stockholders—thus leaving them in a disappointed state of mind, should a shadow of obsoleteness gather rather suddenly and darkly over the special property in which their money is invested. Just how far to carry surplus-keeping in general, as a matter of self-insurance against all kinds of losses, is a question for individual judgment, the discussion of which is scarcely germane to the purpose of this paper.

DISCUSSION.

Mr. Henry R. Towne.—The subject of this paper is one which interests nearly every one who is engaged in the conduct of producing works, and can surely be profitably discussed by a number who are present.

It is a subject to which I have given a good deal of thought. The method contemplated by Mr. Smith of a catalogue inventory, and an appraisement of each special tool, involves a great deal of labor in a business using many such tools, and almost any business largely consisting of the production of specialties involves so many special tools, many of them quite small and of proportionately small value, as to make it really a very great labor to go over them one by one annually and say what they are worth. I tried that plan myself for a number of years, and finally discarded it as involving entirely too much cost in proportion to the benefit received. The discarding of that method raises the question as to how else it can be done. With some special classes of tools it can be done, approximately and with fair accuracy, by weighing them—taking the tools by weight. In that case, of course, the tools must be graded somewhat according to their kind, that is, according to the original amount of labor expended on the material to convert it into the tools; but a few classes will usually cover all of this kind of tools which can be treated in that way. But where the number or variety of tools is too great to admit of that treatment, I think that the best result with the least expenditure can be reached by simply keeping a ledger account with each class of tool, and at the end of a year considering how much shall be abated from that account for deterioration. For special tools, strictly such, which are applicable to nothing else, for instance, jigs, milling fixtures, special cutting tools, which are applicable only to a particular product and not to general use, in my judgment, twenty per cent. per annum is not too large a rate of deterioration. That does not mean, of course, that the entire value is sunk in five years, but it means that at the end of five years you have got the valuation down to a pretty low and a proportionally safe basis. I think, however, there is something to be said against this plan or any of the plans considered so far. There is a tendency by the adoption of any of them to deceive one's self as to the real value of your property by classing as assets things which would be of almost no commercial value in the event of a sale, and whose future value is quite uncertain; and it has occurred to me lately that perhaps the better method would be to treat the cost of all such special tools primarily as an expense, keeping it under a separate expense account or table during the year, but letting it appear in the monthly statements of the cost of operating the department for which the tools are made, and then at the end of the year ascertaining from your books how much the total charges to this account have been, and consider then what proportion of that total shall be transferred to a betterment account and credited to the expense account. This plan has the conservative value of calling all of these items expenses until they have been put into use and tried, and their value somewhat known, and then considering how much of their cost you will take out of the expense account and put into the betterment account. The question is really one of great consequence to most manufacturers who are producing a specialty, and I trust that some of the other members present will favor us with their views upon it.

Mr. Oberlin Smith.—I would say to Mr. Towne that it has not been my habit to keep the valuation of these tools in the inventory very much in detail, as it does involve too much work; but in getting up a lot of tools, for instance, a set of jigs and gauges, for the production of some particular machine, I find approximately what they cost at first. The account is very easily kept by having a printed blank on the corner of the drawing from which the tools are made, to be filled in with the cost when it is returned to the office. Thus I know in the first place what that set of tools cost. They are then depreciated a proper amount and treated as one item in the inventory; that is, one set of tools, for machine so and so, is worth so much.

I would like to ask Mr. Towne regarding the case supposed in this paper, where a new corporation is started, the cash paid in by the stockholders, and the need of these special tools having been foreseen, they have been ordered and can be procured in a week, or a month, by making or buying them. If his idea of charging them to the expense account is carried out, what becomes of the capital of the concern? How do the managers account to the stockholders for the property they supposed they owned, if the books show it is *not* property?

The Chairman.—Special cases require special treatment. I think that a business having so large a proportion of its capital invested in special tools would reasonably require that a wholesale abatement from first cost should be made, and even then I would prefer making a liberal abatement to the end of the first year—at least ten, possibly twenty per cent. on the value or cost of the special tools.

Mr. John J. Grant.—I should like to know why a tool which is just as good (for the purpose for which it is designed) at the end of ten years' use, as it was the first year, should be depreciated twenty per cent, or even ten per cent. We are going through a period of inventorying special tools in our establishment, and we are bothered about inventorying those tools. We find jigs and fixtures for sewing-machine work, exactly as good for our present work as on the day they were made, yet they were made fifteen years ago, and if they have depreciated twenty per cent. a year, they would have no valuation, and the stockholders I think would complain.

Mr. T. J. Borden.—The valuation of tools, whether special or general tools, should, I think, be determined by charging off each year a fixed rate of depreciation, the percentage being computed each year on the value at which they stood on the books at the beginning of such year, or on their cost if put in during the year.

The rate of depreciation must necessarily be much greater for special tools than for general, and although it is difficult to determine the exact rate to be fixed on each tool, yet the number of special tools in most establishments is large enough to warrant treating them as a class, and the exercise of one's best judgment on the rate of their depreciation as a class will usually reach more correct results than can be attained by treating each tool separately.

I submit herewith tables showing, at the several rates of depreciation named therein:

1. The value at the end of each year for thirty years for each $1,000 of original cost.

2. The reduction in value each year for each $1,000 of original cost.

It will be noticed that the original cost is not exhausted as rapidly as might, at first thought, be expected.

A heavy line is drawn across Table No. 2 to indicate the points in each column where the amount of reduction at the lower rate exceeds that at the higher rate for the corresponding year. This feature is apparent in all of Table No. 2 below the heavy line.

Value at the end of each year for each $1,000.00 original cost.

Rate of Depreciation.	5 per cent.	6 per cent.	8 per cent.	10 per cent.	12 per cent.	15 per cent.	20 per cent.
Original Cost.	$1000.00	$1000.00	$1000.00	$1000.00	$1000.00	$1000.00	$1000.00
End of 1st year	950.00	940.00	920.00	900.00	880.00	850.00	800.00
" 2d "	902.50	883.60	846.40	810 00	774.40	722.50	640.00
" 3d "	857.38	830.58	778.69	729.00	681.47	614.13	512.00
" 4th "	814.51	780.75	716.39	656.10	599.70	522.01	409.60
" 5th "	773.78	733.90	659.08	590.49	527.73	443.71	327.68
" 6th "	735.09	689.87	606.36	531.44	464.40	377.15	262.14
" 7th "	698.34	648.48	557.85	478.30	408.68	320.58	209.72
" 8th "	663.42	609.57	513.22	430.47	359.64	272.49	167.77
" 9th "	630.25	573.00	472.16	387.42	316.48	231.62	134.22
" 10th "	598.74	538.62	434.39	348.68	278.50	196.87	107.37
" 11th "	568 80	506.30	399.64	313.81	245.08	167.34	85.90
" 12th "	540.36	475.92	367.68	282.43	215.67	142.24	68.72
" 13th "	513.34	447.37	338.25	254.19	189.79	120.91	54.98
" 14th "	487.68	420.52	311.19	228.77	167.02	102.77	43.98
" 15th "	463.29	395.29	286.30	205.89	146.97	87.35	35.18
" 16th "	440.13	371.57	263.39	185.80	129.34	74.25	28.15
" 17th "	418.12	349.28	242.32	166.77	113.82	63.11	22.52
" 18th "	397.21	328.32	222.94	150.10	100.16	53.65	18.01
" 19th "	377.35	308.62	205.10	135.09	88.14	45.60	14.41
" 20th "	358.49	290.11	188.69	121.58	77.56	38.76	11.53
" 21st "	340.56	272.70	173.60	109.42	68.26	32.95	9.22
" 22d "	323.53	256.34	159.71	98.48	60.06	28.00	7.38
" 23d "	307.86	240.96	146.93	88.63	52.86	23.80	5.90
" 24th "	291.99	226.50	135.18	79.77	46.51	20.23	4.72
" 25th "	277.39	212.91	124.36	71.79	40.93	17.20	3.78
" 26th "	263.52	200.14	114.42	64.61	36.02	14.62	3.02
" 27th "	250.34	188.13	105.26	58.15	31.70	12.43	2.42
" 28th "	237.83	176.84	96.84	52.33	27.89	10.56	1.93
" 29th "	225.94	166.28	89.09	47.10	24.55	8.98	1.55
" 30th "	214.64	156.26	81.97	42.39	21.60	7.63	1.24

Amount of reduction in value each year for each $1,000.00 *original cost.*

Rate of Depreciation.	5 per cent.	6 per cent.	8 per cent.	10 per cent.	12 per cent.	15 per cent.	20 per cent.
1st year.....	$50.00	$60.00	$80.00	$100.00	$120.00	$150.00	$200.00
2d “	47.50	56.40	73.60	90.00	105.60	127.50	160.00
3d “	45.12	53.02	67.71	81.00	92.93	108.37	128.00
4th “	42.87	49.83	62.30	72.90	81.77	92.12	102.40
5th “	40.73	46.85	57.31	65.61	71.97	78.30	81.92
6th “	38.69	44.03	52.72	59.05	63.33	66.56	65.54
7th “	36.75	41.39	48.51	53.14	55.72	56.57	52.42
8th “	34.92	38.91	44.63	47.83	49.04	48.09	41.95
9th “	33.17	36.57	41.06	43.05	43.16	40.87	33.55
10th “	31.51	34.38	37.77	38.74	37.98	34.75	26.85
11th “	29.94	32.32	34.75	34.87	33.42	29.53	21.47
12th “	28.44	30.38	31.96	31.38	29.41	25.10	17.18
13th “	27.02	28.55	29.43	28.24	25.88	21.33	13.74
14th “	25.66	26.85	27.06	25.42	22.77	18.14	11.00
15th “	24.39	25.23	24.89	22.88	20.05	15.42	8.80
16th “	23.16	23.72	22.91	20.59	17.63	13.10	7.03
17th “	22.01	22.29	21.07	18.53	15.52	11.14	5.63
18th “	20.91	20.96	19.38	16.67	13.66	9.46	4.51
19th “	19.86	19.70	17.84	15.01	12.02	8.05	3.60
20th “	18.86	18.51	16.41	13.51	10.58	6.84	2.88
21st “	17.93	17.41	15.09	12.16	9.30	5.81	2.31
22d “	17.03	16.36	13.89	10.94	8.20	4.95	1.84
23d “	16.17	15.38	12.78	9.85	7.20	4.20	1.48
24th “	15.37	14.46	11.75	8.86	6.35	3.57	1.18
25th “	14.60	13.59	10.82	7.98	5.58	3.03	.94
26th “ . . .	13.87	12.77	9.94	7.18	4.91	2.58	.76
27th “	13.18	12.01	9.16	6.46	4.32	2.19	.60
28th “	12.51	11.29	8.42	5.82	3.81	1.87	.49
29th “	11.89	10.61	7.75	5.23	3.34	1.58	.38
30th “	11.30	9.97	7.12	4.71	2.95	1.35	.31

Mr. Oberlin Smith.—To answer partly what has been said, I would say that in my inventory of special tools they are very much depreciated; but a good part of this is the difference between what they did cost and what it would cost to reproduce them. I would not usually attempt to mark anything of this kind at what it did cost, because one can always build a duplicate considerably cheaper. But it does seem to me, as Mr. Grant says, that if a thing has been entered in the inventory at a lower price than it costs to reproduce it, the price should be marked up to about such cost of reproduction, being very careful to depreciate for any possible obsoleteness, and of course depreciating for wear and tear. This latter does not occur so much with special as with ordinary tools, because they have to be kept up to do their work in the very best manner. I should like to hear the views of members in regard to inventorying patterns. Of course, jigs are not as familiar to everybody as ordinary foundry patterns, but every one who has had anything to do with machine-shops has dealt with the latter. Probably many of you who are machinists have systems of valuing patterns. They are just as much special tools as are jigs, templets, or gauges. They are, if obsolete, worth nothing except for kindling wood, but if in full use, they are of great value in conducting a machine-shop and foundry, if we judge value of articles positively needed by cost of reproduction. I think it would be of interest to us all if we could hear the experience of the different members as to how they value their foundry patterns.

The Chairman.—It will contribute very much to the value of this discussion if any of the members will state what their practice has been, and the practice of any establishment they know of—we would be glad to know the simple fact of what really has been adopted by them in the valuing of special tools.

Mr. T. R. Almond.—I would say that I have considered many of the special tools which I have in use for my purposes as being really of more value after the first year's use than they were during the first year. I find that by careful treatment the tools improve in value. Of course, this is in relation to what they are for. If they are for very fine work, where the half-thousandth of an inch or a quarter of a thousandth of an inch comes in, then the tools, many of them, will undoubtedly increase in value. They have to be, as we say in the shop, tinkered with or humored until we get them into a correct condition for the purpose. If the tools are destroyed, you have to reproduce them again; you have to exercise an amount of ingenuity in reproducing them. In many instances you have to invent them over again. It would seem to me that the matter of invention should be considered in connection with the subject somewhat. It is not a matter of simply destroying a tool, and being able to reproduce it again. We have no means of doing it in many instances. Of course, I am speaking mainly with regard to the small tools which are so common in the production of fine work, and, as Mr. Grant says, I think the value increases in many instances instead of decreasing—I do not mean for a continuous period, say ten or fifteen years, but during the first three or four years of their life.

Mr. J. W. Cole.—Speaking of twenty per cent. reduction per annum, some of us may get the idea that the value will be wiped out in five years; whereas, if one looks at it, he will find, as Mr. Grant stated, that the depreciation is on a decreased value; it runs on your inventory for years.

The Chairman.—And will never disappear entirely.

Mr. Cole.—No, sir; not entirely.

Mr. A. C. Walworth.—I would say in regard to the practice in the establishment with which I am connected, that it is our custom to charge off on standard tools, ten per cent. a year; on special tools, twenty per cent. Our wooden patterns we charge to expense. I think we call all our wooden patterns, varying from one to twenty years old, worth five thousand dollars, and they have stood at that on the books during that time, although every year we are adding to them, and we have a great many more than we had then. Still, they are such a changeable, perishable property that we think the safest way is to charge the new ones to the expense of running the business, because if you close your business out, your wooden patterns will realize very little money. In regard to well-made brass patterns for molding machines, etc., the case is a little different. We consider them as we do tools, and charge off ten per cent. a year. I think that we have been rather conservative in that way, and have kept on the safe side.

Mr. Grant.—I would like to ask Mr. Smith in case there is a slight change in any part of the machine that requires a new jig, what would he do? Would he wipe that jig out entirely and put the entire cost of the new jig in as stock?

Mr. Smith.—Yes, sir; I would wipe out the other jig at a cent a pound, and charge the new one at what it cost, or less. You have the experience, and probably the drawing of it, and all that, and perhaps you can make a duplicate one quicker; so I would always put each necessary jig at the cost of reproducing it in its present condition, and thoroughly wipe out the other one. When obsoleteness comes, you must cut such tools right down as quick as you can, or else you will get caught.

CCXXXIX.

CAPITAL'S NEED FOR HIGH-PRICED LABOR.

BY W. E. PARTRIDGE, NEW YORK.

(Member of the Society.)

THE labor troubles of employers, both in England and America, are largely due to a wrong theory in regard to wages. This theory is so deeply rooted in the minds of both the laborer and the capitalist, that it has passed unquestioned from the early days of manufacturing to the present. Stated in its simplest form, it is this: The less the price paid for labor, the less will the product cost. Consequently the manufacturer, when he wishes to lessen the cost of goods, first considers the question of a reduction of wages. This is an obvious method, within the comprehension of any one. It calls for no investigation of methods and involves no intricate estimating.

Among the mechanical trades it is a pretty generally recognized fact that the easy way, and that into which the apprentice generally falls, is always a wrong way. In the case of wages, no exception is found to the rule. A present reduction in wages may for the time reduce the cost of the product, whatever it is, but the reaction is bad. The Old World stands to-day distanced by America, and exceedingly anxious, because wage-reduction has been pushed a little beyond the point where starvation begins. In spite of having thus carried the principle to the extreme, America faces her and produces in many lines, for less money and of better quality. The reason is that here we have, to some extent, been forced to adopt the true theory of work and wages. Our present labor troubles largely come from the introduction of Old-World ideas and Old-World methods. The very existence of the trade union depends on the wide-spread belief that cheap labor makes cheap products.

The truth in this case happens to be at precisely the opposite pole. The correct theory is, that high-priced labor makes a cheap product. The manufacturer or employer who carefully considers the ultimate success of his undertaking must study how he may increase the individual earnings of those whom he employs. He must seek for the highest-priced labor as giving him the best returns for his expenditure. His problem is to adopt a system which shall utilize skill and intelligence. These are imperative and universal laws, and apply to all branches of business.

That high-priced wages make cheap goods seems a paradox to many who have only considered the other side. The question naturally arises, Is this possible? Has such a thing ever happened, and can it happen again? The answer is, Yes. England has for many years held markets on the continent of Europe, in the face of the fact that wages were much higher in England. From England, cottons are sent to India and China in spite of the cheap labor of the latter countries.

The view usually taken is that machinery enables the English to manufacture so cheaply as to enable them to compete with the cheap labor of China. In one sense this is a mistake. Before the cheap labor can run the machines, it must be educated and drilled. It must become skilled to such an extent as no longer to deserve the title of cheap. Skilled labor must be exported to China to superintend, if successful cotton factories are to be erected in that country.

Some years since, a manufacturer of presses for making articles from sheet metal advertised his machines abroad, and, basing his statements on what was habitually done in America, said they would run 120 strokes per minute. Foreign purchasers complained. They could not make half so many articles in a given time as he had promised. Visiting the shops in person, he found no difficulty in feeding the blanks to the press for minute after minute at the guaranteed rate, but he discovered that the cheap workmen whom they employed could not keep up the American rate. After that, the guaranties advertised abroad were only half as large as the capacity of his machines in America. Similar cases are within the knowledge of every manufacturer of machinery who has an export trade.

The use of machinery presents the greatest advantages in those countries where general intelligence is greatest and skill is most common, and where, in consequence, labor is most highly paid, and in countries where labor is cheapest, machinery cannot be run by the natives. Not long since, an American engineer in Russia, who had been superintending the erection of machinery for making oil cans, wandering about Batoum, saw at a distance a hundred or more camels receiving loads of a peculiar form, and all alike. Approaching, he found a caravan about to start for Persia, and the loads were Singer sewing-machines—two to each animal. Skilled and high-priced labor alone has made a sewing-machine possible that can be shipped half way around the globe and transported a thousand miles upon camels to a land of low wages. Without such labor the machinery for its manufacture could neither be built nor operated. Could skilled labor be obtained, capital would soon erect in Asia the necessary machinery, and save the cost of transportation.

From general cases it is well to turn to those which are more particular, in order to demonstrate the advantage and imperative necessity of seeking to increase the earnings of labor. Some years since, an industry was started in the eastern part of the country, which grew to considerable proportions. The distance from consumers, cost of transporting bulky articles, and high wages, led to a transference of the factory to Boston. It was then concluded that the cheaper labor obtainable in New York and its advantages as a business center would pay for another removal. The factory was then brought to New York. It was found, however, that both changes were for the worse. Fine materials, like ornamented paper, silk, satin, scrap-pictures, etc., were employed, and the waste of these in cheap hands was so great that, in spite of higher wages, long freightage, and cold, long winters, it was cheaper and best to remove the factory for the third time, to the village in Maine where it started.

An example of the inevitable evils of seeking cheap workmen happened a few years ago in a shoe shop in the western portion of the State. At the regular price for piece-work exceedingly good wages were made by the smarter girls in the factory: $25 and even $30 per week were paid to single operatives. A large number of girls were employed, some able to earn these wages, while the rank and file averaged at the time about $8. Cut after cut was made in the wages, because the employers thought that no girl ought to earn more than $10 per week. The result was hardly what was expected. The capable women whom they hoped to cut down to $10 left them. Only the poorer operators remained, the best of whom could earn only $5 or $6 per week. The productiveness of the plant had been cut down in about the same ratio as the cut in wages, and the most expert and valuable laborers were forced to leave, and quality suffered as well as quantity. The profitableness of such an operation is certainly questionable. In contrast to this is the testimony of one of our shrewdest and most intelligent Eastern manufacturers, that within fifteen years, in several departments of his establishment, systems have been in operation which have reduced "the labor cost on certain products without encroaching upon the earnings of the men employed." In this establishment records are kept with great care and system, and the accuracy of the statement is beyond a doubt.

It is a fact, known and recognized for years, that in certain lines of metal work, goods were most cheaply produced in parts of New England where the earnings of the men and wages in general were the highest, and the same goods were most expensive to make in parts of the West where the daily earnings were only a third as large. The facilities were equal. A manufacturer, in speaking of this, said, "My men are respectable citizens and property owners, with children who will go through the high school, and perhaps through college." He characterized those of another section as but one remove from the day laborer, and of no account socially.

The workman who earns large wages is valuable to his employer, because he returns a large product for a given outlay. Such a sale of labor is in one sense a wholesale transaction. When two men sell a dollar's worth of labor, the buyer rarely expects more than 90 per cent. return, while if one man performs the work, something over 100 per cent. would not be unusual. When the earnings are large in proportion to the number of workers, the value of the plant, as well as of the product, is increased. Cases are on record where an increase of the earning power of the men has been equivalent to an increase in the capacity of an establishment. Men who earn large wages become property holders; they are respected citizens, conservative, self-respecting, intelligent, and temperate. Their children do not grow up to swell the criminal classes. Such citizens are a valuable purchasing power in the land; their prosperity is of a stable character. Men of this class have no labor troubles; they have too much at stake and are too well assured of the necessity of continued work, and too successful in it, to take part in strikes.

Manufacturing can only be prosperous through long periods in large, thrifty communities, where the average income of the masses is high, and where wealth is widely distributed; under such conditions, consumption is enormous. The tax imposed by poverty bears lightly, because there is no poor class—no pauper labor.

It must not be understood that the suggestion of high wages means that the manufacturer or employer should at once make an advance in the wages paid, or increase the price of piece-work. Such a course is disastrous alike to employer and men. Advancing prices without a corresponding advance in earning power, results ultimately in a permanent decline in earnings. This is a rule which appears to have had no exceptions in this country. It is a result greatly to be feared.

In Great Britain a strange thing has happened, which may be interpreted in many ways. As the price of wages in the great iron-producing establishments has been reduced, so has England begun to feel Belgian and German competition. This has often been looked upon as a case of cheap labor against that which is higher priced. Others take the ground that it is the irrepressible conflict between machinery and man; in which they say that man must be ground to powder if machinery is tolerated. Examine the case a little further, and it will be found that it has been a contest of machinery alone. In such a contest, the latest comers, and those having the most capital, are always winners. Belgium and Germany had only to construct superior plants, and employ as much or more capital, and they could command the markets of the world, the natural facilities being nearly similar. The only reply to this attack was to reduce wages and increase and improve the machinery. But the Belgians could reduce wages even more easily than the English, and the improved machines of one country could be duplicated by the other within a year or two.

Had the English iron-masters turned their attention to the improvement of their labor as well as their machines; had they made one Englishman in their iron works as good as three Belgians, competition would have been extinguished. They had not learned a lesson which the ship owners of the last generation could have taught them; that one English or American sailor was worth two of any other nationality, and English and American ships, though paying high wages, cost less to handle than those of other nations. They habitually sailed with fewer men and with greater safety and more profit. Labor of a high class cannot be exported; it is not duplicated by capitalists of a foreign country, and it is exempt from competition of machinery or men.

The general problem which the employer of the present day in this country must solve is a simple one in its general form. It is to increase the earning powers of his men from year to year, and to do it in such a way that the men not only earn more, but are more profitable to him. Though simple as a general statement, it becomes complex when applied to individual industries. The method which has been employed in a single instance will serve as an illustration of how the problem may be attacked in relation to piece-work. In a large establishment in Pennsylvania, embracing a great variety of trades, piece-work is almost the rule. When a man devises any method by which a saving of time, labor, or material is effected, he calls for a trial of the improvement. When it is found successful, a new schedule for that class of work is made and the price is reduced, but the men get one-half of the gain and the establishment the other half. Cost of production has been reduced and the earnings of the men increased. This stimulates every man to study processes and machinery. Every motion is criticised; the operation of every machine watched with the utmost care by intelligent and interested eyes. Ambition is at work, there is an incentive for men of skill to stay, because they feel assured that there is an ultimate market for what they have to sell. Every man is as interested in cutting down the price of piece-work as the proprietor himself. Under this system no limit is placed on the sum a man may earn by piece-work. The higher the wages the more profitable is the plant.

When improvements in processes are designed in the office or by the heads of departments—that is, when they may be said to originate with the proprietor—the establishment takes two-thirds of the saving effected, and the men one-third. It must be understood that such tools, appliances, or alterations in tools as may be needed to introduce improvements, are made by the works, for the men; hence a man when he has designed an improved method of doing his work is not at any expense in demonstrating or introducing it. How profitable this system has proved for the works can be judged from the fact that a few years ago the labor on one article cost $6, and 18 months ago the same thing, in an improved form, was being turned out for $2.50. The men at the latter price were earning higher wages than before. This method is capable of being applied in many lines of business. In some it has a wider range than in others, but it is profitable in all.

The underlying principle, however, is applicable in every line of business, and the employer will find it profitable to enlist upon his own side, and virtually take into partnership with him, the hope, ambition, and self-interest of his men.

DISCUSSION.

Mr. Wm. E. Partridge.—(In opening the debate, and in the formal presentation of his paper to the meeting.) More than one hundred years ago France found herself in a condition not unlike that which is prevailing to-day. The community was separated by a greater extreme perhaps than at the present time. The nobles and the king were France. The people were animals, of which a noble lady said "it is doubtful whether they have souls." The feudal system had thrown the money into the hands of the nobles; the people had nothing, and were, perhaps, infinitely worse off than the poorest of our laboring classes at the present time. A preacher arose—in fact, many of them—and preached just such a crusade as we are hearing to-day, and the result was 1798. It is a question which has great interest to us whether 1898 will mark a period of equally disastrous uprising with us. Certainly there are some things in the history of labor and capital which make it seem almost probable. Then the king said, "I am the State;" and the nobles said, "After us the deluge;" and after them the deluge came, and it was by no means a pleasant one. To-day, in the eyes of labor, capital is in a position not dissimilar, but the facts are that capital has robbed labor of nothing. The workingman cannot say that in this country anything has been taken from him by the wealthy class as a body. He has no right to ask anything of capital and of brains, for brains and capital must be classed together to-night in this discussion. We then come to a question written in our oldest book—"Am I my brother's keeper?" No, perhaps not; perhaps not in a business sense. Strict morality will not ask you to take your property and give it to some one else; but the sharp business foresight says, in the light of what may happen, "It will be very much better for us to do something more than our duty." The laborer to-day in masses is not capable of thinking for himself. If he was, he would not be a day laborer. If he could do the planning necessary to organize a great railroad system, and bring it from bankruptcy up to six per cent. on watered stock of double its original value, he would not be working for you for a dollar and a quarter per day. If your man at the machine tool was capable of inventing quickly, habitually, and well, he would be getting his $3,000, $6,000, and $7,000 a year, instead of $18, $20, or $25 a week. It is the old story of McCullough's asking the "supe" why he could not announce "My Lord, the King!" "Can't you do it as I do?" "Naw; if I could, I wouldn't be a 'supe' on six dollars a week." Now, the capitalist and the thinker must take these men into partnership with them as a matter of safety—not partnership in a business sense, but a partnership in a humanitarian sense. Instead of devoting just 100 per cent. of your business time to making your own money, you must give him a portion of the time, and attempt to make his condition better for him. The business man can do wonders for his men. I have attempted to show, in the preceding paper, that greater earning power is as valuable to capital as it is to labor. There is not a man I think here who has had charge of a shop who cannot give, chapter and verse, that the greater the earnings the more valuable is the stock; it increases the value of your plant—of your factories. But I need not tell this; you know all about it. But there is one thing that you will have to do for your men, and that is to think for them in ways which will enable them to earn more. In other words, it may be necessary here and there to follow the style of some of the French co-operative establishments. Nominally co-operative, they are really establishments where the owner has taken his work people into a participation of the profits of the business, and enabled their earnings to be greater than they possibly could be had he stopped his thought about and planning for them when the six o'clock whistle blew.

Mr. John T. Hawkins.—In a brief discussion of this paper, I desire, principally, to emphasize the position taken by Mr. Partridge; and, by extending into slightly other channels the application of the aphorism given in his first paragraph, to add to the much-mooted labor questions of the day something which may possibly be of advantage to the earnest men throughout the country who are now discussing the present or late disturbed and unsatisfactory relations of the employer and employee.

There is little, if any doubt, that to-day the average employer, to a great extent at least, accepts the mistaken axiom: "The less the price paid for labor, the less will the product cost;" and it will probably be more emphatically and universally accepted by the average mechanic or laborer. I do not think, however, so far as the employee sees it, that "among mechanical trades it is a pretty generally recognized fact that the easy way is always the wrong way," if applied to the actual energy displayed by the average workman in a given time; and it is the grand and constant error of his life that the laborer does not recognize this truth; and that he does not, goes without saying with every one who has had any extended experience as an employer.

The two principal factors, in the present strained relation between employer and employed, are, I think: First, that the former assumes the right to get the greatest number of *hours* of labor out of the employee for the least possible wages, disregarding, in a large measure, that the dissatisfied laborer, working a given number of hours, is vastly less efficient than he who is satisfied with his lot and will do a fair hour's work in every hour; second (and by far the most formidable factor), that the latter considers it to be his bounden duty to do the least possible in the hours he has agreed to work. There is more excuse for the laborer than the employer, perhaps, because in the main he is naturally not so situated as to become so well informed on social economics as his employer, and is biased by all such imaginings as that, if he does too much, he will be keeping some other fellow out of a job; or that, if he makes a given labor-saving tool produce all that it is capable of doing or intended to do, he will be adding to the already untold wealth of the employer, without anything accruing to him for the extra exertion. "What difference will it make to me," he reasons, "if I make this machine bore ten of these wheels a day, where I have been doing only five? It will only be more money in *his* pocket, which already has so much more than I, without adding one cent more to my wages at the end of the week. So the less I do the better and easier for me."

I do not pretend to say that there are not many employers who, in a rather general or perfunctory way, approve of paying not only the most skillful, but the most industrious, somewhat in proportion to their merits; but that there is a lamentable lack of a proper recognition of this principle, on the part of employers, there is, in my opinion, not a doubt. But much as I believe this fact tends to the restlessness now reigning in the world of labor, this is, by far, the less of the two disturbing factors indicated above. The one undeniable fact that the average workman or laborer to-day considers it to his gain that he does the least instead of the most he can do in a given time, is one of the fundamental causes of the present labor troubles.

Not that I think a workman should be driven up to the capacity of his thews and sinews or his brains to perform, but merely that a man in such a capacity ought conscientiously to do all he can reasonably and properly do in the hours he is employed, and exert himself in every way to increase, rather than to diminish, the product of his labor. If workmen would adopt this principle throughout the country for one year, every man honorably striving to outdo his fellow, and employers would, during the same time, reward in shape of higher pay to the most skillful and energetic and industrious men, as measured by the quality and quantity of the product, and advertise to the men freely that this course would be followed, I believe that greater good would come to both sides than can be gained by a century of trades-unions on one side and manufacturers' combinations on the other. I quote from a recent daily paper that "the best kind of a labor union exists in the man who combines in himself pluck, economy, foresight, and common sense;" and there might be added *energy* and *industry.*

"Our present labor troubles largely come," says Mr. Partridge, "from the introduction of Old-World ideas and Old-World methods." There is one Old-World method, however, which is not adopted, to any extent, by American workmen; and that is, that it is incumbent upon a workman of any kind to do something more than the least possible in working hours. It is notorious that imported artisans or laborers are a great deal more productive than Americans when they first find employment in this country; but, unfortunately, it is equally true that after they have become Americanized in this particular, they outdo their exemplars and become the most assiduous do-nothings and agitators of how not to labor.

The greatest want of to-day, as I see it, in this connection, is that employers should counsel more with their employees, and to the effect of letting every man understand that energy, industry, and skill—in other words, productiveness—will meet its reward in increased wages and permanence of employment, and that the reverse of this will be similarly dealt with. If this state of things be once fully appreciated by workmen, and as thoroughly carried out by the employers, the days of trades-unions and similar organizations as now conducted and the antagonisms they engender would be numbered; and generally the employer who paid the highest wages would be he who got his product the cheapest. So that I most emphatically believe in "capital's need for high-priced labor" as the most effective method of cheapening its product. It is one of the most singularly perverse attributes of the wage-earner that he does not realize the force of this and what it means to him, as compared with the work of trades-unions and the like, attempting to force employers to pay more for a given amount of product. If they would organize for the purpose of instructing one another how to become the most productive and how to command the highest rates of pay, instead of getting the most pay for the least product, there would be a measure of good for them in such organizations incomparable with their present objects and methods.

I call to mind a typical instance, although, perhaps, an exaggerated one, which illustrates very well this conviction, on the part of the average workmen, that they are morally obliged, in justice to themselves, to produce the least possible in a given time when working by the hour or day. In a certain machinery establishment in New York city, there was required to be made and finished, as a part of the regular product, a large quantity of a certain small part of a printing-machine, which was common to nearly all kinds and varieties of these machines. The time was when two men were found necessary to be constantly employed upon finishing these articles in sufficient quantity; and, as the business increased, a third man became necessary. The proprietors had, for a considerable time, been convinced that they were costing too high in wages; and, being of that particular range of character which permitted of great skill being acquired by long practice on the part of the men, and feeling that the product of these men ought to be largely increased in this way, concluded the trying of making them upon the piece-work plan, employing the same skilled men to do the work, and to which they readily assented. The result was that, after the first week, two men were found to be easily equal to the demand, and soon thereafter one man made too many, although the demand was constantly increasing; and, in point of fact, increasing numbers were made. After about six months, this one man asked for some other work by the piece with which to fill in his time; and all without any attempt on the part of the employers to introduce labor-saving methods or appliances, but considerably due to devices of the man himself, which enabled him to produce more, and which, added to his increased assiduity, brought about the above result. Of course, every reduction in the number of men employed upon this work was attended with a commensurate reduction in the price of the piece-work, which was at first based upon the need of three men to produce the sufficient quantity, and was a clear saving to the employer. The man ultimately reserved to make them by the piece received from fifty to seventy-five per cent. more pay than when working, or rather idling, by the day. The employer in him had high-priced labor, and both were benefited by the simple fact that the workman performed what he was capable of doing easily, and used his brains to perfect and improve his methods. Yes, one of capital's greatest needs is high-priced labor; and to this end the employer's next greatest need is systematic and comprehensive effort on his part to create it by encouraging and instructing workmen and laborers how to become higher-priced men than they now are.

As another very recent instance of the workman's purblind determination to be less productive, and therefore a lower priced laborer, than he might be if he would; and what will be quickly recognized as quite a typical one by any one familiar with the interior working of a machine-shop, I quote from the New York *Tribune* of November 29th last: "Elmira, November 28.—For some time trouble has been brewing between the employees and the proprietor of Payne's machine and molding shops, one of the largest concerns of the kind in the city. The Paynes have determined to run their shops to suit themselves, and although the principal number of their employees have been Knights of Labor, yet a number of non-union men have been employed. The machinists say that about a year ago they were asked each to run two lathes instead of one for the same pay as for running one, that the best of them left the shops, and that the order proved a failure. Yesterday a machinist was asked to run two lathes, but refused. David Payne became angry, and believing that the Knights of Labor were fomenting trouble, discharged the entire force in the department."

To enter into any argument here as to the effect upon the laboring man, of the employment of labor-saving machinery, would be out of place; every one who has given this subject any considerable thought is agreed as to that. But here we have men employed by the hour or day to operate labor-saving machines in a way to permit of their saving the labor they were designed to do. They deliberately undertake to insure that these machines shall produce the least they are capable of keeping them down to, insisting that the employer shall pay double (or nearly so) for the product for their machines that which he should pay if the workmen would intelligently and willingly apply themselves to making them produce the most that could be gotten from them. They refuse to see that if they each willingly attended to the running of two or three or ten of these lathes—if within their reasonable powers to accomplish the task—they would so far be increasing their value to their employer as to make it to his interest to pay them higher wages; and to intensify their obtuseness, they also play the dog in the manger; refusing to allow others to gain what they reject.

To be sure, the above-quoted report says that "the men were asked to run two lathes instead of one for the same pay as for running one;" but, while the undeniably correct principle is, that every man who could as well run two lathes as one, when paid by the day or hour, should consider it his bounden duty to run the two without any consideration as to the amount of pay he should receive; who can doubt but that the employers in question would be vastly more likely to increase the wages of a man who carefully ran the two lathes than if he mulishly insisted upon sitting or standing idly by his one lathe when he might, without any deprivation to himself, nearly double the product of his labor? Of all the antagonisms between capital and labor, I believe the greatest to be that labor strives to give the least equivalent for the wages it receives, while capital as persistently, perhaps, operates to keep the wages of labor at a minimum; the employer being as blind to the fact that by encouraging his labor to become high priced he puts dollars in his pocket, as the laborer is oblivious to his own interests by refusing to earn all that he is capable of.

Mr. Wm. Kent.—We should be obliged to Mr. Partridge for having brought this question up, and I wish to comment on the fact that possibly this Society is about as well qualified to discuss this labor question, in its various phases, as any body of men in this country. The labor question has been gradually developed by newspaper articles and magazine articles, in which one man writes what he has to say at one time, and at another time another man writes something else, and the country reads it. But when we meet, as we do to-night, each man rubs his ideas against another's, and probably better results are reached than by reading stray articles. I think more is to be gained in the knowledge of this subject by having a discussion, such as we are likely to have to-night, than in any other way.

The American capitalist and employer has to employ high-priced labor because he can get nothing else. He employs inventors, who invent machines which take the place of the high-priced skilled labor. He pays enormous wages sometimes for such inventors—ten thousand dollars a year, perhaps, or more; which enables him to dispense with a large amount of highly-paid labor, and so to reduce the labor cost. After the works are fully established, and the plant has become a commercial success, the question arises, what shall we do to obtain a foreign market? If the company is rich enough, it goes over to England, or to Scotland, and duplicates its American factory there, and turns out its machines much more cheaply there. The Yale & Towne Lock Company, I have no doubt, could to-day duplicate their plant in England, and turn out locks cheaper than they can in America. By taking over to England the machines with which they make locks, and operating them there with lower priced labor, they could make locks cheaper.

I am in full sympathy with the apparent object of Mr. Partridge's paper. It is certainly commendable from the stand-point of the philanthropist, and many of its statements are consistent with sound political economy. That "the workman who earns large wages is valuable to his employer, because he returns a large product for a given outlay," is true in a great number of instances, and the cost of production in many industries might be reduced if this truth were more generally appreciated. Capital is undoubtedly in need of high-priced labor. The market is always overstocked with low-priced labor. In mechanical pursuits, for instance, there is rarely any difficulty in getting workmen at from two to three dollars per day, who may or may not be worth what they ask, but a five-dollar-a-day man, who earns his wages, is harder to find, and a ten-dollar-a-day man is always scarce.

Agreeing thus far with Mr. Partridge, I regret that much of his argument assumes a rather paradoxical form, and that his fancy for a paradox leads him to make assertions which, if true at all, are true only to a limited extent, or in a limited number of special instances, and not generally.

He states broadly that it is a wrong theory that "the less the price paid for labor, the less will the product cost," and that the correct theory is that "high-priced labor makes a cheap product." It is difficult to frame any argument to meet this style of assertion, almost as much as it would be to combat the statement of a learned mathematician, that it is a wrong theory that two and two make four, and the correct theory is that two and two make five. The problem is simply one of arithmetic. The cost of a manufactured product is made up of rent, including royalty, interest on capital, wear and depreciation of plant, insurance, taxes, and the wages of labor, including the labor of superintendence. Thus the cost of a steel rail is made up of the royalty on the ore, the coal and the other raw materials, as they are found in the earth, or the interest and depreciation of value of the land which contains them, the wear and tear, depreciation, insurance, and taxes on all the plant involved in the manufacture, including the mining, the transportation, and the manufacturing plant proper, and the labor of mining, transporting, and manufacturing. This labor costs by far the largest percentage of all the items of cost. It is a simple matter of arithmetic, then, that if the cost of labor is diminished, the total cost is diminished.

"The manufacturer, when he wishes to lessen the cost of goods, first considers the question of a reduction of wages." If this means the daily wages of the individual employee, it is very far from true in this year 1886. It may have been true in former years, but this year, when every manufacturer is endeavoring to reduce the cost of his product, a reduction in daily wages is about the last thing he thinks of, for such reduction, in the face of strikes of all sorts for advance in wages, for reduction of hours, and for control by the employees of the workshops, is a practical impossibility. He is glad if he can consider the rate of daily wages a fixed quantity, and his effort to reduce cost takes far different forms. He so organizes his factory that the high-priced man is kept on work requiring intelligence, that the average man is kept on average work, and that the machines which such average man tends are kept in good running order and speeded up to their maximum capacity; that machines which merely require feeding, and but little or no skill, are manned by low-priced workmen or by boys; that whenever possible, the low-priced workmen and the boys are both displaced by the substitution of machines which feed themselves. Mr. Partridge mentions a case, of the press for making articles from sheet metal, requiring high-priced labor to feed it. I noticed a similar machine recently fed by a cheap, small boy, who was not very skillful, and commented on the high labor cost of the articles produced. A few days later I saw the same machine fitted with an automatic feed, and the same boy, at the same wages, turning out about five times as many of the articles in the same time.

Here it was good economy to employ a cheap boy rather than a high-priced man to feed the machine, but it was still better to make the machine feed itself. Mr. Partridge mentions the sewing-machine. If he would investigate the question of its manufacture a little closer, he would find that, while it is true that its cheap production has been rendered possible only by the employment of high-priced labor to design and perfect the machinery necessary for its manufacture, its cheap production now depends upon the operation of that machinery by low-priced labor. The most difficult operations, apparently, are those which are done by automatic machines, in which the machine tender only inserts the blanks and removes the finished pieces. In many instances, as in making the screws, a coil of wire enters at one end of a machine, and the finished screws come out at the other, without any hand labor at all. Further, the time was when the Singer Manufacturing Company exported sewing-machines from America to all parts of the world ; a grand instance of America distancing the world in cheapness of production ; but now the same company has a factory near Glasgow, Scotland, much larger than the American, which can turn out sewing-machines more cheaply than the American factory can, simply because labor is cheaper there than here. It has another factory in Vienna, which makes machines still more cheaply, because labor is cheaper in Austria than in Scotland. Westinghouse, Armington and Sims, and Corliss engines; Westinghouse air-brakes; Worthington and Blake pumps; Babcock and Wilcox and Root boilers, all of which originated in America, are now made cheaper in England than they can be made here—all because labor there is cheaper. America has the best blast-furnaces and the best iron and steel works in the world, yet it never has been able to make iron and steel as cheaply as England, because labor is cheaper in England. In the same way, England has better iron and steel works than Belgium, yet Belgium frequently underbids England in English markets, and only because Belgian labor is cheaper. In 1882, the lowest price paid to unskilled labor, recently imported Poles and Bohemians, in the iron works of Pittsburg was $1.25 per day. The lowest class in a Scotch steel works, a more intelligent set by far than those Poles and Bohemians, earned only 62 cents per day. In a Belgium iron works, a similar class, apparently fully equal if not superior to the Scotch, only got thirty cents a day, and the same price was paid to women in the same works, who were engaged in the work of wheeling coal, iron, ashes, etc., work which in a good English or American works would be done by a locomotive. According to Mr. Partridge's theory, the cost of iron would be lowest in America, and highest in Belgium, but the facts were exactly the reverse.

Mr. Partridge.—As I have to leave shortly, I will ask the chairman to allow me to say a word or two, which I might claim to say later, chiefly in reply to Mr. Kent, who is, I believe, according to the law of "foreordination" or "predestination" on the other side of any ordinary question. (Laughter and applause.) However, friend Kent is on the right side this time. Perhaps I am a little to blame for not making it a little more clear, that "cheap" and "high-priced," as I have used them, should not mean the amount of money, but relatively how much you get for your value. Friend Kent's figures all fit my paper, and I would have been very glad to have had them while writing it. The chief question is not whether you pay $2.50 or $5 for what you buy, but the percentage of value received for the outlay. If $3 worth of value is obtained for $2.50, or $7 for an expenditure of $5, the purchase is cheap. A twenty-five cent article is high if its value is but six cents. It is the percentage of value received. Some low-priced labor returns more money than the high-priced; that is, the large wages return less than the low. Usually the reverse is true—that the large wages return many times as much value to the shops as the low wages. This statement will meet all the figures and all the cases which Mr. Kent mentions. (Applause.)

Mr. G. L. Fowler.—I had recently an opportunity of inquiring into and examining a system which is employed by one of the largest railroad companies in this country, in the treatment of their employees in their large shops. They do everything by the piece-work system—I mean everything, from the work of a day laborer to that of the most skillful machinist. All of the work about the shop, in taking in wheels from the platforms on the outside, carrying them to the lathes, the unloading of cinders, the carrying of boiler tubes from the boiler shop out into the yard, the handling of scrap iron—everything of that kind is done by piece-work. There is so much paid for handling a driving wheel, say, from the lathe out into the yard where it is kept. This work is done by gangs; gangs of from seven to nine work under the charge of a foreman, who works with them, and the amount of work which is done by a gang is divided up among them at the end of the week, and the result is, that they will not allow any one in among their number who loafs and is not willing to work as hard as he can all the time. I remember seeing a case of this kind as I was going through the shop. A gang of men were loading car wheels. There was a man in the gang who was an extra hand. There was no place for him to work with the rest, and he was on a car-load of cinders unloading that. The very moment the men had finished with the wheels all of them were up on the car-load of cinders with that extra hand and at work. It is just so with the machinists. They have a system of piece-work for every individual thing which can be done about a locomotive or a car. That is all scheduled off and the men know exactly what they are to receive for it. Of course, when you take the work of repairing a locomotive, the schedule must be very long. My recollection is that it covers about fifty pages of foolscap written on a type-writer. The pay of the day laborers run on an average from $1.65 to $1.75 a day. Machinists are about $3.50 a day. The superintendent of the shops, who was showing me the record, said that he had a very amusing incident occur some time before. There were two men who before the piece-work system was introduced made $2.40 a day apiece. After the piece-work came in, one of them ran his wages up to $3.50 and the other dropped back to $1.75. The $1.75 man came around and said he thought his piece-work should be allowed a little more; that it was an imposition to make a man work for $1.75. He said, "Here is this other man making $3.50." The superintendent said, "That doesn't make any difference; it is none of my business; you want to go to God about that matter; he is the person for you to go to. He didn't make you quite as sharp as that fellow. You manage the matter with him." And the man looked at him but didn't say anything. The superintendent went on, "It is just the same as it is with me. I have to go to the same authority. I cannot get the same fees for doing my work that some of the big lawyers in New York get. I ain't as smart as they are." The man said he didn't know but what that was so, and he went off perfectly contented. (Laughter.)

The result of that case is, that the men are unwilling to work on anything but piece-work. Of course, in a large railroad corporation there are continually arising circumstances where work will be coming in which is outside of the regular work, and is not scheduled. A man will look at it and make an estimate on it, and if his estimate is any way reasonable, they let him go to work on it. Nine times out of ten, the men will make a fair estimate on the work, and work like beavers to get it done. And it is working more than satisfactorily. When the company made their schedule price, of course they wanted to reap some advantage from piece-work as well as the men, and after long and careful consideration of the subject, I think they cut the rates down about twenty per cent. below what it was actually costing them by the day, and gave the men this work to be done at those rates. While the men worked to so much better advantage that they have run their actual receipts up in the way that I have indicated, averaging about twenty-five per cent. increase to the men, there is a saving to the company of about twenty per cent. That shows there is a saving on one side and an increase of wages received by the men on the other side. Everything is working in that way with perfect satisfaction to both sides, and the company think that they have, in that way, rather solved the labor question. (Applause.)

Mr. T. R. Almond.—In connection with that subject, it has seemed to me that if you give the workmen some incentive to do better, they will do better. I had a good illustration of that during the past year. I was getting dissatisfied with certain help and suggested that they had better seek employment elsewhere, unless they were willing to make the goods at a price which I had made two years previous. There seemed to be no inclination whatever to make them, and being displeased with the help, I said: "Now you must make those goods at that price or get work elsewhere." The result is my machinery is in better condition, more care is exercised to keep it in good condition, the help get considerably better pay, and during the last nine months I am certainly $1,000 better off than I was, because of that, and that alone. I considered very carefully, prior to this experiment, the cost of the goods, and since making the experiment I have found that the help are—say in the case of four men—between $500 and $600 better off themselves, and I myself am at least $1,000 better off because of that simple little experiment.

I was very much pleased to read Mr. Partridge's paper, because it was in such strict accord with my idea, and I believe that if you give any employee some incentive to do better, he will exercise his brains sufficiently to enable him to do better.

Mr. J. L. Wells.—In the matter of this labor question, there is no doubt but that the true solution of it is in some system of rewards, or some incentive for emulation. Here in New York, in my business, the whole incentive to competency, or to working a full day, is withdrawn from the men by this trades-unionism. We are to pay every man, big or little, great or small, old or young, the same rate of wages per day and no less. The incentive to a man to do more than his neighbor is lost, and we do not seem to have any redress for it. In the scheme of a participation in the profits, the hope engendered is not followed out in the distribution. You take a hundred men. Perhaps the legitimate profits for the year would be $15,000 or so, and say you would give them half the profits; that makes $75 apiece, and it is rather disappointing. I d not think that that is the true solution, but I think that it is to b that general lines of business shall be paid *pro rata.* Take th mason, or brick-laying business for instance. If a combination master-builders in New York could agree to make a rate of wag for laying 1,200 brick, or 1,500 brick for a day's work, at a rat per day which the business will stand, and it could be understoo that the man who can lay only 1,000 brick would get so muc less, and have such rates established, then you will have an incen tive for the man who lays the thousand brick to work so hard th in a short time he will be able to lay twelve hundred brick, et It seems to me that that is the true solution of the business.

Mr. Oberlin Smith.—In order to carry on what has been term the "discussion" here—though I do not think many of us hav discussed each other much—I will say that I believe all that th other fellows have said. We have certainly a remarkable unanim ity of opinion among us. There is no question in my mind b that some form of piece-work is the true solution of the difficulti in question—that is, if it can be carried out. It is easy in th large railroad shops, or in a gun or lock factory, where the produc is made in large quantities in duplicate, but it is very difficult i the ordinary machine-shop—especially in small ones. I hav found, in die-making (a class of work with which I have had muc to do), great difficulty in introducing any kind of piece-work, be cause each article was different from anything that had been ma before, or was likely to be made again. The men have refused to bi upon them. They have often been urged to, but they would no because they were afraid they would lose. Some improvemen have been made by instituting a series of premiums to be pa over and above the day wages, in proportion to the excess of profi over losses which have occurred in that particular class of good I believe it should be the study of every machine-shop owner to systematize his business as to give all possible piece-work, or, lieu of it, to have some arrangements by which the men ca a percentage of the profits. A small percentage of the pı each particular job may be set over to the premium account. may apply to gang-bosses and foremen as well as to journ In this way their personal interest in each job is more dir apparent than where they get a percentage upon *yearly* pro

Mr. J. J. Grant.—We employ about three hundred n Philadelphia, and there are only nine men in the shop who Knights of Labor, who are usually reckoned among the op of the piece-work plan. There is not one who objects to work. They all want it. They do object to contract work one man hires a gang of other men; but they do want work.

Mr. T. J. Borden.—One of the large industries of this c that of cotton manufacturing, pays its employees chiefly b work. About three-fourths of all the operatives in such es ments have been so paid for at least forty years, but the much controversy among them in relation to the rate of w prevails among other classes of labor. Piece-work leads to larger product per hand than can be attained where pay made by the day or hour, and in that respect is decidedly cial. In lines of manufacturing or mechanical work, in large numbers are engaged and the nature of the operation but little from year to year, competition gradually brings t of wages to a minimum, and under those conditions there is as much friction in the adjustment of the rate of wages on work as on day labor.

In new enterprises, or where so few are engaged in a branch as to be comparatively free from keen competiti wage question can be handled with much less friction by work than by day labor.

As large a proportion of the operatives employed on piec even those receiving the highest wages under that syst members of the Knights of Labor and kindred organizat are found among day laborers.

Piece-work is not by any means a cure-all for labor tr but only a slight alleviation of the difficulty. Controver this subject will exist so long as there are two sides to the q dependent upon frail human nature to decide between them.

Mr. Henry R. Towne.—The intense present interest question is shown by the discussion which we have had. W simply opened too large a volume for us to get through with in this session. It is *the* question of to-day, but it is going to be still more the question of to-morrow. It is the great question. But we are not going to see this question treated and settled as was the one Mr. Partridge referred to, which was settled one hundred years ago. We are upon the eve of a revolution, in my opinion, but I believe that it will be a peaceful one, and that it will be settled without the world having to pay so dear a price for it as for the one accomplished a hundred years ago.

There has been so much time taken up by this discussion that I will not now say what I would like to, but I will refer to one or two points which, while germane to the question as originally proposed, are also quite within what is covered by the range of discussion since. And in the first place, I want to refer to a little book I have recently read which will interest any one who considers this question at all. The title of the book is *Profit-Sharing*, by Sedley Taylor [London, 1884]. The book is a small one, of perhaps a hundred short pages, giving a *résumé* of the experiments in profit-sharing in England, but chiefly in France, where the plan has been tested longer and better than anywhere else. The French system consists of a plan by which the entire corps of any establishment, from the head to the youngest boy, has an interest in the profits of the business, that interest being divided at the end of the year in the shape of a percentage on the earnings of each individual during the year. In this way the relative values of the services of different men are left entirely undisturbed. They may work by the day or by the piece, but each receives a percentage upon his earnings representing his interest in the profits of the business. In no other way can you so completely identify the workman with the proprietor, and interest him, not merely in the product of his own work, but in the product of his fellows around him and in the entire economy of the place, the economy of material, and in everything which tends to reduce cost. This, it seems to me, is the ultimate and final solution of this question. But while we are endeavoring to reach some such final solution, there are many intermediate steps which can be taken slowly, one at a time, all tending to improve the condition of things. Obviously, piece-work is the first and most important, and it is consistent with "profit-sharing;" one can exist within the other. There is a broad distinction, however, between piece-work and contract work. Many kinds of business do not admit entirely of the application of piece-work, in which each man works for himself, but necessitate the introduction of a system where one man works for himself and has two or three or twenty more working with him as assistants, but being paid by the day. The piece-work system, if possible, should be applied throughout, simply for the obvious reason that it makes every man interested directly in his work.

A good many years ago, in organizing a piece-work system, which has since become somewhat large, it occurred to me that an improvement could be obtained by adopting a specific time, a definite period during which piece rates should hold. Prior to that it had been the practice to reduce piece rates whenever the earnings of the piece and contract hands seemed to be larger than reasonable. The men appreciated that fact and they knew perfectly well that when their earnings reached a certain point they would probably be cut down in their piece rates. The result was a tendency, and a very strong one, to keep the men from pushing their work beyond a certain limit. On the first of a certain January, a good many years ago, I had it announced to the piece and contract men that the rates then adopted, which constituted a revision of those previously in force, would hold for twelve months, and that during that interval whatever earnings the men made would belong to them. At the end of that twelve months we made the heaviest reduction in piece rates we had ever made, simply for the reason that the men, knowing that they would keep all that they got in the mean time, went in with a will and did the best they could, not only in the amount of energy put into their work, but the best also in the way of improving their appliances and methods of doing work. That system has now been in continuous operation for some fifteen years or more with the most unqualified satisfaction, and as jobs become old and the cost of work pretty definitely known, the period during which piece rates hold has been increased in many cases to two years and sometimes to three years. It seems as though there was no end to the possible reduction in the cost of work where machine processes come in. Where the work is purely manual, there is a limit. Again, where new work is to be started by piece or contract and you have no accurate data to go on, and where you have to recognize the timidity of workmen in incurring risks (indeed, they have not the means, they have not the right, many of them, to incur the risk of losing money or of earning much less than their average), that trouble can be met by fixing limitations. I have pursued that course in many cases and with very good results. The method is simply to say that the rate adopted is assumed to be the fair rate for a particular job. If at the end of the period for which the rate is fixed, the workman's earnings are less than the *minimum* named at the commencement, the difference will be made up to him, that difference being usually the amount required to make good his regular day pay for the term. On the other hand, if his earnings exceed a certain *maximum*, which should be much larger than the minimum, the excess beyond that should belong to the employer. The *maximum* and *minimum* may be put at any reasonable distance apart, but they fix a limit; they guarantee the workman against loss, and they guard the employer against the occurrence of a blunder by which he would pay an extravagant price for the work. In almost any kind of productive work a great portion of it can be done by piece-work or by contract, and one element of the "conflict" between capital and labor be thus far settled, for the present at least. But I do not regard this system as a final one, for I believe that before long we are going to see the principal of profit-sharing introduced very much more generally than has yet been done.

Mr. Partridge's proposition is abstractly true, as Mr. Kent has explained. High-priced labor, if you get value for it, is the cheapest in the end, for many and obvious reasons.

I dissent, however, to the statement that has been made by Mr. Hawkins that it is the disposition of workmen to do as little as they can. I know that this is true of some,—that it is true of men low down in the scale as mechanics. But that it is true of, or generally applicable to, the American mechanic I emphatically deny. He is an honest man; he means to do what is right; and in my experience I have found that, in the long run, he does fairly what is right. He will do more, however, when he feels that he is working for himself and not for an employer who pays for his work by the time.

Mr. Fowler.—I would like to add one word. This piece-work system is used in all the repair work upon the road to which I have made reference. If a man does any work whatever; if he only tightens up one nut on a car or locomotive, he has a scale for tightening up that one nut, and is credited on the pay-roll for it.

Mr. J. T. Hawkins.—Mr. Towne, I think, misapprehends the tenor of my remarks, if he understands from them that I impugn the honesty of the American workman or laborer. I by no means intended to convey that the tendency among workingmen of all kinds to minimize the products of their labor comes from anything worse than ignorance on their part. It is more or less a matter of traditional feeling with them that a gain to the employer through their efforts is a corresponding loss to them, and it is this practically unconscious or thoughtless condition of mind on their part which I deprecate, and which I have suggested might be replaced by more logical action through educational means in their own organizations, looking to making them the most productive, instead of, as now, aiming to give the employer the least possible for his money.

My remarks, in this connection also, should be taken in a more general sense than Mr. Towne has seemed to have understood them; but, while the average American workman is honest in his intentions to do justice to his employer, there are doubtless many exceptions which only go to establish the generalization. There are, and always will be, dishonest American as well as other workmen; and there are also many who perfectly understand that it is to their own interest to render the fullest equivalent in effort made for compensation received; but fortunately in the first, and unfortunately in the last case, these both are comparatively rare, and the rule is, as stated in my remarks, that the average wage-earner thinks (and honestly so, perhaps), that it is his gain that he produces the least for a given amount of wages received, or in a given number of hours worked; and any means which can be devised which shall reverse the condition of his mind in this respect will be productive of more good for him than any possible scheme to effect a reduction of the employer's profits; which latter is, in almost every case of antagonism between capital and labor as represented by its organizations, if not the main and direct object in view, at least the result of the success of the laborers' organizations in such contests.

Mr. Towne.—After noting Mr. Hawkins's comments, I must still express my dissent from the views he holds as to the average disposition of workingmen in regard to their obligations toward their employers. Admitting, however, that they hold such views, the remedy for the evil resulting therefrom lies in the direction of interesting them in the results of their labor. This end can be, in great part, accomplished by the piece-work system, if so arranged that each man's earnings are controlled and affected by the work accomplished by him individually. The final and most perfect solution of the case, however, lies in the direction of "profit-sharing," or its equivalent; that is, a system in which the interests of the employer and employee, not only in the results of labor, but in the entire economy of production, are harmonized and united.

In closing the debate, I want to call attention to the fact that the two discussions of to-night are practically in the line approved by the Society at the meeting in Chicago last spring, when it was proposed that economic questions should be introduced, the suggestion being at first that they should be taken up somewhat as a side-issue. By practically unanimous action of that meeting, it was decided that such papers are as pertinent to our work as any others, and should be treated in the same manner. The two papers of to-night both relate to economic subjects, and it has been interesting to see how the Society considers their value and interest to compare even with those of our usual and ordinary papers.

transferred the drawing to the asphalt by means of red transfer paper; then traced the lines with a needle-point and had an acid bath to do the rest of the work. In other instances we coated only those parts of the metal which were to form the templets, leaving the spaces blank on one side, and used the acid bath as before.

Mr. Oberlin Smith.—I also have used the etching method for thin templets, but I find that it is difficult to eat through, if the metal is of much thickness. Another method I have used is a punching press with a pair of shear-blades set in it, of which the vertical section is something like my thumb nail, both for upper and lower—that is, the cutting edges are both considerably convex. Thus, when the two come together, each goes barely through the metal for a very short distance. In following curved lines, you can by this means get through without mashing the metal, by taking a series of contiguous cuts all along the lines. A third method, and one which is perhaps the most successful, is to use a jig-saw. I once rigged up one of those little toy jig-saws with a very fine watchmaker's saw-blade, and had perfect success in jigging out thin brass plates—just as you would a piece of board.

Mr. P. A. Sanguinetti.—I have had occasion to cut templets from thin metal, and have used very much the same process as Mr. Smith, with a little alteration. Instead of cutting directly on the metal with the jig-saw, I placed it between two pieces of poplar boards about $\frac{1}{8}''$ thick, with the design to be cut glued to the upper side of the top board. By this method I was enabled to produce the templet without any burr on the under side, and perfectly straight. I used sheet zinc.

Mr. Oberlin Smith.—I accept the amendment; it is a very good one.

No. 240.—29.

What is the best way to build annealing furnaces for small gray iron castings?

Mr. J. M. Dodge.—I would like to state to the Society that after considerable experience with annealing furnaces I have found that the reversal of some of the accepted methods has been advantageous. In the first place, suppose the castings are in a pot two feet high; I would have the top of the furnace five and a half or six feet high. Instead of taking the heat in at the bottom, I would bring it in at the top, and in the next place I would divide the flues at the bottom so as to get an even draft. An even draft can be obtained with a number of flues very readily with pieces of brick to vary the openings and a handkerchief as a guide to the strength of the draft. With a natural draft—no fire at all—a handkerchief held near one of the openings will be deflected, and then you try the others, holding the handkerchief in the same relative position, and if the handkerchief is deflected too much, place a piece of brick partly over the flue-hole. The advantage is, that you have even action, whereas, if you draw the heat in at the bottom, you will naturally have hot streaks through your furnace. If you take it in at the top and then it leaks out at the bottom as it has to, it makes perfectly even heating all around; and the advantage of having this large reservoir over your castings is simply that you get this soaking action of the heat with perfectly even distribution and very little loss; because your radiation, after all, amounts to very little compared with the amount of heat which you actually use in annealing. If you do lose a per cent. more or less by radiation, it does not make much difference. You gain more by paying proper attention to the damper.

Mr. H. R. Towne.—I may mention briefly an experience of my own in this particular direction. Some five years ago, I built an annealing furnace in which I took the gases from the combustion chamber over the bridge-wall into the heating chamber on top of the pots, as Mr. Dodge proposes. The down-takes into the flue were in the floor, so that the heated gas came in at the top and flowed down and went out at the bottom. It worked well in some respects, but had this defect, that the boxes in the upper part of the chamber were heated more rapidly and to a higher degree than the others. The chamber contains twenty-four boxes, in rows three high, and the upper tier of boxes always showed a different result from those at the middle and bottom. Last spring I altered the furnace by having the bridge-wall carried up to the top of the chamber, and checker-board openings made through it, so that the gas was delivered in a number of small jets through the whole furnace, the down-takes being left in the original condition. We now get uniform work from each pot, regardless of its position. I am at present designing a new furnace, but am going to make the outlets correspond with the inlets, the areas of the different openings in the checker-board bridge-walls being proportionate to the intensity of the draft at the top and bottom of the furnace, and I think that this will give a still better result.

Mr. A. H. Emery.—One excellent point in regard to the method of carrying the draft which Mr. Dodge has mentioned is this—that in carrying off at the bottom he is all the time taking off the coldest gases instead of the hottest; in this consists much of the economy of taking off the gases at the bottom.

No. 240.—30.

Which do you prefer, a feed-pump or an injector?

The Chairman.—I presume the point of the inquiry is to know which is the better to adopt for ordinary purposes. For any important steam plant the question is already settled pretty conclusively. But it is not always clear to persons who have occasion to buy small steam-engines or boilers whether they need a steam pump or injector, and I take it that that is the point of this inquiry.

Mr. W. C. Kerr.—We furnish both of these articles in connection with steam power plants. Our experience has been that we furnish more pumps than injectors. It is also our experience that sometimes engines and boilers are put into incompetent hands, and that in such cases the feeding will be better done by an injector. It is merely a fact without any reason why. I think one reason why, is, that small pumps are generally so cheaply built. We have had occasion in buying small pumps for several purposes to ship a pump from the factory to the repair shop and have it repaired first and then send it out for use.

Mr. C. M. Giddings.—It has been pretty hard for us to decide which we like best; we have now come to putting on both. In our electric light plants we put on both as a matter of caution and necessity. We find it difficult to get a small pump well enough made for us to be willing to pin our whole faith to that alone. As to the amount of trouble we get with injectors and pumps, it is pretty nearly divided after the observation of several years. We cannot find any kind of jet apparatus or pump which will give us less trouble than the other one. We cannot trust either alone, and the tendency seems to be pointing towards the adoption of two. We now prefer two injectors of the simplest possible type to run, believing that we are escaping much of the trouble which is common to the average cheaply made pump.

Mr. J. W. Cole.—Some years ago, I favored putting in the hands of the ordinary engine driver and fireman, for a boiler feeder, a small steam pump, and to have it a plunger pump, simply for this reason: that if a piston in the water cylinder began to leak, the man who was firing the boiler and runing the engine frequently discovered it first by having low water in his boiler; but if he had a plunger pump, there would be a spurt of water coming out, and he would find it out before his boiler had low water.

Mr. W. B. Le Van.—About ten years ago, I made a series of experiments in regard to the economy of belt-driven and steam pumps and also injectors. With the original Giffard injector, which was first made in this country by William Sellers & Co., I found that the excess of cost with an injector, by actual test, was ten per cent. over that of the belt-driven pump, and over that of the Worthington pump, in good condition, it was twenty per cent. The advantage of the injector over the belt pump was, that it could be run at any time. The injector, by partially acting as a heater, and heating the water up, proved the better one of the two, although it was a little more expensive. The steam pump's loss was due to the fact that the exhaust was not utilized for heating as with the injector.

No. 240.—31.

What is the best way to separate grit from grinding rooms and prevent its dissemination in yards and shops?

Mr. J. M. Dodge.—I would state that we exhaust all the dust from tumbling barrels and emery wheels. The first method which we used was to take pipes from the barrels right to the exhaust fans, but now we have introduced this change, that between the fan and the barrels we built a room thirty-five feet long and ten feet square, and made it air tight. I brought the pipes into the room, and from a crevice on the other side I brought a fan-shaped pipe to the exhaust fan. The dust would come into that room with just as much velocity as though it was a small pipe, and when it got in there it would come to rest. It would settle there, and then the air would go on out through this long slit. The consequence was that the fan did not wear out, and what little dust went past that was caught by running over a series of boxes where there were alternately hanging partitions and upright partitions, and finally the current was blown on the surface of water.

Mr. C. M. Giddings.—I would say in regard to this subject that at Massillon, our people, after having been guilty of manslaughter for several years, determined to adopt some method of abating the dust nuisance. We covered each one of our tumbling barrels and grindstones with boxes. From the exhaust fan we carried it outside of the building to an eighteen-inch sheet-iron pipe leading out into a vertical chimney (Fig. 104), of about eight feet in diameter, the total height of which would be in the neighborhood of eighteen to twenty feet, A B representing the ground line. The inlet pipe was brought into the chamber a little below the center and opened at a right angle. A half-inch water pipe with a pressure of about forty to fifty pounds per square inch, having a cap at the end, with an ⅛ hole in it, opened directly against a small plate under the apex of the cone. This served to divide the water into a spray throughout the entire cross-section of the chamber which moistened the dust. The result is, that a very great proportion of it falls to the bottom in the shape of a thin mud which can easily be washed out with a hose. The portion of the dust not moistened sufficiently to be precipitated was caught again in the filtering chamber (C), there being two perforated plates, one above and one below, as shown, filled with coke and similar substances from the size of a man's fist down to the size of an egg or larger, with some fine material mixed in among it. This should be renewed in about sixty days. In connection with this we were enabled to get rid of nineteen-twentieths of the dust, the balance,

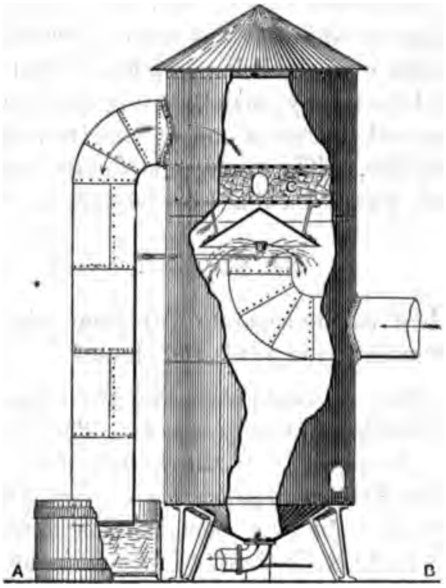

Fig. 104

which went through the escape-pipe, being so slight as to form no objection whatever. That has been running successfully for the past eight or ten months, which is due in a very large measure to our worthy member, Mr. Charles Bauer, of Springfield, who gave the suggestion. I have used it very successfully, so much so that I can well recommend it to others.

Mr. W. E. Partridge.—Excelsior has very successfully replaced, on a small scale, the combination of coke and other materials in the upper part of the chamber. The excelsior is usually placed on wire gratings, so as to keep it from packing and to enable it to be moistened uniformly. The air is forced through it as in the case mentioned; the great extent of moist surface acts in the same way that prepared cotton does in sifting germs from atmospheric air. It takes up the dust and holds it very closely, and where it has been used in very dusty air, and a large quantity going through, a comparatively small amount of excelsior has done very much good. The excelsior does a greater amount of separating than would be expected by one who has had no previous acquaintance with it. The use of the material in some such way may be valuable in the construction of separators in a large way.

Mr. Henry R. Towne.—Both methods which have been described embody a common feature which I have used for a long time with good results, namely, that of a settling chamber, a great enlargement of the pipe with the proportionate decrease in the velocity of the current, thus allowing more or less of the material to settle. I have used this plan in connection with emery wheels and with buffing wheels for various kinds of metal. There are still difficulties remaining, however, which make it no easy matter to extract the whole of the metal from a current of dust-laden air.

Perhaps at another session I will have something more to say on this subject, supplementing what Mr. Giddings has presented.

No. 240.—32.

How may drawings be made and the contraction and expansion of the paper be avoided?

Mr. Oberlin Smith.—I have found by experience that the best way to prevent drawings from expanding is to let them expand, and the best way to prevent them from contracting is to let them contract. If you do not let them, they will do it anyhow. (Laughter.)

The next question is, how to get rid of the *evils* due to the expansion and contraction. I know of no kind of drawing paper—I wish there was some—which is not very materially affected by the hydrometric condition of the atmosphere. I have recently had my attention called to this matter in a very practical way by having to deal with gangs of dies for cutting playing-cards, fifty-six at a time, from 22″ × 28″ paper. In thus cutting cards out of such large sheets, it was found that a slight difference of dampness in the paper while lying piled in different parts of the room—or in different positions in the pile—some of it with air blowing on it from windows, etc., made a difference of about twenty hundredths of an inch between the largest and smallest of the various sheets. In this work they print both sides of the cards before cutting, either from a stone or from a relief plate; then they pile them up and get them as nearly of a uniform dryness as possible. If, however, they are not cut perfectly true by the printing, there is a nice chance to make money by players who watch the backs of the cards, a fact with which you are all doubtless practically familiar. (Laughter.) Cards which are cut unevenly with the printing of the backs have to be sold as "seconds" and "thirds," at a low price. The way decided upon for the case in question was to sort the paper into four or five different grades, just before cutting, and then bring it to one gang of dies for one degree of expansion and to another set of dies for another degree, and so on—using five gangs of dies varying by about five hundredths of an inch from each other in total length.

Many years ago—I think over twenty—Professor Webb and I, who were then in partnership, started a system of using cross-ruled paper for drawings. This is, of course, frequently used by civil engineers and others for profile work, but we got up our system for ordinary machine-shop practice. I have used it ever since with great succcess, and would not be without it now, on any account. My paper is ruled in inches and eighths usually, the latter marks being the finest, and all of them in a pale pink color, so that at a distance they are not noticed enough to disfigure the drawing. This is very convenient for quarter-scale or for eighth-scale drawings; or it can be ruled to order to suit any particular kind of work. The measurements do not get wrong; you do not have a number of errors added together in a long dimension—I mean in cases where the drawings lie about a good while and have not been "figured up." And this ruled paper is of great advantage in laying off work rapidly, especially in symmetrical work, where any one of the red lines running through the paper can be selected as an axis, and work laid off either side of it by the eye, without having to measure. I think for ordinary laying out purposes, an average of half the time is saved. This is an important advantage, in addition to its merits in regard to expansion and contraction. I would not make scale drawings on plain paper at all. It is, moreover, not only useful for permanent scale drawings, but for sketching free hand, approximately to size.

A Member.—Do you know if there is such a paper in the market?

Mr. Smith.—I have not been able to find any, except some that Queen in Philadelphia had, ruled in tenths, which he charged fifteen cents a sheet for, where it costs me about six. The paper I use is the heaviest Western ledger-paper, and is sold at $50 a ream, in some very large sized sheets which I have cut to 24″ by 36″. It is strong and tough; I had it ruled to order and sent a draughtsman to the place where the ruling was done to superintend the work. He took steel scales with him and spoiled several sheets of paper until he got the rulings right. I intend some time to make a plate by which I can print the lines accurately. I attempted to have it printed by the steel-plate process; either sinking the lines in a steel or brass plate, or in a lithographic stone. I finally decided on the latter, but was told by the lithographic printer that the paper would shrink nearly a quarter of an inch, on account of the exceeding dampness necessary for the printing. I think there ought to be some good relief plates made to different scales, for dry printing, by somebody, at any necessary expense. These should be accurately and carefully made, and a paper printed and put upon the market for the benefit of all machinists. It would be a very valuable boon, I think, to all those designing people called draughtsman.

Prof. J. B. Webb.—As Mr. Smith has used my name, I would like to say something on this subject. In giving instruction in technical schools, I have felt the necessity of using cross-section paper in a variety of ways, and believe it is desirable that the student should commence to use it at once, and learn the many uses which it will serve, not only in making drawings to scale, but in working all sorts of graphical problems, such as getting the strains on a bridge, where we wish to measure the results to scale. I found no paper in the market which was at once accurate, cheap, and in sheets small enough for class instruction; I therefore got up a ruling-machine of my own, and put it in charge of a student, and so got along for a number of years. The paper was ruled in ten per inch squares with a set of accurately made bookbinders' pens, kept for the purpose; but it was not as good as I wanted, and I have therefore had engraved, for use in this Institute, a relief plate as accurate as could be made; to enable you to judge of it, I will pass some of the sheets around. You will notice that the lines are numbered along the edges, so that I can give a definite problem to a class; I can say "Draw a bridge; put one abutment at 10-23, and the other at 25-23," etc., etc., and so I can lay out the problem in such a way that when the student works it he will not find that part of his drawing runs off the paper. This paper can be furnished for two or three cents a sheet. There is another size twice as large as the sheets you are examining. When we get out an edition, at the same time, and on the same paper, we print a few sheets with darker ink, and these are cut up for scales. I was led to this by finding that the paper scales in the market would agree neither with wooden scales nor with any paper. I found also that if we made the scale of our paper half-inch, some students would make their drawings partly with a paper scale and the rest with a wooden one, if it happened to be at hand, and tell me they didn't know that it would make any difference. Students will make all the mistakes they can, you know, so as to find out something. Now, I did not want to be troubled in that way, so I made the scale of the paper half way between the centimeter and the half-inch. The smallest division, therefore, is slightly larger than a millimeter, of which you can estimate tenths, and thus get easily as great a degree of accuracy as is practical in working on paper.

Mr. W. E. Partridge.—There are a good many more advantages in paper of this kind to the advanced engineer than are seen in first taking up the paper. Two years ago, I had occasion to ask a draughtsman to make me a scale drawing from a machine. The first thing, of course, was to make a free-hand sketch, and then put the dimensions on it. He had started and got an elevation partly finished. I found that his estimate of four or five days' labor was absolutely correct. I felt perfectly satisfied at the price I was to pay for the job, but I did object to waiting so long for my sketches. I therefore obtained some ordinary cross-section paper, and suggested that he make a free-hand sketch to scale. The result was that, instead of five days of drawing, the first set of drawings were finished in three days, and were accurate enough for the engraver. If the professor would have the paper printed in Prussian blue, a drawing made in India ink can be off, and then accurately ground, would shrink so differently that the roll would be all ridges and hollows in a very short time. The only remedy was to be very careful that the disks were placed with the lengthwise direction of the sheet all one way, so that when they contracted through, the roll was not round, it was smooth on the surface.

Mr. L. Herman.—As an improvement on Mr. Partridge's method, I would suggest the use of the white of eggs instead of glue. This is spread uniformly on the back of the paper after beating it to a foam, or dissolving with a little ammonia. The paper is then stretched on the board and allowed to dry. While the sheet so treated will adhere firmly to the board for any length of time, it can, when desired, be easily removed by lifting it from one corner.

Mr. W. B. Le Van.—The difference of shrinkage along and across the sheet is due to an unequal tension of paper on the drying rolls. A friend of mine found that a paper machine would vary the lengthwise shrinkage by increasing or decreasing the tension of the sheet in drying. By carefully manipulating the tension, he found that he could make the paper shrink on an average nearly equally in both directions.

Mr. A. H. Emery.—I have been using, for a number of years, a paper called linen woven paper, made by Byram, Weston & Co., of Dalton, Mass. The size which I have been using is 31 inches by 23. I use this on a white pine drawing-board, and I find that my white pine drawing-board will shrink, in a year, several times as much as the paper will. The paper changes itself but very little. It will change, from time to time, between morning and noon, and noon and night, as much as it does change in the whole year. If I take the Whatman papers, we have a different experience; the drawings figure up wrong. They shrink and change dimensions greatly. But from the paper I speak of I can take off measurements with the scales after years of preservation with accuracy.

Mr. W. O. Webber.—I would like to say that we have been using the same paper which Mr. Emery speaks of in our shop for nearly four years, and I can corroborate exactly what he said. Besides, it has the advantage that we are able to make blue prints from it without making any tracing.

Mr. C. T. Porter.—I would like to ask, Is this matter of so much consequence as it would seem to be from the discussion? As all drawings are or ought to be completely figured, and no direction, and what we have done began in connection with our experiments on gearing, to determine the power required to drive a 48″ lathe, so as to compare the efficiency of spur and worm gearing. In making those experiments, I noticed a peculiar phenomenon, which seemed altogether paradoxical, but which was nevertheless repeatedly confirmed, namely, that when running at full speed, it required more power to drive the lathe empty, with the cone disconnected and loose on the spindle, than it did to drive with the back gear and the spindle thrown in. In the former case, the belt pull on the small step of the cone 14″ diam. was 27.5 lbs., and in the latter 25. 8 lbs. The face plate and spindle were very heavy, and the reduction was about 38 to 1. The only explanation I can suggest is, that the extra work of driving the back gear and spindle was more than offset by the improved lubrication of the cone pulley due to the revolving spindle. A similar phenomenon was again noticed in the machinery for driving a traveling crane, in which a cut-off was used to stop the motion of a long train of gearing allowing the first set of gears to stand upon a revolving sleeve when it was found that the friction of the sleeve alone was very often sufficient to start up the whole train of gearing. The experiments on the lathe were continued to determine the power required for cutting various metals and the relative amount o power with different forms of tools, but I do not know whether tha question is in order.

The Chairman.—We should be very glad to hear from you, Mr Lewis, on the subject.

Mr. Lewis.—The power required to remove a given amount o metal depends very much on the shape of the cut and the shape o the tool used. If the cut is very nearly square in section, the amoun of power required would be a minimum. If it is very wide and thin it would be a maximum. The amount of power required to take a cut depends upon the sharpness of the tool and upon the form o the cut. If it is comparatively square in section, if the feed i about equal to the depth of the cut, it is not a very serious matte to have a dull tool. The metal is broken away from the head o the tool as though it were a wedge, and the edge of the tool hardly reaches the metal at that point. It piles up on the edge of the too and forms a wedge. The resistance per square inch in the meta varied in these experiments from 700,000 to 180,000 for steel; cas iron, about one-third.

The Chairman.—That is, per square inch of section removed?

Mr. Lewis.—Per square inch section removed. I think the best form of tool is one in which the principal amount of cutting is done in advance of the point. The point ought to droop, so as to split off the heavy amount of metal and leave only the light amount that remains for the point of the tool. The dullness of a tool affects but little the power required with a very heavy cut. It affects it with a light cut, but not so much with a heavy cut.

Mr. A. C. Hobbs.—I do not know that I can give you anything very scientific on this question, but I can give you some data from my own experience as to the power required to turn modern American machine tools. We have an engine with an eighteen foot fly-wheel, on which are two loose running belts, each three feet wide. Around through the factory I have belts which I know will drive any machines that we have to work. I find that very satisfactory. I think if the gentlemen put on their belts wide enough, and have them good, that they will find all their tools cut right. (Laughter.)

Prof. Gaetano Lanza.—I have at home a few data which bear upon one phase of this question. Last year there were two of our students who took up as an investigation the question of the pressure on different shop tools used in a lathe and planer. The lathe and planer were of medium sizes, and, of course, the chips were not very heavy, and they weighed the presssure of the tools, taking a number of different shaped tools. I merely mention it so that if there is any one present who is interested in these data I shall be happy to let him see them.

Mr. T. S. Crane.—The power required I think could be readily deduced from the speed of the belt and the width of it. It is very well known that some lathes will not run with a two-inch belt; you have to put on a three-inch belt. So it seems to me that any one who is interested in determining the power required to drive different sized lathes already in operation, with maximum resistance, could ascertain that readily by ascertaining the speed of the belt used and its width.

Mr. G. H. Babcock.—I do not suppose that any of us have experimented sufficiently to answer this question with the degree of accuracy desired in these discussions. The older class of engineers have been in the habit of depending on the "rule of thumb" a good deal, and have mostly used guess-work to determine how many horse-power to put in to drive a certain machine-shop. Those who have made investigations find that the power required to drive the tools in such a shop is very small. I have driven a good-sized machine shop with, I should think, not less than twenty men at work, witl two horse-power. I have a sort of a rule for general work, whicl of course has to be varied according to the kind of work done an the character of the tools. At an average in ordinary machin work we may calculate roughly that one horse-power is sufficien to drive machine tools necessary to keep ten men at work. Thi does not necessarily, however, include shafting, engine, etc., no blowers for foundry work.

Mr. A. B. Couch.—The question of how much power is require to drive modern machine tools is about as indefinite as any tha could be put. (Laughter.) What a machine tool is, is slightly i doubt. What a modern one is, is very much in doubt; and wha a modern machine tool is required to do, is still more so. I appears to me, at least I am in the habit of regarding the matte in that light, that the power which can be profitably and properl supplied to any machine tool should be measured by that whicl can be transmitted to it by the belt by which it is driven unde ordinary tension and under ordinarily favorable circumstances that the first thing that should happen, in case too much wer required of it, should be the slipping of the belt. That woul require a separate answer for each individual tool. I cannot see ho it would be possible to arrive at any definite statement of th power required to drive, for instance, a given number of planing machines or lathes. For, in business of some kinds, all of then will be in nearly continuous use and to nearly their full capacity while in other cases, the duty demanded of them would be light and perhaps intermittent.

The matter of drilling can be considered somewhat mor definitely. For instance, if the product of the diameter of a pulle in inches, by the width of its belt, by the ratio of its velocity t that of the spindle, be about sixty, there will be belt force sufficien to drill one-inch holes in steel. And that product should var about as the squares of the diameters of the holes, since the meta removed at a revolution of the drill thus varies; and when th velocity of the drill appears as a factor, it will be found that th velocities being nearly inversely as the diameters, the power required will vary about as the diameters; and though the force involved in drilling cast iron is less, yet, a greater velocity bein admissible, the power actually expended remains much the same So that, the work intended for a drilling-machine being known the required power may be stated very approximately. But as for answering the question, What amount of power is requisite to drive modern machine tools, I give it up. (Laughter.)

Mr. Wm. Kent.—Mr. Babcock, I think, gave an instance where twenty men in a machine-shop required two horse-power. I have an instance in my own experience where the power required was only forty times as much as that. There was only one workman, and the tool was an emery planer. When the steam pressure in the boiler registered sixty-five pounds, the planer would run; when it got to sixty pounds, the planer would stop by slowing down a four horse-power engine.

Mr. W. H. Doane.—I think we ought to have the maker's name of that first engine, as well as the manufacturer of the tools, placarded where we all could see it, and order a few when we get home.

The establishment with which I am connected, I regret to say, have not yet been able to secure such results. I do not think it possible, with two horse-power, to run any shop equipped with modern iron tools, and do the work sufficient to give employment to ten men, or the amount of work every first-class shop is expected to produce in a certain length of time with its tools. It may have been done twenty-five years ago when one eighth of an inch cut in wrought iron with a planer was considered unusual. With modern first-class tools, we are not satisfied with this limited amount, and cuts even in steel are made in some cases now from one to two inches wide at one cut. To do this requires power. The size of the belt and its amount of slippage are not the only conditions.

It is well known that, with two tools of the same size, but of different designs, with the same work given to each to do, the result is different. One requires more power to displace the same amount of metal than the other. You ask, why this is so? It may be accounted for in the more perfect construction, as well as better adaptation for the work which it has to accomplish, and which gives it greater ability to perform the allotted work.

The works of J. A. Fay & Co. use fully one hundred and fifty horse-power to drive their tools, and with this power the productive ability of the tools is about sufficient for furnishing the work for four hundred men only, and I think these results about as good as the average of best shops.

Now, while I am much gratified to hear the report given of the success of the two horse-power engine, I am also so much surprised to hear that there are shops where two horse-power is sufficient to run tools sufficient to get out the work for so many men, that I would like to secure the receipt for the process and put it into application.

Mr. Wm. Kent.—I want to placard the name of the man who built that four horse-power engine. It was Mr. Babcock. (Laughter.)

Mr. Babcock.—Mr. Chairman, you will allow me to answer the question of Mr. Doane. The engine which drove the machine-shop I was speaking of was a Wilcox caloric engine, which could not by any possibility do more than two horse-power, and it drove the shop. Of course, it was not doing such work as Mr. Doane has to do in his shop. The power required depends on the kind of work you are doing and the tools used, so that in general you must "cut and allow" more or less if you are going to do close guessing. (Laughter.) For instance, milling tools require more power than lathes and planers, and they will do more work in a given time. I have an example in point. The Singer Manufacturing Company turn out a thousand or more sewing-machines per day. The work is largely done by special tools, very many of which are milling machines, and each man attends a number of tools. There are in the machining department some 2,200 men, and 460 horse-power is used to drive the tools. This is nearly five men to the horse-power, under circumstances where a minimum of men for the work done is carefully maintained.

Mr. A. H. Raynal.—At the works where I am engaged, there are employed about 500 men, working at tools, which are driven by two engines, one of 75 the other of about 30 horse-power. The principal part of this force is, however, consumed in the transmission of power, and in the driving of large traveling cranes and elevators. I have tested the power absorbed by large tools when running loaded, without doing work, and again while cutting, and have found the difference hardly perceptible.

Mr. Daniel Ashworth.—I agree with one of the speakers who preceded me in stating that a small amount of power will be absorbed under ordinary circumstances. When we come to the factor of a fan, then that of course changes very radically the power absorbed in an establishment, and the question brings to my mind a personal experience of my own. In an establishment where it was represented by the maker that a fan would take six horse-power at a certain speed, an engine of fifteen horse-power was purchased with a view to introducing at a future period an electric light system. It was found, when everything was erected, that it was as much as a fifteen horse-power engine would do to drive this fan alone; and careful tests made showed that this fan absorbed $15\frac{3}{10}$ horse-power. This is a very common error as regards rating the power necessary to drive high speed machinery. Wood-working machinery requires, of course, a great deal more power than that of the iron tool departments. In the case of the Rochester Tumbler Company, of Western Pennsylvania, a large fan was rated as needing twenty horse-power to drive it, but when tested it absorbed nearly fifty horse-power—48 and a fraction, I think. The statement and the tests were questioned very much, and immediately afterward an independent engineering company tested that plant again without any knowledge of the previous test, and the test revealed the fact that it actually took fifty horse-power to drive that fan alone. So in estimating the power necessary to drive these tools there is a wide departure from the idea of driving simple lathes and iron-working tools. There is nothing so erroneous, as the masses take it, as the idea of the power absorbed in the driving of high speed machinery and fans. I know of one case of a fan with a 44-inch wheel, driven at 1,005 revolutions, absorbing 44 horse-power. The engine to drive that establishment in all its departments would have very little effort to do it; but throw the fan upon it and it is greatly taxed; so that these points are very important in considering the power necessary to drive machines.

Mr. G. L. Fowler.—I have in mind a little shop I had charge of, about which I can give very accurate data. The engine was a 5″ × 12″, with a cut-off at ¾ stroke. The boiler carried 75 pounds of steam. We had two lines of shafting not more than 20 feet long each, and underneath there were a 42-inch swing lathe, two lathes with 20 inches swing, and two with 10. Besides this, there was a drill and planer and a Sturtevant fan capable of supplying the air for three ordinary blacksmith's forges. That engine would run 125 turns with perfect ease, but right near the engine was a little nine-inch emery wheel belted down from a countershaft, which, when thrown on, would lag the engine every time. We had taken all the power out of the engine we could possibly get, and the addition of a single emery wheel would invariably decrease the speed.

Mr. Arthur Falkenau.—It is possible I may be able to give something that may be somewhat reliable. I had a shop out at Leadville, Colorado, and had an engine there which I indicated at seventeen horse-power, running all the tools, which were three lathes of ordinary size, from fifteen to twenty-six inches, planer, bolt-cutter, emery wheels, and a fan for supplying a cupola; and when taking that diagram, I found that it was all I could do to keep the engine to its work. But as another gentleman just remarked, fans generally require more power than is supposed, so that I stopped all the machinery, suspecting that the fan was probably taking more power than everything else, and I applied the power solely toward driving the fan. It was a Sturtevant fan said to require ten horse-power. I was satisfied that it required more, and I found that my fan took about twelve horse-power, leaving about five horse-power to drive the tools.

It should be said, by the way, that it takes there a greater power and higher velocity to acquire a certain pressure—the altitude of Leadville being ten thousand feet—than it would here, although in that instance I was not able to verify the question. I had occasion in a smelting works to observe that. I calculated in advance that it should take a higher speed and more power than the Sturtevant people rated their fan as needing at the sea level, but the mining engineer thought that he would base his work on the data given by the Sturtevant people. When the fan came, they had to increase their power.

Mr. W. H. Weightman.—From my own experience I find that the answer to this question depends considerably upon a second question as to who wants to know? I had a case some time since in which the dispute was between a landlord and tenant. When the regular measurement was made, all the tools were apparently in good use and working to good capacity—the landlord and his representatives being on hand. The test developed some five horse-power required. Two or three days later, thinking this five horse-power rather small, I made a second test. This time the landlord was not around. I found that with the same tools in operation, same busy appearance and capacity, there was a demand for over twelve horse-power. Upon such experiences as this we may safely affirm that the answer to the question under discussion depends greatly upon "who wants to know?"

The Chairman.—I may mention that in the Yale and Towne Works we use from 125 to 150 horse-power, and are in the habit of indicating it weekly. By reason of the large number of fan blowers in use through the works, our constant resistance, including friction of shafting, etc., is more than one half of the total power used, so that the remainder, which will vary from 60 to 75 horsepower, is all which is utilized in the driving of machine tools. We are employing over five hundred hands; a considerable part of the work is light, but two of the shops are running on heavy work. I agree with some of the previous speakers as to the fact that the amount of power required is often overestimated. Undoubtedly it is due in part to the fact that there is hardly ever a time when all the tools are in operation at once; the work is intermittent with each, and is thus averaged.

No. 240.—34.

What are the best conditions for flying rope for transmission of power? Are there limitations to its use?

Mr. T. S. Crane.—In using such ropes, I have been annoyed more by the vibration of the ropes between pulleys than anything else. If any species of support were put up to catch it, I found it would cut it away. If any one has been able to use such a construction without meeting that difficulty, or has learned how to overcome it, I would be glad to hear it.

Mr. C. C. Hill.—I do not know what the effect would be of what I am about to suggest: but a little simple remedy occurs to me which is sometimes employed on band saws, that is, to put a disk pretty closely in contact with the flying rope with its flat face against the rope and the center of motion of the disk either below or above, so that you will have the whole face of the disk to wear away instead of having the groove in a pulley. It would be continually changing its face, and probably give pretty fair results. It does on band saws I know.

Mr. Allan Stirling.—Some years ago, I arranged to run a small machine and blacksmith shop (using a power hammer, a fan, and a few machine tools) from a mill which was driven by a large waterwheel. Wire rope transmission was adopted as the best thing. It was soon after it had been brought out by Mr. Hirn, and everything was arranged to the very best advantage from the latest information obtainable. Large sheaves with leather filling were used. The whole arrangement proved unsatisfactory and troublesome, and the ropes wore rapidly. I have had more experience of the same kind since, and my impression is that the advantages to be derived from this mode of transmission have been very much overestimated.

Mr. J. T. Hawkins.—In answer to the question of the first speaker on this subject, I would say that I have never seen any experiments tried for the purpose of doing away with the vibrating movement which is generally found in such ropes, but I have observed them very frequently, and noticed that this is a great detriment to their proper action; but it has occurred to me several times in connection with them that the most feasible thing to do in such cases, to prevent that peculiar pendulous motion they acquire, would be to put some guide—either pulleys or some other form of lateral guide—at certain points in the length of the rope between the pulleys. It would break up that oscillating or vibratory movement which is set up in the rope, due to its length, probably conjoined with its velocity. There appears to be no doubt that in such ropes we do get something like the vibration of a musical string; that there are certain nodal points in them, and a certain vibration is set up as governed by the tension of the catenary and the velocity. The methods used to avoid that, I notice, are to place some lateral guiding object near the pulleys, and it invariably fails, by the ropes cutting into the guide. It fails even if they put lateral pulleys at that point. The rope continues to sway at other points, simply because they do not carry the point of contact of the lateral guides to the place in the rope which will break up the vibrations of the rope due to its length, and the condition of the curve and the velocity. My idea is, that it might be advantageous to place one or more lateral guides in such positions as to divide it up in such a way as to prevent the nodal points forming under any conditions of speed to which it is subjected.

Mr. C. M. Giddings.—In the early history of Sibley College, Cornell University, we had the problem of transmitting power by a $\frac{5}{8}''$ wire rope, a distance of eleven hundred feet from a water-wheel in the gorge below, and I remember very well the difficulties encountered, first, in putting up the rope, but more especially in maintaining it, and in keeping it in running order, and in getting it to last a reasonable length of time. I regret that Professor Sweet is not here to give the facts connected with it. It took us in the neighborhood of two or three years to find out how to run that long line and to avoid vibrations, and by repeated splicing and renewing of cables, and finding they would last but a short time, we finally attempted to discover the reason of this by looking into the cause of the vibrations. Some thought it was due to the wheel itself, and the professor suggested the idea of refilling the wheels, then putting up a rest, driving the shaft and its wheel by any outside appliance, and turning the groove (with the wheel in its own bearings) so that it ran absolutely true. We found, in putting the rope on then, that there were no vibrations, and that the wheel was not the cause of the trouble, but it was simply due to a slight tremor in the groove of the wheel, and when *that* was turned true, it obviated nine-tenths of the trouble. I have been able, in the last year at our works, to corroborate this. We have had no trouble whatever in the vibration of the cable in a span of 250 feet, but did have trouble from another source. The cable was used for driving the machinery connected with a pneumatic molding-machine, and we could not afford to lose a day's time on it, and in the extremity of the cable's parting, we put up an inch and a quarter Manilla rope. After protecting that from the weather, we have obtained the most satisfactory results from it. We find that it is flexible, enabling it to endure the short bends made necessary by the location of the driving pulleys. It is still running very successfully, and its life is not determined yet. To be sure, the load is not great, probably not over 22 to 25 horse-power; but I am so happily disappointed with it that I should not think again of putting up a wire rope to drive the same amount of power the same distance.

Mr. Crane.—Were the ropes put in the same sheaves?

Mr. Giddings.—Run into the same sheaves, but with the groove altered.

Mr. Crane.—Made larger, I suppose?

Mr. Giddings.—Yes, sir; the grooves were filled with alternate layers of leather and rubber on end, and turned true in their own bearings.

Mr. A. H. Raynal.—I have charge of three lines of rope transmission, which are all in successful operation. The ropes have a long life, except at the splices, where there is always additional thickness. Projecting fibers are pulled out in passing the grooves of the pulleys, are torn, and the splice gradually wears out. The quality of the ropes employed is of the utmost importance. The maker should know the purpose for which they are intended, as pliability more than strength is the requisite. For a one and a quarter inch diameter rope I find a three-ply Manilla preferable to a four-ply.

Mr. A. C. Hobbs.—When I went to the works with which I am connected, we had a Manilla rope of about sixty feet crossing the yard. Sometimes it was very good, and sometimes very bad. I made up my mind that it was bad so often that I got rid of it. I laid a shaft two and three-quarter inches in diameter across the yard, took the Manilla rope down, put the shaft in, and everything has worked right ever since.

Mr. H. R. Towne.—I happen to have had a somewhat wide experience with rope transmission, and it leads me to concur heartily with everything which Mr. Giddings has said. The trouble with wire rope, in nineteen cases out of twenty, is lack of perfection in the parts. The rope is running at a pretty high speed usually. The wheels are also, of course, and any original irregularity tends to enlarge and increase, and to reproduce itself. That irregularity may come from slight eccentricity of a wheel, from slight want of truth in the other sense—not being a true plane—is still more likely to come from irregularity in the "filling" of the rim, which is always used for wire rope, usually rubber, although wood or lead makes a better filling—the least irregular of any; and these conditions set up a vibration in the rope which is increased, and under some conditions I have seen becomes really terrific, sufficient to pull down the structure, almost, if not checked. The trouble almost always arises from want of balance, or want of regularity, in the parts. That corrected, the trouble will disappear. Of course, where the rope is of great length, it may be necessary to put in intermediate idlers to take up the slack of the rope. Where that is properly done, it tends to check vibration, and should introduce no other trouble. As Mr Raynal has stated, the *kind* of wire rope is a very important factor, and persons proposing to use wire rope transmissions should always study the conditions with the maker of the rope. A rope which is to be used for such purposes is very different from the rope used for standing rigging. A rope of small wire, and many parts to the strand, is a great deal better than one of large wire and few parts. Again a soft core is sometimes used, and is thought to be advantageous—a hemp core instead of wire. Where the use of the rope is in-doors, and it is not exposed either to the weather or to heat, my experience is, that a cotton rope is very much more satisfactory than a wire rope. It involves a much higher speed, in order to reduce the tension to the limit which is practicable with cotton. But the higher speed brings with it ordinarily no difficulties. The counter-shafting, of course, must be well made and well put up; for I have seen a case of rope transmission fail where the parts themselves were all good, but where the framing to which one of the shafts was attached was not sufficiently stable, and a vibration was established in the driving-wheel which was transmitted to the rope. With cotton rope we get rid of a great source of trouble found with wire rope, and also a very considerable item of expense, namely, the packing or filling of the rims of the wheel. With cotton rope we can use cast-iron wheels, the driving and driven wheel having a V-shaped groove in which the rope nips, while the idlers have a round-bottomed groove slightly larger in radius than the radius of the rope, so that the rope bottoms in it and does not nip. The relative speed which practice shows can be utilized for these two kinds of rope is practically as two to five, that is to say, a wire rope running at 2,000 feet a minute can be replaced by a cotton rope running about 5,000 feet a minute, the tension being proportionately altered. The cotton rope does not so readily take up the vibratory motion which is complained of, and the wheels, being entirely of metal, can be made true in the first place, and once true will remain so, except as to the wear of the shafts, and that can be easily taken care of. The matter of splicing these ropes is of considerable consequence, and it is not every rigger even that knows how to do it. Two kinds of splice, which sailors and riggers use, are known as the long and the short splice. The short splice involves a thickening of the rope; the two ends of the rope are partly uncoiled, and are put together and interlocked again—laid up, as it is called—cutting off of a portion of the strands on each end, but the result is a thickening of the rope at the splice. In the case of a cotton rope about five-eighths or one-half quarter in diameter, the short splice would be, perhaps, three or four feet in length. What is technically known as the long splice, on the contrary, is formed by uncoiling the strands of the two ropes, removing an equal portion from each, and putting the two together again and laying up the strands precisely as the original rope was made. This, if done neatly by an experienced hand, can be so well done as to be hardly perceptible, not at all perceptible when the rope is in motion. A long splice in such a rope as I have spoken of, should be not less than fifteen feet, and it should always be resorted to with high speed ropes, for the reason that any thickening of the rope at the splice tends to establish vibration of the rope itself and of the wheels. It involves a serious shock or blow to the wheels each time that the splice passes over them, and that itself tends to disturb the balance of the wheels and causes trouble. So far as my experience goes, where the circumstances admit, cotton rope is more satisfactory than wire, but, with a proper proportion of the parts and reasonable care in maintaining their condition, either can be resorted to with entire confidence. All that is required is good workmanship and proper attention and care.

Mr. Raynal.—Do you use any soaps on your cotton rope—I mean a mixture of tallow and black-lead, or something of that sort?

The Chairman.—Always; we use not tallow, but beeswax, which is much better than tallow. The purpose of any dressing on a cotton rope is simply to prevent the fiber from rising, and also, to some extent, to prevent the absorption of moisture by the rope. This coating is water-proof, practically, and it keeps the interior of the rope in a perfectly dry condition. I have taken a cotton rope which has run for more than a year, which is perfectly black on the outside, looking like a bar of iron, with a smooth polished coat, and on cutting it squarely across, it would show the entire interior of the rope to be as clean as the day it was made, the coat of beeswax and black-lead, with a little tallow, serving to protect the core.

Mr. W. E. Partridge.—At the town of Proctor, in Vermont, ropes have been used largely in transmitting power from central stations over considerable distances, for the purpose of running mills for sawing marble, carrying power to derricks, hoisting and various purposes in and about a marble-sawing establishment and quarry. The distances there were great; the angles were sometimes very sharp, in one case up a hill at an angle of 45 degrees, and unless the rope was completely protected, no amount of balancing which they were prepared to do would prevent disastrous whipping in the winds of the winter and spring. In other seasons of the year they had more or less trouble with it. They transmitted power up a bank from the top of a 75-foot tower, and while I do not know the exact amount, it was the power of a turbine supplied by a penstock six feet in diameter, and a head of water of something like fifty or sixty feet. The shaft, I think, was six inches in diameter; the conditions were rather peculiar; the tower in which the turbine was placed was at the bottom of a ravine, from the top of which the power was to be taken up a bank 275 feet, I think, at an angle of approximately 45 degrees. The bank goes down very sharply. The rope having failed completely, they laid a shaft from the tower up the bank to the mill; it was four or five inches in diameter, had expansion couplings and thrust-blocks in each bearing. It was carried tainly shows that he at any rate has thought of carrying the thing a good deal farther even than we have dared to do on this side of the water.

Mr. W. F. Durfee.—There is one form of rope transmission which is not very generally known; I refer to the form used in manufactories of cordage. There are many "rope-walks," which are one-quarter of a mile in length, in which the whole power for "twisting" and "laying up" the ropes is conveyed from one end of the building to the other by ordinary Manilla ropes. Starting from a driving pulley, or its equivalent, near the source of power at one end of the building, the rope passes to the other end, supported at intervals by grooved pulleys, turning in standards bolted to the floor;—thence returning, it is taken three or four times around a conoidal head, or a series of horns, on the extremity of the driving shaft of the rope-twisting engine, and thence the rope passes to and around a pulley on a weighted tightening frame, and then in its original direction to the driver whence it started. Endless Manilla ropes have been used in the way described with great success for the past fifty years, and it would practically be impossible to run the older form of rope-walk without this method of transmitting the required power. The use of ropes or cords for transmitting power, instead of being a modern invention, is among the oldest known mechanical devices, and was used for that purpose long before the invention of leather belts. The earliest application of ropes or cords as power conveyers is, doubtless, found in the bow-string of the savage races. We then find the cord used in the primitive reciprocating mandrel lathe and in the bow-string drill, followed by its employment in the modern rotating lathe and spinning-wheel, and ropes for the "running rigging" of ships, and for funicular machinery generally, which have been used from the beginning of history. Among the more modern applications of ropes as power conveyors, has been classed the use of moving cables as actual bearers of burdens from place to place; but this, even, is much older than is generally supposed.

In a work published in London in the year 1702,* we find the

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* *Mechanic Powers; or, the Mystery of Nature and Art unveil'd.* Showing what *Great Things* may be perform'd by Mechanick Engines, in removing and raising Bodies of vast Weights; with little Strength or Force; and als the making of Machines or Engines, for raising of Water, draining o Grounds, and several other Uses. Together, With a Treatise of *Circular M* *tion* artificially fitted to Mechanick use, and the making of Clock-work, an

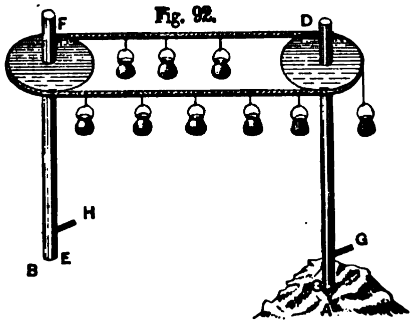

Fig. 92.

cut reproduced in Fig. 92, and the following description: "Let the mountain or hill, or heap of stones be A, to be removed to the place B; to save time in going and returning from one place to the other, as also that the motion whereby the earth or stones is transferred from A to B may be swift, we make use of the following industry: Erect at the foot of the mountain or in its middle, a great and solid wooden column, or piece of timber, C D, and erect such another in B, *viz.*, E F, affix at the top of each piece or column, the wheels D and F, and make hollow each wheel in the circumference, and put about them a great strong rope, extended parallel to the horizon; but if the distance from A to B be great, least the rope should be too much stretched or bent, raise other such like pieces, or columns, in the middle, with their wheels made hollow, as aforesaid, to sustain the rope parallel to the horizon; on the rope thus doubled, here and there hang baskets, which must be so far distant from each other that they hinder not one another; and the ends of the pieces must be so placed that the power applied to the levers G and H may be turned about their centers, for so the whole rope, with the baskets hanging on it, will be turned about successively. Wherefore, if men keep filling the baskets in A, and others unload them in B, the whole hill will be easily transferred from A to B. Where note, that the greater the wheels D and F are, the swifter the rope and baskets will be turned about; which motion about the axis, or piece of timber, being easy, may be accomplished by means of short leavers, that so the motion of the baskets may be greater than the motion of the power about the piece of timber. Besides the saving of labor, and the gaining of time, which is effected by this engine, it hath likewise this conveniency, that if between the two places, A and B, there should be a river or stream, or such like inaccessible, as if the earth were to be

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other Engines. A work pleasant and profitable for all sorts of Men, from the highest to the lowest Degree: And never treated of in *English* but once before, and that but briefly.

The whole comprised in Ten Books, and illustrated with Copper Cuts. By Venteri Mandey, Philomat. London. Printed for Tho. Shelmerdine, at the Rose tree, and Tho Ballard, at the Rising Sun in Little Brittain, 1702.

transferred from a mound, or hill, to the next adjoining fields, and there were a large deep mote or ditch between them, you could scarcely obtain your desire any other way."

No. 240.—35.

What is the practical value of the sand-blast process for sharpening old and new files?

Mr. Chas. N. Trump.—The sand-blast has a very great advantage in the sharpening of either new or old files. As applied to new files, it has the effect of removing the burr which is formed by the chisel, thereby preventing the future breaking of the teeth, and in addition to the sharpening of the teeth it adds about fifty per cent. to the life of the file in use. Re-sharpening in the majority of instances will cause the file to do more work than a new file not sand-blasted; and if again applied to the same file, the second re-sharpening will cause it to do more work than the first sharpening. I have records of files which were tested in Trump Brothers' shop, and in the shops of the Betts Machine Company, and the English records are also in print; I know nothing of the last except in that way. The files which I have tested through the hands of workmen in our own shop (I refer particularly to what is ordinarily known as a mill-saw file) were taken new to finish pieces which were first turned to gauge, merely to put a surface on them. The amount of surface finished was 44¾ square feet. That file was sand-blasted after it was too dull to use, probably at a cost of a cent and a half. The result of that test of the file was 66 feet of surface. The second re-sharpening enabled it to finish 107 feet. It was simply brushed up the third time, finishing 31 feet. The following statement shows the details of the test:

No. 1. New American file (not sand-blasted) finished the surface of five pulleys, 14" × 4" face diam.

No. 2. Worn "Moss and Gambel" file, re-sharpened by sand-blast, eleven pulleys, 14" × 4" face diam.

No. 3. New "Spencer" file, not sand-blasted, three pulleys, 14" × 4" face diam.

Nos. 1 and 2, re-sharpened, did about equal amounts of work on a second lot of larger pulleys.

No. 3 was probably soft.

Files tested in Betts Machine Co.'s shops.

About double the quantity of metal is removed by the same number of strokes under the same pressure by the new file, which is sand-blasted, as compared with the file as it comes from the temper—

ing bath. The process is merely one of grinding. It does not amount to anything more than that.

Mr. H. R. Towne.—In regard to the present subject, I can say that in the Yale & Towne works we have adopted the sand-blast for sharpening files, and have had it in use for some six months with very satisfactory results, but I am not able to state any percentage of economy. There is an unquestioned value in applying it to new files. A new file will wear longer by sand-blasting it before you begin using it. The reason for this is very simple. The edge of the tooth, as cut by the chisel, has a little burr left upon it—a thin fin of metal, and this fin, as soon as the file is put into use, bends down and clogs the cutting edge, just as a thin fin of metal on the edge of a chisel would bend over and dull it. The sand-blast removes this thin fin of metal and leaves a fine, sharp edge. Examining a file which has been used, you find somewhat the same effect—a burr raised on the edge of the teeth; subjecting it to the sand-blast removes this burr. Of course, there is a limit to the number of times that this can be done, as the teeth gradually decrease in depth, and the file becomes useless.

It would be very interesting if any one could tell us from records what the actual economy of the process is, so as to compare that with the cost of providing the apparatus.

Mr. F. W. Taylor.—In the Midvale Steel Company we have had the sand-blast in use between three and five years. During that time, our works have largely increased in size; and our file bills have been less since having the sand-blast in use than they were before. Just what the economy is I don't know. We have experienced a very decided economy in its use, however.

Prof. J. B. Webb.—The explanation which you have made naturally raises the question as to the comparative value of this process and the method of taking off the burr by biting the file with acid.

The Chairman.—That is being done, Professor Webb. I am not able to answer from experience with both; but I can say I looked into both before adopting either, and was quite satisfied that the sand-blast was better to adopt.

Mr. Trump.—I think I can give some points in reference to the acid process. The Singer Sewing Machine Company, at the time the apparatus was put up there for sand-blasting files, concluded to try a process which was introduced by one of their chemists, which was the application of a galvanic battery in connection with the acid for eating away the teeth of the file, and he thought that he had accomplished the purpose so that the sand-blast was thro out. Since that time, a large lot of files was put on the market, it was found that they were files that had been bought as sc from the sewing-machine company; they had been sharpened that acid process, rejected, and sold as old iron. Some sharp n got hold of them and put them on the market as new files.

Mr. W. H. Doane.—I would like to ask Mr. Trump to state w amount of power is necessary to drive a sand-blast machine for sha ening a file up to its full capacity.

Mr. Trump.—I can answer that question in several ways. 1 pressure is of considerable importance; from 65 to 75 pounds v work nicely, 90 or even 100 pounds is better. The amount power required is given by Tilghman in some of his experime in England as being about six horse-power. I have run the sa blast, and run it effectively, from an agricultural boiler out W which was rated at six horse-power. In our own factory we u a twelve horse-power boiler, and we supposed that we were us about eight horse-power for the shop. At noon, which is the ti that I sometimes take for sand-blasting files, I have had to cl the ash-pit doors in order not to have the steam run up on me high. I think Park Benjamin wrote an article for the *Ameri Encyclopædia* in reference to this matter.* He gives four as horse-power required. The application of the blast to the file is intermittent one, so that the steam is not actually being used m than half the time, while the operator is standing by, as the must be tested, and perhaps taken out of the holder more than on

Mr. Doane.—We carry eighty pounds of steam on our Babcock Wilcox boiler, but did not get the best results. We supposed was because the apparatus used so much steam that it reduced p sure below what we desired to carry. Not having a sufficient ste pressure, we estimated it took about twelve to fifteen horse-power work it properly.

I asked the question for information, as we have not been able obtain quite as good results as we had anticipated.

Mr. G. H. Babcock.—If the gentleman will tell us the size opening required with a given pressure of steam, we could 1 approximately how much steam will be used.

The Chairman.—If Mr. Trump will allow me, I can answer t question briefly. In the first place, the apparatus is driven b direct jet of steam. There is no intermediate mechanism, an

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* *Appleton's Cyclopædia of Applied Mechanics.* Page 692, Vol. 2.

small valve, I think, of not over ¾-inch pipe is used for the steam connection, and the valve is opened a very small way the most of the time.

Mr. Babcock.—Does the steam escape through an open pipe or through a small nozzle?

The Chairman.—The steam is delivered through an injector, so that a combined column of steam and sand is thrown on the file.

Mr. Babcock.—But there is a certain amount of opening in the ejector?

Mr. Trump.—A quarter of an inch.

Mr. Babcock.—According to Rankine's ready rule, the steam flowing out of an opening, into a pressure less than two-fifths of the original pressure, will be equal to one seventieth of the absolute pressure in pounds per second, per square unit of area. That is, with a pressure of seventy-five pounds, or ninety pounds absolute, 1¼ pounds of steam per second will flow through an inch of area; with a quarter of an inch, it would be one-sixteenth of that, or in round figures, $\frac{1}{12}$ of a pound per second. This, multiplied by 3,600 and divided by 30, will give the horse-power of steam which that opening would deliver. These figures give ten horse-power, which would be reduced slightly by the friction of the passage.

No. 240.—36.

How should a laboratory of mechanical engineering be equipped?

Prof. Gaetano Lanza.—The equipment of a mechanical engineering laboratory must depend on the more fundamental question as to what are the objects to be accomplished by such a laboratory. Of course, the requirements of different schools will make more or less difference, but it seems to me that the tendency of the age points to specializing as much as we can, and the more specializing we do, as a rule, the better we can do our work. Consequently when I speak of a mechanical engineering laboratory, I do not mean a shop nor a laboratory where shop work is taught. I believe thoroughly in the importance of shop work for the student, but a mechanical engineering laboratory is a separate thing from that. In my own case, I make also a distinction between the mechanical engineering laboratory and the laboratory of applied mechanics which is devoted to testing the strength of materials; nevertheless, what I have to say will apply to this latter also.

I think that the objects of the mechanical engineering laboratory are three. The first is, of course, the education of the students, and the thing of first importance is, that the student should receive instruction in that class of work which, as an engineer, he is very liable to be called upon to perform. He should learn how to make boiler tests, engine tests, dynamometrical measurements, calorimeter tests, and such other work as an expert engineer is constantly liable to be called upon to perform in the industrial world. He must become a careful and accurate observer, be able to get his results, to work them out, to draw his conclusions without bias and without prejudice. The above is of course the first thing that a laboratory must accomplish.

The second object is, it seems to me, to teach the students to make original investigation. Taking up some subject which needs investigation, they should seek to determine what experiments are needed, and then proceed to make them, and from them deduce correct conclusions.

The third object is that of carrying on investigation in the laboratory. Now, in carrying on investigation when you have a large number of students, you have at any rate a large number of hands to use, and can, therefore, make investigations which can not be made well in other places.

The three objects then it seems to me are: first, to teach the student to make such tests as he will have to make when he goes into the practice of his profession, giving him such a drill as will make him a reliable experimenter; secondly, to teach him to make investigations; and, thirdly, to carry on investigations on engineering subjects and publish results. The equipment must be such as to enable us to accomplish those objects. One of the requisites is, that the machinery and the apparatus used should be on a practical scale. It does not do to make tests in miniature, as such tests do not generally give results applicable to practical cases. As to the details of the equipment, this will be determined by the objects to be accomplished, and the means of the institution. If steam work is to be done, there should be as many and as large engines as the institution can afford; there should be, of course, dynamometers, brakes, indicators, gauges, and other needed apparatus, such as will readily suggest itself at once to any of you, and there should be the apparatus needed for the work and for the lines of investigation which it is decided to pursue, for the number of possible lines of investigation is almost infinite. In my own laboratories, we have undertaken several steam investigations; also, investigations on belting, strength of materials, etc. The institution ought to take up just as much as it can do thoroughly, and then it would be very desirable if there were a number of laboratories in different parts of the country, all pursuing investigations in mechanical engineering subjects, and keeping a correspondence with each other, so as to play into each other's hands, and thus by their united efforts to accomplish much more than could possibly be done by one alone.

Prof. G. I. Alden.—It seems to me that, as has been said, the equipment must be such as to enable you to carry out the work you are required to do. I thought, upon seeing that topic, that the engineers here might perhaps make some suggestions which would be valuable to teachers in the schools, by showing what kinds of work they would like to have graduates able to do when they come out to work for them; and seeing that our time in technical schools is very limited, they might aid us by suggesting what kinds of mechanical laboratory work should take precedence of other kinds in the short course. Of course, if the technical schools could keep their students longer, and have time to enter in small classes into investigations, that would be a thing they would like very much to do, but cannot now do to any great extent. If the practical engineers present would say what they would like to have the graduates know of engineering, each one in his own department, it might possibly be suggestive to teachers as to what they should attempt in starting a laboratory, which, I understand with Professor Lanza, is not a shop; it is an entirely different department. These suggestions would be valuable to teachers as indicating what they should do first in steam engineering, electrical engineering, strength of materials, and various other investigations of that kind—and how in importance these subjects are related one to the other.

The Chairman.—In answer to Mr. Alden's question, all that can be said, I think, is that the field of investigation is too wide and too varied, in different shops and under different circumstances, to specify easily what the student is likely to be wanted to do after he has graduated. The main thing is, to teach him *how* to do any experimental work which may be wanted; to teach him how to reason and investigate, and, after all, that is the chief aim of almost all instruction; it is not alone the teaching him how to do the school work, but how to do other work.

No. 240.—37.

What are the best problems for students in mechanical engineering in the last year of their regular course?

Mr. T. S. Crane.—In my experience in carrying on a consulting business and designing machinery for various purposes, I hav frequently had assistants who were graduates of technical school I found that they were constantly embarrassed in attempting t design anything, by not knowing the proportion of various part and it has occurred to me if they could be forced to exercise thei originality in the last year of their course, in making various d signs for various purposes, and having an instructor competent t point out the defects in those designs, they might be helped t think and use their knowledge more usefully afterward. I do n know how long it would take, or how much time could be allotte in an ordinary course; but if the student could be require to design a pedestal, for instance, to design a train of gearing, t design some simple tool, to design the simplest form of turnin lathe, or the simplest form of planing machine that he could thin of, and throw in the strength of the metal in different par according to his own judgment, some one of experience might poi out the defects. It seems to me that a few days would suffice f a student to sketch out some design such as that, and the critici which he would afterwards receive would be extremely valuable him.

Mr. Wm. Kent.—This subject is very much like the one in t thirty-sixth question. It is a very large one. The problems a so many that you can not get them in in the last year. The teac ing of designing, I should say, ought not to be done in the last yea but in the junior year; but, unfortunately, on account of the d fects in our educational systems, before the students get into t technical schools, they do not know enough of drawing, of geom try or anything of that kind, so that you have to spend the first t years in giving them elementary instruction in geometry a drawing before they can enter into a course of designing. As M Crane has pointed out, even after they have graduated they ha not got the instruction in designing. We ought to begin down the beginning, in the primary schools, almost, to educate the boys think with pencils in their hands. Starting thus early with el mentary drawing, we may be able to expect that in the technic schools we can teach designing in the junior year. I think th in the senior year we should pick out the problems requiring dif cult calculations, which are most apt to arise in practice. If po sible, I should say that the senior year should be the hardest in t whole course. The use of the indicator, the use of the brake, the te of boilers—those problems which require thinking power, should be the work of the senior year, so that the student would get in the senior year those things which he is most likely to get in his earlier practice. In the colleges there are certain facilities offered; there are dynamometers and indicators and testing-machines of all kinds, and mechanical laboratories, which, after a man goes into the machine-shop, he does not find. So, I would say, give him in the senior year those things which he ought to have as an engineer, but which he is not likely to get in the first ten years of practical life afterward, until he has leisure to devote himself to some of these problems which can be best thought out in the senior year in the schools.

Prof. Gaetano Lanza.—This subject is a great deal broader than the preceding topic, because it opens up not merely the laboratory question, but the question of design and the question of the studies of the senior year. I will first say a few words in reference to design, which I consider to be a matter of very great importance, and I will state what we are doing in that regard at our school. The subject of design is introduced at the beginning of the senior year, when the students devote to it eight hours per week. At that time, they have had instruction in mechanism, in thermo-dynamics and steam, including steam boilers, and, of course, have had a considerable amount of drawing and sǒme laboratory practice, and they have also had the mathematical side of the strength of materials. They begin usually with the design of a boiler. Professor Schwamb, who has charge of that work, is with them most of the time, passing from one to the other, criticising their work and showing them where the designs are faulty and might be improved, making them realize the fact that in practice, when a man has made a drawing, he must not hesitate to rub it out and begin all over again. When they have made their boiler designs, they proceed to other designs, the character of which is intended to be practical and not of too great extent. We should not approve of giving a design on such an extensive scale that they could not make a thorough study of all parts. By way of example, a design that has been used by us is one for the main shaft of a mill from which power is to be taken off at different places, the student being required to consider that shaft with reference to its strength, to the friction of the bearings, etc. Another example of a design which may possibly be given this year is the following: Make a thorough and careful design of a hanger with reference to strength, bearing.

surface, etc. Then deduce from this one the suitable dimensi for a whole set of regular hangers. Now, of course, we cannot to a very great extent in this kind of work for lack of time; but as the Chairman has so well said, we can succeed in making students begin to think, as men outside have to think, we are doi so much toward preparing them for their future work.

In regard to the other studies of the senior year, I dare not go i details, or I shall consume too much time; I will, therefore, o state two or three general principles. In my opinion, the sen year should be the student's busiest year. They should get much of their theory as they can during the second and third ye so that (although a considerable amount of theory must still rem for the fourth year) they should be in as good condition as po ble to take up and discuss practical questions in the light of elementary theory, and not be troubled at that time to go back study the elementary parts of the theories on which the subj depends. We try to bring the questions of the senior year as n the questions of practice as the advancement of the student warrant, and try to teach him to look at them from the poin view which must be taken by a man in the practice of his pro sion.

Mr. W. E. Partridge.—There is one thing that stands in way of all instructors in mechanical engineering, and that is attempt to impart, at one and the same time, experience, knowled and theory. The student is turned out, and the effort is too of made to put him into the world in such a shape that he can something at once. In other words, there is an attempt mad combine a post-graduate course with one which is elementary one which is intermediate. The student comes out without s ciently understanding the difference between having a chest of w sharpened, properly fitted tools, and a trade. The boys in schools can get very little more than a chest of tools with theor cal directions as to how to use them. To learn the trade, they h got to take hold of the work in the shop, or they must have a v extensive post-graduate course. The boy usually graduates young to be expected to design intelligently, even though he been designing under instructors during the whole of his last y It is not altogether a mistake which those schools make when t abandon to some extent what we think so important—the prac —and say to the boys, We will give you all the theory that need; we will give you all the experimental results and the exp mentation which we can; but for the practical application of these, you must go into the engineering establishments and come in contact with work which is made to sell. If we bear in mind the distinction between designing to sell and designing to please the teacher's ideas, we shall find that the problems to be laid out for the boys are a good deal easier for the teachers, and perhaps quite as valuable to the youngsters themselves.

No matter how well prepared a young man may be in the schools, he must serve an apprenticeship in the shop before he can be valuable. The sooner the shops recognize this and stop asking for impossibilities, the better it will be for both shops and graduates. A higher class of apprenticeship is unavoidable whenever an inexperienced man steps into any department, and well-organized establishments are beginning to recognize this fact, and to see that the schools cannot teach practice.

Mr. G. C. Henning.—I think the remarks made heretofore show that we have two things to contend with: first, the great amount of work; and, secondly, the average age of those who want to prepare themselves for a professional life; and I think that, although our common schools are not as high as they might be in order to allow the graduate to go directly into a technical school, the right course has been pursued by one place of study to help get over this difficulty. We cannot think of improving our public schools throughout the country so as to make the graduates fit for a technical course; so we must do this—call for such requirements of admission that none but those prepared to undertake the work specially laid down in the schools can enter. Of course, it is conceded that before a man can undertake professional work he must be prepared for it. The only way he can be fitted for taking up such work is to get a good foundation for it by devoting all his spare time to examinations of actual work, and studying the principles involved—giving his principal attention to those problems which he will be most unlikely to meet with in the first years of his professional life. I think if we look at what the Stevens Institute is doing, and has planned to do next year, we have the basis for educating engineers so that, when they leave the school and enter upon actual work, they will be well fitted for it. There they admit men not less than eighteen years of age. There are so many to apply that only a part of those who pass the examinations can be admitted, and such are selected as seem to be best fitted, from their general knowledge and the training of their minds, to do the work thoroughly, rather than those who have shown the most book learning; those men who have trained minds are preferred, as only they can do the work well. At that place, in the junior year the young men must go out and see the shops in a great many places, and then describe what they see, and work up special problems—not large problems, but parts of large problems, perhaps, dividing the work amongst several students. In that way, the graduating class obtain actual experience such as they require when they enter upon professional work. They begin to see and think for themselves what is needed to undertake and execute a certain piece of work.

Mr. P. A. Sanguinetti.—I quite agree with Mr. Crane, who spoke in the early part of this debate, as to the desirability of students in engineering being well grounded in the proportions of parts of machinery. I have noticed that young draughtsmen who have had a workshop training give better service, and are more proficient in designing, than those who have graduated from Polytechnic Schools where such practice was not afforded. Generally junior draughtsmen are given the details of machines to work out, and while they are able to produce finely executed drawings, there is some difficulty experienced in getting them to make a plain working drawing which the men in the shop can readily understand. In trying to account for this, I have been led to conclude that, whilst they have paid great attention to shade lines and scientific projections, they lack the knowledge of constructive detail which close contact with the parts of machinery imparts. For that reason, I think that the last course for the student in engineering should be, if not actual practice in the workshop, at least such familiarity with the proportions of parts of machinery as will give him the confidence necessary sooner to become useful to himself and to those parties with whom he seeks employment.

In this connection, I would like to mention an incidental subject which has occupied my attention for some time. I find among draughtsmen the greatest divergence of opinion as to how a drawing should be projected, and the results arising from it are sometimes very serious. Now, as this Society is endeavoring to secure uniform standards in engineering matters, it would not be out of place, in my opinion, if its good offices could be interposed to further the adoption of a universal manner of projecting working drawings. Mr. Coleman Sellers has advocated the idea, which I think a most natural one, that having made a front elevation of a machine, the projection of the end view to the right of the front elevation shoul be shown on paper on the right-hand side of such elevation; but as I have met with some gentlemen who express opinions at variance with this theory, I for one would be very glad if the Society would use its influence toward reconciling the differences.

Mr. J. L. Gobeille.—I think one of the problems should be to design simple machines to do special work. I have found that a young man just out of a technical school is very apt to make a machine with four spindles where two spindles would do better, and I think they should be required to design a tool for a special purpose, in the simplest possible manner, so that when they get in their positions as engineers (it will not probably be in charge of works), they can use those positions as a post-graduate course, and learn the more intricate things in machine design.

Prof. G. I. Alden.—In regard to this matter of machine designing, I think it is the point where all graduates will be likely to be called on for more than they can do. It is where they show their weakness first. At the Free Institute, where I am, in connection with the shop, we do a little work for the students which I think meets this case. Some of their practice is in the draughting-room of the shop, where they take a problem like the designing of some simple tool, and they work and rub out until they get it done, and as the tool is going to be made and used, the work of designing it proves admirable practice. We only need more time to do it; and I know of no attainment which would improve the value of the students, as they go out to work, more than greater proficiency in machine design. The higher work of engineers they may not, as has been said, be called to do for ten years.

No. 240.—38.

Is aluminium bronze corroded by exposure to weather, etc.?

Mr. Thos. D. West.—It seems to be well established that aluminium bronze will withstand the action of moisture and oxygen better than any other known metal, except gold and platinum. Nor does the sulphurous acid gas of the air attack it as it does silver. A very slight tarnish forms over the surface, which does not alter materially the beauty of the burnished surface. It is due more to dirt than to chemical action on the metal. Mr. Eugene Cowles, of the firm which is making this bronze in Lockport, N. Y., called on Dr. Percy, in London, a couple of weeks ago, and the Doctor, during conversation, opened a drawer in his desk which he said had not been opened for fourteen years, and he took out some burnished samples of aluminium bronze, and ordinary brass which had not been protected with lacquer. The bronze was as bright as when placed in the drawer. The brass was black and covered with verdigris.

Mr. P. A. Sanguinetti.—I remember about fifteen years ago seeing in London specimens of what was called then aluminium gold, which was probably the same thing as the metal under discussion. It was made up into propelling pencil cases, the advantages claimed being that the metal would not corrode nor tarnish, and had all the appearance of gold without being as expensive. I purchased one of the pencil cases and carried it in my pocket for several years, and found it was all that the makers claimed for it.

The Chairman.—The opportunity is so unique, and the subject so pertinent, that I am going to ask Professor Thurston to show us a model, now in this room at the Stevens Institute, which I believe he originated, indicating the values of the different alloys of copper, tin, and zinc.

Prof. R. H. Thurston.—The model exhibited by the Chairman is one which belongs to the collections of the Stevens Institute of Technology, and was devised by myself a good many years ago. In making the researches on the copper-tin-zinc alloys for the United States Test Commission, it was found very difficult to express the law connecting the mechanical properties with the composition of the alloy, so that any one studying the results of the work should be able to say, with confidence, what would be the precise character of any possible alloy of the three metals which he might propose to make.

It seemed evident that the only system of collating results which would attain these essential objects was some graphical method. To represent with satisfactory precision, completeness, and intelligibility a series of researches on the character of triple alloys of all desired proportions appeared, at first, a most difficult, if not insolvable problem. A very perfect and most satisfactory method was, however, finally devised:

In any triangle, as at A, Fig. 106, let fall perpendiculars upon the three equal sides. The area of the whole triangle B, C, D, is measured by the product of the altitude, C E, by one-half the base, B D. Draw lines A B, A C, A D, to the vertices of the triangle, thus forming three smaller triangles, the sum of which equals, in area, the original triangle. We now have:

$C E \times \frac{1}{2} B D = A F \times \frac{1}{2} B D + A G \times \frac{1}{2} B C + A H \times \frac{1}{2} C D$; or, *the* sides of the triangle being equal, $C E \times \frac{1}{2} B D = (A F + A G + A H) \frac{1}{2} B D$. Hence, $A F + A G + A H = C E$.

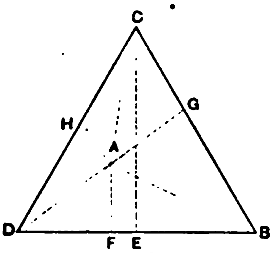

Fig. 106.

But the area of the whole triangle may be conceived to represent a triple alloy composed of the three components in proportions represented by the area of the three several small triangles which together make up its total area. But these smaller triangles have areas proportional to their altitudes, A F, A G, A H; the proportions in which the three metals are combined to form the given triple alloy may, therefore, be measured by the ratio of their representative triangles to the whole triangle in area and in altitude. Then, dividing the height of the large triangle into one hundred equal parts, the altitudes of the small triangles, measured in the same units, will represent the percentages of the three elements in the given alloy.

Every point in the triangle thus represents some certain triple alloy; there is no possible triple alloy which has not its representative point in our triangle.

Let it be proposed to discover what is the strength of all the possible alloys of copper, zinc, and tin: lay out, within the principal figure, a series of concentric triangles, as in Fig. 107, of which the vertices are placed at distances representing ten per cent., twenty per cent., thirty per cent., and so on, from the vertices of the large triangle; select along the sides of these triangles, excluding the exterior figure, points ten per cent. apart for examination. The points taken in the outline of the principal triangle represent the double alloys of copper-zinc, copper-tin, and tin zinc, in proportions also varying ten per cent. They are alloys in each of which the proportion of the third element of the variable triple combination to be studied has become zero. Now, determine the strength of each of these alloys, and, upon the point which represents it in the figure, erect a perpendicular having a

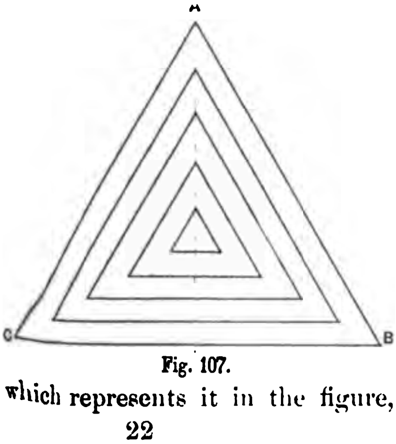

Fig. 107.

height proportional, on any convenient scale, to that strength Having completed this work, we have, upon our triangular base plane, a forest of verticals, each of which is an ordinate of a poin in a surface which may now be conceived to pass through them all Curves of sections, running in any desired direction across this field may now be made, and they will be the graphical representation of the law which connects cohesion and composition in the serie of alloys so selected; just as the surface is representative of th law for all possible alloys of the three metals selected for experiment.

Lines connecting points of equal altitude may be drawn, as o topographical maps, and, on these lines of alloys of equal strength that which meets any given requirement in other respects, as i cheapness or in ductility, may be selected. The same method wil evidently answer equally well in the representation of any othe quality, as the resistance to transverse fracture, to shearing forces to compression, or in the exhibition of ductility, elasticity, or o the values of the moduli of elasticity or of resilience, whether elasti or total.

In many cases, it would be found that, at sharp culminations, i points or lines, forming peaks or ridges in our topography, it would be necessary to take another set of points nearer together, and thu to feel out with greater exactness the sudden changes of resul which follow the operation of the discovered law at such "critical" points or lines.

The result of an investigation, such as has just been described, may be very beautifully exhibited to the eye by making a *model* o the surface thus determined, such as is here seen in Fig. 108. In carrying out these researches, the following plan was found perfectl satisfactory:

Lay out a triangle, as above described, upon a surface of shee brass. At the points at which determinations have been made, erect wires of which the lengths have been made carefully proportional to the ordinates of the representative surface at those points, screwing them firmly, or otherwise fixing them, in their places. When all the wires are in place and are found to be of the exac length required, place bits of board along the outside to form th boundaries of the triangle, and pour in plaster of Paris until the wires are all covered. When the plaster has set, remove the boards and carefully cut away the upper part of the plaster, working carefully down to the tops of the wires, just exposing their points. The surface thus produced is a model of the strength, or other quality represented, of all the alloys.

Mr. M. I. Coster has prepared for the investigator such a model of the cohesive strength of the alloys of the three metals, copper, zinc, and tin, as determined for the United States Board appointed to test metals, as in Fig. 106, and it is one of these models to which the Chairman has referred, and which he has exhibited.

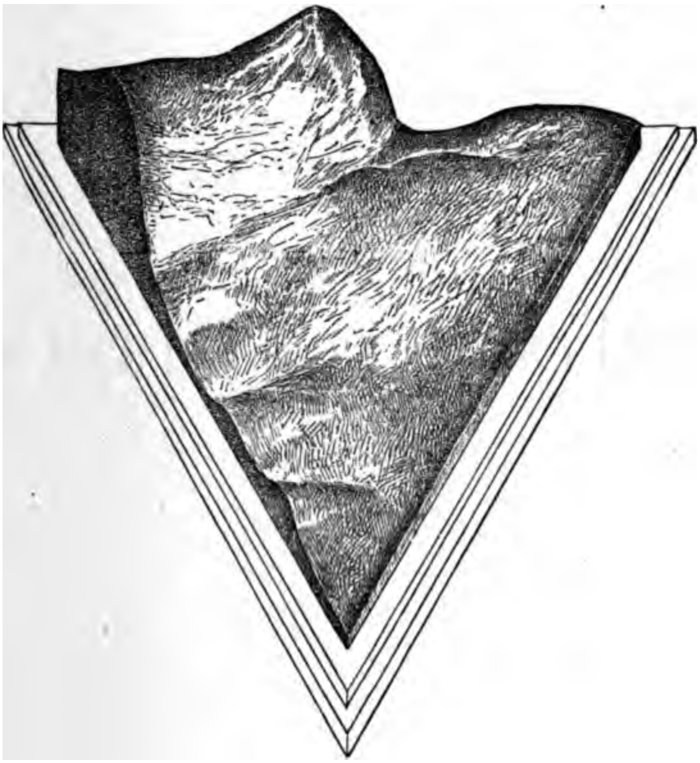

Fig. 108.

Mr. Wm. Kent.—I have a personal interest in this matter, because I did much of this work myself, under Professor Thurston's direction, some ten years ago.

I had occasion, only a few months ago, to have some castings made, and wanted as strong a brass as I could get, and told the founder to make the brass of a composition deduced from this model. I wrote Professor Thurston to ask if he knew a better alloy than that. He advised me to use a little more copper and a little less tin. I told the man to make the best alloy he could out of the three metals—the best tin he could get, the best copper and the best zinc, and in the specified proportions. He said, "If you will take the responsibility, I will do it for you." I told him that I would take the responsibility. He made the castings, and they were very bad castings. They were full of blow-holes and full of cracks; but I finished them down, and, notwithstanding the flaws, they showed 36,000 pounds to the square inch. They would have shown over 60,000, I have no doubt, if they had been cast right.

PAPERS

OF THE

WASHINGTON MEETING

(XVth),

JUNE, 1887.

CCXLI.

PROCEEDINGS

OF THE

WASHINGTON MEETING

(XVth)

OF THE

AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

May 31st to June 3d, 1887.

THE XVth meeting of the American Society of Mechanical Engineers was held in the City of Washington, D. C., during the week from May 31st to June 3d, 1887. The sessions were held in Willard Hall in connection with Willard's Hotel, in whose Parlor Ten were the Headquarters of the Society during the Convention.

FIRST DAY, TUESDAY, MAY 31st,

The opening session was called to order at ten o'clock and was introduced by an address of welcome by Commissioner Wm. B. Webb of the District of Columbia. To this a brief response was made by President Geo. H. Babcock.

The Secretary's Register showed the following members in attendance:

Almond, Thomas R	Brooklyn, N. Y.
Ashworth, Daniel	Pittsburgh, Pa.
Babcock, Geo. H., *President*	New York City.
Bailey, Reade W	Pittsburg, Pa.
Bailey, W. H	New York City.
Baldwin, Stephen W	New York City.
Barnaby, Chas. W	Salem, O.
Barrus, Geo. H	Boston, Mass.
Bond, Geo M	Hartford, Conn.
Borden, Thos. J	Fall River, Mass.
Brady, James	Brooklyn, N. Y.

Brooks, Morgan	Boston, Mass.
Butterworth, James	Phila. Pa.
Capen, Thomas Wells	Stamford, Conn.
Christensen, August C	N. Y. City.
Colwell, Augustus W	New York City.
Couch, Alfred B	Phila. Pa.
Crane, Thomas S	Newark, N. J.
Crouthers, James A	New York City.
Curtis, Gram	Pittsburg, Pa.
Dagron, James G	Baltimore, Md.
Dent, Edward L	Washington, D. C.
Denton, James E	Hoboken, N. J.
Dicey, Elmer C	Chicago, Ill.
Doane, Wm. H	Cincinnati, O.
Downe, Henry S	Fitchburg, Mass.
Dutton, C. Seymour	Youngstown, O.
Emery, Charles E	New York City.
Fawcett, Ezra	Alliance, O.
Forney, Matthias N	New York City.
Francis, Harry C	Phila. Pa.
Fraser, Norman D	Chicago, Ill.
Gilkerson, J. A	Homer, N. Y.
Gobeille, Jas. Leon	Cleveland, O.
Gould, W. V	Norwich, Conn.
Hand, S. Ashton	Toughkenamon, Pa.
Hawkins, Gardner C	Boston, Mass.
Hawkins, Jno. T	Taunton, Mass.
Hazard, Vincent G	Wilmington, Del.
Hewitt, William	Trenton, N. J.
Higgins, Milton P	Worcester, Mass.
Hillman, Gustav	City Island, N. Y.
Holloway, J. F	Cleveland, O.
Horton, Jas. A	Boston, Mass.
Howard, Chas. P	Hartford, Conn.
Huston, Chas. L	Coatesville, Pa.
Hutton, Frederick R., *Secretary*	New York City.
Kent, Wm	New York City.
Kirkevaag, Peter	Youngstown, O.
Leavitt, Frank M	Brooklyn, N. Y.
Loring, Chas. H	Washington, D. C.
Lyall, Wm. L	New York City.
Mackinney, Wm	Phila. Pa.
Magruder, Wm. T	Baltimore, Md.
Mahony, James	New York City.
Manning, Chas. H	Manchester, N. H.
Miller, Alexander	New York City.
Moore, Lycurgus B	New York City.
Morgan, Joseph, Jr	Johnstown, Pa.
Mumford, Edgar H	Omaha, Neb.
Odell, Wm. H	Yonkers, N. Y.
Parks, Edward H	Providence, R. I.

Plamondon, Ambrose	Chicago, Ill.
Porter, Chas. T	New York City.
Ramsay, H. Ashton	Baltimore, Md.
Randolph, L. S	Mt. Savage, Md.
Richards, F. H	Springfield, Mass.
Ridgway, J. T	Trenton, N. J.
Robinson, A. Wells	Phila. Pa.
Robinson, J. M	New York City.
Rose, Joshua	New York City.
Sargent, J. W	Scranton, Pa.
Scott, Olin	Bennington, Vt.
See, Horace	Phila. Pa.
Schuhmann, Geo	Reading, Pa.
Schutte, Louis	Phila. Pa.
Sheldon, Thomas C	Boylston, Mass.
Smith, Oberlin	Bridgeton, N. J.
Snell, Henry I	Phila. Pa.
Sorge, A. Jr	Rochester, N. Y.
Sperry, Chas	Port Washington, N. Y.
Springer, J. H. Sr	Hamilton, O.
Stearns, Albert	Brooklyn, N. Y.
Stetson, Geo. R	New Bedford, Mass.
Steward, John F	Chicago, Ill.
Stewart, W. G	Reading, Pa.
Stiles, Norman C	Middletown, Conn.
Stillman, Francis H	Brooklyn, N. Y.
Stirling, Allan	Yonkers, N. Y.
Stratton, E. P	New York City.
Strong, Geo. S	New York City.
Sweet, Jno. E	Syracuse, N. Y.
Taylor, J. Archie	Wilmington, Del.
Tilden, James A	So. Boston, Mass.
Tompkins, S	Crozet, Va.
Towne, Henry R	Stamford, Conn.
Townsend, David	Phila. Pa.
Underwood, F. H	Tolland, Conn.
Van Duzee, Harold	Bayonne, N. J.
Walker, John	Cleveland, O.
Webster, John H	Boston, Mass.
Weeks, Geo. W	Clinton, Mass.
Weightman, Wm. H	New York City.
Wellman, Sam'l F	Cleveland, O.
Whitehead, Geo. E	Providence, R. I.
Whiting, Geo. B	Washington, D. C.
Whitney, Baxter D	Winchendon, Mass.
Whitney Wm. M	Winchendon, Mass.
Wilcox, Stephen	New York City.
Wilcox, Jno. F	Pittsburgh, Pa.
Wilder, Moses G	Phila. Pa.
Wiley, Wm. H	New York City.
Woodbury, C. J. H	Boston, Mass.

Woolson, O. C.	Newark, N. J.
Wright, John Q	New York City.
Wyman, Horace W	Worcester, Mass.
Zell, Robt. R.	New York City.

Mr. Henry R. Towne of Stamford reported on behalf of the Committee on Uniform Methods of Test, and Uniformity in Test Specimens, as follows :

Mr. H. R. Towne.—Mr. President, I have to report, on behalf of the Committee, that during the past year our Chairman, Prof. Egleston, was compelled by ill-health to resign the chairmanship and at one time it was feared his membership, of the Committee. The Committee induced him, however, to remain a member and saw fit to elect me as Chairman of the Committee in his place. On behalf of the Committee I have to report that a good deal of work has been done, but it has not yet been brought to a point which makes it expedient for us to submit a written and final report. The Committee obtained material for making tests from several manufacturers who kindly contributed it, and distributed that material, together with a circular of request and explanation, to a large number of manufacturers and individuals having testing appliances, with the result of obtaining from them reports of tests, amounting, I think, to fifteen or more in number. Thereupon the Committee attempted to deduce from these returns some satisfactory result. It was found, however, that the methods of making reports differ so widely among different users of testing appliances as to make it impossible to reduce many of them to any comparable form. An attempt was made to do this by Mr. Henning, one of the active members of our Committee, who found, however, that it required a table having something over thirty columns in order to provide for reports under all of the different headings stated by the different experimenters, and that these results were so discordant and so differently designated as to make it impossible to put them all together with any useful result. The Committee thereupon decided to make the endeavor in another way. Additional material was obtained and again distributed, but this time there was sent with it to each party having a testing machine a blank form of report, which the Committee prepared with great care, and which had on it the proper explanations as to how the tests were to be made and the report entered; and the persons to whom the material was sent were requested to make the additional tests and to report them in this manner.

Returns have been received from one-half or more of the parties to whom the material was thus sent, and are very much more satisfactory than the original set of returns. They have not yet been received, however, from the whole number, and we are endeavoring to obtain the missing ones. There is still a great deal of discrepancy in the returns, and the Committee proposes to make a third effort in the same direction by again distributing material, and issuing the form of report which they have prepared, but also giving more specific directions as to the manner in which the tests should be conducted.

This much can be said, that the methods of testing in vogue are exceedingly divergent; hardly any two investigators pursue exactly the same method or make their returns in exactly the same way, and that therefore the work which is being done by one is rarely comparable with the work done by others, and, as a result, the immense amount of time and labor and expense which is being devoted to the investigation of material is largely wasted. It has, of course, its value in each particular case for the particular object in view, but is of little or no use to the engineering profession at large by reason of this lack of comparability. If, therefore, any means can be devised whereby the investigations of the many individuals and firms who are every day of the year carrying on tests of materials, can be brought to a basis which shall make them comparable one with another, we shall all of us be largely the gainers thereby. This work the Committee has addressed itself to and is prosecuting so far as it reasonably can, and hopes by the time of the next meeting of the Society to be able to make a written report embodying the results of its work in a form to enable the Society to understand it fully and to take action thereon.

The final report of the Committee on Uniform Sizes of Pipe and Pipe Threads, was presented by Mr. Bond as follows: *

FINAL REPORT OF THE COMMITTEE ON STANDARD PIPE AND PIPE THREADS.

Your Committee to whom was referred the consideration of a standard for pipe threads, for purposes other than that which is covered by the Briggs formulæ and tables, the consideration of

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* See pages 29 and 48 of Proceedings, Vol. VIII., and pages 20, 311, and 414 of Vol. VII.

which, with results, being contained in the report of your Committee presented at the VIIth Annual Meeting of this Society, have the honor to report the following conclusions:

At a meeting of your Committee, held in New York, Wednesday, May 25, 1887, at 11 o'clock A. M., the advisability of attempting to standardize pipe and pipe thread sizes, for thin brass tubing, oil and salt well casing, and special tube for oil, natural gas, and other pipe lines, was carefully discussed. Your Committee would respectfully report that, as pipe for this class of work is of a character which might properly be termed "special," and is not a product of universal application in the trades, as is pipe which is covered by the Briggs standard, your Committee are of the opinion, that the consideration of the matter, and the action necessary in order to harmonize the various pipe thread sizes for this class of work, properly belongs to the manufacturers and users of this special pipe, rather than to your Committee.

Your Committee would further say, that while the Briggs standard covers in its application the sizes of pipe and pipe threads for purposes in which every engineer, manufacturer, and consumer in the country, is more or less deeply interested, and while uniformity is certainly a necessity for the various special pipe thread sizes for oil and salt well casing, pipe line and thin brass tube, also for the couplings and fittings for fire engines, and water work connections, and that such a desirable result should be brought about with as little delay as possible, still, as already stated, it would seem to your Committee to lie entirely in the hands of the manufacturers and the consumers of such pipe and tube.

The advantage and the convenience of an interchangeable system, which should include the results of intelligent experience, is unquestionable, and your committee would earnestly recommend the matter to the attention and consideration of the manufacturers of this special tube and pipe.

Brass tube for steam and water pressures is now made of sufficient outside diameter and thickness, to permit the thread being cut to the Briggs standard, and is the practice in New York, for all sanitary plumbing and for steam and hot water, wherever brass tube is used for the purpose.

Your Committee would also state that brass and iron fittings for thin brass tube, such as would not permit the application of the Briggs standard thread, are not carried in stock by the fittings trade, brass tube of this kind being special, and used only to a limited extent for elaborate sanitary or ornamental work, and on which tube a finer thread is necessarily cut, often no thread whatever, soldered joints being used instead. This shows conclusively that thin brass tube of special size and thickness is not a product in universal use, and hence, is not to be considered by your Committee in this connection.

In conclusion, your Committee would also state that although there is made and used an enormous quantity of oil, and salt well casing and pipe line tube (referring to the latter only in sizes on which a special thread is cut, other than the Briggs standard), and great as is the desirability of a standard uniformity in sizes, thus to insure practical interchangeability for this product, yet the use of this class of tube and pipe is in a certain sense local, and not of universal application, and does not, in the opinion of your Committee, come within the province of the work of the Society, but should be carefully considered by those whose interests are directly concerned; the members of this Society individually aiding, in every possible way, their efforts to accomplish the desired result.

Respectfully submitted,
FREDERICK GRINNELL, *Chairman.*
GEO. M. BOND, *Secretary.*
B. H. WARREN,
WM. J. BALDWIN,
GEORGE SCHUHMANN.

On motion of Mr. Towne, the report of the Committee was accepted, and the Committee was discharged with the thanks of the Society.

The report of the Council was presented by the Secretary as follows:

The Council would report to the Society, the consummation of the generous intentions of Mr. Stephen W. Baldwin of New York, member of the Society, by which it has come into possession of much of the expert apparatus belonging to the late Mr. John C. Hoadley of Boston.

A detail list follows of the collection:

TEST AND STANDARD GAUGES.

Crosby Test Gauge	8''	to 800 lbs.	grad. to 5 lbs.
" " "	6''	" 100 "	" " ½ "
" " "	5''	" 200 "	" " 1 "

Ashcroft Test Gauge	6″	to 120 lbs.	grad. to 5 lbs.
Crosby Vacuum Gauge	5″	" 15	" " " ¼ "
" " "	5	" 15	

This latter gauge is in a special case, is graduated finely for lbs., for inches of mercury and for feet of water, and on the lid is a table of constants for volume and pressure and curves of expansion, plotted by Mr. Hoadley himself. Most of the gauges have cocks.

THERMOMETERS.

Chemical, reading from	— 10° to 100° C by 1°
" " "	32° " 270° F " 1°
" " "	10° " 540° F " 2°

These are of glass, graduations on the tube.

Maximum and minimum, tin case from	— 40° to 140° F
Thermometer in brass case reading "	200° to 740° F
Hydrophant, wet and dry bulb from	20° to 120° F
Crosby Pyrometer 5″ dial reading by 20° from	0° to 1,400° F

U-GUAGES.

Mercury U-Gauge 4″ long, graduated to $\frac{1}{10}$ inch.

Wollaston-Prentiss Anemometer for light pressures.

This is a special U-gauge, 3 feet long, and was described by Mr. Hoadley, in Vol. VI., Trans. page 725.

INDICATORS.

Pair of Thompson indicators in case, nickel plated, with cocks, and springs 50 and 60 lbs.

An extra 3-way cock goes with these, and a wooden Ashcroft pantagraph for reducing motion, as well as an Amsler planimeter, and an instrument for proving its accuracy or error.

A McNaught indicator of the earlier style is also in the collection.

Crosby Revolution Counter to 999,999, with slotted lever complete.

SCALES.

One triangular steel, graduated by 48ths, 50ths, 60ths, 72nds, 96ths, 100ths.
One flat scale, graduated to 64ths and 100ths, *on the bevel of the edge.*
Wooden scales 2′ long, one shrink for brass.
" " " " " " cast-iron.
" " " double " " steel.
" " " one standard, graduated 1⅛″ to the foot.
" " " " " " 2 ″ to the foot.
" " " " " " 3 ″ to the foot.
One steel straight edge, nickel plated ⅛″ thick.
One Lyman T-square metal blade, vernier-head.
One Lyman vernier-head, with wooded blade to the T-square.
One hygrometer for oil, reading, from 10° to 45° Beaumé.

There should also be reported, that since the last meeting there has been added to the library, the complete series of the transactions of the Institution of Mechanical Engineers of Great Britain, from 1847 to date. The journal of the Iron and Steel Institute of Great Britain, from 1873 to date, has also been secured, and the list of current exchanges has been enlarged also.

Gifts have been received from the firm of Messrs. Thomas Shanks and Co. of Johnstone, near Glasgow, Scotland, of a series of photographs of very large machine tools which have been acknowledged by the Council, and they have directed that the Secretary solicit from the members gifts of photographs of other special tools, engines, and machinery, with the understanding that they will be filed in suitable portfolios and indexed for reference by visitors.

The matter of soliciting from absent members written discussions of papers which have been presented, or from non-members who are specialists in the subject of which any paper treats, has been committed to the Publication Committee with power to issue such invitations where they may deem it advisable in any case.

The Council would also present the Report of its tellers as follows:

The undersigned were appointed a Committee of the Council to act as tellers under Rule 13 to count and scrutinize the Ballots cast for and against the Candidates proposed for membership in the Society of Mechanical Engineers, and seeking election before the XVth meeting of the Society, in May, 1887.

They would report that they have met upon the designated days in the office of the Secretary and proceeded to the discharge of their duties.

They would certify, for the formal insertion in the records of the Society, to the election of the appended named persons to their respective grades upon Lists No. 1, 2, and 3, respectively Pink, Yellow, and Green.

There were 320 votes cast in the Ballot upon the Pink List, of which 7 were thrown out because of informalities. There were 304 votes cast upon the Yellow Ballot, of which 8 were thrown out because of informalities; and there were 304 votes cast in using the Green Ballot of which 4 were thrown out because of informalities.

The Lists are appended below.

CHAS. T. PORTER, } *Tellers.*
WM. KENT, }

NEW YORK, *May* 26, 1887.

Members.

Ashburner, Chas. A.
Barnum, Geo. S.
Bellingrodt, M. O.
Bigelow, Frank M.
Bole, Wm. A.
Coffin, John.
Crane, Wm. E.
Dixon, Geo. E.
Dodds, Elihu.
Dutton, C. Seymour.
Engel, Louis G.
Erwin, John M.

Freeman, John R.
Geoghegan, Stephen J.
Gould, Webster V.
Hardie, Robert.
Henry, Wm. F.
Horton, James A.
Kent, Edmund.
Lambert, W. C.
Leavitt, Frank M.
Lewis, James F.
Maillefert, G. J.
Mansfield, Albert K.
Metcalfe, John.
Meyer, J. G. Arnold.
Nicolls, J. O.
Owen, Frederick N.
Parsons, Harry de B.
(Promotion from Jr.)
Prentiss, Fred. H.
Radford, Benj. F.
Rider, George S.
Ridgway, J. T.
Roberts, T. Herbert.
Schutte, Louis.
Scott, Walter W.
Simpkin, Wm.
Smith, Chas. H. L.
Steel, Charles
Stewart, Walter G.
Stillman, Francis H.
Thomson, John.
Tobey, Geo. A.
Tuttle, Edgar G.
Van Vleck, Frank.
Warrington, Jesse.
Whitehead, Geo. E.
Wilcox, John F.
Wood, De Volson.

Associates.

Brooks, Thos. H.
Gilkerson, James A.
Huston, Chas. L.
Noye, Albert A.
Temple, W. C.
Zahm, A. F.

Juniors.

Conrad, Hugh V.
Garfield, Alex. S.
Lyall, Wm. L.
Mumford, Edward H.
Tompkins, S.

The Committee to Nominate Officers was appointed by Chair as follows:

Mr. H. A. Ramsay, of Baltimore.
Mr. T. J. Borden, of Fall River.
Mr. Wm. H. Doane, of Cincinnati.
Mr. W. H. Weightman, of New York.
Mr. F. H. Underwood, of Tolland.

Invitation to visit the Baltimore Manual Training School v received from Mr. John D. Ford, and a communication from I John W. Weston, Commissioner-General for the United States the Paris International Exposition of Railway Appliances and dustries urged the Society to coöperate in making that exhibit a success.

No other new business being presented, the professional pap were taken up.

The paper of Mr. Samuel Webber, of Charlestown, N. H., v entitled "Tests of the Comparative Value of Different Kinds of Belting." Messrs. Underwood, Dutton, Towne, Olin Scott, Ashworth, Hawkins, Emery, Woodbury, and Richards took part in its discussion. The paper by Mr. L. H. Rutherford (non-member), introduced by the Secretary, entitled "Should a Piston Packing Ring be of the same Thickness at every Point?" was discussed by Messrs. Fawcett, Porter, Sweet, and Emery.

Prof. R. H. Thurston's paper on "The Systematic Testing of Turbine Water Wheels in the United States," received discussion from Messrs. Samuel Webber and Babcock. The other paper by the same author, "Note on Helical Seams in Boiler-Making," received no discussion.

The topical discussions were then taken up until the hour of adjournment. Messrs. Stirling, Almond, Barnaby, Tilden, Scott, Towne, and Hawkins replied to the query: "What have you found the best methods for removing smoke from blacksmiths' shops?"

Messrs. Stirling, Couch, Richards, Dingee, Babcock and Joseph Morgan spoke on the topic, "What data can you give about the working pressure of gear teeth?" At the conclusion of this discussion the session adjourned.

The afternoon was devoted to visits of some of the public buildings of interest in the city.

The evening session was called to order at eight o'clock. The paper by Mr. John T. Hawkins, of Taunton, was entitled, "The Education of Intuition in Machine Designing." Messrs. C. A. Smith and Denton discussed the subject.

The two papers by Mr. C. E. Emery, entitled "Notes for Discussion on Cylinder Condensation and the Reduction of the Same by the Use of the Compound Engine," and the other, "Notes for Discussion in Relation to the Development of the Compound Engine and the probable Limit of Steam Pressure in Marine Engines and Boilers," were presented and discussed together. Messrs. Barrus, Porter, Denton, Joseph Morgan, Jr., Stirling, Strong, Babcock, and Emery spoke in discussion.

Mr. Emery's third paper, entitled "The Comparative Value of Steam and Hot Water for transmitting Heat and Power," was discussed by Messrs. Porter, Bond and Babcock. The last paper of the session was that of Mr. Albert Stearns, of Brooklyn, entitled "A Method of Evaporating by Exhaust Steam," and was discussed by Messrs. Babcock and Miller.

SECOND DAY, WEDNESDAY, JUNE 1ST.

The morning was left without assignment to admit of visits to points of interest which were not open in the later part of the day. The professional session was set down for two o'clock, and the economic topics had been allotted to this afternoon. The first paper was by Mr. H. R. Towne, of Stamford, Conn., entitled "Steam and Power: the Commercial Determination of Costs." It was discussed by Messrs. Denton and Babcock. The second paper of the series was by Mr. William Kent, of New York, entitled "A Problem in Profit-Sharing." It was discussed by Messrs. Hewitt, Hawkins, Towne, Doane, Stirling, Emery, Woolson, Olin Scott, Green, Fowler and Ashworth, the direction of debate being so closely in the line of the topical query on this general subject that the two matters are presented as one discussion. The question was: "What system of regulating wages of labor in ou manufacturing establishments will tend to make that labor mos efficient and produce the largest returns both to employer an employee?"

In continuation of the Topical Discussions, Messrs. Town Bond, and Supplee answered the question, "What is the be method and form of tool for drilling small deep holes in stee say a hole half inch in diameter drilled four feet into the e of a steel shaft?" Messrs. Halsey, Towne, Richards, Bon Scott, and Emery answered the query, "Have you made any u of calculating machines or tables, either in professional or co mercial work?" A large blue print was also exhibited and t method described by which it had been made. The plan w proposed by Prof. E. C. Cleaves, of Cornell, and the descripti and print were sent by Prof. Thruston for the information of t members. At the close of the discussion, the session adjourn at an early hour to allow the members to attend an evening ception tendered to the Society by the Hon. Josiah Dent, a Mr. Edward L. Dent, at their residence in Georgetown. T house belongs to the colonial period and is notable as the one which General Lafayette was entertained by Mr. John C. Ca houn. The reception was much enjoyed by all present.

THIRD DAY, THURSDAY, JUNE 2.

This day was devoted to an excursion to the home of Geor Washington at Mount Vernon. The party boarded a boat do the Potomac, at ten o'clock, and spent the afternoon at Marshall's Hall on the Maryland side of the river, returning in time for the fourth professional session in the evening.

This session was opened by the paper of Mr. H. Ashton Ramsay, of Baltimore, entitled, "What are the needs of our Navy." Messrs. Crane, Stratton Kent, and Oberlin Smith took part in discussion.

The other paper of the session was that of Mr. Joseph Morgan, Jr., of Johnstown, Pa., and was entitled, "Our Coast Defense, its Cost and its Mechanical Problems." The discussion was opened by Commander F. M. Barber, U.S.N., by invitation, and Capt. Rodgers Birnie, Jr., of the Ordnance Dept., U.S.A., and Lieutenant Beehler, U.S.N., had also accepted a request to contribute. Messrs. Oberlin Smith, Grimshaw, and Stirling, took further part in the discussion. At the close of the paper the session adjourned.

Fourth Day, June 3, 1887.

The morning was left without assignment and parties visited points of interest in the city. The largest party visited Cabin John Bridge, which is one of the largest stone arches in the world.

The fifth and concluding professional session was held in the afternoon. The first paper was by Mr. Thos. S. Crane, of Newark, entitled "Direct-acting Steam Veneer-Cutters," and was discussed by Messrs. Stirling, Walker, and Stratton.

Mr. George H. Babcock, of New York, presented his paper on "A New Method of Making Tubes from Solid Bars," and it was illustrated by samples of such tubes in steel and in brass. The discussion was given by Messrs. Hewitt, Stillman, Crane, Schuhmann, Kent, Hutton, Grimshaw, Bond, J. F. Wilcox and Stirling.

A paper by Mr. James Dredge, of London, England, honorary member of the Society, had been sent in, entitled "Gaslighting by Incandescence." The paper had not been received in time to be distributed among the members, but it was presented under a suspension of the rules in reference to debate on papers, and was read in brief by Mr. W. H. Wiley. No discussion being elicited from those present, the Secretary was directed to solicit written discussion from absent members.

The remaining Topical Queries were then taken up and occupied the time until adjournment. Mr. Stratton spoke on a "for safety-valve on the weight and lever principle for boilers on s vessels in rough water." Messrs. Stirling, Stratton, Kent, Wal Dicey, Bond, Randolph, and Woolson discussed the question : 6,000 lbs. per square inch as provided in the U. S. Inspection L for steam vessels a necessary limitation for stays in marine boiler In this connection Mr. Stirling proposed that a committee of society be appointed to look into this matter of boiler stays g rally, and to make the necessary experiments, so as to advise members on these points. The motive to appoint such a c mittee was put and was lost, the vote being very light upon b sides of the question.

Messrs. Dutton, Kent, Walker and Woolson, discussed "limit of stress for chains as used on cranes:" Messrs. Thurs Walker, Kent, Bond, Woolson and Babcock gave experience to the "average efficiency of a man when turning a crank; Mes Bond and Walker spoke on the average loss of efficiency f friction in ordinary hoisting machines operated by hand.

This discussion concluded the professional material of the sion. The following resolutions were thereupon presented Mr. C. T. Porter:

Resolved, That the American Society of Mechanical Engineers tender most sincere thanks to the members who have acted as a Local Committe Arrangements for the Washington Meeting, for their efforts and for their su in making our meeting in that city one of the most enjoyable of the conven of the Society.

Resolved, That the Society desire to express their most hearty recognitio the Hon. Josiah Dent and to Mr. Edward L. Dent for the courteous rece extended to them on the occasion of the enjoyable evening in their hospit and historic mansion.

These motions being put by the President, were carried enthusiasm, and the XVth meeting stood adjourned.

CCXLII.

A NOTE ON HELICAL SEAMS IN BOILER-MAKING.

BY R. H. THURSTON, ITHACA, N. Y.

(Member of the Society.)

In some cases the seams of the shells or the flues of boilers are put together in helical form, and some increase of strength is thus secured in the longitudinal at the expense of the girth seams. If n represent the ratio of the projected length of the seam on the circumference to the corresponding length of the projection longitudinally, the ratio of strength, as compared with the common seam, is measured by the ratio,

$$m = \frac{2n^2+2}{n^2+2}.$$

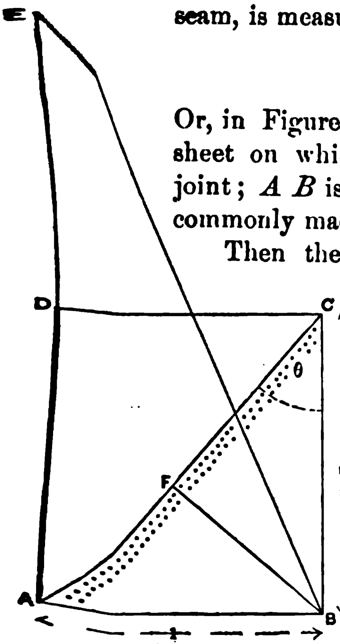

Fig. 150.

Or, in Figure 150, let *A B C* represent a part of a sheet on which the diagonal *A C* is the line of the joint; *A B* is the corresponding longitudinal joint, as commonly made, and *B C* the girth seam.

Then the stress per unit of length of *A B* will be unity; that on *B C* will be 2, and the total stresses will be, respectively, 2 and n, where n measures the ratio of *B C* to *A B*, or the "rake" of the seam. The total resultant stress will be *B E* on the joint *A C*, and its normal component will be *B E*, the sum of the components of those on the longitudinal and girth seams, *A B* and *B C*, resolved perpendicular to *A C*, and the intensity of that stress is the quotient of this sum divided by the length, *A C*, of the seam. Hence, the intensity on *A B* will be,

$$t_1 = \frac{2 \times AB}{AB} = 2;$$

that on $B\ C$ will be

$$t_2 = \frac{1 \times nAB}{nAB} = 1;$$

that on $A\ C$ will be

$$t_3 = \frac{2 \sin \theta + n \cos \theta}{\sqrt{1 + n^2}}.$$

But

$$\sin \theta = \frac{1}{\sqrt{1 + n^2}};\ \cos \theta = \frac{n}{\sqrt{1 + n^2}};$$

and

$$t_3 = \frac{n^2 + 2}{1 + n^2},\ \text{and}\ \frac{t_1}{t_2} = \frac{n^2 + 2}{2n^2 + 2} = \frac{1}{m}.$$

When n is given the values below, the ratios of strength of se are as tabulated.

STRENGTH OF HELICAL SEAM.

(Common longitudinal seam = 1.)

n	m	n	m
0	1·0	1·75	1·6
½	1·3	2·00	1·7
1	1·4	3·00	1·8
1·25	1·5	∞	2·0
1·5			

When $n = 0$, the joint is parallel with the axis of the cylind it becomes a longitudinal seam. When $n = \infty$, it becomes a gi seam of twice the relative strength. When the angle of "rake' 30°, the gain is 10 per cent.; when 45, the gain becomes 0·4. is obvious that this form of seam is very wasteful of metal, if much inclined as to secure any considerable gain of strength, the boiler or the flue is built of a succession of ring courses l side by side; in such constructions as Root's "spiral pipe," which the courses are helical, this objection does not hold.

CCXLIII.

THE SYSTEMATIC TESTING OF TURBINE WATER-WHEELS IN THE UNITED STATES.

BY R. H. THURSTON, ITHACA, N. Y.

(Member of the Society.)

THE habitable portion of the United States of America (excluding Alaska) extends over nearly 45 degrees of latitude and 60 degrees of longitude. The middle portion of this enormous territory is level and without streams of rapid fall; but the Atlantic coast is supplied with an enormous water-power by large streams and great rivers draining the lines of the Cordilleras Mountains, a range extending from Canada on the north to the Gulf of Mexico on the south, and divided into several parallel ridges at many points in its length. Great rivers and many smaller streams also run from these mountains to the westward as feeders to the Ohio and the Mississippi. On the west, the main line and the parallels of the Rocky Mountains, and the Sierras on the Pacific Coast, traverse the country throughout its entire length, and extend into British America and Alaska on the north, and through Mexico into South America on the south.

On the borders of the great lakes, and here and there in the more level country adjacent to the Mississippi and its feeders, are occasionally to be found streams of considerable magnitude, affording, at long intervals, where moderate falls occur, sufficient water-power to drive many mills and workshops. Throughout New England, the water-power thus available is utilized to a very great extent; but in the South and West, the tremendous energies offered to the people by the bounty of Nature have been utilized to but a very small extent.

The total amount of steam and water-power employed in the United States was reported in the United States census of 1880 at 3,410,837 horse-power, an increase of 45.4 per cent. since 1870.

Of this power, steam-engines furnished 64 per cent. and w wheels 36; in 86,000 establishments, there were at work a 55,404 water-wheels of a collective power of over 1,225,000 h power, and over 56,483 steam-engines of 2,185,458 horse-po supplied with steam from 72,000 boilers.

A considerable change in the relative magnitude of the two k of power has occurred during the ten years which elapsed bet the census of 1870 and that of 1880. At the earlier date the p furnished was very nearly equally divided between water steam-power. The use of steam has increased more rapidly that of water-power, and this change will undoubtedly continu an indefinite period. Nevertheless, the use of water-power is g ing, and will continue to grow so rapidly as to furnish an enor and an extending market until the steady decrease of water-p available throughout the country shall, ere many generations sume such serious proportions as to affect the business interest the nation.

The number of water-wheels in use in the United States to exceeds the number of steam-engines; and, of the total amoun power furnished by the latter, the principal part is obtained fr half-dozen large rivers. The amount of power so available ma imagined, when it is known that the Merrimac River supplies 000 horse-power at Manchester, N. H., and an equal amoun Lowell, under heads of about 50 and 35 feet, respectively; Mohawk is capable of furnishing, in good seasons, 10,000 h power at Cohoes, N. Y., under a maximum head of 100 feet; Androscoggin gives, at Lewiston, Me., an equal amount unde feet head, and the Connecticut River at Holyoke, 18,000 h power; while the Falls of Niagara will, undoubedly, in time called upon to yield a part at least of their 3,000,000 horse-po with a head of 160 feet.

These great water-powers are usually under the control of a poration, empowered, by act of the legislature of the State wi which the fall is situated, to erect and maintain dams and ac sories, and to use, or to rent, the power so obtained at such rat they may find most profitable.* Thus it happens that the w power of the Merrimac at Lowell, and that of the Connecticu Holyoke, is controlled by a "Water-power Company," at

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* At Lowell the six or seven principal manufacturing corporations form Water-power Company, owning shares of stock in proportion to the power o by each company.

place, and all consumers of power are compelled to deal with this corporation on such terms as it may dictate. The water-power companies are, however, directed by good business men, who recognize the advantages of a policy which encourages the introduction of a variety of manufactures into the place, and the encouragement of the use of the available power to the best advantage. The method of sale of power is substantially the same at all the principal falls. Power is sold by the "mill-power," which is a quantity of slightly variable amount. At Lowell it is the equivalent of 30 feet per second under 25 feet head; at Minneapolis, where the whole current of the Mississippi pours over the Falls of St. Anthony, the mill-power is 30 cubic feet per second, under 22 feet head, or its equivalent; at Holyoke, it is the equivalent of 38 cubic feet under a head of 20 feet. The prices are very variable with location and time of sale. The mill-power at Lowell is equal to about 85.2 theoretical H. P. 16 hours per day. None is sold, all being used by the mills included in the Water-power Company. At Holyoke the payment* is but $300 per annum for the equivalent of 90 H. P., or $4.17 per horse-power.

The forms of motor employed in the utilization of the water-power of the United States, although very numerous, are almost universally of the class known as turbines—wheels in which the current is received at one extremity of a bucket-channel, and discharged at the other end, traversing the conduit continuously and not with reversal, as in the older and more cumbersome "vertical" water-wheel. These wheels are all small as compared with the other class, quick-motioned, light, and of inexpensive construction. A very large proportion are of the type which may be designated as the "inward and downward" flow. This differs from the inward flow-wheel, of which that of Professor James Thomson is good example, in the fact that the bucket channels are so constructed as to turn the water downward, discharging it in the line of the axis. The very best work done in the United States has been usually done by turbines of this kind. Some of these, as have many of other kinds, proved inefficient and unsatisfactory in their operation; this is but a natural consequence of the fact that a large number of builders have gone into the business without possessing the most elementary knowledge, either of the principles or

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* This is not the *price* of the power; it is only that *portion* of the purchase price of land and power which the Water-power Company demands in the form of an annual rental. The balance may be, and generally is, paid in money or notes.

the working of such wheels. In fact the best work done in the country has been that of makers who have gradually, and at great cost, in time and money, experimentally felt their way, in the improvement of these motors, up to a state of excellence in their wheels which has probably never been equaled elsewhere. It may undoubtedly be the fact that efficiencies, such as will be given later, as obtained by test, have been occasionally attained by single wheels, in instances in which the wheel has been constructed with extraordinary care and at great expense; but the American turbines are usually cast with buckets in place, and are set at work very nearly as they come from the foundry. A sharp competition in the market usually forbids the expenditure of much time and labor in finish, or in machine-shop work. In a few cases, including one which will be referred to at some length presently, the buckets are cast separately and bolted in; but, even in this case, the surfaces of the guides and buckets are left with the skin upon them, just as they leave the foundry.

The magnitude of the business of turbine-construction, and the closeness of the competition among the leading builders, has led to the most remarkable success in their efforts to reduce the cost of construction, while increasing efficiency. Purchasers, also, have gradually come to understand that no wheel can be safely taken for use, in localities in which water-power is valuable, or of limited amount, without careful test of its power, efficiency, and regulation. It has thus occurred that makers and users have slowly, but steadily, approached a basis of agreement, in the buying and selling of these wheels, which seems now likely to become universally accepted on both sides. Those makers who do not come to this standard are certain to be compelled to take a secondary place in the business, and to submit to losses, both in prices and of business.

The larger falls of the Eastern section of the United States are usually, as above stated, under the control of powerful corporations which have secured the control of the water-power, and rent the power to users, taking, as rent, a certain sum per "mill-power," the power being a gauge of the quantity of water used. The water-power companies have also come to use the wheel as a water-meter, and are thus interested in determining the quantity of water used at the various openings of gate, as well as in ascertaining the efficiency of the forms of turbine in use among consumers. In several ways, therefore, influences have been at work tending to bring about a system of testing turbine water-wheels, and this system is now thoroughly established in certain localities, and is already controlling the character of wheels, and the methods of sale, in the whole country. The testing of turbines is no new thing, in itself, but the general use of this method of determining precisely what is the value of the wheel bought and sold is, so far as the writer is aware, peculiar to the United States.

The systematic testing of turbines was begun many years ago, and the following account, based upon statistics and information from scattered sources, but principally gathered through the assistance of Mr. James B. Francis and of Mr. Clemens Herschel, may prove interesting.

The first work of this character was done long before the turbine became well known in the United States. As early as 1823, Fourneyron had begun to study the then rude forms of turbines, with a view to their improvement, and when, in 1827, he brought out his own invention, he introduced with the wheel a systematic test, by means of the Prony Brake, as a no less important matter than the introduction of the Fourneyon wheel itself. Using the now common method of determining efficiency, he obtained, with somewhat uncertain measures of water used, efficiencies reported as from 65 to 80 per cent. for different wheels, under varying heads, and at varying speeds. The use of the brake, and the careful test of wheels of this class, has been continued from that day to this. "La Société d' Encouragement pour l' Industrie Nationale" is to be credited with the introduction of an intelligent general system of comparison of wheels, and correct methods of determining efficiency, in the establishment of a "Concours" into which Fourneyron entered, receiving from the Society the prize of 6,000 francs, offered for the best wheel of this class.*

Contemporary engineers at once took up this method of comparison of wheels. Thus, Morin, in 1838, reports† the results of a trial of a Fourneyron wheel, as giving an efficiency of 69 per cent., with but slight change of value for a wide range of speeds, and it ran equally well, whatever the degree of submergence. With another wheel, he obtained 75 per cent. Combes, at about the same time, found that he could get an efficiency of above 50 per cent. with his reaction wheels;‡ and Redtenbacher made similar tests of turbines

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* *Memoire sur les Turbines Hydrauliques;* H. Fourneyon, *Bruxelles*, 1840.

† *Experiences sur les Roues Hydrauliques;* Paris, 1838.

‡ Weisbach.

in Germany. From that time forward, this was a standard method of testing turbines in Europe.

The earliest systematic test of turbines in the United States, so far as can be ascertained, was made by a then well-known engineer, Mr. Ellwood Morris, of Philadelphia, in 1843, and in the State of Delaware, as reported in the *Journal of the Franklin Institute* for December, of that year. The maximum efficiency reported is 75 per cent. This was reached when the velocity of whirl, at the interior of the wheel, was 45 per cent. of the velocity due the whole head. In the following year, in February and March, Mr. James B. Francis, who has since become a distinguished engineer, and well-known for his extensive and valuable work in this direction, and in establishing useful hydraulic formulas, determined the power and efficiency of a high-breast water-wheel, in the city of Lowell, using a Prony Brake, fitted with a dash-pot, to prevent irregular operation; this "hydraulic regulator" is said to have been suggested by Mr. Uriah A. Boyden, the great engineer, who, later, designed turbines of extraordinary efficiency. This was probably the first application of this important detail in the use of this brake.

In the months of January, February, and March, 1845, Mr. Boyden made a trial of a turbine, designed by himself, in the city of Lowell, using the Prony Brake, and obtained an efficiency of 78 per cent. as a maximum. In December, 1846, he made similar trials of one of his turbines, at the Appleton Mills, in Lowell, and there obtained 88 per cent. These wheels were of the Fourneyron type, with certain improvements effected by Mr. Boyden, including diffusers and other peculiar devices.

Mr. Boyden, in 1846, contracted with the Atlantic Cotton Mills of Lawrence for three large turbines, to be designed by him, and built under his superintendence, the compensation for services, and for patent rights, to be $2,000 if the efficiency should attain 76 per cent., and $350 for each additional one per cent. gained above that figure. The trial, under the contract, was made in 1851 by the designer; but as the parties to the agreement could not come to an understanding, a law-suit followed; but the case was sent by the presiding justice to a Board of Referees, consisting of Professor Joel Parker, of the Law School of Harvard College, Benjamin Pierce, the distinguished Professor of Mathematics at the same institution, and Mr. Francis. This Board received the evidence in the case, and found that the various methods of reckoning the water consumed, as presented by the attorneys for both sides, produced calculated efficiencies varying from 88.5 per cent. upward. This minimum quantity, as given by their measurement, was claimed by the proprietors of the mills, and was accepted by the Board, and the contract was so settled. One of these wheels more recently tested gives the following figures, which are more nearly representative of recent practice.

TESTS OF AN 81″ BOYDEN TURBINE.

Gate.	Prop. Disch.	Head.	Revs.	H. P.	Efficiency.
1.000	1.000	12.90	51.06	160.51	79.37
0.870	0.994	12.88	51.28	156.74	78.17
.747	.959	13.01	50.12	149.20	76.18
.493	.786	13.28	51.80	107.54	64.97
.251	.535	13.42	50.10	45.99	39.80

Mr. Francis continued the work of determining the efficiency of turbines from this time forward, testing many wheels of several types. His most extensive and most important work is described, in great detail, in his *Lowell Hydraulic Experiments*.* In this book, he gives the data upon which he bases the formula for flow of water over weirs, which is now used by all American engineers in work of this character. He also deduces rules of construction which have since been very generally used in the design of turbines of the Fourneyron type, and which have been found to give excellent results. They are, however, like all empirical rules, to be used with caution where the conditions to be met differ from those of the tests upon which they were based. Mr. Francis has used these rules in the construction of a large number of wheels built for the mills at Lowell, and with great success.

The system of trial of wheels proposed to be used by purchasers of water from the water-power company with which Mr. Francis has been connected, and for the purpose of making them meters, was first introduced by him, and has now been found an essential

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* New York, D. Van Nostrand, 1880.

feature of the successful management of this kind of property; all large water-power companies now practice it. This system will be referred to again later. The regular testing of water-wheels has, from this period, never ceased. Many tests have been made at Lowell by Messrs. Francis and Mills, and similar trials have been made at Philadelphia, and at other places at which considerable water-power is available, and in use.

One of those engineers who have done much work of this character is Mr. James Emerson, whose history illustrates the peculiarly American habit of passing from one business to another with remarkable facility, and yet contriving to accomplish something, wherever he may find himself. Mr. Emerson was a seaman during the earlier part of his life, and in 1868, while contriving a new form of dynamometer, of the transmitting kind, he was requested, by Mr. A. M. Swain, to design a Prony Brake embodying his improvements, for the purpose of testing a form of turbine then constructed by Mr. Swain, and to be tested in a flume built by that engineer from designs by Mr. Francis. Mr. Emerson, at this time, as he states, was entirely ignorant that such an instrument had been earlier invented, and had never heard the name of the Prony Brake.

The instrument was designed and constructed, and the wheel was tested by Mr. Emerson, at Lowell, at a flume specially built for the purpose. The results were so encouraging, that it was concluded to employ a professional hydraulic engineer to repeat and revise the work with Mr. Emerson. The engineer selected was Mr. H. F. Mills, of Boston, and the Swain Turbine Co., having decided to open their flume for the purpose, a competitive test of all turbines which might be offered for the purpose was announced, June 16, 1869. The pit was 14 feet wide, 30 feet long, and 3 feet deep, measuring from the crest of the weir. The best results were obtained with the Swain and Leffel wheels, ranging, for the first, from 0.668 up to 0.789, and for the second, from 0.619 up to 0.799. The Swain wheel was stated by Mr. Emerson to have exhibited great internal friction, and to have lost efficiency seriously from that cause. Later tests, especially those reported upon by Mr. Mills,* confirm this conclusion, giving efficiencies exceeding 80 per cent. as follows:

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* *Journal Franklin Institute*, 1870.

TESTS OF A 42" SWAIN TURBINE.

Gate.	Prop. Disch.	Head.	Revs.	H. P.	Efficiency.
1.000	1.000	14.25	119.13	62.81	82.20
0.750	0.853	14.45	119.81	51.20	77.50
.714	.774	12.55	115.04	39.22	75.30
.476	.589	13.00	108.58	27.08	66.20

The competitive test just described seems to have been the beginning of a series of such tests, as well as of a general system of public tests of turbines at testing flumes open to all users and builders of wheels, and these tests have continued, in the United States, to the present time. The reports upon the results of tests made, as just described, at Lowell, attracted the attention of the late Mr. Stuart Chase, then agent of the Holyoke Water-power Company, who at once saw the immense importance of securing the adoption of such a system, at Holyoke, for the benefit of the company with which he was connected. He wrote to Mr. Emerson: "The testing of turbines is the only way to perfect them, and that is a matter of great importance. Move your works to Holyoke, and use all the water that is necessary for the purpose, and welcome, free of charge." At Lowell, Mr. Emerson had been compelled to pay for the water used. At this time, Mr. Emerson was conducting the tests as a matter of private business. He at once accepted the liberal offer thus tendered him, and removed to Holyoke, where he continued the testing of turbines until it was taken in hand by the Holyoke Water-power Company. The reports of work done were published; and undoubtedly were the means of bringing a number of wheels up to a state of high efficiency. The reports were found, by hydraulic engineers, to be full of valuable data, and, although not systematically arranged, or as complete in the analysis of the distribution of useful and lost work as a professional might have made them, form an extensive and valuable collection of figures. These, with such reports as those of Mr. Francis, embodying the most painstaking analysis, supply the engineer with much of the essential data needed in his work. The later work of the Holyoke Water-power Company, which combines the facts and data with careful and skillful analysis, is likely to prove still more valuable. It will be described at considerable length presently.

One of the earliest tests of turbines, at the flume at Holyoke, was witnessed by the writer in August, 1872, he having been invited to be present at the test, with two of the oldest hydraulic engineers in the United States, Messrs. Samuel Webber and L. M. Wright—the latter of whom was well known as the engineer who so boldly and skillfully gauged the Niagara River, at the Falls, driving the little steamer, *Maid of the Mist*, up under the falls, as closely as her power would carry her, and sounding a line directly across the stream at that point. The best figures obtained at this trial, which were those of a wheel designed and built by Mr. N. F. Burnham, of York, Penn., were as follows: the wheel was 36 inches in diameter, the head above the wheel 18 feet. The efficiency at full gate was from 78.4 to 81 per cent., the revolutions ranging from 156 down to 147 per minute, and the best work being done at the latter speed. The power of the wheel varied between 53.18 and 55.45 H. P., discharging from 1994.52 to 2005.73 cubic feet per minute. A second wheel gave efficiencies closely approximating 80.5 per cent. Each was tested five times under the same head, and at full gate, the variation of efficiency proving to be unimportant at successive trials under similar conditions. Part-gate tests, made by blocking up guides, were also made, giving lower figures which are of no value as indicating the efficiency of the wheel as ordinarily used with its usual method of governing. Mr. Burnham, whose wheel is here referred to, was one of the first builders in the United States to put up a flume, and to determine the efficiency of his own wheels by test. His flume was built in the early part of the year 1870.

In 1876, the Centennial Commission authorized the judges at the International Exhibition to conduct a series of trials of turbines there on exhibition. The apparatus for use at these Centennial tests was constructed for the purpose, under the supervision of one of the most experienced hydraulic engineers in the United States, Mr. Samuel Webber, of Charlestown, N. H., one of the judges in that department, and a member of the American Society of Mechanical Engineers.

A large tank was built under the floor of the "Hydraulic Annex," and at a height of 33 feet above it was placed another tank supplied from the lower one by a set of Andrews centrifugal pumps, capable of raising nearly 2000 cubic feet of water per minute. The pen-stock was 4 feet in diameter to the wheel casing, which was 8 feet in diameter, and 6 feet high. The tail-race was 14 feet wide and 8 feet deep, terminating at the large tank under the floor. The measuring weir was 9 feet long, the water flowing over an edge formed on a cast-iron plate, planed down to ⅛-inch thickness and then beveled to an edge at an angle of 45 degrees.

The method of test was essentially that practiced by Mr. Francis, and will be more fully described later in the account to be given of the testing flume at Holyoke. Many of the wheels offered for test were found, as is very usually the case, to be more or less defective in fitting and workmanship, and the results obtained were not always such as to reflect great credit upon American builders of turbines. All the wheels tested, with two exceptions, were made in the United States. Two Canadian wheels gave a very good record at full, or nearly full, gate. The competitive tests and the private tests made since that time by builders desirous of improving their wheels have brought about great changes in design, and correspondingly great changes in the efficiency of the wheels made by the best constructors in the United States.

The best results reported at the Centennial Exhibition were given by a Risdon Turbine, a wheel of the inward and downward flow type, the most successful type now known in the United States. The Swain wheel tested at Lowell, as above, is also of this kind. The Risdon wheel was 30 inches in diameter, 1.006 square feet area through the minimum sections of wheel buckets, 1.257 feet through the guides, and delivered the water from the bucket orifices at a velocity relative to the wheel, of 62½ per cent. that due the total fall—say 70 per cent. that due the effective head at the entrance to the guides. These proportions may seem peculiar to European constructors of turbines; but the judges making the tests (Report, p. 10) reported 87.68 per cent. at full gate. This figure is subject to the criticism made below, but so remarkable nevertheless that it can hardly be questioned that the peculiar proportions of the wheel are such as are well adapted to reduce loss, by friction and disturbance of current, to a minimum. This wheel had previously been tested in the old flume at Holyoke, with quite different results, giving 90.22 per cent. at full gate. At this trial, it discharged the water from the exit orifices of the wheel with a relative velocity of 61½ per cent. that due the total fall. Tested once more by Emerson, after the exhibition had closed, and at the old flume, the wheel gave a maximum efficiency of 86.41 per cent. These differences in efficiency are attributed to variations in the setting of the wheel; but they would seem to th writer quite as likely to be evidences of variation of accuracy of apparatus, or of tests, amounting to several per cent. At the exhibition, the wheel was so set that its loss by friction and leakag was made a minimum.

The velocity of whirl of these turbines, at the receiving side, exceeds that of the water at entrance from the guides, givin unusual power to the wheel for its size.

The following is a tabular statement of the results reported b the committee to the authorities, at the close of the tests of al turbines entered at the Centennial Exhibition:

TESTS OF TURBINE WATER-WHEELS AT THE U. S. CENTENNIAL EXHIBITION, 1876

MAKER'S NAME, OR NAME THE WHEEL IS KNOWN BY.	Per cent. at full gate or discharge.	Per cent. at about 9-10 of full discharge.	Per cent. at about 7-8 of full discharge.	Per cent. at about 3-4 of full discharge.	Per cent. at about 5-8 of full discharge.	Per cent. at about 1-2 of full discharge.	Per cent at about 4-10 of full discharge
Risdon Wheel...........	87.68	.....	86.20	82.41	.....	75.35	.....
National Wheel	83.79	.....	....	70.79	.....	.....	.....
Geylein Wheel (single)...	83.80	.....	.....	.....	.....	.....	.....
Thos. Tait	82.13	.....	.....	70.40	66.35	.....	55.00
Goldie & McCullough....	81.21	.....	71.01	55.90	.....	.....	.....
Rodney Hunt Mach. Co...	78.70	71.66	.....	68.60	51.03	.....	.....
Tyler Wheel............	79.59	.....	81.24	79.92	67.23	69.59	.....
Geylein's (duplex) *......	77.57	.....	.....	.....	74.74	.....	.....
Knowlton & Dolan.......	77.43	74.25	.....	.....	62.75	.....	.....
E. T. Cope & Sons.......	76.94	.....	69.92	.....	.....	.....	.....
Barber & Harris	76.16	73.33	.....	.....	70.87	71.74	.....
York Manufacturing Co..	75.70	.....	67.08	67.57	62.06	.....	.....
W. F. Mosser & Co.......	75.15	74.89	71.90	70.52	.....	66.04	.....
A. N. Wolf..............	74.89	.....	.....	74.15	65.00	61.82	.....
A. N. Wolf..............	72.50	.....	71.66	.....	.....	64.30	.....
O. J. Bollinger...........	70.46	68.78	65.33	.....	60.20	55.52	.....
American Turbine.......	68.59	.....	69.29	.....	.....	.....	60.14
Chase Turbine Co........	68.30	67.79	.....	57.52	.....	.....	.....
York Manufacturing Co. (experimental)	67.63	.....	51.15	.....	61.42	.....	.....
J. T. Noye & Sons.......	65.68	65.59	.....	64.80	.....	.....	.....

The limits of error are evidently here very uncertain; they a undoubtedly considerable as compared with the later work done

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* This wheel was a double wheel with double guides, having no gate. On one part of the wheel and guide was uncovered to get part-gate results. The fore the part-gate result reported is not a part-gate compared with the oth wheels, but a full-gate test of a wheel venting about five-eighths of the water the combined wheel.

the permanent flume at Holyoke—possibly they may be as much as 4 or 5 per cent.

The later great work of improvement of American turbines, by systematic test, has been principally effected at the Holyoke flume.

The object aimed at by the Water-power Companies of Lowell and of Holyoke, in the establishment of testing flumes for turbines, is the determination of the power and efficiency, the best speed, and the quantity of water flowing at from whole, to, say, half gate, so exactly that the wheel may be used as a meter in the measurement of the water used by it. The quantity of water passing through the wheel, at any given gate-opening, will always be practically the same at the same head, and the wheel having been tested in the pit of the testing flume, and its best speeds and highest efficiency determined, and a record having been made of the quantity of water discharged by it at these best speeds and at all gates, the turbine is set in its place at the mill, speeded correctly for the head there afforded, and a gauge affixed to its gate to indicate the extent of gate-opening. The volume of water passing the wheel at various openings of gate having been determined at the testing flume, and tabulated, the engineer of the Water-power Co. has only to take a look at the gauge on the gate, at any time, or at regular times, and to compare its reading with his table of discharges, to ascertain what amount of water the wheel is taking, and to determine what is due the company for the operation of that wheel, at that time. The wheel is thus made the best possible meter for the purposes of the vender of water.

Knowing, also, the efficiency of the wheels drawing from the canals controlled by the corporation, it soon becomes to the interest of both user of wheel and vender of water carefully to test wheels proposed for use at that site, and to select by such test the most economical wheel, both as a matter of economy of cost in purchasing water, and as a means of making the most of the available water-power, when, as is now the case at all large centers like the cities here referred to, the demands for water at market rates may exceed the supply. The interest of the proprietors of the water-power, as well as of the citizens of the place, dictate the adoption of a policy which will encourage the development of manufactures, and will lead to the extension of the privilege of water consumption to as many takers as possible. The introduction of efficient wheels thus becomes the declared policy of every water-power company. It has thus happened that old wheels of Boyden, which are modified Fourneyron turbines, and others which were also once famous for their efficiency, have, during later years, been displaced, at Lowell, by more modern wheels. At Holyoke, as at the former city, the tendency is very strongly exhibited to use only a very small number of makes of wheel, selecting the best, by test at the flume established for the purpose by the company supplying the water. The work of the distinguished engineer of the Lowell Company, Mr. J. B. Francis, at that place, and the work recently done at Holyoke by Mr. Herschel, in this direction, is having a wonderful effect for good, in the encouragement of the development of efficient motors of this class, and in the opening to such efficient wheels of a large and a healthy market. The trade is settling, after a period of uncertainty and irregularity, into a very satisfactory condition—to those who are so fortunate as to be able to build a wheel that comes up to the very stringent specifications of the buyer, in price and performance. Fortunately, experience and direct test of the wheels in the market, show that the efficiency of turbines has been increased without at all complicating them in design, or adding to the cost of their construction.

The Holyoke Water-power Company, having arranged for testing turbines at the flume constructed for Mr. Emerson, at Holyoke, for the purpose of determining the standing of wheels offered for use at that place, by their tenants, determined to issue a general call to builders and makers of wheels, asking them to send in their turbines for test. Accordingly the following circular was sent to the principal builders:

HOLYOKE WATER-POWER COMPANY,

Holyoke, Mass., April 10, 1879.

NOTICE TO TURBINE BUILDERS AND MANUFACTURERS.

The practice of testing turbines, so common the past ten years, has undou edly done much toward bringing the best into use; but there has been o serious defect in the system; that is, the practice has generally been confined the trial of small wheels, owing to the great expense that would be caused by t tests of large sizes. As it is a matter of vast importance that the best turbi plans should be established beyond chance for doubt, this Company has provid means for a thorough competitive test of the various kinds of turbines that m be offered for trial, and invite Water-power Companies, cities that pump th water supply, and all others interested in the matter, to take part therein. Ea builder shall superintend the setting of his wheel—the setting and testing to done at the expense of the Water-power Company. *Capacity of each wheel to be sufficient to discharge about 5,000 cubic feet of water per minute, under 18 feet head. Each wheel will be thoroughly tested from half to whole gate, and, if deemed best, under at least two different heads; also under several feet of back water. At the conclusion of the trial, a full report will be made of the results obtained and of the workmanship and probable durability of each kind of wheel tried. Turbine builders of this or any other country are invited to furnish wheels, and those proposing to do so should give notice of such intention as soon as possible.

Tests to commence the first day of September next.

Builders were also requested to furnish "draught-tubes," or "suction-pipes," in order that the value of such appendages might be ascertained, and manufacturers of gearing were desired to send in gearing for the purpose of giving an opportunity of measuring the loss of efficiency in the machinery of transmission. The wheels were set under the direction of Mr. Emerson; the tests were directed by Mr. Webber, and the builders were represented at the flume by Mr. Theo. G. Ellis, since deceased. The report on the tests made was prepared by Messrs. Ellis and Webber, and certified to by Mr. Emerson. The work was done publicly, in the presence of the representatives of the builders of the wheels and of invited witnesses, including a number of distinguished engineers. The following is an abstract of the results reported, as published by the Holyoke Water-power Company.†

TESTS OF TURBINES.

Holyoke, Mass., U. S. A., 1879.

TABLE SHOWING AVERAGE EFFICIENCIES AT PART-GATE.

NAME.	½ to ¾. Per cent.	¾ to full. Per cent.	½ to full. Per cent.
Hercules	.737	.805	.771
New American	.732	.795	.763
Success	.708	.786	.747
Tyler	.665	.766	.715
Tait	.680	.744	.712
Thompson	.696	.721	.709
Nonesuch	.619	.712	.666
Houston	.397	.717	.557

During the tests, the best speed was found for full-gate, and the

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* Builders who have not patterns for wheels of so large capacity may enter their largest size, but it is better that all should discharge about the same quantity.

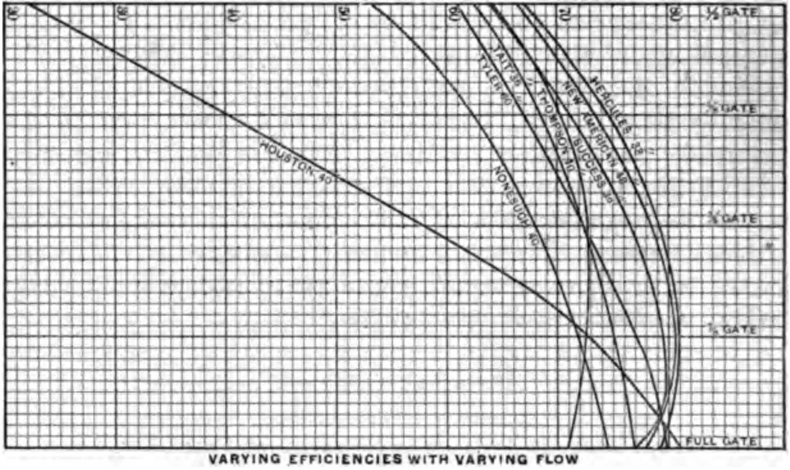
† Holyoke Hydrodynamic Experiments, 1879–80. Springfield, 1880.

water then gradually shut off to part-gate, falling to about one-half the maximum, the engineers noting the efficiency at th several rates of supply.

The results of these tests are also represented graphically in th accompanying illustration, Fig. 130, in which the abscissas measur the proportion of water used, and the ordinates are proportional t the efficiencies obtained. The diagram so obtained gives a bette idea of the relative standing of the wheels than does the table, an shows better the effect of variation of gate upon each.

Examining the diagram and the foregoing table, it will b observed that the Houston turbine, which has the highest percentage of effect at full-gate, is the least efficient at from half to three quarters, and from half to full gate, of all those shown on th diagram, and is only superior to the Nonesuch at from three-quarters to full-gate, and that by a very trifling amount. The whee which apparently has the highest percentage is really the leas desirable for actual use. Taking the average useful effect of th wheels shown from one-half to full gate as a measure of their efficiency, their relative value is in the order shown in the table.

TESTS OF TURBINES
Holyoke, Mass., 1879.



VARYING EFFICIENCIES WITH VARYING FLOW

Fig. 130.

The experiments with "draught-tubes," or "suction-tubes," whic were actually "diffusers" in their effect, so far as the writer ha been able to analyze them, indicate the loss by friction whic should be anticipated in such cases, this loss decreasing as the tub increased in size, and increasing as its diameter approached that of the wheel—the minimum diameter tried. It was sometimes found very difficult to free the tube from air completely, and next to impossible, during the interval, to control the speed with the brake. Several trials were often necessary before the power due to the full head could be obtained. The loss of power by gearing and by belting was variable with the proportions and arrangement of the gears and pulleys, length of belt, etc., but averaged not far from 30 per cent. for a single pair of bevel gears, uncut and dry, but smooth for such gearing, and but 10 per cent. for the same gears, well lubricated, after they had been a short time in operation. The amount of power transmitted was, however, small, and these figures are probably much higher than those representing ordinary practice. Introducing a second pair—spur gears—the best figures were but little changed, although the difference between the case in which the larger gear was the driver, and the case in which the small wheel was the driver, was perceivable and was in favor of the former arrangement. A single straight belt gave a loss of but 2 or 3 per cent., a crossed belt 6 to 8 per cent., when transmitting 14 horse-power with maximum tightness and transmitting power. A "quarter-turn" wasted about 10 per cent. as a maximum, and a "quarter-twist" about 5 per cent. Mr. Emerson states that the

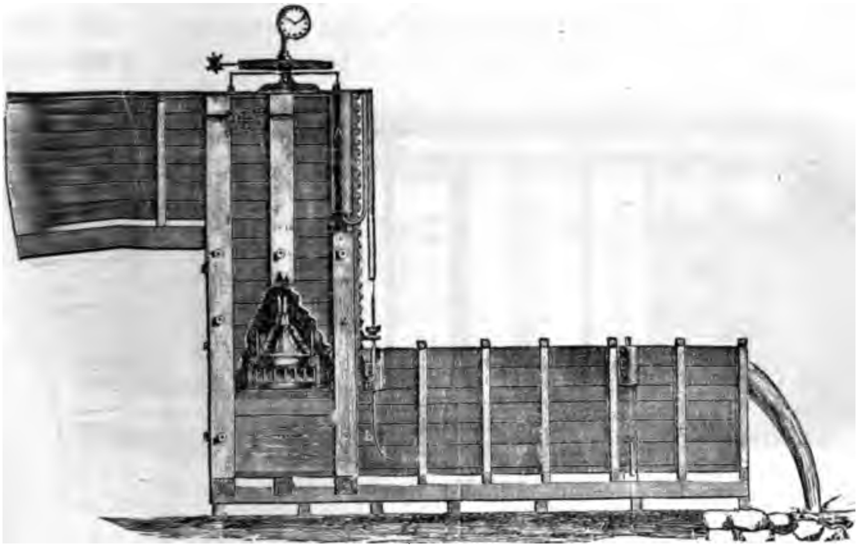


FIG. 131.

tests showed that a turbine could be tested, under such conditions a were here secured, at about *one* per cent. of the cost of testing wheel ten years previously, and more accurately than was possible, at an cost, at the earlier date. The average cost, in 1869, is stated t have been $2,500 per wheel; the cost for water alone, at Lowel in testing three Swain wheels, is given at $500. The lowest charg for water was $25 per wheel at the flume there in use. The usu charge, to-day, for a single test, is 10 per cent. of the list-price o the turbine, with the minimum figure set at $30.

These earlier testing flumes, although capable of doing goo work, were not very elaborate, or very expensive, construction The two figures here given (Fig. 131 and Fig. 132) represent th elevation and the plan of the flume in use at Lowell in 1869. Th channel-section at the junction with the fore-bay, or penstock, wa four or five times the section of the largest wheel to be tested i the flume; the penstock had such a depth that the maximum fal might be utilized, and the tail-race to the weir was given a suffi ciently large section to reduce the velocity of approach to so smal a magnitude that it may be neglected.

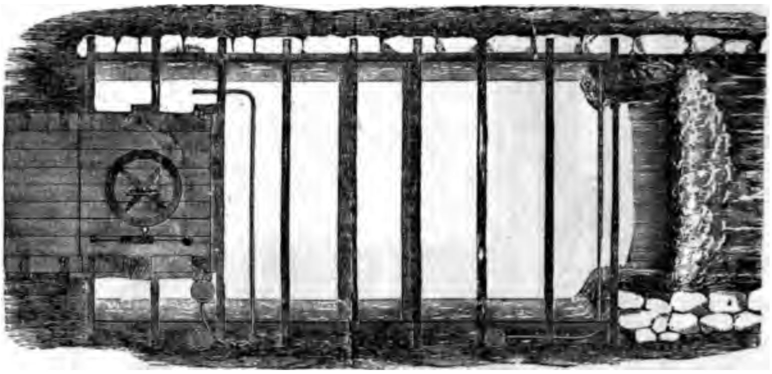


FIG. 132.

The wheel to be tested was set at the bottom of the penstock usually, and the dynamometer was mounted directly upon the hea of the turbine shaft, as shown in the engraving. Small tanks, A, B C, were set at the sides of the structure, communicating, by mean of small pipes, with the water in the flume at the head and the foo of the fall, and at the weir. The water in A stood at the level o that in the head-race; that in B at the level of the tail-race curren leaving the wheel-case; that in C at the height of the water at th weir. The connecting pipes were so small that all oscillations were intercepted, and the surface of the water in either tank was perfectly still. A flexible rubber pipe from the bottom of the tank, A, led to the lower end of a vertical glass tube, at the right, which served as a gauge and gave the height of the water-level in the penstock. The scale on which readings were taken was movable, and was adjusted vertically, together with a "hook-gauge" pendant from it, which was set to give the height of water in the lower tank B. The scale was thus arranged to read differences of level in the two tanks, *i. e.*, total heights of available fall. The reading on the scale was the measure of the heights of the marks read, above the lower level, and the head was obtained by taking this reading at the level of the water-surface in the glass gauge-tube. The zero-mark of the stationary lower scale, at the tank C, was at the level of the edge of the weir, and the hook-gauge there placed gives the height of the water in the flume at that point above this zero, and the depth of water on the weir. Both tanks were made adjustable, in height, in order that the surface of the water in each might be kept near the top of the tank, and thus be easily and accurately seen and gauged. It is advised that, in the construction of such flumes, the proportions adopted by the designer of that here described be

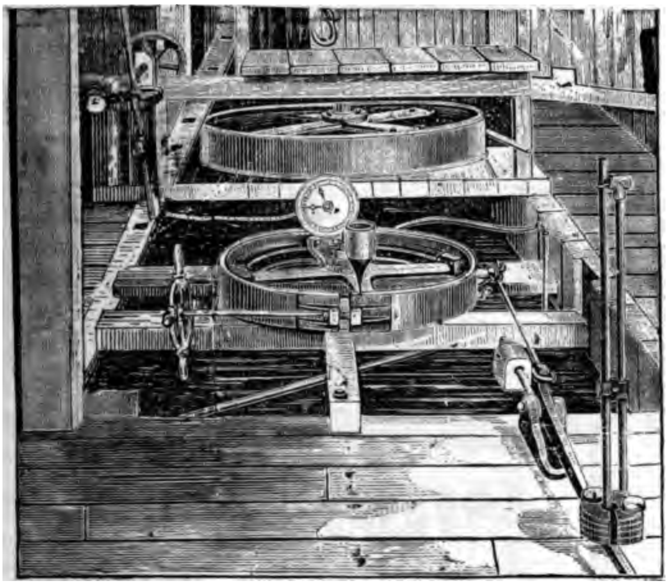


FIG. 183.

adopted, *i. e.*, that the fore-bay be made at least twice as wide as any wheel proposed to be tested, the width of the tail-race at least one and a half times that of the weir, and its depth four times that of the current over the weir at maximum flow. The weir should be 20 feet, or more, from the wheel, and precisely at right angles with the thread of the current.

The arrangement of the flume at Holyoke, in 1872, is seen in the two illustrations which follow, Fig. 133 giving a view of the observing room at the head of the penstock, and Fig. 134 showing the pit, or tail-race, below the penstock, and the weir at its terminal end. The flume was constructed in an unused canal lock of the Holyoke Water-Power Company. The first view exhibits the arrangement for test of a turbine set as in the last figure above given with a vertical shaft; the second the arrangement for testing a wheel with horizontal shaft.

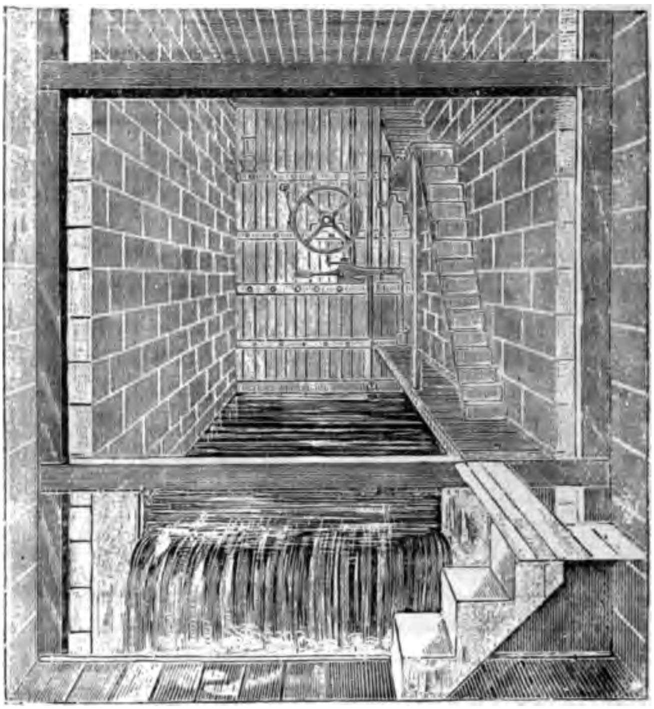


FIG. 134.

The hook-gauge used in determining the height of water i the tanks, A and B, is shown in Fig. 135. It consists of a sharp pointed hook, carried upon the end of a vertical rod, to which is secured a scale and vernier, the former movable with the hook, the latter, when at the tank C, fixed to the frame of the instrument, which is, itself, fixed to the side of the flume. The scale and hook are caused to traverse the frame by means of a carefully cut screw, having a milled head by means of which it may be easily and accurately set. When in use, the hook is raised from a lower position until its point just pricks the surface of the water, and thus indicates, with singular exactness, the water-level and the height of the surface of the current above the weir. At the penstock, the vernier may be dispensed with, as the total head is determinable with sufficient accuracy without it. The first forms of the Boyden hook-gauge were cumbersome and heavy; that illustrated in Fig. 135 is much lighter and more portable, as well as more convenient; it weighs but 14 pounds, and is but 30 inches long. A still more compact and handy apparatus is that exhibited in Fig. 136, which is the later form used, and weighs but 4 pounds. The last style is used n the later Holyoke flume, and was found by the writer to be a ery convenient instrument. This gauge is "self-contained," and

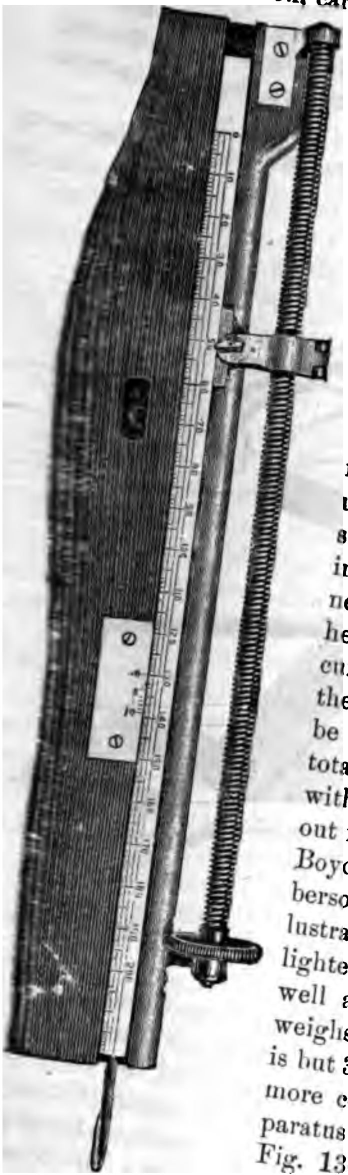


FIG. 135.

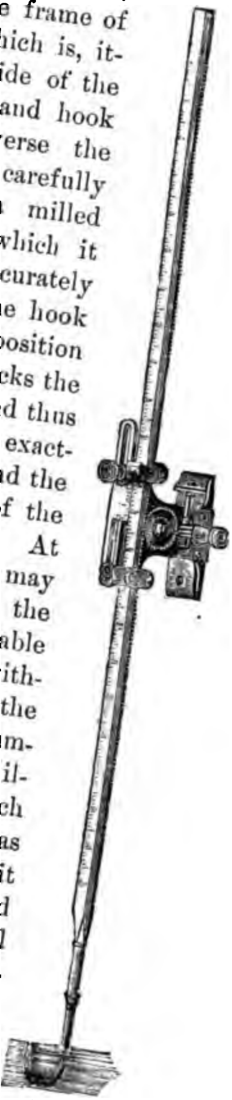


FIG. 136.

is secured to the frame of the water-way, or to the wall of the flume, by screws which fasten the bracket firmly in place. A milled headed spindle carries a pinion which engages a finely cut rack, on the back of the scale rod, and thus permits as accurate adjustment as is desired, while the vernier mounted on the bracket gives exact readings.

The form of Prony Brake, or band dynamometer, designed for the work of testing at the Lowell flume, is shown in the accompanying engraving, Fig. 137.

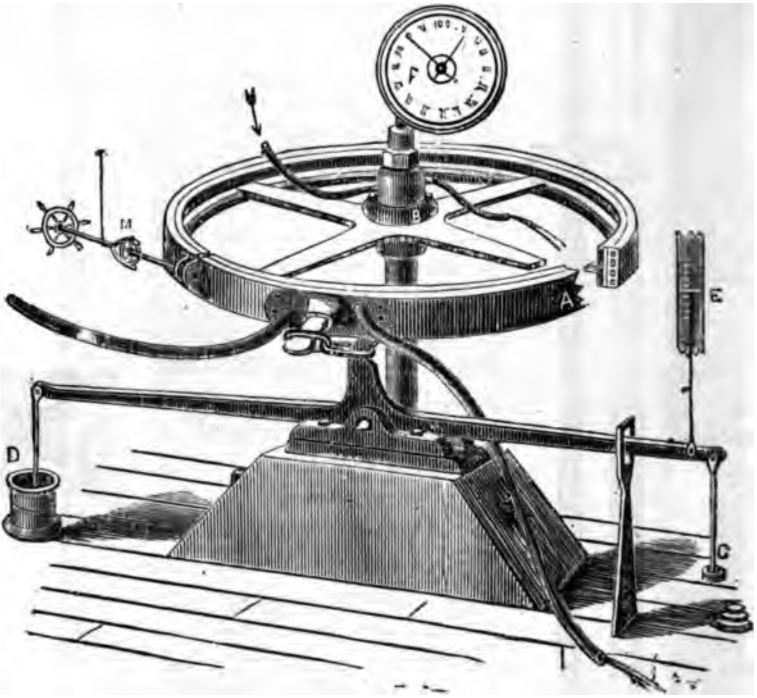


FIG. 137.

The wheel B is secured to the shaft of the water-wheel, and its speed is controlled by the friction-band A, which is connected to the scale-beam as shown. The rim of the wheel and the friction-band are hollow, and kept cool by streams of water passing through them; the water in the rim of the wheel being supplied through its hollow arms and the pipe, shown in the engraving. The wheel B is made of cast iron and the friction-band of "composition" or "gun bronze." The hands of the "counter" F, are so arranged in connection with a worm gear, that they can be made to rotate in the same direction with the hands of a clock, whichever way the wheel being tested may revolve.

The hand-wheel for operating the friction-band through the screw M has a "universal joint" in its shaft. The connection of the band with the scale-beam is made by knife-edged links, and the pivot of the beam is also knife-edged. The weights are suspended at one end of the beam as shown at C; at the other end is the "dash-pot," D; it is better to have a "dash-pot" at the same end as the weights; it is filled with water to hold the beam steady. The dash-pot is made of cast iron. The plunger on the end of the rod is a thin disk of iron turned to fit loosely, so as to allow it to move perfectly freely; it has six three-eighths inch holes through it, stopped with brass thumb-screws; one or more of these may be removed at any time, to render the beam more sensitive; the screws are left lying on the plunger, that the weight may not be changed.

The method of test at the flume last described, as conducted during the visits of the writer, was as follows: The dynamometer was first carefully balanced and adjusted before starting the wheel. The start was made with a light weight upon the scale-pan, and the velocity of the wheel noted, and its effect roughly determined. The load was then increased, at intervals of two or three minutes, by 25 pounds at a time, until the speed of the wheel had fallen below that of maximum efficiency for the head; the weights were then reduced again and the velocity of the wheel allowed to increase until the maximum was again passed. The same process was then repeated within a smaller range of speed, and with smaller variations of load, until the speed of best work had been more exactly ascertained, and the performance of the turbine at maximum efficiency, under full head and at full gate, had been very precisely determined. This was repeated at each of the part-gates, usually down to one-half maximum discharge. All doubtful measurements were repeated, and all unsatisfactory tests, as well. The readings of the dynamometer, the revolution-counter, and of the two-hook gauges, were taken as nearly as possible simultaneously. The results were finally carefully calculated and verified, and the reports made up on blanks which had been printed for the purpose. The effect of variation of speed upon efficiency, the actual efficiencies, and the same points as affected by variation of head, were the subject of investigation, as well as the variation of efficiency with change of gate or of flow. There is rarely found to be any very close proportional correspondence of quantity o water discharged with position of gate.

The following are data obtained, in the manner described, fron the Burnham wheel at Holyoke, June 15, 1883, from a 36-incl wheel:

TEST OF A 36-INCH BURNHAM TURBINE.

Gate.	Water discharged.	Rev.	H. P.	Efficiency.
Full	1 000	139	66.50	81.82
0.788	0.943	134	61.08	80.78
0.500	0.760	136	47.23	75.26
0.339	0.588	131	32.90	65.80
0.237	0.456	118	23.10	58.60

The flume constructed in the canal at Holyoke, by Emerson, wa finally ceded over to the Holyoke Water-Power Co., and was ope rated by that Company, testing the wheels proposed to be used i the mills drawing water from the canals controlled by that corpc ration, and also in testing wheels brought to Holyoke for test i the interest of other users, makers, or intending purchasers. It wa very soon found advisable to erect a flume to be especially designe for the work of testing turbines. The importance of securing acci rate data, not only to the Holyoke Water-Power Company, but t users of such wheels generally, as well as to their makers, had be come so evident, the limits of error, though but three or five pe cent. at the old flumes,* were becoming so important, and the exten of the business of testing turbines had become so great, that it wa decided to go to considerable expense, and to erect a flume an buildings which should fully meet all present demands, and prol ably all likely to arise for years to come. This work was unde taken by the Holyoke Water-Power Co.; the designs were made and the construction supervised, by their engineer, Mr. Clemen Herschel, and the flume was completed and opened for use i April, 1882.

At this time, the Company furnished to its tenants 15,000 horse power by day and 8,000 by night. There were in use 139 turbin wheels—no other kinds of water-wheels were used—of which 5 ran 10 hours per day, while the other 80 ran 144 hours per wee

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* The writer has before him the reports of the official tests in old flumes two wheels, similar to that later tested under his own eye, as described herei which gives efficiencies ranging up to ninety and ninety-four per cent.; certain several per cent. too high.

i. e., from midnight on Sunday, to midnight Saturday. These wheels were driving the machinery of about 70 factories and mills. At all of these establishments, the engineer of the Company supplying the water directs a system of observations by means of which he keeps himself informed of the amount of water that each tenant is using. He secures, each day, a record of the opening of the regulating gate of each wheel, and a similar reading is taken every night, when the wheels are in operation night and day, and also a reading of the head acting at each wheel. These records are preserved, and furnish the data from which to compute the quantity of water used by each tenant throughout the year. Any surplus power above that granted is paid for at the end of each "quarter." At times of small flow, and when the use of "surplus" power is limited, the tenants are thus kept under surveillance, and are held to strict accountability for water used in excess of that allowed by their contracts with the Water-Power Company. By the preliminary test of each wheel, it is made a meter, as already stated, and it thus gives the engineer of the Company a very accurate means ot measurement of the flow. It was for the accomplishment of this very essential part of its system, that the Holyoke Water-Power Co. finally, as just stated, determined to erect a new and carefully constructed flume, in which this work could be done with equal facility in winter and in summer, day or night, and one in which the measurements could be made with the utmost precision, and with the greatest possible convenience.

The new testing flume was constructed between two of the canals of the company, where a difference of level and a working head of 17 feet could always be obtained, and, in some cases, 19 feet. The flume is covered by a large brick building, in which are the repair-shops, a blacksmith shop, a store-room and offices. It is fitted up with all the apparatus required in the work of testing any wheels that can be supplied with water at that point, including dynamometers of various sizes, gauges, measuring scales, thermometers, clocks with electric bells to strike the minute and half-minute, and minor conveniences, all together forming the most complete outfit yet devised or anywhere in use.

The plan of the flume is exhibited in the accompanying drawing, Figures 138 to 141.

The underground portion of the testing flume consists of the trunk or penstock, A, bringing in the water; a sort of vestibule, B; an antechamber, C; the wheel-pit, D; and tail-race, E. The trunk, A, is of boiler iron, about 9 feet in diameter, laid low down in ground, so as to pass longitudinally under the center of a street, thus take up no land suitable for building purposes. The obj of the vestibule, B, is to afford opportunity for the application the two head-gates, G, G. Besides these, there is a head-gate the point of entry of the trunk, A, into the canal whence it tak the water. A small trunk, F, about 3 feet in diameter, tal water from this vestibule, independently of the gates, G, a leads it to a turbine wheel, H, set in an iron casing, in the cha ber or pit, C, so that this wheel can run, even when C is emp This wheel discharges through the floor at the bottom of thence through the arch, I, and the supplementary tail-race, into the second level canal. It is used to operate the rep shops; also to lift and lower the gates, G. The chamber, C, bounded on one side by a tier of stop-planks, L, and, on anoth side, by a tier of stop-planks, M. The object of the stop-planks, is to afford a waste-way out of the chamber, C. This is of espec use in regulating the height of the water when testing under l heads; also to skim off the oil floating on the water, which oil h dropped down from the dynamometer. The water thus pass over the planks, L, falls directly into the tail-race, K, and pass into the second level. The stop-planks, M, come into use wh testing scroll or cased wheels. In that event, D is empty of wat and the wheel-case in question is attached to the planks, M, over proper opening cut for the purpose. Large cased wheels are set the center of D, and the water is led to them by a short trunk penstock, leading from an opening cut in the planks, M. Flum wheels are set in the center of the floor of D, and D is filled wi water. They discharge through the floor of D (not shown on t drawing) and out of the three culverts, N, N, N, into the tail-ra E. At the down-stream end of this tail-race is the measuring we O, the crest being formed of a strip of planed iron plate. It c be used with or without end contractions. The depth of water the weir is measured in a quarter cylinder, P, set in a recess, fashioned in the sides of the tail-race. These recesses are wate tight, and the observer is thus enabled to stand with the water-lev about breast-high, or at convenient height for accurate observatio A platform, R, surrounds the tail-race, and is suspended from t iron beams that carry the roof. Above the tail-race is the stre The wheels to be tested arrive on this street, are lifted from t wagon by a traveling windlass that runs out on a frame-work ov

which forms the down-stream end of the wheel-pit, D. To enabl the observer at the brake-wheel, the one at the head gauge, and th one at the measuring weir to take simultaneous observations intervals of one minute, an electric clock rings three bells, simult neously at intervals of one minute, or of half a minute if desired.

The whole structure is built in a durable and efficient manne The pits and tail-race are all lined with brick laid in cement. Th stone masonry was intended by careful work and grouting to b water-tight without the brick lining, and the brick lining was the carefully laid up with joints full of mortar as an extra precautio As a consequence, the front of the wall forming the down-strea side of the pit, D, is barely damp with 20 feet head of water upo it. The floor of the pit, D, is so tight that an exact measuremen of the leakage of the wheel-gate can be made if desired. An ap proximate estimate is readily made by filling the pit before th tail-race is allowed to fill up, and apportioning the total measure leakage between the leakage of the wheel-gate and that of th flume.

U, V, W show three waste-pipes, from the vestibule antechan ber and tail-race respectively. Another, not shown, serves to dra the water out of, and through, the floor of the pit, D. To close c open these waste-pipes, they are fitted with cases of small wate wheels, which form convenient valves for the purpose. The wast pipe, V, serves also to help regulate the height of water in the ant chamber, C, and pit, D, during tests under low heads.

The pipe, W, leads into a sewer on the other side of the secon level canal, and thence into the river, and permits the tail-race to b emptied of water down to within ten inches of the bottom planking

A view of the main working floor is given in Figure 142, show ing the dynamometers in place.

When the flume was completed, and had been satisfactorily ir spected and tested, the proprietors issued the following notice, an commenced the testing of turbines for the benefit of the public:

HOLYOKE, MASS., JULY 18, 1883.

MANUFACTURERS, WATER-WHEEL BUILDERS, AND LESSORS OF WATER-POWE

GENTLEMEN: We have completed our new Testing Flume, and furnished with apparatus to test wheels of any of the usual diameters (the pit is 20 fe square) and power up to 230 H. P.

The measuring weir has a capacity of 200 cubic feet per second.

Wheels can be tested under any head from 4 to 17, and in some cases, near 19 feet. Parties sending wheels should comply with the following directions:

The wheel should be sent ready to set.

For wheels of 25 H. P., or less, have distance from platform on which wheel rests, to top of shaft, 4 feet 3 inches; have 4 inches of top of shaft 2 inches in diameter; with key seat $\frac{1}{4}$ inch wide and $\frac{3}{16}$ deep.

For wheels 26 to 75 H. P., platform to top of shaft, 5 feet 6 inches; 5 inches of top of shaft 3 inches in diameter; key seat, $\frac{9}{16} \times \frac{3}{16}$ inches.

For wheels 75 H. P., or over, platform to top of shaft, 7 feet; 6 inches of top of shaft 4 inches in diameter; key seat, 1 inch × $\frac{3}{8}$.

The price of test and of a full and carefully computed report is 10 per cent. of list price of wheel, but no wheel for less than $30. The sender to pay freight and cartage. Scroll wheels, or wheels set in iron cases, may cost from $10 to $15, possibly more, in addition.

HOLYOKE WATER-POWER CO.,
HOLYOKE, MASS.

The new flume was opened in July, 1883, as above, and shortly after the writer was employed to visit it for the purpose of witnessing tests of wheels, observing the methods of test, examining the structure and the working of the flume and apparatus, and following the engineers in their calculations of efficiency, as obtained from the data secured by test, with orders to report upon the suitability of the flume for its work and the reliability of the results so obtained. He thus had an excellent opportunity to study the system and the methods adopted by the engineer. In his inspection he was given every facility, and was aided by the chief engineer of the company in every possible way.

The method of test practiced at this flume is in some respects similar to that already described, as adopted at the old flume, the use of which has been now given up. A number of dynamometers were kept on hand for use with wheels of different sizes and power. The water measurements were made by the use of the hook-gauge, and the calculation of flow by Francis's formula, as is customary throughout the United States, wherever weir measurements are made. The wheel is usually set at the bottom of the penstock, as in Figure 131, with the dynamometer mounted upon the head of its shaft. Attendants are stationed at the dynamometer, who are expected to adjust the weights and the clamps on the band of the instrument, under the direction of the engineer in charge of the work, and to take observations of the speed at one minute intervals, usually at the stroke of an electric bell rung by the clock. Other observers are stationed at the weir, to observe the depth of water on its crest, taking their readings from the well in which the hook-gauge is set, and at the upper and lower water-levels, where other gauges give the height of the surface of the water at those points. The stroke of the bell occurs at the same instant at every post of observation, and all the readings are taken simultaneously, or, purposely, one-half minute after noting the speed of the wheel.

When the test is about to begin, the engineer having charge of the work assigns his assistants to their posts, and sees that the dynamometer is clamped fast, and a load applied at the scale-pan greater than the maximum possible effort of the turbine. The water is then let on, and the weights are carefully readjusted until they precisely balance the effort of the wheel, which is now at rest, with maximum flow of water at full head passing through it. A record is made of the weight on the dynamometer; and each assistant at the weir gets a reading from his gauge, and a record of the depth of water on the crest of the weir. Should there be any leak in the flume, the amount of the flow due to leakage is deducted from the quantity of water given, by calculation, as total flow. The weights are now reduced, and the brake-screw slacked off until the wheel is running at maximum speed. A second set of readings is recorded by all observers, and a new adjustment is made, giving a new weight, and a new and lower speed with a new measure of the volume of water passing the weir. This process is continued until it is seen that the wheel has passed a certain speed of rotation at which efficiency is a maximum. This speed is quite closely found by a few trials, and the turbine is then tested, at speeds closely approaching this, and at various degrees of gate-opening, as described in the account already given of operations at the old flume. Every test of a wheel is preceded by a careful measurement (in the presence of the proprietor, or his representative, if he so desires) of the weir and all parts of the brake. The weights used in the adjustment of the dynamometer are always examined and certified by the official "sealer of weights and measures," and every weight and measure, and every part of the apparatus, is examined and standardized with equal care. The records taken at the flume are transferred from the observers' rough note-books to the engineer's records, at the office of the chief engineer, and the data obtained at the trial of each turbine are there worked up, and the results placed on permanent record. They are not published, unless by the proprietor of the wheel tested, who sometimes publishes the engineer's certificate, which he receives, as an official copy of the determinations recorded at the office, over the signature of the chief engineer.

In the working up of the data, the most scrupulous care is taken to secure exact, and practically useful, results. The figures recorded by the observers at the trial are transferred to blanks, in a form found by experience to be most convenient and useful. The computer checks such as are capable of being verified, and then calculates the efficiency of the wheel for each set of conditions represented on the record. In making this computation, he first easily calculates from the recorded figures representing the weights on the dynamometer, and the speed of rotation given for the same time, together with the known dimensions of the brake, the power developed by the wheel, and compares this with the total available power of the fall, as calculated from the recorded head acting on the wheel, and the volume of water at the same time passing the weir. As these calculations are made, they are represented graphically on section-paper, and the curve of efficiencies thus obtained exhibits precisely at what speed maximum efficiency is to be found under the given head. The curves so constructed are a very useful check upon the accuracy of observation, and are a very interesting and important feature of the system of working up the data which has been adopted by Mr. Herschel. In addition to the data obtained at the weir, during the test of the wheel, careful measurements of the dimensions of the wheel itself are taken, and are put on record with the date of the trial.

In illustration of the system adopted by the Holyoke Water-Power Company, in the determination of the efficiencies and capacities of wheels tested for its tenants, or for makers and owners of wheels who may bring them to Holyoke for trial, the table and curves which follow are presented. For example, the figures in Table I. are derived from a test of a Boyden turbine—a Fourneyron wheel with Boyden's modifications—which was in operation in the No. 2 Mill of the Merrick Thread Company, and which was tested in its own penstock, April 26, 1882. This wheel was of large size, 90 inches in diameter, fitted with brass buckets, and constructed with all the care and skill for which its designers and builders were noted. It may be taken to represent the best work which has been done, in late years, upon the "outward flow" turbine.

The measurements obtained at the weir, previous to the test, are given at the bottom of the table. The weir was 11 feet long and was practically tight, the leakage being but 0.05 cubic feet per second. The "unit quantity," *i. e.*, that discharged when the wheel was giving the best effect at full-gate, was 148.85 cubic feet per second; the temperature of the water was 42° Fahr. The dimensions of the wheel and of the dynamometer are recorded, and the references to the original notes in the engineer's records are entered on the same sheet.

TABLE I.

RIGHT-HAND BOYDEN, FROM HOLYOKE MACHINE COMPANY, FOR MERRICK THREAD COMPANY, NO. 2 MILL. (BRASS BUCKETS.)

Tested in Mill, Wednesday, April 26, 1882.

Number of Experiment.	Proportional Gate Opening.	Penstock Gauge.	Tail-race Gauge.	Head on Wheel.	Depth on Weir.		Quantity of Water passing Weir. C. f. p. s.			Quantity of Water passing Wheel. C. f. p. s.	Weight on the Dynamometer Lever. Lbs.	Revolutions of Wheel per Minute.	H.P. developed by Wheel under actual Head.	Efficiency of Wheel. Per cent.	Quantity of Water discharged by Wheel reduced to Head of 17 feet.	Ratio of Discharge of Wheel to full-gate Discharge at maximum efficiency.	Ratio of Velocity of Periphery of Wheel to Velocity due the Head.
					North.	South.	North.	South.	Total.								
25	1.000	99.53	82.92	16.61	1.789	1.421	84.85	60.55	145.40	145.35	620	57.00	216.79	79.17	147.05	0.988	0.560
24	"	.51	.94	.57	.790	.433	84.93	61.30	146.23	146.18	595	59.33	216.55	78.82	148.07	.995	.584
23	"	.49	.89	.60	.798	.439	85.47	61.68	147.15	147.10	570	63.50	222.04	80.17	148.86	1.000	.624
22	"	.50	.88	.62	.807	.448	86.13	62.24	148.37	148.32	540	66.50	220.28	78.79	150.00	.008	.653
21	"	.54	.90	.64	.824	.454	87.30	62.62	149.92	149.87	510	71.12	222.50	78.66	151.48	.018	.697
20	0.883	.54	.88	.66	.761	.399	82.92	59.17	142.09	142.04	595	55.67	203.19	75.71	......	0.964	.546
19	"	.52	.89	.63	.765	.407	83.19	59.68	142.87	142.82	575	58.75	207.23	76.93	......	.970	.577
18	"	.52	.87	.65	.769	.410	83.47	59.85	143.32	143.27	550	62.50	210.87	77.94	......	.973	.614
17	"	.52	.88	.64	.783	.421	84.43	60.55	144.98	144.93	525	65.17	209.88	76.73	......	.984	.640
16	"	.51	.88	.63	.790	.426	84.93	60.86	145.79	145.74	500	68.37	209.71	76.29	......	.990	.671
15	0.773	.60	.86	.74	.716	.353	79.83	56.34	136.17	136.12	550	56.00	188.94	73.11	......	.922	.548
14	"	.58	.86	.72	.722	.367	80.25	57.20	137.45	137.40	525	58.75	189.21	72.61	......	.931	.576
13	"	.56	.87	.69	.729	.372	80.72	57.51	138.23	138.18	500	62.33	191.17	73.09	......	.937	.611
11	"	.58	.85	.73	.736	.388	81.20	58.50	139.70	139.65	475	66.17	192.81	72.76	......	.946	.648
12	"	.57	.84	.73	.749	.390	82.09	58.62	140.71	140.66	455	69.25	193.29	72.42	......	.953	.678
10	0.662	.59	.79	.80	.654	.303	75.62	53.29	128.91	128.86	495	55.75	169.28	68.95	......	.871	.545
9	"	.60	.79	.81	.658	.312	75.90	54.84	130.74	130.69	470	59.67	172.04	69.04	......	.883	.583
8	"	.60	.79	.81	.665	.324	76.37	54.57	130.94	130.89	450	62.00	171.15	68.58	......	.884	.606
7	"	.60	.80	.80	.673	.337	76.91	55.36	132.27	132.22	425	66.17	172.51	68.47	......	.893	.647
6	"	.61	.85	.76	.685	.340	77.73	55.54	133.27	133.22	400	69.67	170.95	67.51	......	.901	.682
5	0.442	.70	.60	17.10	.456	.127	62.69	43.01	105.70	105.65	365	50.63	113.36	55.32	......	.708	.490
4	"	.70	.62	.08	.461	.137	63.02	43.58	106.60	106.55	345	54.17	114.64	55.54	......	.714	.525
26	"	.65	.61	.04	.464	.151	63.20	44.36	107.56	107.51	320	58.50	114.84	55.27	......	.721	.568
3	"	.70	.60	.10	.470	.153	63.59	44.49	108.08	108.03	320	59.17	116.15	55.44	......	.724	.573
2	"	.72	.63	.09	.474	.157	63.84	44.71	108.55	108.50	300	60.33	111.03	52.79	......	.727	.585
1	"	.74	.66	.08	.490	.177	64.86	45.86	110.72	110.67	280	66.50	114.23	53.28	......	.742	.645

Leakage into measuring pit = 0.05 c. f. p. s.

Length of weir = { North = 11.00[illegible] / South = 11.0[illegible]1 } Experiments No. [illegible]
{ North = 11.007 / South = 11.018 } Experiments No. 16 to No. 28, inclusive.

Temperature of water = 42° Fahr.
Unit Q = 148.85 c. f. p. s., head of 17 feet.
No. 3 lever, ratio of lever arms = 10.072.
No. 3 wheel, circ. = { 20.0915 / 20.1045 } av. = 20.098.
Bucket area = 5.660 sq. ft.
Guide area = 6.814 sq. ft.
Coef. of bucket disch. = 0.7949.
Diam. inside of garniture = 67.20″.
Diam. inside of gate = 71.68″.
Diam. outside of gate = 78.45″.
Diam. outside of disk = 78.45″.
Diam. inside of wheel = 73.60″.
Diam. outside of wheel = 90.00″.
Ht. of disk guides, outer end = 9.875″.
Ht. of buckets, outer end = 9.125″.
Ht. of buckets at lowest pt., abt. 8.64.

MEASUREMENT OF WHEEL.

Angles.

Angle between tangent to circle at guide discharge, and tangent to curve of guides = 24°.

Angle between tangent to circle at inner edge of buckets, and tangent to curve of buckets = 90°.

Angle between tangent to circle at outer edge of buckets, and tangent to curve of buckets = 26°.

No. of guides = 34. (Brass.)
No. of buckets = 54. (Brass.)

Examining the table, it is seen that the tests having been ma at a number of different speeds, and at a wide range of gate-ope ings, including all needed in practical operation of the turbine, th wheel is made a good meter, and can be made to give a very a curate measure of the amount of water used by it, when in pla in the mill.

The efficiency of this wheel was found to become a maximu at full-gate, when making 63.5 revolutions per minute, developin 222 horse-power, and discharging 148.9 cubic feet of water pe minute. The velocity of its periphery was 0.624 that due th head. This wheel was 90 inches in diameter; and it will be in teresting to compare its power with that of the less costly inwa and downward flow wheels which have so largely superseded tl Fourneyron class in the markets of the United States. As th gate is closed, and the flow becomes less, it is seen that the ef ciency falls off in a much higher ratio. Thus, at 0.883 gate, about two per cent. less flow, the efficiency is a maximum at 62 revolutions, developing 211 horse-power, and discharging 0.9 the flow at maximum efficiency, the velocity of outer circumfe ence of wheel being 0.614 that due the head. At 0.773 gate, t efficiency falls to 73 per cent., at 0.662 gate, to 69 per cent., a at 0.442 gate, although discharging 72 per cent. of the quanti passing the wheel at full-gate, the efficiency falls to 55.5 per ce At the best speed at this gate, the periphery of the wheel ha velocity 0.525 that due the total head.

The points that seem here particularly noticeable are the rap loss of efficiency as the gate is closed and power reduced, the co paratively slow decrease of volume of water used, the small amou of power obtained from so large a wheel, and the gradual reducti of velocity of best efficiency as the flow decreases.

Tables II. and III. present the results of tests made upo "Collins Turbine," constructed by Messrs. J. P. Collins & C at Norwich, Connecticut, in 1883, and those made to det mine the efficiency of the wheel with and without a "draft-tub or "suction-pipe," at the testing flume. Table II. gives the d obtained by test of the wheel without draft-tube, and Table gives the same figures as obtained when the draft-tube was place, first with an interior inverted cone inserted to gi smoothly widening passage from the lower side of the discharg the wheel to the full size of the pipe, and, secondly, without cone. This work was purely experimental, and does not repr usual work; it showed the cone not to be here of advantage, although it should act as a diffuser. The measurement of wheel and weir, etc., are given in the tables, and all figures needed for calculations of efficiency as before. The best figures are the following, the head varying from 16.6 to 17.5 feet as the demand decreases:

TEST WITHOUT DRAFT-TUBE.

Gate.	Discharge.	Rev.	H. P.	Efficiency.
Full	1.00	89	97	80 4
0.748	0.90	96	88	78.9
0.600	0.82	94	71	68.8
0.503	0.69	80	58	65.9
0.303	0.52	79	36	53.77

TEST WITH DRAFT-TUBE AND CONE (FIG. 143).

Gate.	Rev.	H. P.	Effic.
1.00	90	103	83.78
0.548	87	73	71.79
0.297	76	40	54.98
	TEST WITHOUT CONE.		
1.00	96	103	84.34
0.548	90	70	71.10
0.297	70	37	53.64

The draft-tube is evidently, in this case, of service, giving three or four per cent. increased efficiency; while the reduction of loss by contraction, when the inverted cone is introduced, is unimportant. The method of setting the cone is shown in Figure 143, and its dimensions are there given. Fig. 144 shows the curve of the wheel buckets.

These figures are not strictly comparable with those given by this and other wheels as ordinarily set.

The following record of the test of a large Jonval turbine, in place in the mill, will be interesting in this place as exhibiting the efficiency which may be attained in this form of wheel, even when of exceptionally large size, and also as showing what success may be secured in the regulation of this type of wheel. It will be ob

TABLE II.

HOLYOKE TESTING FLUME.—60 LEFT-HAND COLLINS'S JOURNAL. FROM J. P. COLLINS & CO., NORWICH, CONN. *BRASS* BUCKETS.)

Tested Thursday and Friday, June 21 and 22, 1883.

Number of Experiment.	Proportional Gate-Opening.	Head on Wheel.	Depth on Weir.	Quantity of Water passing the Weir. C. f. p. s.	Quantity of Water passing the Wheel. C.f.p.s.	Weight on Dynamometer Lever. Lbs.	Revolutions of Wheel per Minute.	H. P. developed by Wheel under actual Head.	Effic. of Wheel. Per cent.	Quantity of Water Discharged by Wheel, reduced to Head of 17 ft.	Ratio of Discharge of Wheel to Full-Gate Discharge at Max. Eff.	Ratio of V. P. W. to V. D. H.
7	1.000	16.58	1.807	64.80	64.31	192	82.00	96.24	79.71	65.12	0.998	0.551
6	"	.57	.868	64.84	64.35	185	85.25	96.41	79.85	65.18	.999	.573
5	"	.55	.869	64.89	64.40	178	89.17	97.03	80.40	65.27	1.000	.600
4	"	.56	.869	64.80	64.40	170	93.00	96.65	80.03	65.25	.000	.625
3	"	.56	.871	64.98	64.49	157	99.00	95.02	78.57	65.34	.001	.665
2	"	.56	.871	64.98	64.49	142	106.50	92.45	76.45	65.34	.001	.716
1	"	.58	.873	65.09	64.60	130	110.67	87.95	72.52	65.41	.002	.743
21	0.748	.79	.750	58.99	58.50	174	81.50	86.69	77.94	......	0.902	.544
20	"	.78	.750	58.99	58.50	167	85.67	87.46	78.68	......	.902	.572
19	"	.77	.750	58.99	58.50	159	90.00	87.48	78.75	......	.902	.601
18	"	.77	.751	59.03	58.54	150	95.67	87.73	78.92	......	.903	.639
17	"	.77	.749	58.93	58.44	135	105.00	86.65	78.08	......	.901	.701
16	"	.78	.747	.84	58.35	115	115.75	81.37	73.40	......	.900	.773
32	0.600	.86	.650	54.10	53.61	146	77.90	69.53	67.93	......	.825	.519
31	"	.89	.650	54.10	53.61	138	83.50	70.44	68.70	......	.824	.556
30	"	.89	.650	54.10	53.61	121	86.00	68.87	67.17	......	.824	.572
33	"	.90	.650	54.10	53.61	127	89.75	69.68	67.92	......	.824	.597
29	"	.95	.652	54.20	53.71	123	94.25	70.87	68.75	......	.824	.626
28	"	.99	.651	54.16	53.66	115	100.00	70.30	68.10	......	.822	.664
15	0.503	17.18	.468	45.64	45.14	132	69.67	56.22	64.02	......	.688	.460
14	"	.18	.467	45.60	45.10	125	75.67	57.82	65.91	......	.687	.499
13	"	.19	.466	45.56	45.06	118	80.17	57.83	65.93	......	.687	.529
12	"	.19	.466	45.56	45.06	111	84.75	57.51	65.57	......	.687	.559
11	"	.18	.466	45.56	45.06	104	88.67	56.37	64.31	......	.687	.585
10	"	.18	.465	45.50	45.00	96	93 25	54.72	62.51	......	.686	.615
9	"	.17	.465	45.50	45.00	88	98.00	52.72	60.26	......	.686	.647
8	"	.17	.465	45.50	45.00	80	104.50	51.10	58.41	......	.686	.690
26	0.303	.41	.218	34.75	34.25	80	71.00	34.72	51.42	......	.519	.465
27	"	.42	.217	34.71	34.21	78	76.00	36.24	53.70	......	.518	.498
25	"	.41	.216	34.67	34.17	75	79.00	36.22	53.77	......	.517	.518
24	"	.40	.215	34.62	34.12	70	83.50	35.73	53.15	......	.517	.548
23	"	.43	.217	34.71	34.21	65	88.67	35.23	52.18	......	.518	.581
22	"	.55	.219	34.79	34.29	60	94.00	34.48	50.60	......	.517	.614
39	0.161	.84	0.984	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	......	[illegible]	[illegible]

LEAKAGE.

	No.	Time taken.	Head. ft.	Depth on Weir. ft.	Quantity passing Weir. c. f. p. s.	Quantity red. to Head of 16 feet.	Est. of Gate Leak. c. f. p. s.	Leak of Flume. c. f. p. s.	REMARKS.
June 21	a	P. M. 1-37	18.50	0.221	2.75		80 per cent.	0.48 used.	
" 21	b	6-17	.71	.247	3.25				Doubtful. Wheel gate probably open.
" 22	c	A. M. 9-11	.68	.205	2.46				

Weir = 8.000 feet, end contractions. {No. 2 Lever ratio of arms = 9.979. No. 3′ Wheel = 6.394.

Temperature of water = 69° F.

Unit Q = 65.27 c. f. p. s. head of 17 feet.

Guide area = 2.912 sq. ft. coef. of Guide Discharge = 0.6778.

Backet " = 2.822 " " " " Bucket " = 0.6994.

ANGLES.

Angle of tangent at upper edge of guides, with horizontal plane = 21¼°

Angle of tangent at lower edge of guides, with horizonal plane = 17¾°.

Angle at tangent at upper edge of buckets, with horizontal plane, = 77½°.

Angle of tangent at lower edge of buckets, with horizontal plane = 19°

No. of Guides = 24.

". " Buckets = 30 (Brass).

TABLE III.

HOLYOKE TESTING FLUME.—60″ L. H. COLLINS'S JOURNAL, NO 172. FROM J. P. COLLINS & CO., NORWICH, CONN.

SUBMERGED DRAFT-TUBE EXPERIMENTS { WITH INVERTED CONE, UPPER HALF OF SHEET. WITHOUT " " LOWER " " "

TESTED SATURDAY AND MONDAY, JUNE 23 AND 25, 1883.

Number of Experiment.	Proportional Gate-Opening.	Head on Wheel.	Depth on Weir.	Quantity of Water passing the Weir. c. f. p. s.	Quantity of Water passing the Wheel. c. f. p. s.	Weight on Dynamometer Lever. lbs.	Revolutions of Wheel per minute.	H. P. developed by Wheel under actual Head.	Efficiency of Wheel. Per cent.	Quantity of Water discharged by Wheel reduced to Head of 17 Feet.	Ratio of Discharge of Wheel to Full-Gate Discharge at Max. Eff.	Ratio of V. P. W. to V. D. H.
4	1.000	16.58	1.880	65.43	65.13	205	77.50	97.12	79.46	65.95	0.997	0.521
3	"	.56	.881	.48	.18	193	86.00	101.46	83.05	66.04	.998	.578
5	"	.57	.884	.60	.30	186	90.25	102.62	83.78	66.14	1.000	.606
2	"	.58	.885	.67	.37	175	95.87	102.56	83.60	66.19	.001	.644
1	"	.58	.885	.67	.37	157	106.00	101.73	82.92	66.19	.001	.712
15	0.548	.96	.613	52.39	52.09	154	74.00 -	69.66	69.66	.....	0.788	.492
14	"	.98	.612	.31	.01	146	79.25	70.73	70.76	.....	.787	.526
13	"	17.19	.619	.66	.36	137	87.33	73.14	71.79	.....	.787	.576
12	"	.01	.611	.29	51.99	127	92.25	71.62	71.54	.....	.786	.612
11	"	.01	.612	.31	52.01	115	99.50	69.95	69.85	.....	.786	.660
10	0.297	.40	.257	36.41	36.11	94	67.00	38.50	54.13	.....	.540	.439
9	"	.63	.261	.56	.26	86	75.67	39.78	54.98	.....	.538	.493
8	"	.61	.260	.52	.22	78	82.12	39.16	54.23	.....	.538	.535
7	"	.61	.259	.49	.19	72	87.12	38.34	53.15	.....	.538	.568
6	"	63	.260	.52	.22	65	93.25	37.05	51.26	.....	.538	.608
5a	"	.64	.260	.52	.22	60	97.33	35.70	49.36	.....	.538	.634
Without inverted cone, June 25, 1883.												
4	1.000 •	16.56	1.869	64.87	64.57	205	78.00	97.75	80.75	65.42	0.994	0.594
3	"	.57	.871	65.00	.70	193	85.50	100.87	83.12	65.58	.996	.575
5	"	.56	.875	.18	.88	186	89.87	102.18	84.01	65.74	.999	.604
2	"	.55	.877	.29	.99	175	96.00	102.70	84.34	65.87	1.001	.646
1	"	.56	.875	.18	.88	157	105.50	101.25	83.25	65.74	0.999	.709
15	0.548	17.01	.591	51.34	51.04	154	68.87	64.83	65.97	.....	.775	.457
14	"	.00	.591	.34	.04	146	76.00	67.83	69.05	.....	.776	.504
13	"	16.99	.590	.29	50.99	137	82.50	69.09	70.45	.....	.775	.548
12	"	17.00	.589	.22	.92	127	80.75	69.68	71.10	.....	.774	.595
11	"	.02	.588	.19	.89	115	98.50	69.25	70.62	.....	.773	.653
10	0.297	.53	.221	34.88	34.58	94	62.37	35.84	52.23	.....	.518	.407

LEAKAGE.

	No.	Time taken.	Head.	Depth on Weir.	Quantity passing Weir. c. f. p. s.	Quantity red. to Head of feet.	Est. of Gate Leak.	Leak of Flume.	REMARKS.
June 23	*a*	P. M. 2-40	18.70	0.157			90 per cent. both days.	0.80 c. f. p. s. used both days.	Little weight given the *b* leakages, as wheel-gate was found hard to shut with the flume filled, for *a* gate was shut before filling.
	b	5-01	.68	.310					
June 25	*a*	A. M. 10-22	.76	.129					
	b	P. M. 12-12	.65	.263					

Weir = 8.000 ft. with end contractions.

Temperature of water { 72° F., June 23d.
71° F., June 25th.

Unit Q = { 66.15 c. f. p. s. under head of 17 ft., June 23d.
65.80 " " " " " " 25th.

No. 2 Lever, ratio of arms = 9.979.

No. 3 Wheel, diameter = 6.394.

Guide area = 2.912 ft., coef. of Guide Discharge = { 0.6870 June 23d.
0.6868 " 25th.

Bucket " = 2.822 " " " Bucket " = { 0.7069 " 23d.
0.7051 " 25th.

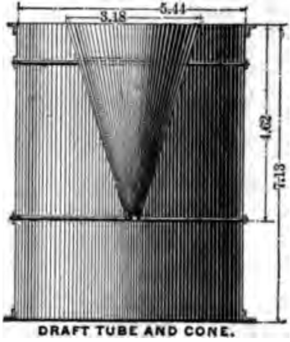
Angles see sheet No. 172.

No. of Guides = 24.

" " Buckets = 30 (Brass).

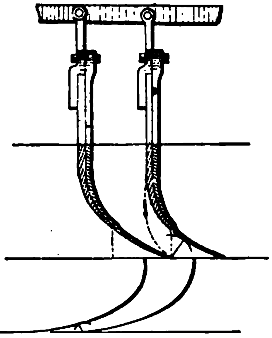
For method of using cone see sketch bearing same No. as this sheet (173 & 174) served that the efficiency is remarkably well sustained at gates even as low as 60 per cent. of full flow. The test of this wheel also was made by the engineer of the Holyoke Water-Power Co., who certifies the copy from which the figures here given are abstracted. The wheel was, when tested, set in the working flume of the Merrick Thread Co., ready for its work of driving the mill, and, when the test began, had not been in use an hour. It may be expected, therefore, that it will improve perceptibly, after working long enough to get its bearings smooth, and its connections in good shape. The performance is remarkably uniform. Altering the speed of the wheel at full-gate, from 47½ to 76¾ revolutions per minute, the efficiency varied but 3½ per cent., while the quantity of water discharged varied less than 1 per cent.

Fig. 143.



DRAFT TUBE AND CONE.

Fig. 144.



BUCKET, GUIDE AND GATE.

The regulation of this wheel is secured by adjustment of the guides. This was one of the regular patterns of its makers. As they state, it was not specially prepared for the test. Two 96-inch wheels of this class have been adopted by the same mill proprietors, recently. These figures will be the more interesting, as a really efficient system of guides, giving good part-gate efficiency, has usually been considered as something to be hoped for, but hardly to be expected, with this class of wheels.

TEST OF A COLLINS-JONVAL TURBINE.

Gate.	Disc'ge.	Head.	Rev.	Water.	H. P.	Eff.
1.00	0.999	16.59	63.38	113.46	131.49	85.06
0.778	0.892	16.75	63.00	101.67	160.83	83.27
0.631	0.764	16.95	58.93	87.66	133.56	79.26
0.547	0.670	17.12	54.82	77.19	111.36	74.64
0.422	0.565	17.26	49.65	65.36	84.40	65.96

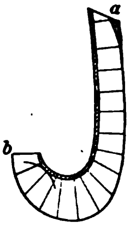
The above is the best performance of a Jonval wheel, on the whole, of which the writer has been able to obtain authentic records. The test is "No. 33," on the books of the Holyoke Water-Power Co., and was made November 16th, 1884. The wheel was a "right-hand" pattern.

Tables IV., V., VI., are taken from the record books of the Holyoke Water-Power Co., for the purpose, principally, in this connection, of showing the accuracy of the work done at the new flume. The wheel of which the performance is given in these tables was 36 inches in outside diameter, and of the Inward and Downward Flow class. When first tested, its performance was so much better than the makers had anticipated, that it was concluded to repeat the test. The second trial giving an equally good result, it was feared by the constructors that there might be some fault of construction in the flume or of method in calculation; and the writer was requested to investigate these points, and determine, if possible, whether it would be safe to accept the values of efficiency reported. The third trial was made in the presence of the writer, and the data taken under his eye. The calculations were made in the customary way, and the results were then compared with those given by the earlier trials, as the best way of ascertaining the limits of probable error in the whole operation. The tests of the wheel were made in the usual manner, and the data sent into the office and worked up precisely as is customary in all cases, with the results given in the tables. Figures 145 and 146 give the section of buckets and guides measured as per Table VII. The whole series of figures, with the data, were furnished to the writer, from the books of the Company, in the customary form, with such additional details as were desired.

Fig. 145.

a–Top width.
b–Bottom width.
c–Height.
GUIDES.

Fig. 146.



BUCKET.

TABLE IV.

HOLYOKE TESTING FLUME, 36″ R. H. HERCULES, FROM HOLYOKE MACHINE COMPANY. NO. 190.

TESTED MONDAY AND TUESDAY, AUGUST 13 AND 14, 1883.

Number of experiment.	Proportional gate opening.	Head on wheel.	Depth on weir.	Quantity of water passing the weir. c. f. p. s.	Quantity of water passing the wheel. c.f.p.s.	Weight on dynamometer lever, lbs.	Revolutions of wheel per minute.	H.P. developed by wheel under actual head.	Efficiency of wheel. per cent.	Quantity of water discharged by wheel, reduced to head of 17 feet.	Ratio of discharge of wheel, to full-gate discharge at max. eff.	Ratio of V. P. W. to V. D. H.
1	1.000	16.97	1.316	101.45	101.16	350	Still.	......	......	......	1.141	......
7	"	17.12	.237	92.42	92.13	211	113.00	145.39	81.44	91.81	.035	0.535
6	"	.12	.234	92.07	91.78	204	115.00	143.05	80.44	91.46	.031	.544
5	"	.15	.227	91.31	91.02	197	123.60	148.47	84.04	90.62	.021	.585
4	"	.16	.221	90.64	90.35	188	127.50	146.16	83.30	89.93	.014	.603
3	"	.16	.217	90.20	89.91	182	132.50	147.05	84.21	89.49	.009	.626
8	"	.18	.213	89.75	89.46	176	138.00	148.10	85.14	88.99	.003	.652
2	"	.20	.210	89.43	89.14	170	142.00	147.20	84.83	88.62	0.999	.671
9	"	.18	.201	88.44	88.15	163	147.00	146.11	85.24	87.69	.988	.695
26	0.800	.26	.135	81.20	80.91	194	110.67	130.92	82.83	......	.905	.522
25	"	.31	.127	80.34	80.05	187	117.00	133.41	85.07	......	.894	.551
24	"	.31	.122	79.79	79.50	180	122.33	134.27	86.21	......	.888	.576
23	"	.33	.116	79.14	78.85	174	126.50	134.22	86.78	......	.880	.595
22	"	.35	.112	78.73	78.44	167	131.50	133.91	86.94	......	.875	.617
21	"	.37	.111	78.63	78.34	160	136.50	133.17	86.47	......	.874	.641
20	"	.40	.104	77.90	77.61	150	144.00	131.71	86.18	......	.865	.676
31	0.645	.55	.025	69.65	69.36	160	112.00	109.27	79.31	......	.769	.524
30	"	.55	.019	69.03	68.74	154	121.50	114.09	83.56	......	.763	.568
29	"	.56	.010	68.12	67.83	146	130.00	115.74	85.85	......	.752	.608
28	"	.58	.004	67.52	67.23	138	136.00	114.44	85.55	......	.745	.635
27	"	.58	0.995	66.62	66.33	130	142.00	112.56	85.29	......	.735	.663
19	0.490	.83	.901	57.35	57.06	133	107.00	86.78	75.36	......	.628	.496
18	"	.50	.892	56.50	56.21	125	113.60	86.59	77.77	......	.624	.532
17	"	.53	.888	56.10	55.81	119	120.00	87.08	78.64	......	.619	.561
16	"	.80	.881	55.45	55.16	112	130.00	88.78	79.89	......	.608	.603
15	"	.82	.870	54.43	54.14	105	136.00	87.08	79.74	......	.596	.631
14	0.390	.82	.795	47.52	47.23	110	100.67	67.52	70.89	......	.520	.467
13	"	.82	.790	47.08	46.79	103	109.50	68.77	72.88	......	.515	.508
12	"	.85	.785	46.63	46.33	96	118.67	69.47	74.22	......	.510	.550
11	"	.97	.774	45.63	45.33	87	129.75	68.83	74.66	......	.497	.599
10	"	18.01	.768	45.11	44.81	81	135.67	67.01	73.36	......	491	.626

LEAKAGE.

	Number.	Time taken.	Head.	Depth on weir.	Quantity passing weir. c. f. p. s.	Quantity reduced to head of feet.	Estimate of gate leak.	Leak of flume.	Remarks.
		P. M.							
Aug. 13...	*a.*	2-21	18.88	0.022	0.22		¼ total.	0.80	
Aug. 13....	*b.*	8-26	.91	.087	.48				
		A. M.							
Aug. 14....	*c.*	10-09	.52	.066	1.12				
					Av. = 0.61				

Full length weir, no end contractions.

Temperature of water = 73° F.

Unit Q = 88.72 c. f. p. s. head of 17 feet.

No. 8 lever, ratio of arms = 10.043.

No. $\frac{8}{8}$ Wheel. Diam. $\frac{6.371}{6.385}$. Av. 6.378.

Bucket area = 7.925 square feet.

Guide area = 4.752 square feet.

Coefficient of guide discharge = 0.5646.

Weir Length.

Crest = 20.086 feet.

0.5 up = 20.089 feet.

1.0 up = 20.080 feet.

1.5 up = 20.016 feet.

2.0 up = 20.015 feet.

Angles.

Angle between tangent to circle at guide discharge, and center line of guides = 14¾°.

Angle between tangent to circle at outer edge of buckets, and tangent to curve of buckets = 98°.

Direction of the water is a component of inward and downward discharge.

No. of Guides = 21.

No. of Buckets = 17.

TABLE V.

HOLYOKE TESTING FLUME. RETEST OF 36″ R. H. HERCULES, NO. 190, FROM HOLYOKE MACHINE COMPANY.

TESTED WEDNESDAY, OCTOBER 10, 1883.

Number of Experiment.	Proportional Gate Opening.	Head on Wheel.	Depth on Weir.	Quantity of Water passing the Weir. c. f. p. s.	Quantity of Water passing the Wheel. c.f.p.s.	Weight on Dynamometer Lever. Lbs.	Revolution of Wheel per Minute.	H.P. developed by Wheel under actual head.	Efficiency of Wheel. %	Quantity of Water discharged by Wheel, reduced to head of 17 feet.	Ratio of discharge of Wheel, to full-gate discharge at max. eff.	Ratio of V. P. W. to V. D. H.
7	1.000	17.19	1.228	91 42	91.36	200	121.25	147.82	83.01	90.85	1.028	0.573
6	"	.18	.223	90.86	90.80	198	125.67	147.85	83.59	90.32	.022	.594
5	"	.20	.217	90.20	90.14	185	131.25	148.01	84.20	89.61	.014	.620
4	"	.21	.211	89.55	89.49	177	136.50	147.28	84.34	88.94	.007	.644
3	"	.24	.205	88.88	88.82	170	142.00	147.15	84.75	88.20	0.998	.670
2	"	.28	.202	88.55	88.49	163	147.12	146.18	84.31	87.77	.993	.693
1	"	.31	.196	87.89	87.83	155	152.50	144.09	83.59	87.04	.985	.718
19	0.796	.42	.128	80.44	80.38	187	117.50	133.94	84.36	.....	.899	.551
18	"	.43	.123	79.90	79.84	180	122.75	134.69	85.36	.....	.892	.576
17	"	.43	.119	79.47	79.41	174	126.75	134.44	85.66	.....	.888	.595
16	"	.46	.114	78.94	78.85	167	131.75	134.12	85.89	.....	.881	.618
15	"	.47	.108	78.30	78.24	158	138.67	133.56	86.18	.....	.874	.650
14	"	.49	.102	77.69	77.63	150	144.25	131.90	85.68	.....	.866	.676
30	0.654	.65	.029	70.08	70.02	160	118.25	115.33	82.30	.....	.778	.551
29	"	.65	.024	69.57	69.51	153	125.75	117.28	84.31	.....	.772	.586
28	"	.66	.019	69.05	68.99	146	131.75	117.26	84.88	.....	.766	.614
27	"	.68	.010	68.11	68.05	138	138.42	116.44	85.36	.....	.755	.645
26	"	.73	.002	67.32	67.26	130	143.75	113.91	84.15	.....	.745	.668
25	0.489	.85	0.897	56.99	56.93	133	105.25	85.33	74.06	.....	.629	.488
24	"	.84	.893	56.58	56.52	125	113.33	86.36	75.53	.....	.624	.526
23	"	.83	.886	55.93	55.87	119	120.67	87.53	77.50	.....	.617	.560
22	"	.87	.879	55.27	55.21	112	128.00	87.39	78.12	.....	.609	.593
21	"	.90	.870	54.43	54.37	105	134.75	86.25	78.16	.....	.600	.624
20	"	.94	.860	53.50	53.44	97	142.00	83.96	77.24	.....	.589	.657
13	0.382	18.03	.790	47.07	47.01	109	98.67	65.56	68.22	.....	.517	.455
12	"	.00	.786	46.73	46.67	102	108.17	67.26	70.61	.....	.513	.499
11	"	17.98	.780	46.17	46.11	95	117.00	67.76	72.08	.....	.507	540
10	"	.99	.772	45.47	45.41	87	126.25	66.95	72.28	.....	.500	.583
9	"	.99	.764	44.77	44.71	81	132.50	65.42	71.74	.....	.492	.612
8	"	.99	.758	44.23	44.17	75	138.37	63.26	70.21	.....	.486	.639

LEAKAGE.

Number.	Time Taken.	Head.	Depth on Weir.	Quantity passing Weir. c.f.p.s.	Quantity reduced to head of feet.	Estimate of Gate Leak.	Leak of Flume.	Remarks.
a.	A.M. 9.38	18.82	0.022	0.22		85 per cent. Total.	0.180.07	Leak of Weir forebay found to be 0.07, add this to Quantity or substract from Leak.
b.	P.M. 2.00	.83	.070	1.24				
c.	P.M. 4.29	.82	.065	1.10				
			Av.....	0.85				

Full length weir, no end contractions.

Temperature of water = 52° F.

Unit Q. = 88.35 c. f. p. s., head of 17 feet.

No. 3 Lever, ratio of arms = 10.048.

No. $\frac{3}{3'}$ wheel. Diam. $\begin{matrix}6.367\\6.385\end{matrix}$. Av. 6.376.

Bucket area = 7.925 square feet.

Guide area = 4.727 square feet.

Coefficient of Guide Discharge = 0.5652.

Weir Length.

Crest = 20.036 feet.
0.5 up. = 20.039 feet.
1.0 up. = 20.030 feet.
1.5 up. = 20.016 feet.
2.0 up. = 20.015 feet.

Angles.

Angle between tangent to circle at guide discharge, and centre line of guides, = 14¾°.

Angle between tangent to circle at outer edge of buckets, and tangent to curve of buckets, = 98°.

Direction of the water is a component of inwards and downwards discharge.

No. of Guides = 24.

No. of Buckets = 17.

TABLE VI.

HOLYOKE TESTING FLUME. THIRD TEST 36" R. H. HERCULES NO. 190, FROM HOLYOKE MACHINE CO. TESTED SATURDAY, OCT. 13, 1883.

Number of Experiment.	Proportional Gate Opening.	Head on Wheel.	Depth on Weir.	Quantity of Water passing the Weir. c. f. p. s.	Quantity of Water pass'g the Wheel. c. f. p. s.	Weight on Dynamometer Lever. Lbs.	Revolutions of Wheel per minute.	H.P. developed by Wheel under actual head.	Efficiency of Wheel. %	Quantity of Water discharged by Wheel, red. to head of 17 feet. Last 6 Ex. red. to 10 feet.	Ratio of discharge of Wheel, to full gate discharge at max. eff.	Ratio of V. P. W. to V. D. H.
1	1.000	16.82	1.305	100.20	No flume leak, hence this column is the same as preceding one.	345	Still.				1.139	
7	"	.90	.214	89.87		193	123.50	145.30	84.39	90.14	.019	0.588
6	"	.94	.212	89.66		185	129.10	145.59	84.55	89.82	.016	.614
5	"	.95	.206	89.09		177	135.33	146.02	85.38	89.13	.008	.644
4	"	.96	.200	88.33		170	140.62	145.72	85.80	88.43	.000	.669
3	"	.98	.197	87.79		163	144.80	143.88	85.14	87.84	0.993	.688
2	"	17.05	.189	87.12		155	152.00	143.62	85.29	86.99	.984	.721
18	0.806	16.84	.129	80.56		188	112.33	128.73	83.70	....	.915	.536
17	"	.85	.123	79.88		181	117.00	129.09	84.60	....	.907	.558
16	"	.88	.118	79.38		173	123.00	129.71	85.39	....	.901	.586
15	"	.88	.111	78.63		165	130.25	131.01	87.07	....	.892	.621
14	"	.92	.105	78.00		156	137.00	130.28	87.08	....	.884	.652
13	"	.94	.099	77.38		148	143.00	129.01	86.82	....	.877	.680
29	0.647	17.05	.014	68.53		158	111.50	107.39	81.07	....	.774	.529
28	"	.05	.007	67.83		151	120.00	110.46	84.25	....	.766	.569
27	"	.09	.001	67.25		144	126.50	111.04	85.22	....	.758	.599
26	"	.15	0.994	66.52		136	134.67	111.65	86.33	....	.749	.637
25	"	.16	.986	65.72		128	140.40	109.55	85.69	....	.740	.664
24	0.489	.24	.888	56.11		128	106.60	83.18	75.85	....	.630	.508
23	"	.25	.882	55.54		121	115.60	85.27	78.50	....	.623	.545
22	"	.22	.874	54.80		114	123.00	85.48	79.90	....	.616	.581
21	"	.28	.866	54.07		107	130.00	84.79	80.05	....	.606	.618
20	"	.27	.858	53.31		100	136.00	82.90	79.48	....	.598	.641
19	"	.25	.853	52.85		95	140.67	81.46	78.82	....	.593	.663
12	0.379	.62	.774	45.64		102	104.00	64.66	70.93	....	.507	.485
11	"	.61	.769	45.20		95	112.75	65.29	72.36	....	.502	.526
10	"	.63	.761	44.50		87	122.67	65.06	73.06	....	.494	.572
9	"	.66	.753	43.76		81	129.40	63.89	72.93	....	.485	.608
8	"	.65	.747	43.24		75	135.00	61.72	71.84	....	.480	.629
34	1.000	9.59	1.009	68.01		111	88.67	60.00	81.14	69.45	1.034	.561
33	"	.62	.008	67.41		106	93.33	60.31	82.03	68.73	.023	.589
32	"	.61	0.999	67.02		102	96.80	60.19	82.43	68.37	.018	.612
31	"	.65	.992	66.31		96	103.33	60.47	83.26	67.50	.005	.651
30	"	.65	.985	65.62		90	109.00	59.80	83.80	66.80	0.994	.687
35	"	.67	.981	65.23		85	114.00	59.07	82.00	66.33	.987	.718

LEAKAGE.

27 Number.	Time taken.	Head.	Depth on Weir.	Quantity passing Weir, c. f. p. s.	Quantity reduced to head of feet.	Estimate of Gate Leak.	Leak of Flume.	Remarks.
	A.M. 10.03	18.64	0.015			Entire leak.	None.	
b.	P.M. 2.03	.35	.045					
c.	5.35	10.71	.080					

Full length weir, no end contractions.

Temperature of water = 55° F.

Unit Q. = $\genfrac{}{}{0pt}{}{\text{88.43 c. f. p. s. head of 17 feet.}}{\text{67.18 c. f. p. s. head of 10 feet.}}$

No. 3 Lever, ratio of arms = 10.043.

No. $\genfrac{}{}{0pt}{}{3'}{3}$ wheel.. Diam. $\genfrac{}{}{0pt}{}{6.385}{6.367}$. Av. 6.376.

Bucket Area = 7.925 square feet.

Guide Area = 4.727 square feet.

Coefficient of Guide Discharge = $\genfrac{}{}{0pt}{}{\text{0.5658 head of 17 feet.}}{\text{0.5604 head of 10 feet.}}$

Weir Length.

Crest = 20.086 feet.
0.5 up. = 20.089 feet.
1.0 up. = 20.080 feet.
1.5 up. = 20.016 feet.
2.0 up. = 20.015 feet.

Angles.

Angle between tangent to circle at guide discharge, and centre line of guides, = 14¾°.

Angle between tangent to circle at outer edge of buckets, and tangent to curve of buckets, = 98°.

Direction of the water is a component of inwards and downwards discharge.

No. of Guides = 24.

No. of Buckets = 17.

Comparing the figures given in the tables, as representing the performance of the wheel on the three days of the tests, we obtain the following:

PERFORMANCE OF THE HERCULES TURBINE.

36" HERCULES.

1st Test.

Gate.	Prop. Disch.	Head.	Revs.	H. P.	Efficiency.
1.000	1.000	17.18	138	148	85.14
0.800	0.875	17.35	132	134	86.94
.645	.752	17.56	130	115.7	85.85
.490	.608	17.80	130	89	79.89

2d Test.

Gate.	Prop. Disch.	Head.	Revs.	H. P.	Efficiency.
1.000	0.998	17.24	142	147	84.75
0.796	.874	17.47	139	134	86.18
.654	.755	17.68	138	116	85.36
.489	.600	17.90	135	86	78.16

3d Test.

Gate.	Prop. Disch.	Head.	Revs.	H. P.	Efficiency.
1.000	1.000	16.46	141	146	85.80
0.806	0.884	16.92	137	130	87.08
.647	.749	17.15	135	112	86.33
.489	.606	17.28	130	85	80.05

PROPORTIONAL DISCHARGE.

Line.	Size.	Name of Wheel.	Date of Test.
- - - - - - - - - - -	36"	Hercules.	Aug. 18, 1883.
——·——·——	Same	Wheel, 2d Test.	Oct. 10, 1883.
————————	"	" 3d "	Oct. 13, 1883.

By discharge is here meant the proportion of flow to the flow at full-gate when giving best efficiency. The curve given with the table, Fig. 149, exhibits the method of variation of efficiency with discharge.

Studying the table above given, and comparing tests made under substantially similar conditions, it will be seen that the results obtained are substantially identical. Thus, at full-gate, with equal speeds, as in the second and third trials, the efficiencies are within one per cent., and the speeds within one revolution, in 147, of being alike. The velocity of the wheel was the same to within 0.001 and the measured flow of water differed but one-half cubic foot per second. The best speeds corresponded to within one revolution per minute. The same comparison being made between the first and second trials, the same results are noted. At five-eighths gate, the conditions at the three trials were, as it happens, almost exactly the same. The result is, that the calculated efficiency is the same for all, within one-half per cent. The irregularities to be found in the record of heads, speeds, and flow, in any two cases, are invariably found to give rise to equally slight, and always consistent, irregularities of calculated results. A careful examination of these several sets of figures has led the writer to the conclusion that the Holyoke Testing Flume, and the methods of observation and calculation employed there, are capable of giving the efficiencies of turbines tested correctly within a limit of error of certainly less than one per cent., and probably to within one-half of one per cent. For all practical purposes, the results of the trials of turbine water-wheels, at the Holyoke flume, may be taken as exact, and absolutely trustworthy.

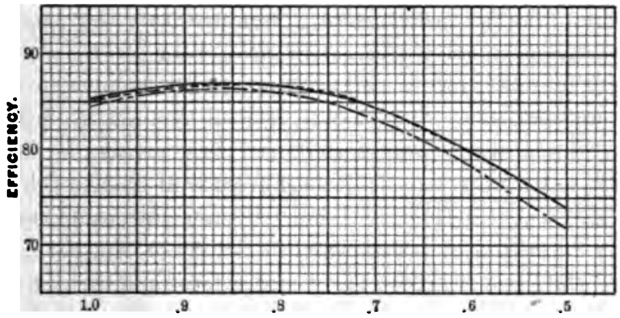


FIG. 149.

Variations do occur, however, in the actual performance of wheels of nominally identical size and form, and even when made from the same patterns; but it is now well known that it is extremely difficult to make two wheels of exactly the same form, or to give the same performance. Makers can rarely construct a right, and a left-hand, wheel that shall give the same efficiency, within two or three per cent.; and equal differences often arise among wheels made from the same patterns, in consequence of the warping of the patterns in the sand, when of wood, or even warping of the castings themselves, if not carefully cooled so as to avoid shrinkage strains. Tests of any one wheel are evidently of no great value in determining the probable performance of any other wheel of even the same size, except within a moderate degree of approximation and they may be entirely misleading, if taken as a gauge of the probable efficiency of wheels of other patterns or sizes. New patterns will always be likely to give new figures when the whee made from them is tested.

The builders of the wheel, the performance of which is above given, have thirty-six different sizes and patterns of wheel, all of which have been subjected to test to determine the efficiency, bes speeds, etc., that the makers may safely guarantee. No two of the wheels thus far tested have given precisely the same figures and, in some cases, the differences have been quite considerable Whenever one of these wheels is found to fall below the efficiency that the makers have fixed upon as a minimum, the patterns are altered and the new wheel tested, and this process is repeated, i necessary, until that size is brought up well above the assumed limit. The wheel is then, and not till then, put upon the market Thus, the following tables exhibit the behavior of a pair of smaller wheels of the same make—a right and a left-hand turbine of fifteen inches diameter each, tested at the Holyoke flume. The result are substantially the same, the left-hand wheel doing a little the better work, but it is not usually possible to secure this accordance in any two newly made turbines.

TESTING FLUME OF THE
HOLYOKE WATER-POWER CO., HOLYOKE, MASS.

Report of Tests of a 15-inch Right-hand Hercules Turbine Wheel, Brass Buckets, No. 185, made January 25, 1883.

Number of the experiment.	Proportional part of the full opening of the speed gate;	Proportional part of the full discharge of the wheel; being the discharge at full-gate when giving best efficiency;	Head acting on the wheel;	Duration of the experiment;	Revolutions of the wheel;	Quantity of water discharged by the wheel;	Power developed by the wheel;	Efficiency of the wheel;
	In per cent.		in feet.	In min.	per minute.	cubic ft. per sec.	H. P.	In per cent.
1	1.000	1.111	15.18	1	Still.	16.54		
8	1.000	.017	.40	4	261.50	15.25	20.71	77.75
7	1.000	.013	.39	3	273.67	15.18	20.84	78.65
6	1.000	.015	.46	3	**292.67**	15.10	**21.39**	**80.80**
4	1.000	.002	.44	4	300.50	15.05	21.05	79.88
5	1.000	0.999	.45	4	314.00	15.00	21.04	80.05
3	1.000	.995	.43	4	329.00	14 93	21.04	80.54
2	1.000	.992	.44	3	337.67	14.89	20.57	78.89
19	0.844	.905	.10	3	266.67	13.44	18.27	79.40
18	0.844	.898	.19	3	281.67	13.38	18.44	80.02
17	0.844	.894	.19	4	**297.50**	13.31	**18.57**	**81.10**
16	0.844	.891	.18	3	302.33	13.27	18.42	80.61
15	0.844	.887	.19	3	315.67	13.21	18.27	80.27
14	0.844	.877	.23	3	344.33	13.08	17.82	78.91
27	0.695	.784	.11	4	270.00	11.64	15.21	76.27
26	0.695	.778	.15	4	290.25	11.57	15.47	77.82
25	0.695	.768	.16	4	**305.50**	11.43	**15.59**	**79.31**
24	0·695	.763	.11	4	310.75	11.34	15.14	77.93
23	0.539	.638	14.86	3	248.00	9.40	10.95	69.14
22	0.539	.633	.86	4	270.00	9.32	11.10	70.68
21	0.539	.625	.85	3	289.00	9.20	11.00	**71.01**
20	0.539	.622	.90	3	299.00	9.18	10.93	70.45
13	0.350	.443	15.65	2	229.00	6.70	6.63	55.72
12	0.350	.438	.67	3	**258.67**	6.63	**6.70**	**56.84**
9	0.350	.437	.64	2	268.00	6.60	6.53	55.78
11	0.350	.433	.65	3	283.00	6.54	6.46	55.60
10	0.350	.433	.67	4	293.25	6.55	6.25	53.71

TESTING FLUME OF THE
HOLYOKE WATER-POWER CO., HOLYOKE, MASS.

Report of Tests of a 15-inch Left-hand Hercules Turbine Wheel, Brass Buckets, No. 149, made March 9, 1888.

Number of the experiment.	Proportional part of the full opening of the speed-gate;	Proportional part of the full discharge of the wheel; being the discharge at full-gate when giving best efficiency;	Head acting on the wheel;	Duration of the experiment;	Revolutions of the wheel;	Quantity of water discharged by the wheel;	Power developed by the wheel;	Efficiency of the wheel;
	in per cent.		in feet.	in min.	per minute.	cubic ft. per sec.	H. P.	in per cent.
1	1.000	1.131	17.13	3	Still.	17.85		
6	1.000	.010	.11	3	305.00	15.92	25.10	81.25
7	1.000	.005	.06	3	318.33	15.82	24.74	80.88
5	1.000	.006	.29	3	321.67	15.94	25.49	81.56
4	1.000	.003	.29	3	333.33	15.90	25.40	81.47
3	1.000	.000	.29	3	**347.33**	15.85	**25.41**	**81.75**
2	1.000	0.994	.29	3	371.00	15.76	24.88	80.50
19	0.803	.872	.24	4	306.50	13.80	22.42	83.10
18	0.803	.867	.25	3	**321.00**	13.73	**22.50**	**83.78**
17	0.803	.864	.32	4	334.00	13.71	22.40	83.16
16	0.803	.858	.46	3	351.33	13.67	22.49	83.08
20	0.083	.853	.27	5	356.40	13.51	21.73	82.11
24	0.625	.726	.46	4	289.00	11.57	17.62	76.90
23	0.625	.724	.40	4	308.25	11.51	17.85	78.59
22	0.625	.715	.41	3	**327.33**	11.37	**17.96**	**79.99**
21	0.625	.709	.43	2	343.00	11.29	17.77	79.64
29	0.513	.625	.61	3	279.00	10.00	14.46	72.38
28	0.513	.621	.59	4	300.00	9.93	14.63	73.85
27	0.513	.614	.59	4	**316.50**	9.82	**14.47**	**73.87**
26	0.513	.610	.57	3	332.00	9.74	14.17	72.9
25	0.513	.603	.56	2	347.50	9.63	13.77	71.8
15	0.388	.500	.91	3	247.00	8.07	10.54	64.3
14	0.388	.496	.89	3	**269.00**	8.00	**10.66**	**65.6**
13	0.388	.492	.86	3	286.67	7.93	10.48	65.
12	0.388	.486	.84	4	307.75	7.82	10.32	65.
11	0.388	.482	.81	3	326.00	7.75	9.94	63.
10	0.388	.475	.79	3	342.67	7.63	9.40	61.
9	0.388	.470	.71	5	355.80	7.54	8.68	57.
8	0.388	.464	.70	4	371.75	7.44	7.93	53.

A 51-inch L. H. wheel tested in this flume July, 1883, was reported thus:

TESTS OF A 51″ HERCULES TURBINE.

Gate.	Prop. Disch.	Head.	Revs.	H. P.	Efficiency.
1.000	0.999	12.77	81.67	191.01	82.69
0.897	.935	12.68	82.00	177.43	83.47
.808	.873	12.85	81.50	170.00	83.40
.732	.809	12.90	81.30	156.30	82.80
.651	.728	13.18	82.00	143.47	81.20
.501	.586	13.70	82.28	116.01	76.80
.369	.454	13.86	82.00	83.46	70.76

The method of working up the data secured at these tests is exhibited in Fig. 147, and well illustrates the nicety of work demanded on the part of the engineer and his assistants. As has been seen, the effort is made, at the trials, to find the speeds of best work for the several heads, and the efficiencies at these best velocities of wheel. As the velocities given on the records of the trial are taken acccording to the judgment of the engineer in charge of the work, and at intervals, it is not likely that the best figures obtained will be those of the very best conditions attainable; but it is always to be expected that the very best speed will fall between two of the speeds observed. If the law of variation of efficiency and of velocity of wheel can be determined, however, it becomes easy to ascertain where the true maximum may be found, and to obtain the exact value of the efficiency of the wheel at that

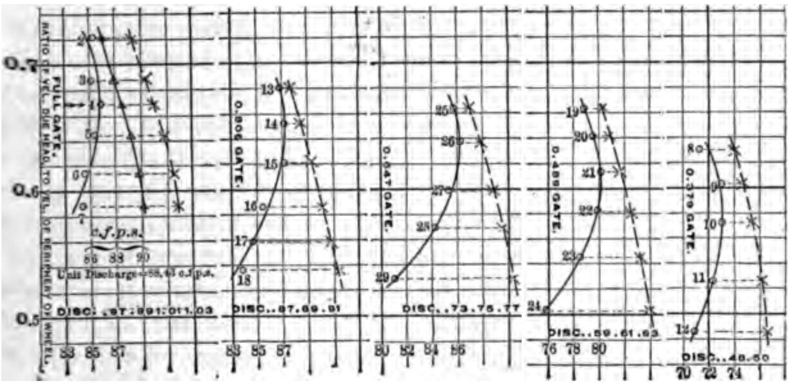


FIG. 147.

speed of maximum efficiency. This is very easily accomplished by laying down a diagram of "proportional discharges" and efficiencies, and thus obtaining a graphical representation of the required law of variation of efficiency, as exhibited in its relation to variation of the quantity of water discharged, and hence to the speeds and gates. This is done as shown in Fig. 147, on section paper of any convenient scale. A diagram is constructed for each series of tests, on which the ordinates represent the ratio of the velocity due the total head to the velocity of the periphery of the wheel; while the abscissas measure the ratio of the actual discharge, at the given speed, to the "unit discharge," *i. e.*, to the quantity of water passing the wheel at the full-gate discharge, when giving the best efficiency of the wheel at that gate. This diagram is constructed by plotting the figures given for several points in the curve, as calculated from the corresponding tests made during the trial of the wheel, and then striking through these points a smooth curve which represents the law required. The maximum abscissa of this curve —which abscissa may, but probably, in any given case, will not, fall at any one of the points determined by experiment—wherever it may come, is the point which measures the maximum efficiency of the turbine at the given gate, and determines the best speed and discharge of the wheel, corresponding to that gate and efficiency. This operation is repeated for each of the gates adopted at the trial of the wheel, and thus the maximum efficiency attainable at each gate is determined, and the best speed of wheel, under the given head and at the given gate-opening, is made known. The law of variation of best speeds, and of highest efficiency, is also ascertained, for the whole range of working of the wheel. Thus, Fig. 147 gives the curves found for the turbine tested for the writer, as already described, for the five different gates at which its efficiency was observed.

Fig. 148, in a similar manner, represents the curves obtained, both for the maximum and for mean efficiency, from full discharge to 0.75, and from full to 0.5 respectively, from the 15-inch wheels of which the "*logs*" of test have been just given. By "gate," in these last cases, is meant the opening of the gate, as actually shown by the wheel at the time, and by "full discharge" is here meant the discharge at full-gate when the wheel is working at full-gate and at maximum efficiency. In the last diagrams, also, the figures above the curve represent the proportion existing between the velocity of the periphery of the wheel, on the receiving side, at the speed which gives maximum average efficiency from full to half-gate, or to three-fourths gate, as the case may be, to the velocity due the full head on the wheel. This last "velocity ratio" is sometimes the same for both curves.

The proprietors of the flume now identify wheels tested by a leaden dial securely fastened to the wheel.

The formula used for computing the flow over the weir is that of Mr. James B. Francis.* $Q = 3.33(L-0.1nd)d^{\frac{3}{2}}$, in which Q is the number of cubic feet flowing per second, L the length of the weir in feet, d the depth of water over the weir in feet, and n is the number of "end-contractions." To secure accuracy in the application of the formula, the up-stream edge of the crest of the weir is made straight, sharp, and smooth—usually by constructing it with an iron edge, chamfered sharply, and fitting similar apertures at the

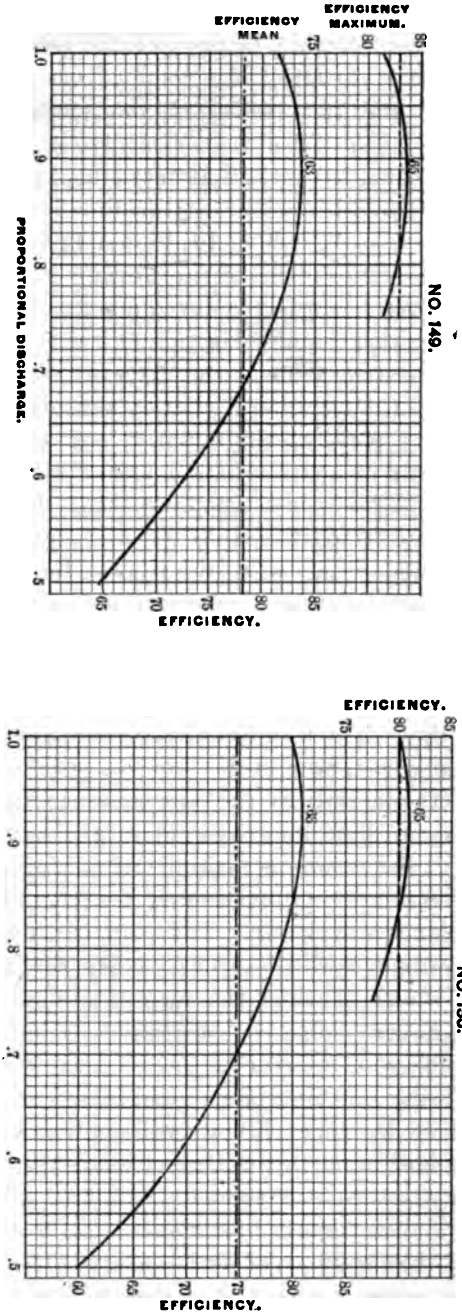


Fig. 148. DIAGRAMS FOR WHEEL TESTS 135 & 149.

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* Lowell: Hydraulic Experiments.

sides; the depth of the water in the race approaching the weir i
made considerable, to prevent the velocity of approach becoming
so great as to affect the results. Occasionally the weir is carrie
out to the full width of the raceway and then has no sid
contractions. The weir at Holyoke is often used without end
contraction, being then 20 feet long, in which case the second ter
in the parenthesis is zero; or the flume may be used with en
contractions, 2, 4, 6, 8, 10, 12 and 14 feet apart.

The establishment of the Testing Flume at Holyoke is the las
great step taken in the development of the system of testing tu
bines, which has now been described at such great length. Its suc
cess, and the excellence of its work, will undoubtedly lead to th
construction of other flumes of a similar kind, public and privat
in other parts of the country, and the plan of working up th
efficiency of new sizes and styles of wheels, by careful test, and th
method of sale now becoming usual—basing the contract upon th
guaranteed performance of the wheel, and determining th
efficiency of the wheel by trial at the flume—will unquestionabl
become the customary method of dealing in such motors, through
out the United States, and probably throughout the world. A
advance of this character once effected, the improvement is neve
lost, but the new ways become a part of the business methods of th
country. The result of the introduction of this system of test ha
already been seen in the improved performance of turbines as sen
into the market by the best makers; and, although the opportun
ity for further improvement of the best wheels is evidently no
great, the custom of making and selling, guided by tests, will lea
to the regular production of turbines of maximum, and unifor
efficiency and power. The gain in the latter direction is alread
very great, as will be seen by comparing the powers and sizes of th
last wheels of which the records are given, with that of the Boy
den-Fourneyron wheel presented on an earlier page. The diffe
ence in cost in favor of the modern wheel is fully commensurat
with the gain in economy and in power.

The Holyoke Flume will probably long be considered th
standard flume of the United States; but it cannot be long befor
every water-power company will have a flume of its own, and ever
large manufacturer of turbines will erect a private testing flume
and the business of building and introducing such motors wil
thus become settled, at a very early date, upon a substantial an
satisfactory basis.

DISCUSSION.

Mr. Samuel Webber.—I have been very much pleased with the clear and full history of systematic turbine testing, in the United States, given by Professor Thurston, and agree with him fully, that all turbines should be tested, and *carefully* tested, before being put on the market; and I know that this can be correctly done at the Holyoke testing flume, as now arranged by Mr. Herschel.

There have been some remarkable tests reported from the Old Flume, which it has been impossible to repeat or duplicate, at a later date with the same wheels, and the 90 per cent. test of the Risdon wheel, referred to by Professor Thurston, is one of them. I have no doubt of the correctness of the Centennial test, which gave .8768 per cent. net effect from this wheel, for other wheels tested by other engineers, in various places, corroborate it very closely, but I have never myself got so high a result from any other wheel; and it should also be noted that the very high efficiency reported from some wheels has been usually found tested of very small wheels, of fifteen or twenty inch diameter, where a very considerable effect might be exercised upon the wheel by the man who handled the lever at the brake.

There was probably more or less error in the tests at the Centennial Exposition, from the thinness of the iron of the testing curb and penstock, which caused them to spring more or less with the load of the water, and made some of the wheels bind in their cases, or on the steps, but the error cannot have been very great, and in the test of the Risdon wheel, which ran very freely, were probably not to be noticed.

There is now and then a wheel turned out which gives remarkable and unaccountable results, as in the case of the Hercules wheel, reported by Professor Thurston, of which I had previously heard from Mr. Herschel, and of the correctness of which I have no doubt, but my own results with this type of wheel will vary between eighty-one and eighty-four per cent. net effect.

I have since that time tested another turbine of the same make as one of those tested at the Centennial, but of much larger size and greater power, and the results confirm each other so closely that I am tempted to quote and compare them here in corroboration of the statement that these records may be made substantial and satisfactory. I refer to the "Geyelin" Turbine, a wheel of the "Jonval," or downward discharge type, built by Messrs. R. D. Wood & Co., of Philadelphia, and the other wheel was eighty-four inches in diameter, and was tested in the pit of the John P. King mill, at Augusta, Ga., and the comparison of the two tests is given below:

Date.	Diameter Wheel.	Diam. Brake Pulley.	Leverage of Brake.	Weight on Brake.	Rev. Wheel per Min.	Length of Weir.	Height Water on Weir.	Cubic ft. Water per Min.	H-Power of Water.	H-Power of Wheel.	Per cent. of net Effect.
Nov. 3, 1876	36"	37."44	132 to 1	80 lbs.	213.5	9 ft.	.896 ft.	1480.4	81.89	68.32	.8343
May 3, 1884	84"	84"	185.88 to 1	1092.5 lbs.	76.19	19 ft.	1.9853 ft.	10018.5	559.95	468.85	.8367

The head of water acting on the wheels happened to be identical in both cases, *viz.*: 29.30 ft., but the result attained under circumstances so widely different in all other respects is remarkable, and shows how accurately turbines can be constructed. These tests were both made at "full gate," and no partial gate tests were made or desired of the wheel at Augusta, which had been guaranteed to 475 HP. under 32 ft. head, the result showing that it would have given 534 HP.

In such a case as this it will probably be always necessary to test the wheel "*in situ*," but, as a rule, for all wheels of ordinary or moderate dimensions, such a testing flume as that of the Holyoke Water-power Co., where wheels can be tried by disinterested and unprejudiced engineers at very much less cost than it can be possibly done on the spot where they are to be used, must be considered as of the greatest advantage, and as a boon not only to the engineering profession, but to the public at large.

The President.—As the paper is historical I do not know but that it would interest you for me to give you an instance of the testing of a water-wheel which may possibly be useful because of the cheapness and simplicity of the means employed.

Some years ago there was a couple of young men in Rhode Island named Brown, who, by the way, were themselves cradled by water-power, their father having attached a connection from the shafting of his mill to their cradle; and, as a consequence, they were geniuses. One day they came to the shop where I was at work and stated that they had invented a water-wheel which was very much superior to Stillman's wheel, which was the favorite wheel of the neighborhood, claiming that they had got thirteen and a half per cent. better results. On being asked by Mr. Cottrell how they got at that result, they said that they had made a very careful test. They had made two models, one of their wheel and one of Stillman's wheel, both of exactly the same size and the same openings. They put them into the same penstock, so that they both had exactly the same head. Then, to make sure that they used the same amount of water, they measured the water that came through in a pail and weighed it. While running under these similar conditions they took the spindle of one wheel between the thumb and finger of one hand and the spindle of the other wheel between the thumb and finger of the other hand, and *as near as they could judge*, there was thirteen and a half per cent. difference!

Mr. Chas. E. Emery.—The paper refers to the turbine tests conducted at the Centennial International Exhibition, 1876. There are some reminiscences of those trials which will be interesting to a body of engineers, but which it was thought better to omit in the general report of the Judges of Group XX., written by the writer, as that work was designed for more general circulation. The elevated tank and pool below it in the Hydraulic Annex to Machinery Hall were designed under the direction of the late John S. Albert, Chief Engineer, U. S. N., and at the time Chief of the Bureau of Machinery of the Exhibition,—one object being to form a water-fall as a feature of general interest in the Exhibition, and another to enable the turbines to be tested by providing pumps of sufficient capacity from the pool to the tank. The writer was appointed by the Judges of Group XX Chairman of a Committee to superintend the tests of water motors, and under his direction the weir and approach were constructed. The approach was built within the pool at one side, being inclosed by brick walls running to the bottom and forming a box with the weir at one end in which the water could be raised to a higher level than that in the pool sufficient to give the necessary fall. It had been intended to make these tests typical ones, utilizing all the published information on the subject and employing as observers some young gentlemen who had previously had personal experience with Mr. Francis. It was generally understood at the Exhibition that all experiments were to be made by and under the direction of the Judges, and that the executive force in the different departments would furnish the necessary facilities, and this was carried out in such of the scientific groups of Judges as made experiments. The Bureau of Machinery, however, had different views and considered that that Bureau should make any experiments with the machinery; the Judges reporting to its Chief in that matter instead of to the Bureau of Awards from which they received their appointments and to which they made their reports. The fact, too, that one of the Judges of Group XX., if he had remained in the naval service, would have been very much the junior officer of the Chief of the Bureau of Machinery undoubtedly had its influence. The final result was that the Bureau of Machinery, ignoring the arrangements of the Judges, suddenly decided that there should be an expert employed to conduct the turbine experiments and, proper authority being obtained (though without the knowledge of the Judges), Mr. Samuel Webber was selected for the purpose to report to the Bureau of Machinery. Mr. Webber being already a Judge in another Group, and knowing nothing of the contention above referred to, the Judges of Group XX., having full confidence in Mr. Webber's professional attainments, simply evaded the difficulty by appointing him an associate member of that Group under authority contained in the Rules; and as the only object was to obtain reliable and creditable results, the writer undertook to co-operate with Mr. Webber in the conduct of the work. This, however, did not prove an easy task, as Mr. Webber had made a great many tests, had acquired certain methods of his own, and did not care to go into many of the refinements which such an opportunity would have made of great scientific value. While he was pleased with the action of the Judges, he evidently felt, moreover, that his authority was from another source, and insisted practically on having his own way, and under the circumstances little else could be done. The original design was such that the water was necessarily admitted to the weir approach at the side, so that, although ample provision had been made in the length of the approach, the current on the side opposite the inlet was very much the stronger. The speaker suggested a series of baffling screens or racks, such as are described in Mr. Francis's work, and finally a single one was hastily applied, but with the spaces between the bars so wide that there was still ample area for the water with greater velocity to pass along the side where it did before. Any further improvements were, however, considered unnecessary by Mr. Webber, and the experiments were conducted with the water approaching the weir at very different velocities on the two sides; and, moreover, on account of the recoil of the current from one side of the approach, floating bits of wood showed that all of the water did not approach the weir at a right angle, and the evidence of this deflected current was sufficiently marked to show a ridge in the crest of the fall at the weir itself. Mr. Francis in his work states that precaution is necessary to obtain uniform velocity of approach to the weir and prevent cross currents from impinging upon the openings in the hook gauge box. Of course, it is impossible to tell just how much difference the neglect of these precautions made in this case. It is satisfactory to know that the general results correspond in general with other tests made of the same turbines at other places, though Professor Thurston calls attention to some apparent discrepancies. The writer was necessarily obliged to retire, and although the younger Messrs. Webber were kind enough to obtain templates of the blades and much of the information which he had intended to analyze and discuss, there was no provision for the employment of proper assistants to do that work in the way that those who are accustomed to make experiments know would be necessary, and there being at the time some doubt as to the value of the experiments thus conducted, nothing further was done.* Mr. Webber reported to the Bureau of Machinery, and his report also appears in the general report of the Judges of Group XX.

This matter is mentioned only for the purpose of showing engi-

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* The writer was also Chairman of the Committee on boiler tests, and after procuring the necessary authority from the Executive Committee of the Exhibition through the Bureau of Awards, the tests were actually conducted under the direction of the Committee of the Judges and in the immediate charge of an assistant from the writer's office selected by himself. The assistant necessarily remained in Philadelphia where he could receive his pay under the eye of the Centennial authorities while computing results, and an arrangement was made with him whereby he consented to report the results of the experiments as an expert to the Bureau of Machinery, entirely ignoring the Judges, and his services, with that of others, were utilized in other work in closing up the affairs of the Exhibition. The boiler report was at first actually printed as made by the writer's assistant, but reported to the Bureau of Machinery, in spite of the protests of the writer and the Judges of Group XX., who refused to do more work until it was corrected. The general body of Judges took the matter in hand, and a protest signed by a large number of them, headed by Professor Henry, failed to accomplish the object. Finally, the writer was enabled to have the original boiler report withdrawn and one by the Committee of Judges substituted in its place, a single letter, from a political friend to the Director General, having apparently more effect than the respectful protest of the representative scientific men called to act as Judges during the Exhibition.

neers the difficulties which arise frequently in attempting work of investigation, and will serve also to call attention to the very complete directions given in Francis's *Hydraulic Experiments* in regard to experiments of this character.

The writer has interested himself in deducing the probable reason why the results were as good as they proved. He had arranged the hook gauge box outside the approach opposite the back of the weir, building practically a short brick chimney on the bottom of the pool, provided with a perforated pipe leading across the bottom of the approach close behind the weir. Mr. Webber immediately declared this position wrong and put a wooden box down inside the approach, further back from the weir. In fact there was no objection to either position, except that his box reduced the area of the approach, as Mr. Francis had made an elaborate series of experiments, showing that the statical pressures close behind the face of a weir and for several feet back of that point are identical, although of course the actual depth of water varies greatly on account of the slope of the fall. It has occurred to the writer that the whole mass of water in motion at or near a fall is necessarily in a state of equilibrium, the water in motion taking positions which will balance the water comparatively at rest in the bottom, and that therefore even the ridge on the crest of the weir, showing a meeting of conflicting currents at that point, was all averaged in with the varying heights at other points, and the average correctly shown by the height in the hook gauge box. Notwithstanding this theory, it is not recommended that other experimenters neglect the evidently wise precaution given in Mr. Francis's elaborate work on the general subject.

CCXLIV.

A METHOD OF EVAPORATING BY EXHAUST STEAM.

BY ALBERT STEARNS, BROOKLYN, N. Y.

(Member of the Society.)

HAVING occasion to evaporate considerable quantities of alkaline liquors, the writer utilized the exhaust steam from an engine to aid in this work, by means of the Evaporating Pan, herein illustrated (Figs. 152 and 153), which has been in successful operation for several years.

The pan is 17′ 6″ × 11′ 0″ × 1′ 6″ deep, supported by timbers resting upon four jack screws, by which it may be lowered for cleaning or repairs. It has thirteen partitions running crosswise of the pan, each 8′ 9″ long, placed equidistant between the ends of the pan, and so arranged that liquors entering at the upper end must travel in a serpentine course fourteen times across the pan to reach the outlet at the lower end, it being set on a small incline, to aid this flow.

Hung between these partitions, from overhead supports, are oval cast-iron flanged pipes, of 9″ × 5″ inside diameter connected by return bends passing around the open ends of the partitions; this pipe has a 5″ inlet on top at the upper end, and a 4″ outlet on top of the lower end for escape of surplus steam, which is led to a coil of pipes in a warming tank (not shown) from which the warm liquors are continuously supplied to the pan. Directly under the 4″ outlet is a 1¼″ pipe for the removal of condensed water; this pipe is turned smooth on the outside and passes down through a stuffing box on the under side of the pan, thus permitting it to be lowered without disturbing the pipes.

The hangers support the cast pipe near the bends, alternately at the front and back. Fig. 152 shows all the front hangers but only one of the back hangers at the letter A. Bolted to the pipe-hangers are brackets carrying the shafts of agitators which reach nearly across the pan; these revolve within a circle of 10″ and are formed of four

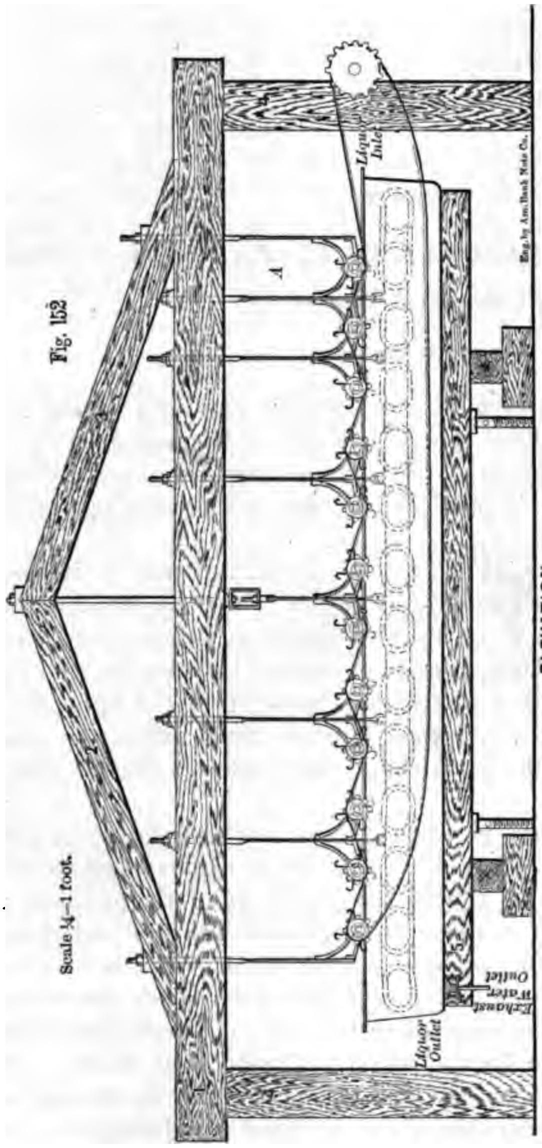

Fig. 152

Scale ¼—1 foot.

blades,
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ceding bl
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metal fro
evaporati
take place

The
being fed
pan at t
end, the
aided by
tators, compels a part of the water to pass off in vapor,
a slow but constant flow toward the lower end where the
solution is drawn off through a pipe in the bottom of th
fast as it reaches the desired strength.

To give an idea of the duty such a pan can do, it may be
it has evaporated one hundred and twenty gallons per h

clear water, condensing only about one-half of the steam supplied by a plain slide valve engine of 14" × 32" cylinder, making sixty-five revolutions, cutting off about two-thirds off stroke, with steam at 75 lbs. boiler pressure, its exhaust steam being carried about eighty feet to the pan on the floor above.

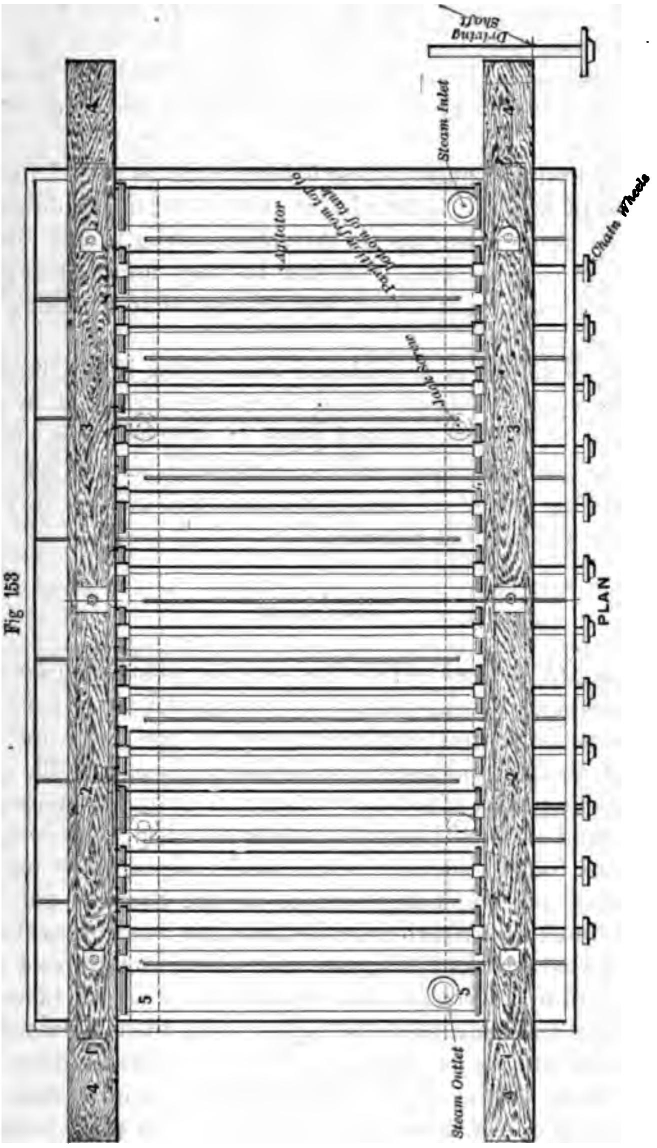


Fig 153

PLAN

It is found that keeping the pan-room warm and letting only sufficient air in to carry the vapor up out of a ventilator adds to its efficiency, as the average temperature of the water in pan wa only about 165 F.

Before building this pan, experiments were made with coils o pipe in a small pan, first, with *no agitator*, then with one having *straight* blades, and lastly, with the *troughed* blades; the evaporative results being about in the proportions of one, two, and three respectively.

In evaporating liquors whose boiling point is 220° F., or much above that of water, it is found that exhaust steam can do but little more than bring them up to saturation strength, but on weak liquors, syrups, glues, etc., it should be very useful, being easily adjusted for continuous action, and costing almost nothing for attendance or repairs.

The pipes acting as a condenser cause little, if any, back pressure on the engine; indeed it has been suggested that by closing the outlets, making the pan and pipes of sufficient area, and using a pump or hot well, they could *economically* perform the double use of adding power to the engine as well as being a very handy evaporator which is not patented.

DISCUSSION.

Mr. Geo. H. Babcock.—This is a very ingenious plan for getting a large amount of evaporation out of a given sized pan, but, as Mr. Stearns is undoubtedly well aware, it gets no more from a pound of steam condensed than ordinary pans. By the condensation of one pound of exhaust steam you cannot evaporate more than a pound of water directly by any mechanism. Nor is the effect per square foot of surface in the steam pipes much increased The heating or condensing surface in Mr. Stearn's pan is not given, but making a rough estimate from the measurements stated it is somewhere about 250 square feet, which would give 4 pounds of evaporation per square foot of surface. Had he taken those same pipes and sprinkled his liquor upon them he would have found them still more efficient. This is what is done in the Yaryan triple effect. In an experiment made in Boston a short time ago with one of those pans, an evaporation of 10 pounds per square foot of surface per hour was obtained; 2½ times as great as in the case given here, simply from the fact that the water was applied in a better way. A similar plan was employed in a concentrator used some years ago in sugar houses, in which the liquor was caused to flow over a stack of steam pipes, similar to the coolers now common in breweries.

Mr. Stearns makes a remark which is worth referring to:—"It is found that keeping the pan-room warm and letting only sufficient air in to carry the vapor up out of a ventilator adds to its efficiency, as the temperature of the water in the pan was only about 165 F." Air at a given temperature will carry off a given amount of water without precipitation, increasing rapidly with the temperature. The less the air required to hold the vapor in suspension, the less the loss of temperature in the vapor to heat that air, and it is evident that there is a maximum point for any given quantity of vapor at any given temperature. To keep the supply of air at this point would be a decided advantage. For instance, if the temperature could be maintained at 165 degrees with the air passing out saturated with vapor, supposing it came in half saturated at 60 degrees, it would require the admission of only 40,000 cubic feet an hour to carry off the amount of vapor which was formed. But if enough air was admitted to lower that temperature to 100 degrees, it would require 400,000 cubic feet, or about ten times as much to carry off the same amount of moisture. Whence we can see the reason why he found it better to close his ventilators.

Mr. Alex. Miller.—My experience with vacuum pans is that about 6½ pounds of water are evaporated per square foot of heating surface per hour. In double or triple effect apparatus 4½ to 5 pounds are evaporated. And in the case of salt pans for producing common salt where the temperatures used are much higher than in the case of sugar work we get as much as 9 or 10 pounds per square foot.

CCXLV.

DIRECT ACTING STEAM VENEER-CUTTERS.

BY THOS. S. CRANE, NEWARK, N. J.

(Member of the Society.)

THE first veneer and lumber cutter illustrated herein is the invention of Dr. H. S. Smith, of Brooklyn.

Being interested in the cutting of veneers, it one day occurred to him, when watching the power of a steam hammer, and the ease with which its movements were controlled, that the knife for cutting veneers might be actuated by direct connection with a steam piston, and the use of the machinery and gearing usually employed, be avoided, while the power of the machine was increased.

He engaged a draftsman to work out his ideas, but after two years of planning, found that his design was still unfit for construction.

The writer was then called in, and found that many patterns had been constructed while the plan was still incomplete; the frame being designed with solid beams of cast iron, eight inches thick, and from twelve to twenty inches wide, and the steam cylinder having been drawn of sixteen inches bore and two and one-half inches in thickness.

As it was imperative that a machine should be rapidly finished, the writer undertook to use the cylinder, which was fourteen feet long, and designed for a machine to cut logs 24 inches square and 8 feet in length, but discarded all the other patterns; and in order to use the direct acting principle required by the inventor, furnished the cylinder with nine-inch balanced piston valves actuated by a four-inch steam piston.

The four-inch piston was reversed at each end of the stroke by the movements of a D-valve, actuated by connections with tappets which were shifted at each end of the stroke by dogs upon the reciprocating knife carrier; and the feed mechanism was also actuated by tappets and dogs.

The appearance of the machine is shown in Figs. 116, 117 and

118, the stay-log *b* being located on a level with the second story in the building in which the machine was erected, and the knife and its carrier reciprocating before the stay-log in ways f^1 at an angle of 23 degrees.

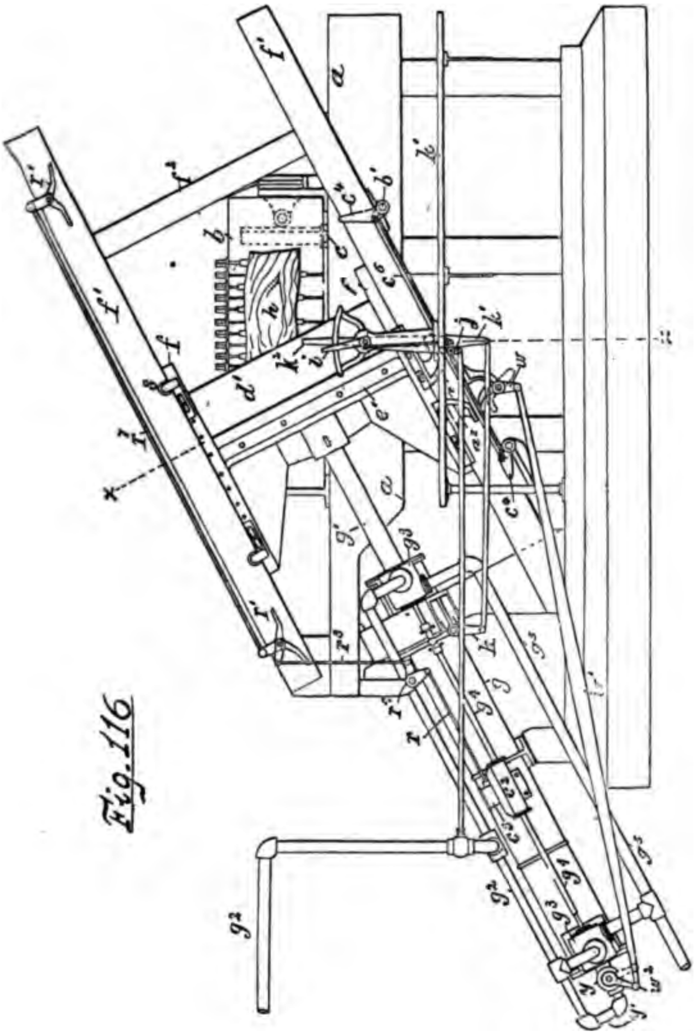


Fig. 116

g is the steam cylinder, and g^1 its piston rod, attached directly to he knife carrier *f*, which was formed as a frame of 6,000 lbs. weight, carrying a compression bar or roller close to the edge of the knife to form a throat like that in a hand plane.

g^3 are the main valve chests; g^4, the rod connecting their piston valves; c^2, the cylinder for the four-inch actuating piston; and c^3, the chest for the D-valve, which is actuated by connections r, r^3, to the tappets r^1, actuated by roller dogs s upon the knife carrier f.

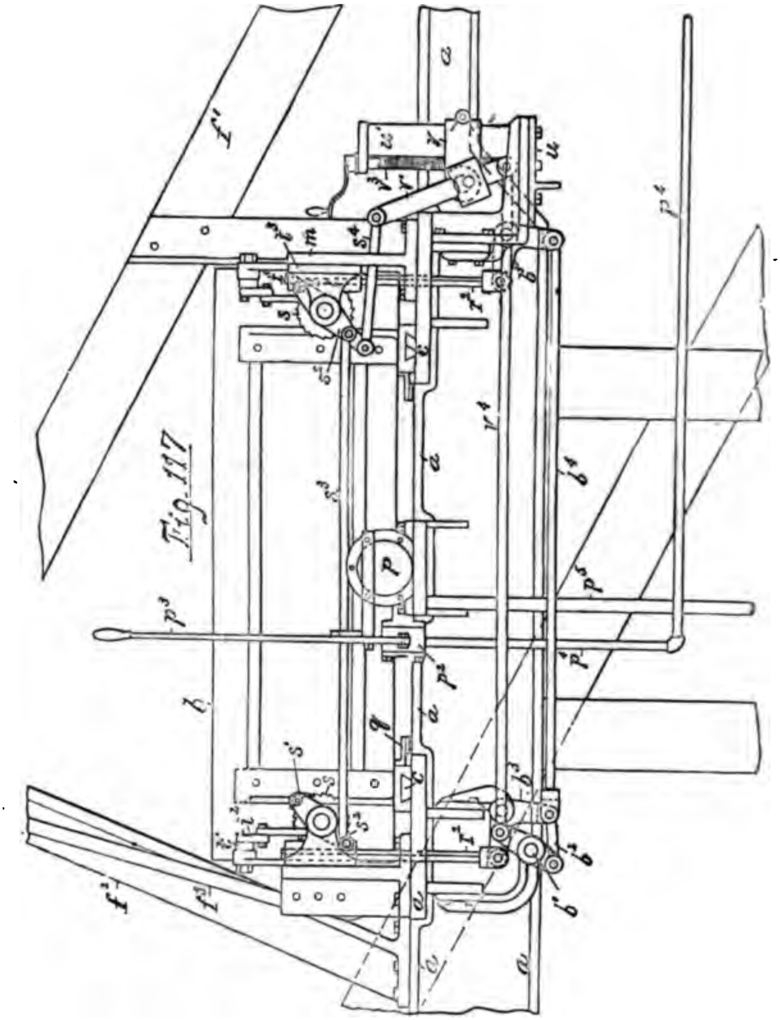


Fig. 117

c^4 are the tappets to actuate the feed mechanism, which consisted in screws n, shown in Fig. 118, which were fitted to separable nuts n^2, which, when opened, permitted the speedy movement of the stay-log by means of a piston rod p^1, and a steam or water cylinder p.

In Fig. 117 are shown the feed ratchet wheels s, actuated by pawls s^1, which were reciprocated, preceding each cut, by the upper

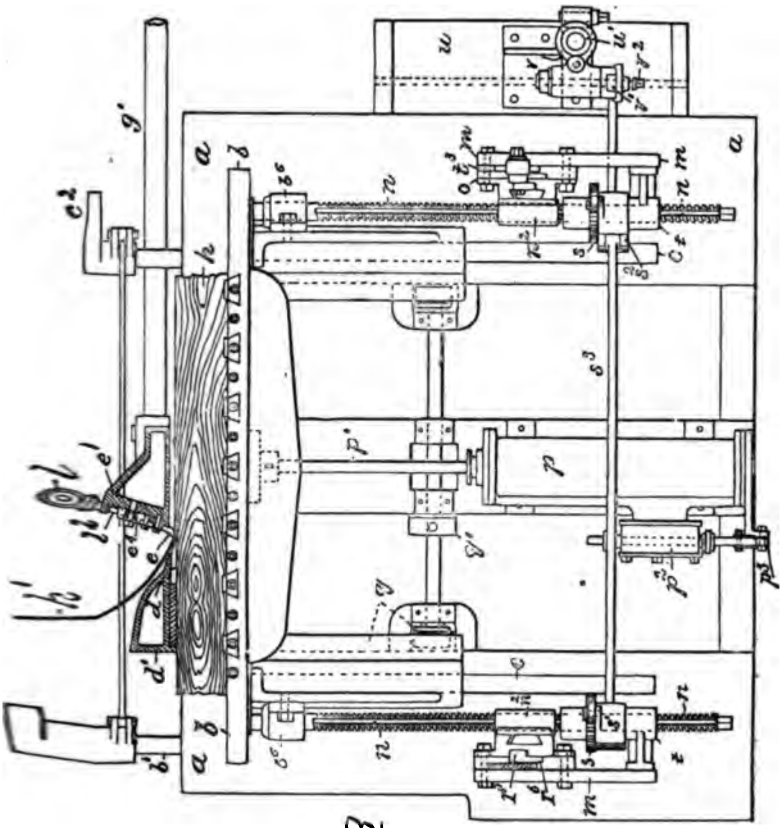


Fig. 118

tappet c^4. The tappet imparted a uniform oscillating motion to its shaft b^1, which, by connections shown in Fig. 117, oscillated a lever v^1, which was fitted within a sliding box which formed its fulcrum, and which could be adjusted and clamped at any point between its two extremities.

A screw v^8, operated to adjust the sliding box by its connection with a collar *v*, movable vertically by the screw upon a post u^1.

The lever v^1 thus converted the uniform motion of the tappet into the variable stroke required, for the pawls s^1, with different feeds.

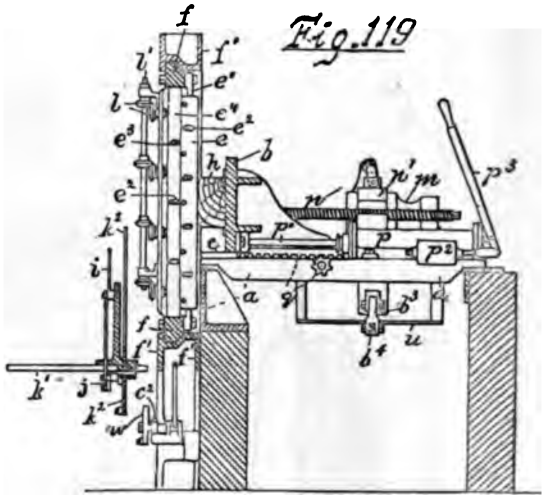


Fig. 119

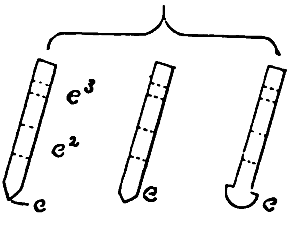
The stay-log *b*, carrying the log *h*, was drawn backward a quarter of an inch at the end of each stroke, to make the log clear the knife while ascending the ways f^1.

This was effected by the means shown in Figs. 117, 118 and 121, the nuts n^1 being fitted by feet *o*, and by a tongue fitted into a groove o^1, to slide longitudinally upon a standard *m*.

A so-called clearance wedge, consisting in a bar r^2, provided with an inclined rib r^5, was fitted in a vertical groove in the standard *m* behind the foot *o*, and the inclined rib was fitted to an inclined groove in the back of the foot.

The clearance wedges were actuated at the end of each stroke by a connection with the lower tappet c^4, by bell cranks b^3, and a rod b^4, shown in Fig. 116; and the nut boxes, when clamped upon the screws, were thus moved back and forth at the end and commencement of each stroke, holding the wood up to the knife during the feed, and retracting it during the return stroke.

Fig. 120



The screws *n* were grooved to slide freely through the ratchet wheels *s*, which were provided with feathers, and the wheels thus operated to rotate the screws, while the latter could be quickly retracted when the nuts were opened to use the cylinder *p* for quickly withdrawing the stay log to insert a new log.

Fig. 120 shows the different forms of pressers used with woods of different hardnesses, and Fig. 118 shows the adjustment of the presser adjacent to the edge of the knife, the presser being set at the required distance by screws l^2, connected by bevel gears with a shaft l.

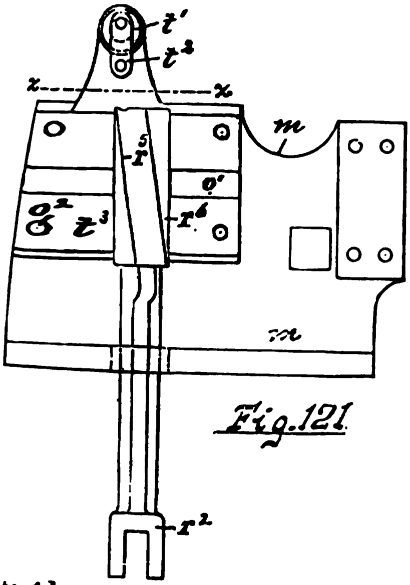


Fig. 121

Fig. 122 shows, in an exaggerated manner, the grinding of the edge of the knife at d^4; such grinding operating to throw the edge of the knife slightly above its plain surface d^2, to prevent the knife from running into the wood during the cut.

The presser *e* operated to compress the wood in advance of the knife, and rendered the cut veneer more sound and perfect than wtthout such compression.

Such compression, however, materially increases the resistance to the movement of the knife carrier, so that when the machine was put in operation, it was found that the elastic force of the steam in the cylinder *g* operated to jerk the entire carrier when it ran off of the wood; and more or less difficulty was found in securing the prompt introduction of the steam at the lower end of the cylinder properly to cushion the piston at that end of the stroke. A balanced governor valve *y* (Fig. 116), was, therefore, applied to the rear cylinder head (at the suggestion of Dr. Smith's son), and connected by pipe y^1 directly with the boiler steam pipe g^2; and such valve was actuated, through an arm w^2 and link w^1, by a special tappet *w*, which was operated by a dog upon the knife carrier just before the end of the stroke.

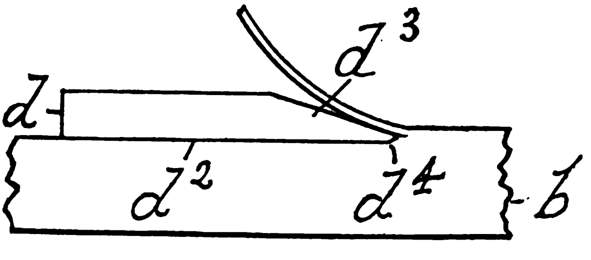


Fig. 122

With this construction the upward movement of the piston was not dependent upon the prompt shifting of the main supply valves in the chest e^3, which were liable, in starting up on cold days, to work stiffly for some time; but the live steam from the boiler was introduced positively into the cylinder before the end of the downward stroke, and thus operated effectually to cushion the piston, and to start it on its return stroke. A spring, not shown, was provided to close the governor valve *y* as soon as the piston had moved a few inches upon its upward stroke.

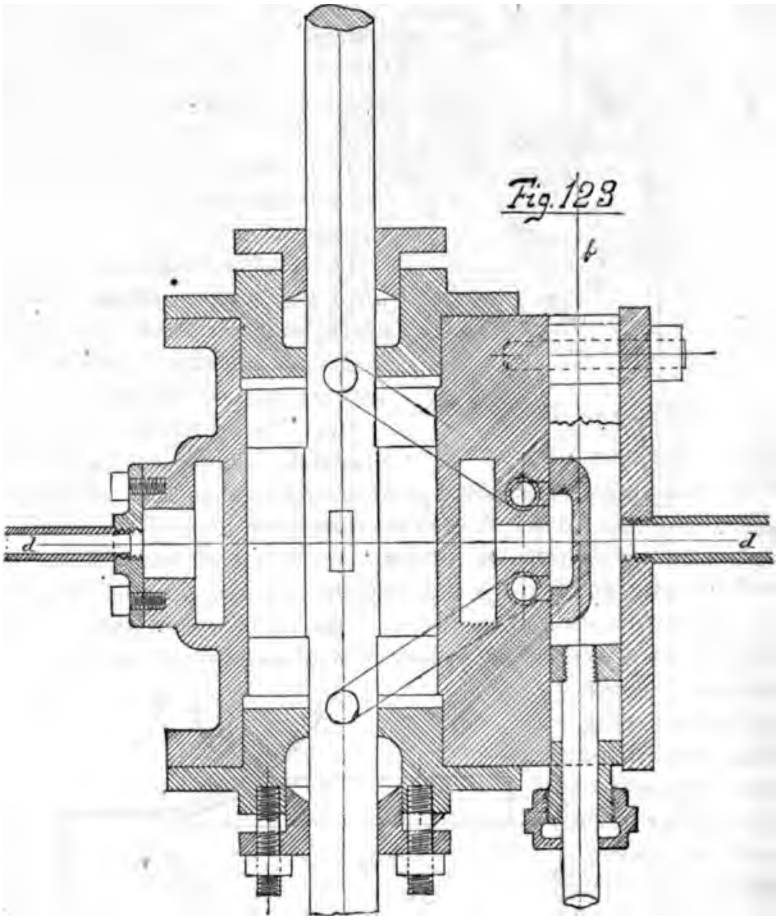
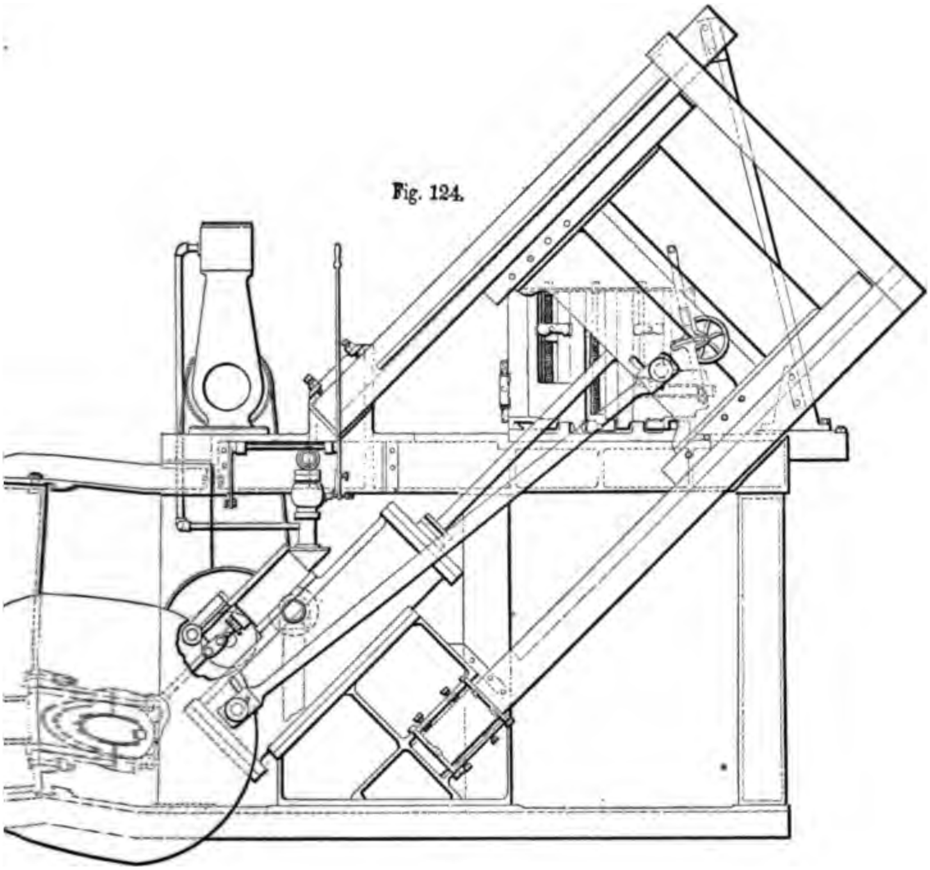


Fig. 123

The inclination of the valve rod g^4 caused the rod and its attached piston valves to shift much more readily in one direction than the other, and a considerable steam pressure was required in the shifting cylinder c^2 (which is shown in section in Fig. 123), to move the valves promptly upward for admitting steam to the lower end of the main cylinder *g*.

To cushion properly the four-inch piston, when moved so rapidly, the exhaust was conducted from each end of the cylinder to the under side of the D-valve exclusively through small steam pipes in which wheel valves were placed to throttle the exhaust as required.

Fig. 124.



With this construction a steam pressure of 70 pounds could be thrown upon the four-inch piston to shift it promptly without creating any jar in the valve rod which it actuated.

These pipes are indicated in Fig. 123 by dotted lines extending from each end of the cylinder to holes beneath the D-valve.

The complication of this machine suggested to the writer the importance of remodeling the design entirely, and Figs. 124 and

125 show a machine recently completed, from the writers designs at the Delamater Iron Works, New York City, and now shipped to Toronto, Canada, in which the feed and valve mechanism are actuated by a rotating shaft, and all the impact and jar of the tappets is thereby avoided.

To rotate such shaft in unison with the knife carrier, it was necessarily coupled thereto, involving a crank plate of the same stroke as the knife carrier; and as the inclined position of the ways which were placed at an angle of 45° produced a downward tendency of the knife carrier when the machine was at rest, it was necessary to provide an auxiliary engine to turn the lower center, in case of the stoppage of the carrier at such point.

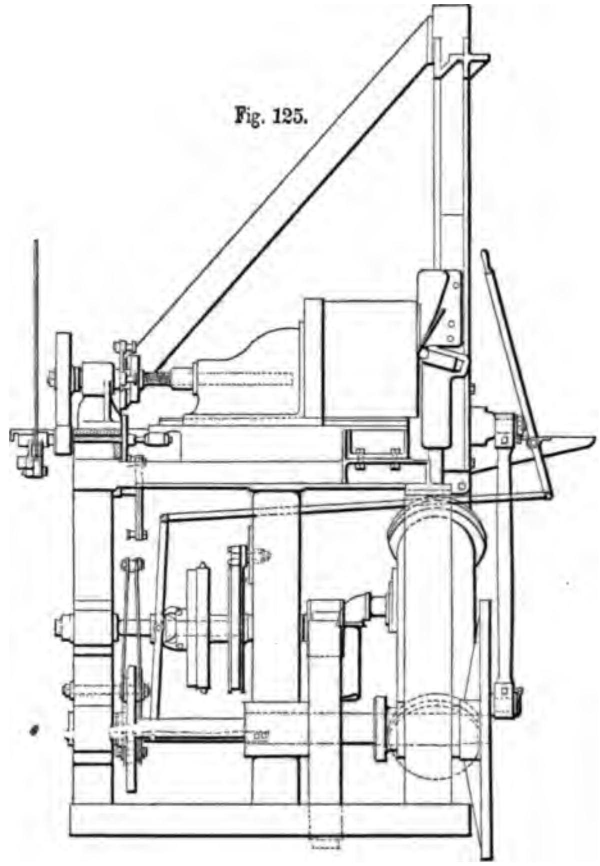


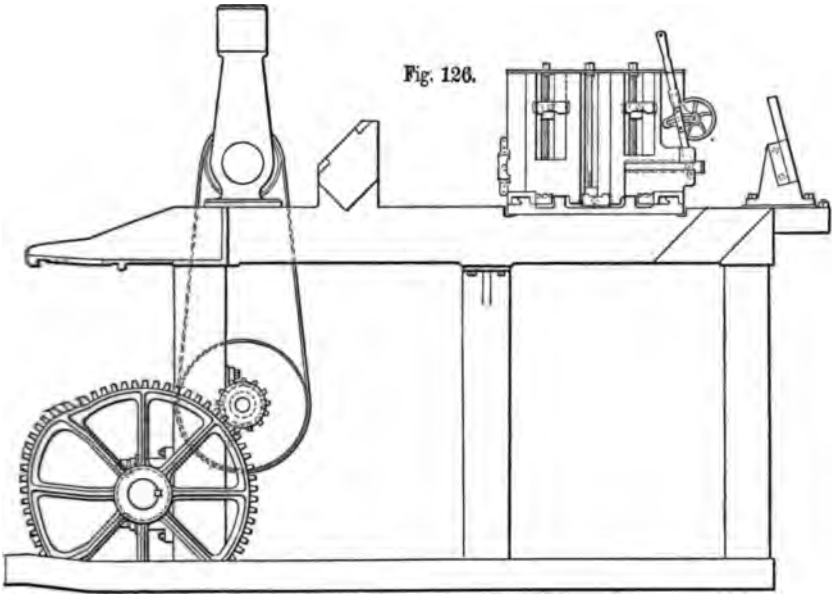
Fig. 125.

By the use of such auxiliary engine, the necessity of a separate steam cylinder for shifting the stay-log backward was avoided, and a connection was readily made from the auxiliary engine to the feed screws, through the medium of friction wheels, which were operated by a hand lever to rotate the screws in either direction, as required.

The auxiliary engine was 8 × 8 inches and stationed upon the upper bed plate at a level with the stay-log.

Its pulley, 36 inches × 12, was connected with a friction clutch pulley upon an intermediate shaft, which in turn was connected by a pinion and 56-inch mortise wheel, 3-inch pitch, and 6-inch face, with the crank shaft (the arrangement of this gearing is shown in Fig. 126). The main steam cylinder was 18 inches bore, and supplied with steam by a single D-valve, actuated by an eccentric on the crank shaft and formed with lap to cut off at two-thirds stroke.

Fig. 126.



A starting lever, mounted upon a platform, in front of the ways and in view of the stay-log, was connected, by separate rods, with the friction clutch, and with the lever of a weighted brake applied to the intermediate shaft, the movement of the starting lever thus operating automatically to throw the brake into operation whenever the friction clutch was disengaged from the shaft.

The auxiliary engine was constantly in motion, and a throttle lever connected with a valve in the main steam pipe, served to admit steam to the main cylinder, after the clutch had been coupled and the knife carrier set in motion by the auxiliary engine.

The opening of the steam valve admitted steam to the main cylinder in the degree required for cutting various thicknesses of wood, as from 1/16 of an inch to 1 inch of pine, and it was found in practice that the auxiliary engine alone, with 80 lbs. of steam, would operate the knife carrier to cut soft wood under 1/8 of an inch in thickness.

The 18-inch cylinder has proved, in practice, unnecessarily large for cutting thick wood, as black walnut could be cut into 3/4 inch boards with a very small opening of the throttle valve.

In fact, the pressure carried (90 lbs.) was too high for readily controlling the machine, and a diaphragm of sheet metal, having a central hole of only 1½ inches, was placed in the 4-inch steam-pipe and supplied all the steam required for the large cylinder.

Samples of wood are on exhibition at this meeting cut from white wood, black walnut, and oak, the oak being cut ½ inch and ¾ thick, and the black walnut and white wood being cut from 1/12 to ½-inch in thickness.

The latter machine was designed to cut boards 4 feet in length exclusively for box stuff, as the consumption of wood for such purposes in some districts is larger than for almost any other use; and the machine is fully adapted for cutting any kind of wood from ⅜ to ¾ of an inch in thickness, although it will of course operate with equal facility and uniformity upon thin veneers.

The device shown in Figs. 127 and 128 illustrates the means employed for stopping the feed automatically when the log was entirely cut, and consisted in a weight connected by a leather strap to the feed pawl, so that the dropping of the weight would lift the feed pawl from the teeth of the feed wheel. Only one feed wheel was used, and the feed screws were connected by sprocket-wheels and a chain manufactured by the Link Belt Company.

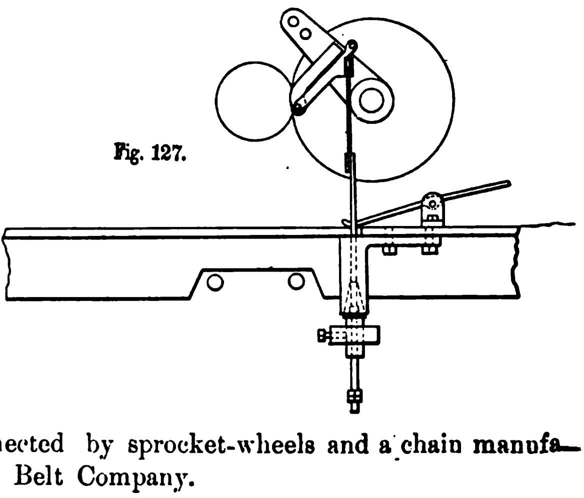


Fig. 127.

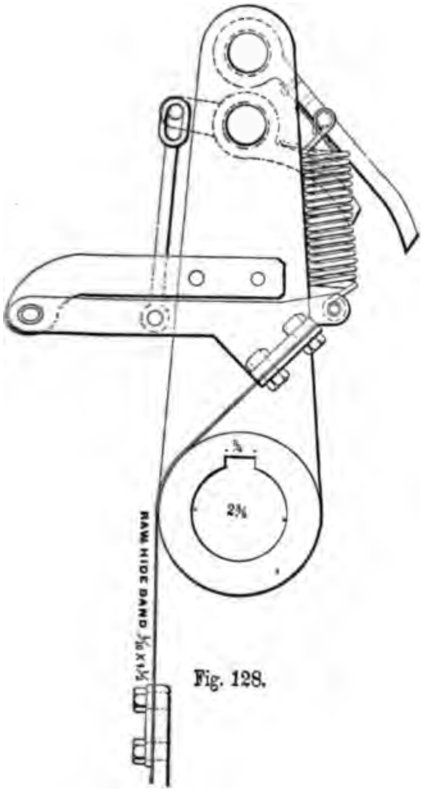


Fig. 128.

As shown in Fig. 127, the weight could be lifted by a treadle, to allow the pawl to operate, to start the feed, and was then sustained by a spring latch, which, when the stay-log had moved nearly up to the knife and the log was entirely cut, was released by a dog upon the stay-log, thus permitting the weight to drop, and lifting the pawl as required.

Fig. 128 shows the pawl crank provided with two pawls so as to take one-half tooth at a feed, and also showing a light spiral spring connected to the pawls to hold them into the gear teeth until the weight tripped by the stay log operated to lift them out.

The feed nuts in this latter machine were made solid and attached to the stay log, and the screws were journaled in eeves (shown in Fig. 129), which were threaded externally nd fitted in standards ehind the stay-log.

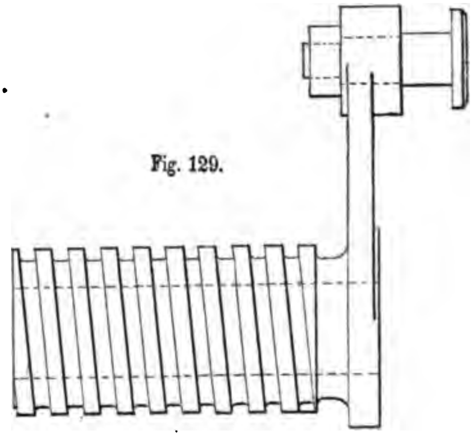


Fig. 129.

The reciprocating d, which actuated the ed mechanism, was upled by suitable nks to each of the ms shown upon one the threaded sleeves, Fig. 129, to rotate hem a quarter turn at ach end of the stroke. Such rotation served, fter the cut, to retract the screws and stay-log without turning the screws, so that the knife might clear the wood upon its upward stroke.

The reverse movement of the feeding rod, prior to each cut, served to actuate the feed, and simultaneously to rotate the threaded sleeve in which the screws were journaled, and to thus restore the sleeves to their initial position, in which they held the wood up to the knife during the cut.

This construction was much more simple than the separable nuts and sliding wedges used in the former machine, and proved equally effective.

DISCUSSION.

Mr. Allan Stirling.—I wish to express my great gratification at the presentation of a paper of this kind, and would call attention to a recent improvement which has been introduced in many large saw mills. In one mill in which I have been interested for many years, and where over 2,000 logs are sawn every day, the slabbing is done largely by a gang of circular saws worked with a steam feed. The work is done very rapidly, and the feed apparatus works beautifully.

Mr. John Walker.—In cutting the thick pieces is there any tendency for the lumber to split as a lath does?

Mr. E. P. Stratton.—Is the log prepared for the machine by being steamed or soaked before it is put in?

Mr. Thos. S. Crane.—The layers are always sound when the pressure of the rollers is properly graduated: there is danger that the wood will be splintered if this pressure is not strong enough.

When the layers are to be cut thicker than a quarter of an inch, the log is usually steamed. The three-quarter inch whitewood sample exhibited, and the quarter-inch oak sample were cut dry, but it is not usually thought desirable to do this for such thicknesses, as wood cut above $\frac{1}{4}$ inch is more liable to be checked or splintered by the process, and the steaming diminishes such tendency.

CCXLVI.

SHOULD A PISTON PACKING RING BE OF THE SAME THICKNESS AT EVERY POINT?

BY L. H. RUTHERFORD, NEW YORK CITY.

INTRODUCTION.

The following paper is an answer to the question whether the ordinary cast-iron packing ring which expands by its own elasticity against the bore of a cylinder can be made to press equally in every direction, so as not to cause unequal wear. The ring is turned a little too large (perhaps $\frac{1}{4}$ inch for each ten inches diameter), and, after being turned, is cut at one point, so that it can be compressed into its groove in the piston and be introduced into the cylinder, where it is to work. Should the inside of such a ring be concentric with the outside, or should the ring be thicker opposite the cut? If so, by how much?

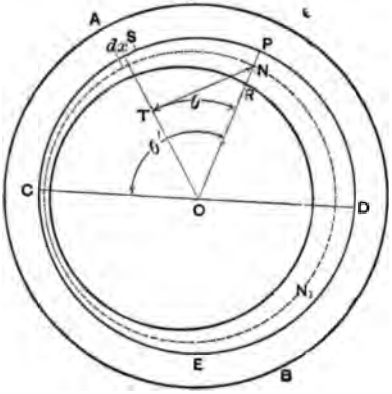
Mr. Rutherford's calculation was undertaken at my suggestion to answer this point, as part of the work of his concluding year as student in the School of Mines of Columbia College, and, incidentally, some other questions were brought in as being of interest. It is presented to the Society to elicit discussion as to whether practice would show that the theoretical discussion has taken account of all the factors which should be considered.

F. R. Hutton.

Let *A B*, Fig. 111, be a transverse sectional view of the cylinder, showing the cast-iron packing ring, *N N*, in an exaggerated state as to relative size. This ring is of constant breadth *b*, in the direction of the axis of the cylinder, of variable radial depth *d*, and is severed at the point *C*. The two halves, *C P D* and *C E D*, symmetrical in every respect, hold each other in equilibrium against external pressures by virtue of the internal strains in the ring.

Let the broken line, *N N*, be the elevation of the neutral surface of the ring, which separates fibers in compression from fiber in tension. Since the cross section is rectangular, this neutral surface will be midway between the outer and inner surfaces of the ring. Cut off any segment of the ring, as $C\,S\,P$, by a plane $P\,R$, perpendicular to the neutral surface, intersecting that surface in the line N, which is the neutral axis of the cross section made by the plane $P\,R$. Let us now consider the conditions of equilibrium of the segment $C\,S\,P$. Let dx, at S, be the length of an infinitesimal element of the outer surface, and call the arc $P\,S$, or the variable distance of the element from P, x. Draw $O\,N$ and $O\,S$, and draw $N\,T$ perpendicular to $O\,S$ at T. Let $O\,N = R_1$; $O\,P = R$ = radius of the cylinder, variable angle $P\ O\ S = \theta$ and angle $P\ O\ C = \theta'$. Let p = pressure per square inch produced by the packing ring on the surface of the cylinder. Now, the area of the element at $S = b\,dx$; the pressure on it $= p\,b\,dx$, and, since this pressure is directed toward the center O, its lever arm, with respect to the point $N = N\,T = R_1 \sin\,\theta$, and the moment of this pressure with respect to that point is $p\,b\,R^1\,dx \sin\,\theta$. But $x = R\,\theta$ $\therefore$ $dx = R\,d\theta$, and we have $p\ b\ R\ R_1 \sin\,\theta\,d\theta$, for the moment of this pressure.

Fig. 111



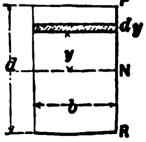
The total pressure on the cylinder from C to P is produced by the reaction of the strained fibers in the cross section $P\,R$, and calling the total moment of resistance of this cross section about its neutral axis N, M, we have

$$M = \int_0^{\theta'} R\,R_1\,p\ b\,d\theta \sin\ \theta.$$

Integrating, $M = R\ b\ R_1\ p\ (1 - \cos\ \theta') = R\ R_1 p\ b$ ver. sin θ' . . . . (1.)

Let us now find the value of M for the cross section $P\,R$. See Fig. 112.

Fig. 112



Let f = the unit strain in the extreme fibers of the section, that is, at a distance $\frac{d}{2}$ from neutral axis. Let dy = the length of an infinitesimal element of the section at a distance y from the neutral axis. Then its area $= b\,dy$ and the unit strain in this element, in accordance with the principle of elastic reaction $= \frac{f}{\frac{1}{2}d} \times y$ and the total force exerted by this element $= \frac{f\,b\,y\,dy}{\frac{1}{2}d}$. Now, the lever arm of this force, with respect to the neutral axis, is y and its moment $= \frac{f\,b}{\frac{1}{2}d}\,y^2\,dy$, and the moment of resistance of the entire section is equal to twice that of one-half the section

$$= \frac{2fb}{\frac{1}{2}d}\int_0^{\frac{d}{2}} y^2\,dy = M.$$

∴ by integration,

$$M = \frac{f\,b\,d^3}{6\,d} = \frac{f\,b\,d^2}{6} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (2.)$$

Equating the second members of (1) and (2) we have $\frac{f\,b\,d^2}{6} = R_1 p\,b$ versin θ'.

Now, $R_1 = O\,P - O\,N = R - \frac{1}{2}d$. But $\frac{1}{2}d$ in practical cases is quite small compared with R, and we may regard the variable $R - \frac{1}{2}d$ as constant and equal to R. Therefore,

$$\frac{f\,d^2}{6} = R^2\,p \text{ versin } \theta' \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (3.)$$

Having now deduced an equation involving the variable depth, we will next seek to impose upon it the condition that, when the ring is removed from the cylinder, it shall spring back into the arc of a circle, as this is required by its method of mechanical construction.

If we take a portion of a ring, as $A\,B\,C\,D$, Fig. 113, of uniform rectangular cross section, and bend it, so that its neutral surface, $N\,N$, assumes the arc of a circle, putting the outer side in tension and the inner in compression, it is obvious, if the unit strain is constant on each side of the neutral surface, in the extreme fibers, that, on releasing the ring, it will spring back into an-

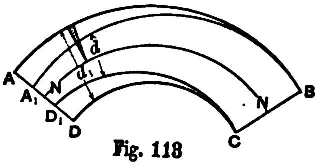


Fig. 113

er circular arc, due to the uniform contraction of the outer side and uniform extension of the inner side. Now, it is further obvious at, if the portion of a ring, $A_1\ B\ C\ D_1$, of constant breadth, but varile depth radially, be bent so that its neutral surface shall occupy e same position, $N\ N$, it will spring back into the same circular arc as far as the neutral surface is concerned) on being released from strain, *provided* that there exist in the fibers of the ring, at the time of strain, the same strains as would occur in them if they formed part of the uniform ring at the time when it was deflected in a circular arc. Now, considering $A_1\ B\ C\ D_1$ a part of $A\ B\ C\ D$ we know that the strain f, in an extreme fiber of $A_1\ B\ C\ D_1$, is to the strain f_1 in an extreme fiber of $A\ B\ C\ D$, as the distance of the first from the neutral surface is to the distance of the second from that surface. That is, $f : f_1 :: \frac{1}{2}d : \frac{1}{2}d_1$, or $\frac{f}{d} = \frac{f_1}{d_1}$ = constant because f_1 and d_1 are both constant.

Therefore, $f = \frac{f_1\,d}{d_1}$ (4) is the equation of condition for the expansion into a *cylindrical surface* of the neutral surface of a spring, which neutral surface is *already* cylindrical, due to a unit strain of f_1 in the extreme fibers at the point where the greatest depth d_1 occurs. Now, the condition which makes Eq. (4) applicable is not strictly realized in the case in hand, for it is the outer surface of the ring, and *not* the *neutral surface*, which is cylindrical; but, unless we find, on further discussion, that the depth of the ring is a considerable part of the radius of the cylinder, we may assume that the neutral surface *is* cylindrical, without material error.

Substituting in (3) the value of f taken from (4), we have

$$\frac{f\,d^2}{6} = \frac{f_1\,d^3}{6\,d_1} = R^2\,p \text{ versin } \theta,$$

or

$$d^3 = \frac{6\,R^2\,p\,d_1 \text{ versin } \theta'}{f_1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (A.)$$

To find the maximum depth of the ring, which occurs when $\theta' = 180°$ and versin $\theta' = 2$, we make $d = d_1$ and have

$$d_1^3 = \frac{6\,R^2\,p\,d_1 \times 2}{f_1},$$

or

$$d_1^2 = \frac{12\,R^2\,p}{f_1},$$

hence

$$d_1 = 2R\sqrt{\frac{3p}{f^1}} = D\sqrt{\frac{3p}{f_1}} \quad . \; . \; . \; . \; . \quad (B.)$$

From a consideration of (A), we see that the depth d varies as the $\sqrt[3]{\text{versin } \theta'}$, where θ' is measured from the small end of the ring, since versin θ' is the only variable in the second member. In equation (A) make $d^3 = d_1^3$ and versin $\theta = 2$, and solve with respect to the constant coefficient of the versin θ, and we have

$$\frac{6R^2 p d_1}{f_1} = \frac{d_1^3}{2}.$$

Substituting in (A), $d^3 = \frac{1}{2}d_1^3$ versin θ, or

$$d = d_1 \sqrt[3]{\tfrac{1}{2} \text{ versin } \theta'} = \frac{d_1}{1.26} \sqrt[3]{\text{versin } \theta'} \quad . \; . \; . \; . \; . \quad (C.)$$

From Eq. (C), we see that the depth of the ring at the small end is equal to zero, as we should expect.

In the foregoing deduction it was assumed that the neutral surface of the compressed ring, which we will call its *second* state, was cylindrical, and, on the basis of this assumption, there was introduced into the equation, involving the depth of the ring, the condition that that form of the neutral surface should have been derived from an initial cylindrical form. This is not exactly what is desired, because it is impracticable to make the neutral surface cylindrical, but it seemed to be the only simple method of securing, though approximately, the desired conditions, and the error of assuming that surface, cylindrical in its second state, is almost completely offset by failing to make it so in its first state, as may be seen from looking at the matter in the following light. If the neutral surface be cylindrical to begin with, it will have to be cylindrical afterward to produce the desired normal reaction, which will not be the case, as the outer surface of the ring will be constrained to conform to the rigid surface of the cylinder, whereby the thinner parts of the ring will not be bent enough to cause the neutral surface to deviate as much from its original form as it ought to.

While, if the neutral surface be not originally cylindrical, all that we need ask is that its second form shall be analogous to its first form, which is realized by making the outer surface cylindrical; for, then, if R_1 is the outer radius of the ring as constructed, and d the variable depth, the radius vector of the neutral surface will be $R_1 - \frac{1}{2}d$, and, after insertion in the cylinder, its radius vector will be $R - \frac{1}{2}d$, d being the same in both cases, and, subtracting the second from the first, we have $(R_1 - \frac{1}{2}d) - (R - \frac{1}{2}d) = R_1 - R$, which is a constant quantity; therefore, *every* point of the neutral surface has been made to approach the central point by the *same* distance $R_1 - R$ by putting the ring inside the cylinder.

Let us now find the relation between the radius of the cylinder and the radius of the outer surface of the ring, which is to fit into it, for a maximum safe unit strain of f_1.

Let $A\ B$, Fig. 114, be a portion of a rectangular ring, whose depth d_1 = the maximum depth of the given ring; and let it be compressed just as the given ring is, inside the cylinder, thereby subtending an angle α at the center, with an outer radius R = the radius of the cylinder. Let R_1 = the outer radius of the ring after the strain is removed and α_1 = the angle at the center. Let l = the expansion or contraction per unit of length due to f_1, which occurs in the extreme fibers on either side of the neutral axis, that is, at a distance R or $R - d_1$ from the center of the ring. Now, in compression, the length of the outer surface of $A\ B = R\ \alpha$ and the length of the inner surface $= (R \quad d_1)\ \alpha$.

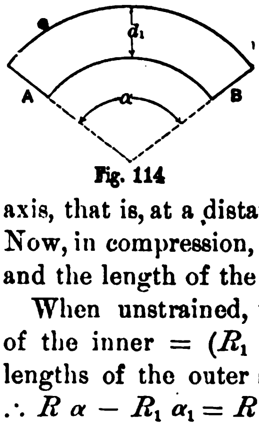


Fig. 114

When unstrained, the length of the outer surface $= R_1\ \alpha_1$ and of the inner $= (R_1 - d_1)\ \alpha_1$. Now, the difference of the two lengths of the outer surface is equal to its contraction $= (R\ \alpha)\ l$ $\therefore R\ \alpha - R_1\ \alpha_1 = R\ \alpha\ l$, or $R_1\ \alpha_1 = R\ \alpha\ (1 - l)$ (5), and the difference of the two lengths of the inner surface equals its expansion $= (R - d_1)\ \alpha\ l$,

$$\therefore\ (R_1 - d_1)\ \alpha_1 - (R - d_1)\ \alpha = (R - d_1)\ \alpha\ l,$$

or

$$(R_1 - d_1)\ \alpha_1 = (R - d_1)\ \alpha\ (1 + l), \quad . \ . \ . \ . \quad (6.)$$

Dividing (5) by (6) we have

$$\frac{R_1}{R_1 - d_1} = \frac{R\ (1 - l)}{(R - d_1)\ (1 + l)}$$

$$\therefore\ R_1\ R - R_1\ d_1 + l\ R_1\ R - l\ R_1\ d_1 = R_1 R - R\ d_1 - l\ R_1\ R + l\ R\ d_1$$

$$\therefore 2l R_1 R - R_1 d_1 - l R_1 d_1 = l R d_1 - R d_1$$

$$\therefore R_1 = \frac{R d_1 (1 - l)}{d_1 (1 + l) - 2 l R} \quad . \; . \; . \; . \; . \quad (7.)$$

But l is so small compared with 1 that it may be neglected in that connection, giving

$$R_1 = \frac{R d_1}{d_1 - 2 l R}$$

or

$$R_1 = \frac{R}{1 - \frac{2 l R}{d_1}} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (D.)$$

Now,

$$R_1 - R = \frac{R d_1}{d_1 - 2 l R} - R = \frac{R d_1 - R d_1 + 2 l R^2}{d_1 - 2 l R} = \frac{2 l R^2}{d_1 - 2 l R}$$

$$\therefore R_1 - R = \frac{R}{\frac{d_1}{2 l R} - 1} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (E.)$$

Equation (E) gives the amount by which the radius of the ring should exceed that of the cylinder, where $l = \frac{f}{E}$, E being the modulus of elasticity for the material of the ring.

In order to obtain more definite results it will now be necessary to assume specific values for p and f_1. Guided by previous trial, in the aim to secure simplicity, let us make $p = \frac{1}{16}$ lb. = pressure of the ring on the cylinder per square inch of area.

Let safe working strain of cast-iron per sq. inch of cross section = 3500 lbs. $= f_1$. This is, of course, for tension, that being the strain to which cast-iron more easily succumbs. E is about 14 200 000 lbs. and $l = \frac{f_1}{E} = \frac{3\,500}{14\,200\,000} =$ about $\frac{1}{4050}$.

From Eq. (B) $d_1 = 2 R \sqrt{\frac{3 \times 7}{15 \times 3500}} = \frac{1}{25} R = \frac{1}{50} D$, . (8.)

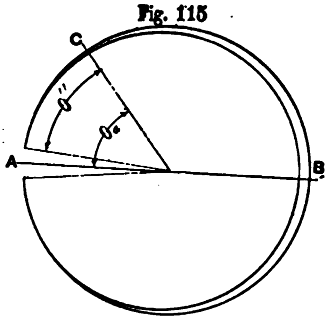
From Eq. $(C)\ d = \frac{2}{63} R \sqrt[3]{\text{versin } \theta'} = \frac{1}{63} D \sqrt[3]{\text{versin } \theta'}$.

" " $(D)\ R_1 = R \frac{1}{1 - \frac{2\,l\,R}{d_1}} \quad R \frac{1}{1 - \frac{50}{4050}} = R \frac{1}{1 - {}^{1}}$

$$\therefore \quad R_1 = \frac{81}{80} R = 1.013\,R, \quad \ldots\ldots\ldots \quad (9.)$$

$$R_1 - R = \frac{1}{80} R = .013\,R, \quad \ldots\ldots\ldots \quad (10.)$$

Fig. 115



The value of $\frac{1}{2} d_1 = \frac{1}{80} R$ woul seem to justify, at least for practic purposes, the assumption that $(R - \frac{1}{2} d_1) = R$. See page 441.

Let us now endeavor to deduce th dimensions of an eccentric ring, whos radial depth shall approximate to th theoretical depth d, as above.

Let $A\ C\ B$ (Fig. 115) be the theo retical ring, in a natural, unstraine state, with an external radius, therefore, equal to R_1. If the end of the ring just touch when the ring is in the cylinder, their distanc apart now is $= 2\,\pi\,R_1 - 2\,\pi\,R = 2\,\pi\,(R_1 - R)$, and one-hal the angle subtended at the center by this arc $= \frac{1}{2} \times 2\,\pi\,(R_1 - R) \div R_1 = \frac{\pi\,(R_1 - R)}{R_1} = \frac{\pi}{81} = .0123\,\pi$. Now, if θ' = the ang subtended by the arc, $A\ C$, to the radius R and θ'' = the ang subtended by the same arc to the radius R_1, we have $R\,\theta' = A = R_1\,\theta''$, or $\theta'' = \frac{R}{R_1}\theta' = \frac{80}{81}\theta'$. From figure $\theta_a = \frac{\pi}{81} + \theta'' = + \frac{80}{81}\theta' = \frac{1}{81}(\pi + 80\,\theta')$, or

$$\theta_a = .0123\,(\pi \times 80\,\theta'), \quad \ldots\ldots\ldots\ldots \quad (11$$

Let d_1 = the greatest and d_2 the least radial thickness of the corresponding eccentric ring. Let t = the distance between the centers of its inner and outer surfaces $= \frac{d - d_2}{2}$. Then, if d = the variable radial depth and θ_a its angular distance from the point of least depth d_2, we know from a trigonometrical deduction that

$$d = R - t \cos \theta_a - \sqrt{r^2 - \sin^2 \theta_a \times t^2},$$

where R = larger and r = smaller radius of the ring. But $R = r + t + d_2$, and $t^2 \sin^2 \theta_a$ is very small compared with r^2 $\therefore d = r + t + d_2 - t \cos \theta_a - r$, or

$$d = d_2 + t\,(1 - \cos \theta_a) = d_2 + t \text{ versin } \theta_a,$$

or

$$d = d_2 + \frac{d_1 - d_2}{2} \text{ versin } \theta_a, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (F.)$$

In Eq. (C), for convenience in subsequent comparison, make $d_1 = 1.26$, and from the resulting Eq. $d = \sqrt[3]{\text{versin } \theta'}$, let us calculate the values of d for $\theta' = 0°, 30°, 60°, 90°, 120°, 150°$, and $180°$.

In Eq. (F), also, make $d_1 = 1.26$. Let $d_2 = \frac{1}{2} d_1$. Then

$$t = \frac{d_1 - d_2}{2} = \frac{1}{4} d_1 = .315\,;$$

$$\therefore\ d = .63 + .315 \text{ versin } \theta_a = .315\,(3 - \cos \theta_a),$$

and from this equation let us calculate values of d for values of θ_a corresponding to $\theta' = 0°, 30°, 60°$, etc., these values being derived from Eq. (11). The following is a comparison of the results:

THEORETICAL.		ECCENTRIC.	
Angle θ'.	Depth.	Depth.	Angle θa.
0°	0	.630	2°13′
30°	.512	.672	31°52′
60°	.794	.788	61°30′
90°	1.000	.945	91°07′
120°	1.145	1.103	120°45′
150°	1.231	1.218	150°22′
180°	1.260	1.260	180°00′

In Eq. (F) let us make $d_1 = 1.40$ $d_2 = .56$, whence

$$t = \frac{1.40 - .56}{2} = \frac{.84}{2} = .42$$

and we have

$$d = .56 + .42 \text{ versin } \theta_\alpha.$$

Instituting a comparison between values of d, calculated this equation, and theoretical depths:

Theoretical....................	0	.512	.794	1.000	1.145	1.231
Eccentric......................	.560	.616	.770	.980	1.190	1.344

These two eccentric rings appear to be as close approxim to the theoretical ring as can be secured with a single in circle. Which would be the more desirable, practically, is a cult matter to decide by surmise alone. If, after turning oı first eccentric ring, the surplus metal in 30° of the ring on of the ends could be removed, so as to leave a gradual taper ring would seem to leave little to be desired.

The practical rule would then be to calculate by the prope mula the value of the maximum depth, that is, $\frac{1}{50}$ diameter o cylinder. Take *one-half* this for the least depth of the rin; *one-fourth* for the distance between the centers. The outer ı is $R_1 = \frac{81}{80}$ radius cylinder, and the inner radius is $r = R_1 - t -$ $R_1 - \frac{3}{4}$ maximum depth $= \frac{81}{80} R - \frac{3}{4} \frac{R}{25} = \frac{81}{80} R - \frac{\;}{100} R =$ 1 $= \frac{49}{50} R$; $R_1 = 1.01 R$; $r = .98 R$; $t = .10 R$.

The distance between the ends of the ring, if they are to in the cylinder, measured *along* the surface of the ring, wheth ends are cut off squarely or diagonally, $= 2 \pi R_1 - 2 \pi R$: $(R_1 - R) = \frac{2 \pi R}{80} = \frac{1}{40} \pi R = .08 R$.

Example : For a 36″ cylinder,

$$R = 18''; \; D = 36''.$$

Greatest depth of ring $= \frac{1}{50} D = 0.72''$.
Least " " " $= \frac{1}{2}.72 = 0.36''$.
Greater radius of the " $= 1.01 R = 18.23''$.
Lesser " " " " $= 0.98 R = 17.69''$.
Distance between centers $= \frac{1}{4}.72 = 0.18''$.
" " ends of the ring uncompressed $= 0.08 R = 1.44''$.

DISCUSSION.

Mr. Ezra Fawcett.—I do not know that I can add anything to the able paper of Mr. Rutherford, except to corroborate its conclusions to a certain extent by observation and practice.

I claim that a piston packing ring, which is divided at one point only, should be of a decreasing section, from a point opposite the cut, in order to obtain a uniform pressure and contact of ring by its own elasticity against the cylinder. In practice in springing in a piston packing ring of uniform section and say ¼'' larger than the bore, for a 10'' cylinder, we find that it bears much harder on the extreme points at the cut, and is not in contact with the cylinder back of the point, owing to its uniform thickness and stiffness at that point and its larger circumference, relative to the cylinder.

By decreasing the thickness of these points in an eccentric form, we come nearer to a uniform pressure of the ring; also in springing the rings over the piston, into grooves turned to receive them in what is termed a solid head, we find it much more easily done.

As to the proper proportion for thickness of ring, this depends somewhat on its width or longitudinal section. For a 10'' cylinder the dimensions should be ½'' to ⅝'' rectangular section at greatest depth and ¼'' at least depth of ring; the ring uncompressed being turned ¼'' larger, gives good results.

In my own practice, one particular high speed engine, attached direct to a large 60'' circular saw is making 400 to 600 revolutions per minute, and has been in general use some thirteen years. It is giving good service yet, and its rings are proportioned according to a rule which seems so nearly that of the solution contained in the paper, that it leaves little more to be desired.

Mr. C. T. Porter.—I believe that a piston packing ring acting by its own elasticity was first employed by Mr. Ramsbottom, the well known Superintendent of the London & North Western Railway, some twenty-five years ago. In 1862 I was exhibiting in London a few American engineering novelties. Mr. Webb, then Mr. Ramsbottom's assistant at Crewe, was a frequent caller upon me, and he described to me a number of Mr. Ramsbottom's inventions. One that interested me very much was this piston ring, which was then in the market for sale in England. The description of it will suggest the progress that has been made in engineering practice since that time. Mr. Ramsbottom's ring was a parallel ring, equal in depth all around. It was a wrought-iron ring, and the form of it was derived by a very peculiar process. A ring was bored and turned of the exact diameter of the cylinder. This was cut on one side and laid on a table, around which were placed a number of pulleys equidistant from each other. Over these pulleys cords were passed, and weights were attached to them, and this circular ring was expanded equally in all directions by the pull of these equal weights. The ring was thus drawn out into a curve of no mathematical character whatever. That curve was traced, and the rings were rolled to that curve. They were not circles, but when compressed to the original diameter they would be circles.

I used the Ramsbottom rings made in that way for some time in England, but with poor results. They wore very unequally. They would sometimes wear quite in two at points perhaps two to four inches from each end. So I ceased to use them, and employed cast-iron rings, sprung into grooves in the piston after Mr. Ramsbottom's method, and which I always bored eccentric. The eccentricity was rather moderate; perhaps the thin side where the ring was cut was half the depth of the opposite side. These rings always wore well in my practice. They were found to bear on every point of the surface. But the precise degree of eccentricity required to give uniform radia expansion, equal in all directions, and so theoretically correct, I have never known how to arrive at. The solution given in this paper is of great interest to me, and I presume will be to al members of the profession.

Mr. John E. Sweet.—To one who cannot form the least conception as to what it all means, Mr. Rutherford's paper seems to present a wonderful array of figures to demonstrate what it would seem ought to be arrived at about as accurately by a simple process of reasoning without any figures whatever.

If it is true that a bar having a straight lower edge, paralle in thickness, with its upper edge formed to a true parabolic curve will, if uniformly loaded, deflect to a true circular curve; then one would suppose if such a bar were bent into a true circle and crowded into a cylinder somewhat smaller than itself, it would press against the surface all around with a uniform pressure.

As an eccentric ring, if cut through its thinnest part and straightened out, would not make a bar of the above form, it cannot be theoretically correct, but as it more nearly approaches that form than a parallel bar, it is so much nearer correct.

Were we to make the packing rings to the true theoretical form, they would possess a feature that would render them worthless in practice. The ends would be so thin and have so little bearing in the groove in the piston, that they would soon wear loose, and by further use wear off the outer corner of the piston and the inner corner of themselves and wedge in, ending in a smash-up.

In this connection it may be pertinent to consider the two leading difficulties with the "sprung in" rings, neither of which come from any constructive difficulties. If made weak and just large enough, or with sufficient tension so as not to leak, a little gummy oil or worn off metal will stick them in the groove. When this happens their usefulness is entirely destroyed, or if given considerable tension to overcome the sticking, they wear unduly fast and are liable to cut. Probably the best which can be done, is to make the rings much stronger, and give the least pressure against the cylinder that will answer, by turning them only slightly larger than the cylinder. By so doing the tendency to wear is no greater, while the power to free themselves from striking in the groove is much greater.

I should adopt very different proportions from those given in the paper, making the ring in its thickest part not less than 1-20th the diameter of the piston (at least for small pistons), and not turn them more than 1-80th or 1-100th larger than the cylinder.

The width of the ring in proportion to its thickness has a great bearing on the subject, the narrower being the more likely to stick and the wider the more likely to wear loose; and also, the quality of cylinder oil has as much to do about the success or failure of the job as any other one thing, for poor oil will upset all theory, and the largest part of good practice.

In our practice we use the "sprung in" rings entirely, as we only build small engines, and for 10-inch cylinder the rings are ½ inch square at the thickest and $\frac{5}{16}$ or ⅜ x ½ inch at the thinnest and turned only about ⅛ inch larger than the cylinder, though just what they are after being sprung on the piston cannot always be told, for unless of strong iron they take permanent set. It is our practice to cut out about ⅛ of the thin part of the ring, which makes it a little nearer the true theoretical form. The thick part of the ring is always at the top (our engines being all horizontal) and the grooves between the ends of the ring are filled in solid, thus restoring the wearing surface to the piston for a small part of its circumference.

The whole arrangement is theoretically pretty good, practically probably as good as any, but bad oil and neglect will induce them to cut and wear out, and hence we are looking and striving for something better.

Mr. Chas. E. Emery.—I think it is due to the young gentlemen who has prepared this paper to say that in the opinion of the speaker it is a very creditable example of analysis practically applied. The results correspond well with the speaker's experience with what are called "Ramsbottom rings." Twenty years ago, in examining the engines in one of the largest flouring mills then in operation in this country, I found that they had been using brass piston rings and were only getting some 600 horse-power out of the engines. By changing the rings and putting in cast-iron ones of the type under discussion, with the points about one-half the thickness of the backs, the engines developed over 900 horse-power with less consumption of fuel.

The general results obtained by Mr. Rutherford are evident from the ordinary investigation of the transverse resistance of beams. With a beam supported at one end and loaded uniformly, the calculated depth for uniform width will result in a parabolic curve, and as each section is proportioned to the moment at that point the angle due to deflection will be the same at each point. It would seem that the results given in the paper would be obtained directly by a simple adaptation of the ordinary discussion to a polar equation. I have not had time to examine this matter, and it may be the formula would practically reduce to the form given.

Prof. J. Burkitt Webb.—As no mention is made of Prof. Robinson's paper at the Hartford meeting,* it is presumable that the author has not referred to it. It covers the same ground, attempting also to approximate to a correctly tapered ring by means of circles; but with regard to both papers there is, in my judgment, a grave criticism to make. In the case of a steam-tight fit between metal surfaces, we have to discuss thousandths, or, perhaps, ten-thousandths of an inch, as affecting the fit. Now, neither of these papers tell us how much the fit between ring and cylinder will be affected by the various approximations introduced into the

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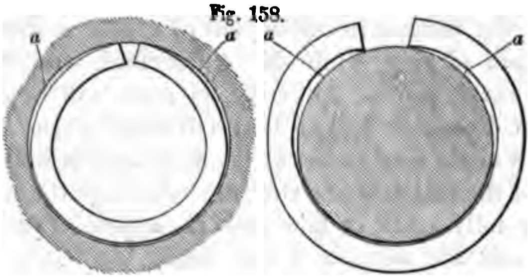
* Trans. A. S. M. E., Vol. II., p. 19.

mathematical analysis. It is safe to say that the practical value of any theory involving approximations is very greatly reduced, if not destroyed, by the neglect to calculate and sufficiently indicate the effect of such approximations, so that the theory may be applied to a practical case with some definite assurance against error. For example, in the paper under discussion, passing over the various approximations involved in the production of the formulæ, we have tables of "theoretical" and "eccentric" depths of rings, but there is no attempt to show how the considerable variations between these depths will affect the fit of the ring, causing, very likely, a leakage of steam. Prof. Robinson advocates an inside circle different from either of those of Mr. Rutherford; Prof. Robinson throws his circle out of center a distance = 0.2 greatest depth of ring, while Mr. Rutherford tries 0.3 and 0.4. Neither, however, proves that rings so made will be of practical value. Prof. Robinson claims that a drawing of the correct ring shows that a circle may be drawn so as to approximate closely to two-thirds of the inside of the ring, but how large the drawing was, and how carefully made, or how close the approximation appeared is not told, and the method of calculating the exact size of this circle is simply to pass it through three correct points—one opposite the slit, and two at 90° therefrom. Both of these proposed simple methods of making rings leave a large and important portion of the ring to be finished by a file or otherwise, and it is this portion which is most likely to fail to make contact with the cylinder. In our judgment the proper way to discuss a ring formed of eccentric circles is simply to call attention to the fact that for a ring whose greatest depth is one inch, the proper inside diameter from the slit across is nearly .6 inch longer than the inside diameter at right angles thereto, and that, therefore, a circle would not be suitable for the inside.

If it be advisable to form piston rings to the exact theoretical shape for a uniform pressure against the cylinder, there are ways enough to accomplish it; they can be milled out inside, or turned out with a tool fed in and out by a pattern bolted to the face plate, or it is possible that a good ring might be made by carefully casting the inside of the proper shape and turning only the outside.

Prof. Robinson refers also to rings which are made of uniform thickness, and says that their ends close to the split will "bear heavily upon the cylinder, with a probable space of no contact for some distance back." A short analysis, an example of which is appended, would have warranted a positive instead of a probable statement and it would have been even more satisfactory if the length and opening of this space had been discussed. We have at the Stevens Institute an eight-inch piston with rings of uniform depth (we should prefer to call it "thickness," and adhere to "depth" only to suit the paper under discussion). The open space is 70° to 90° on each side of the split, and the calculated maximum opening is about half a hundredth of an inch. In springing the ring open to get it off and when it is tightly clasped about the outside of the piston, there are, in the same way, similar open spaces of at least one thirty-second inch maximum opening:

Fig. 158.



These two effects of the uniform depth are shown in Fig. 158. The greater opening in the second case is accounted for by the fact, that in getting the ring off it must be sprung open by an amount four times greater than the amount required to close it in to the size of the cylinder, and that the arc of contact opposite the slit is much smaller than 180°. This ring leaks badly and the extent circumferentially of the open crack is evident by the absence of wear on the exterior of the ring. This ring was turned to a diameter only about a sixteenth larger than the cylinder, and shows that some attention should be paid to forming rings properly. Had it been turned say only a thousandth or two larger than the bore, and had its ends forced apart by a spring acting at the slit, it would most likely have been an excellent ring, fitting the cylinder quite as well as any taper ring could do, and having some advantages which the latter has not.

APPROXIMATE ANALYSIS OF FIT OF 8″ UNIFORMLY-THICK RING.

An examination showed that the ring fitted for 180° opposite the slit. It remains then to determine the width of the opening between the rest of the ring and the cylinder. Suppose the slit at A (Fig. 159); the ring bears against the cylinder for a short distance at the slit with a pressure, which we will call P, but which need not be known. At C the ring is to be supposed tangent to the cylinder. Let B be any differential element of the ring. Then the force P tends to bend this element, and by virtue of the bending of this and the other elements the ring is in the position shown, instead of in its natural (dotted) position. The moment of P tending to bend B is

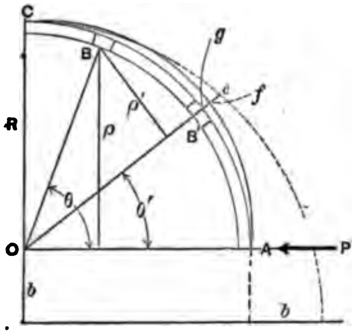


Fig. 159.

$$\text{moment} = P\rho = \text{(approx.)}\ PR \sin \theta.$$

The length of the element being $R\,d\theta$, the amount of the bending will be proportional thereto, and the distance which the end A goes toward O by virtue of this bending will be still further proportional to ρ, so that we may write differential distance due to bending of differential element

$$= m\,PR^3 \sin^2 \theta\, d\theta$$

(where m is an undetermined constant) and the total distance due to the bending of all the elements from A to B will be,

$$\text{Total distance} = m\,PR^3 \int_A^B \sin^2 \theta\, d\theta$$

$$= m\,PR^3\left(\frac{\theta}{2} - \frac{1}{4}\sin^2 \theta\right)_A^B,$$

which for $\theta = \frac{\pi}{2}$ becomes

$$= m\,PR^3 \frac{\pi}{4} = b, \qquad (1)$$

b being the approximate radial distance which the end A is sprung in under the action of P.

At any other point, as at B', we shall have a distance sprung inward due to all the elements between B' and C. The moment will be as before and also the length of the elements, but the lev arm to which the distance is proportional will now be

$$\rho' = R \sin(\theta - \theta'),$$

this gives a differential distance

$$= m\,PR^3 \sin\theta \sin(\theta - \theta')\,d\theta$$

(where m is the same undetermined constant)

$$= m\,PR^3 (\cos\theta' \sin^2\theta - \sin\theta \sin\theta' \cos\theta)\,d\theta$$

which makes the total distance,

$$\text{Distance} = m\,PR^3 \cos\theta' \int_{B'}^{B} \sin^2\theta\,d\theta - mPR^3 \sin\theta' \int_{B'}^{B} \sin\theta \cos\theta$$

$$= m\,PR^3 \cos\theta' \left(\frac{\theta}{2} - \tfrac{1}{4}\sin 2\theta\right)_{B'}^{B} - m\,PR^3 \sin\theta' \left(\frac{\sin^2\theta}{2}\right)_{B}^{B}$$

which for $\theta = \frac{\pi}{2}$ becomes

$$\overline{eg} = m\,PR^3 \left(\cos\theta' \frac{\pi}{4} - \frac{\sin\theta'}{2}\right) = \frac{4b}{\pi}\left(\cos\theta' \frac{\pi}{4} - \frac{\sin\theta'}{2}\right) \quad (2)$$

Now for the ring to fit the cylinder $\overline{eg}$ should correspond wi $\overline{ef}$, but we can calculate $\overline{ef}$ geometrically as follows (Fig. 160):

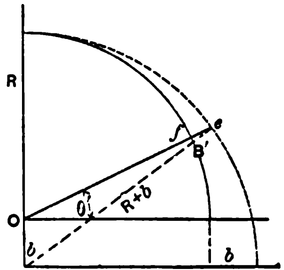


Fig. 160.

$$Of = R$$

$$O'e = R + b, \text{ which approx.}$$

$$= ef + R + b \sin\theta'$$

$$\therefore ef = b(1 - \sin\theta') \quad (3)$$

We have then by subtracting (3) from (

Width of crack at B'

$$= fg = eg - ef = b\left(\sin\theta' + \cos\theta' - 1 - \frac{2}{\pi}\sin\theta'\right)$$

$$= b\left(\frac{\pi - 2}{\pi}\sin\theta' + \cos\theta' - 1\right) \quad (4)$$

Putting this to a maximum we get

$$\sin \theta' = \frac{\pi - 2}{\pi} \cos \theta' \text{ from which}$$

$$\cos \theta' = .94 \text{ and } \sin \theta' = .34 \qquad (5)$$

$$\text{or } \theta' = \text{somewhere near } 20^\circ.$$

These values give in (4)

$$\text{max. width of crack} = b\,(.36 + .34 + .94 - 1) = \frac{b}{16}.$$

$$\text{By measurement } b = .08 \text{ inches } \therefore$$

$$\text{max. opening} = .005 \text{ inches.}$$

An examination of the approximation made in arriving at this result shows that it can be depended upon with .001 of an inch if the ring is truly circular and homogeneous to start with; and a rough circulation based on the measured one-thirty-second of an inch and the greater amount of spring required to get the ring over the piston alluded to in connection with Fig. 158 shows that this result is about right.

CCXLVII.

THE EDUCATION OF INTUITION IN MACHINE DESIGNING.

BY JOHN T. HAWKINS, TAUNTON, MASS.
(Member of the Society.)

IT may, perhaps, at first thought, seem apochryphal to say that a boy's intuitive mechanical perceptions or tendencies may be educated; but a closer look at such a proposition, will, I think, open up to our view an important channel through which the technical education of our boys may be modified to a considerable extent, and to their great advantage as practical men when they begin to assume the responsibilities of that part of their profession involving machine construction.

One of the greatest difficulties in ordinary machine construction (and particularly complicated machines containing a comparatively large number of parts, many of them of insignificant dimensions,) is the proper designing of the various parts, so as to fit them best for the service which they have to perform, without resorting, to any considerable extent, to the mathematics for their solution. In a large proportion of such cases, the matters of strength and wearing and other qualities are important questions of consideration; and others, such as form, outline, and general appearance presented to the eye; accessibility for manipulation in the machine, their congruity with other members of the same machine; the most acceptable manner in which a given piece may be made to depart from what would otherwise be its most desirable form, in order to escape contact with some other member; the facility with which it may be put in its place in assembling the machine, or another substituted for it with least disturbance of the other parts, in case it should require to be replaced; the questions of cost of its construction, and in connection therewith the modifications of it permissible as conducing to cheapening of the methods of its production; what tools are available to be used for its production; and a thousand and one such questions as these depend largely for their solution upon what may be called the intuitive capacity of the engineer engaged in machine construction; and, in a large proportion of such, mathematical investigation can only be resorted to, if at all, in a most unprofitable way. The engineer is met at every turn with questions of this kind, which cannot profitably be subjected to the ordeal of figures, but which are no less important in a vast variety of mechanical constructions than those problems which more decidedly depend, for their successful solution, upon analytical processes.

There are also many questions of proportion which involve strength and wearing and other qualities which are so complicated in the kind of stresses to which the parts may be subjected; and the directions of application of those stresses are so involved with other considerations, such as some of those pointed out above, that analytical methods must fail, if applied to them, and in which the intuition of the engineer must almost entirely constitute the solvent. I take it that the faculty in man which may perhaps be properly defined as mechanical intuition is as much dependent upon education for its full development as a natural tendency to be musical, or an innate talent for painting, or any of the fine arts. It is well understood that, no matter how great a prodigy a boy may show himself to be in either of the directions last mentioned, the cultivation of his particular tendency is indispensable to any marked success in the application of it; and it is doubtless true that a natural mechanic may be developed by cultivation to as great an extent as a natural musician, or artist, or orator; and that, without development, either of them must necessarily be deficient.

Of course, it is admitted that the particular kind of training which will best develop this faculty in a boy is to be had in the practical application of his knowledge and talents after leaving college; but I believe that a very considerable preparatory course in this direction may be followed in the schools, such that a young graduate of a technical school shall not—as is now very largely the case—upon entering upon the practical duties of a mechanical engineer in some manufacturing establishment,—find himself to so great an extent unable to apply and make useful to himself and to his employer that which he has worked so hard to attain while at school.

One of the greatest difficulties attending the début of a newly-fledged technical graduate in the practice of his profession as applied to machine designing is that he commences with a very decided tendency to carry out the exact methods which he has been pursuing at school; and the fact that employing manufacturers immediately find that these methods have to be so extensively reversed or subdued, if the young aspirant is to spend his time in a profitable manner, is a good indication that the education of what has been quite forcibly denominated the boy's "horse sense" by Mr. Dodge,* has been sacrificed during his college course, and that so much prominence has been given to what may be termed the metaphysics of engineering as to leave him very largely an impracticable. It is within my experience to find a young man who had graduated with high honors from one of our best colleges, upon first attempting to use the knowledge there acquired in machine design and construction, becoming completely lost in an effort to apply a comparatively high order of analysis to the designing of some part of a machine which could, in no profitable manner at least, be arrived at in any such way; and, not having had much previous training of that faculty which would enable him to design such parts properly and well, without resorting to a more or less elaborate application of mathematics, the result was anything but satisfactory.

In this way, we find that our young men are very much behind where they ought to be upon leaving school, in the particular direction of machine design and construction, and that they have, upon entering practical life, only begun another course of training as distinct from that which they have followed at college as can well be imagined. They are disappointed, armed and equipped as they feel themselves to be with what they suppose is all that is necessary to enable them to start out as designers and constructors of machines, to find that, in perhaps the very first task set for them, they are all at sea; and it is certain, in the writer's mind, that, if some system could be instituted in our technical schools looking to the greater education and development of mechanical intuition in a boy, schooling his eye to recognize good proportion and proper strength of material and the direction of application of the same, and stimulating and exercising his inventive faculty in such a way as to make him fertile in the resources which are so constantly demanded of the constructing engineer in machine designing, such an addition to the technical curriculum could be profitably made, even at the expense of some of the more analytical knowledge with which his mind is now more exclusively stored.

I am not of those who are inclined to belittle the importance of

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* Trans. A. S. M. E., Vol. VI., p. 548.

the higher mathematics in their proper place; but I do believe that the tendency in our schools is to impress the students with the idea, in too great a degree, that machine construction is dependent upon the application of mathematical analysis, and very much beyond what, in practice, there is any necessity for; and I believe that one of the most important things which could be instilled into their minds to-day would be the fact that, in designing any important machine (while in many ways the application of mathematical processes is indispensable to success) it is essential to know when and where *not* to attempt to apply these methods, and to be able to design, arrange, construct, and create in the proper places without them. Of course, it may be said, strictly, that there can be no piece of machine construction which cannot be subjected to, and its dimensions and other characteristics decided by, strictly analytical methods; but, in a very large proportion, such a course is economically out of the question, and they must be looked and thought out practically by the engineer's intuition—that is, his capacity to judge, unaided by abstruse computations, as to their best proportion and form, the material of which it is best to construct them, and many other considerations, such as indicated above, which are generally to be given their proper weight in the final designs.

One of the principal factors in this educational problem is economy. If it be accepted that, as above admitted, every conceivable problem of machine construction may be solved mathematically, in a vast majority of cases it would be the sheerest waste of time and money to attempt it; and, in this age of close competition and struggle for cheapening of every production, it is one of the very first considerations that a designer does not waste his time over analytical computations where his object may be attained in a more direct and simple manner; and perhaps there is no aspect of the question which applies itself more forcibly to the manufacturer or manager than that the tendency with our technical graduates upon entering upon the practice of their profession is to be too expensive, one of them rarely being able to compete in this respect with a young man who has had his mechanical aptitude or intuitions more thoroughly trained, though not in the possession of a tithe of the higher education with which the college student has been favored.

It may be contended that the manual-training branches of such schools are intended and should be sufficient for the development of this mechanical faculty, but I think not—at least, as they are now conducted; nor do I think it would be possible to combine the two in any desirable way. These are excellent for the training of the hand or the imparting to the student of "finger wisdom," as Mr. Partridge* has it; but very little in the way of deciding proportion, form, strength, and adaptation of parts of a machine can be gotten in the workshop. It is in the mechanical laboratory and the drafting-room where the mechanical intuitions of the designer can best be educated and developed.

I do not know that I am prepared to formulate any system of training which shall better tend to the education and development of this mechanical intuition, in the technical schools. There are, however, I imagine, many ways which will commend themselves to the instructors, in which something of this kind could be more extensively followed out: as, for instance, in all such schools there are more or less machines of various kinds in the laboratories, kept there for experimental or other purposes. From some machine of this kind, for example, there might be removed a single member; the machine turned over to the student, with the problem given to make a sketch or drawing of this member strictly by the intuitive promptings of his brain, and without any computation of its required dimensions or form. Then it might be compared with the removed member; or, if advisable, let him, after designing it, verify or disprove the correctness of his design by computations to any extent or depth necessary. Again, a member of a machine may have a specific form given to it principally for reasons entirely independent of its proportion and strength. In such a case, tender the problem of designing a substitute for it as dissimilar in form as possible, to be equally well fitted to perform its functions, and without resorting to mathematical computation; or perhaps copies or blue-prints of working drawings could be gotten from machine manufacturers, and, in the same way, the student be given a piece to design which he has not seen, such as will be fitted to go with the rest, which he may see and consult. Let him see, in this way, how near he can come to what has already proved to be good in practice; and, if necessary, thereafter criticise both his own and the manufacturer's work by the most crucial methods of which he is capable. Some part of such a machine might be pointed out to which was given a specific form for reasons of accessibility or facility of exchange, and the student might be required to contrive some other means of achieving the particular end to be attained. Almost any

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* Trans. A. S. M. E., Vol. VI., p. 535.

part of a given machine may be produced in a great many ways, each resulting in a different cost of the piece produced. In such a case let the student devise and indicate the proper methods to be pursued to produce it at the least cost, modifying his design to conform to this consideration. Very many such problems as these might be instituted by the instructors, which would constitute a very excellent training of the mechanical instinct in the student, and his inventive faculty be exercised and developed.

One difficulty which might be mentioned in the application of this intuitive or instinctive method so largely indispensable in machine designing is, that working drawings made upon some small scale are, to the eye and judgment of most men, very misleading. A given detail of a machine drawn, for instance, one-quarter size will appear very differently when laid out full size; and, in my opinion, there is no one feature which requires more thorough training where a designer is to rely upon his unaided judgment than this, and I believe that one of the best possible exercises for the student looking to the fitting of him for a machine designer would be to make drawings first to some fractional scale and to draw them out afterward roughly on the black-board or on large manilla sheets, full size; designing the part or parts, to all intents and purposes, first upon a small scale, then drawing them full size, to enable him to verify by his eye and judgment the correctness of the smaller drawing.

I am aware that to all this some will cry, "Rule of thumb!" But I believe, while I deprecate any such exclusive reliance upon it, as is too often permitted and practiced, this same rule of thumb may be made a most excellent one, if properly hedged in, and its good points only developed and applied in machine construction. I believe that, in our technical schools, the rule of thumb is too persistently sat upon, and that it is capable of being brought to a sufficiently high standard, by proper systematic development, to be of incalculable value to the mechanical engineer.

The writer advances these thoughts more with a view of eliciting discussion than of attempting to point out how the object sought could be best attained—in the profound belief, however, that much which may be known under the terms mechanical aptitude, intuition, instinct, or inventive faculty, is now neglected in our schools, while it is what should be educated, nourished, and developed to the greatest degree, in order to turn out successful mechanical engineers.

DISCUSSION.

Mr. C. A. Smith.—While I do not take exception to anything sai in Mr. Hawkins' paper, I do wish to say a word or two in defens of, rather than to criticise, the student and the technical schools

There seems to be a great deal of talk and writing, at th present day, about the "practical inefficiency" of the technica schools, as well as that of the student as he seeks an entranc into the arena of "practical mechanics." I will not deny that great deal which is said is true: I will not deny that many young mechanical engineer, so called, is frequently disappointe when he discovers that he cannot step directly from the rostru of Commencement Day into the position of superintendent o some prosperous manufacturing establishment, but must conten himself for a while by weighing castings, or making tracings, etc Nor will I deny that these young men have acquired their diplo mas honestly, and by hard labor—not eight or ten hours per day but oftener from fourteen to eighteen hours. But in all the discus sions relating to this subject, one immutable law of nature seems t be generally lost sight of, or to be left out of consideration, an that is the fact that man is limited in his abilities, and, conse quently, it takes *time* to accomplish a certain result. The abilitie of individuals to acquire knowledge vary very much, but to expec the most capable man, at the age of entering college, to acquire sufficient amount of knowledge in a four years' course of studie to make him a successful designer of machinery, or manager of factory, is simply thoughtless in the extreme, to say the least Let any man, who is considered a fair designer, stop a few mi utes and count up, "on his fingers," the number of years require for him to acquire those qualifications which he now consider essential in a good designer. Perhaps he can count them on hi fingers, and perhaps he cannot.

The fact is that four years is much too short a time to educat a young man to be a full-fledged mechanical engineer. Fou years may be enough for the collegiate course, but the knowledg gained there is not all which is required. Perhaps the instructio received at college is deficient in this respect, that it leaves th student under the impression that all he has to do, after h receives his diploma, is to walk out upon this broad land an take possession of all he can survey; to tell the Fultons, th Stephensons, the Watts, the Eads, the Ericssons, etc., to step ou of his way, as he is ready to show them "a thing or two." The young man should be given to understand that what he learns in college is not the end, but only the beginning of his studies in mechanical engineering.

The college can only equip the student with a good set of tools, as it were, with which he may start in his life work. After he graduates he is prepared (or at least he should be) to go into practical life and learn—not teach—the other requisites of a good designer. Some of these are: quite a thorough knowledge of the machinist's trade, foundry practice, pattern-making, blacksmithing, etc., the more of each he can get the more valuable he will be as a designer. The technical schools and colleges give instruction in most of these branches, but they cannot do more than give the pupil a start in this direction—simply point out the way in his future studies, and then bid him learn for himself. The college course is too short to do more. For a man to be a successful engineer and designer he must have a fair knowledge of the mathematics in all its branches, pure and applied, including mechanics in its various subdivisions; also the natural sciences to a certain extent; the more he can get out of each the better. These he should get in the college, as he can get them nowhere else so well. The remainder of the requisite knowledge he must get in the world of competition, and it is there where the "mechanical intuition" must be developed.

The sciences and the arts are the two great schools in which a man must be educated in order to make him efficient in the practice of mechanical engineering. The former he can get best in the college and the latter nowhere so well as in the workshop. If he masters the sciences and stops there he will be a failure as a designer, and if he acquires the art without the science he will be only another failure of a different kind. In either case he may be able to copy designs, but he will never originate any to a great extent. It is true that we do find fair designers once in a while who were educated from only one of these two sides, but we can also see under what disadvantages they work sometimes in consequence of not being trained from the other side as well.

In view of what has been said, I may be asked whether I would recommend to abandon the shop practice system in use in the technical schools at the present day. To this I would answer decidedly, No! Although there is abundant work for a student to fill up his time during a four year's course without any shop practice, the shop work is valuable as considered from three distinct stand-points. In the first place it gives the student the necessary physical exercise, to maintain his health, in a more profitable way than he can get it on the base-ball grounds, or in the racing shell. In the second place the shop practice system furnishes the best means of teaching the relation of the sciences to the arts; in other words, it furnishes the means of illustrating and teaching, in a practical way, how to apply the sciences in the daily practice of business. It helps the student to remember the lessons learned, as it multiplies the association of ideas.

I once knew a student who, after having studied plane geometry and recited his lessons well, was unable to apply his knowledge to determine how much to "set over" the foot stock of his lathe to turn a taper of three-quarters of an inch to one foot. If the similar triangles had been drawn on paper he would have understood them at once, but the moment the lines were hidden in the center of a bar of iron, or stretched through the air in an invisible form he could not see any geometry there to apply. He needed some instruction in *practical geometry* to teach him the use of abstrac geometry. Similar instances might be mentioned in relation to the other sciences.

Thirdly: Shop practice is valuable because it gives the studen an introduction into the work which he expects to pursue after he leaves college. It gives him a start in the right direction of hi future professional studies, but it does not and cannot do more than this. If the student gets the idea that he will receive all the training in college which is necessary to qualify him to take ful charge of the shop or of the designing of machinery, then there must be something wrong in the management of the institution which teaches him so deceitfully. The technical schools usually advertise "to fit young men for positions of usefulness in the department of mechanical engineering," or something to that effect This seems often to be interpreted that the schools can manufacture to order, full-fledged mechanical engineers. The student should be frequently reminded that he will have something to learn after he leaves college. We never hear of law schools graduating full fledged lawyers. I believe the young aspirant must always take an after course with some practical lawyer before he is admitted to the bar. Why should the mechanical student expect to fi himself for the duties of life in a shorter time, when the subjects of his studies are certainly much more far reaching? There is no class of men who have contributed so much to the present civilization as the mechanic. No, let us not denounce the technical schools because they cannot make mechanical engineers and designers in the short time of four years. Let us not look upon the graduate with contempt because he has not learned all there is to be known in four short years. The colleges are doing excellent work; let us give them credit for it. Let them continue to give the student that which they *can* give him—a scientific and technical education; after that he will get what else is necessary if he has any ambition and has been properly directed.

Mr. Samuel Webber.—While agreeing very fully with Mr. Hawkins, as to the value of so shaping the education of a young engineer, as to develop and extend his natural intuitive faculties for machine construction and design, I am compelled to admit that I do not believe that any system of study will do so, unless the natural and intuitive faculties are strong in him from the beginning.

The old Latin proverbs, "*Ex quovis ligno non fit Mercurius*," and "*Poeta nascitur, non fit*," are as applicable to the engineer, and I am quite in accord with the remarks of Mr. Dodge at the Atlantic City meeting, which Mr. Hawkins quotes.

The constructive faculty may be so aided by education, as to enable the designer of a machine so to construct his parts of it as to render each best fitted for the stress which is to be put upon it, and not, as I have seen, to place a T-shaped brace or girder wrong side up; but, like Mr. Dodge, I believe a good deal in the great value of "original judgment" in the matter.

The suggestion made by Mr. Hawkins I deem very valuable, and that is, that the young engineer should make enlarged copies of his designs, in a rough way, of full size. I know that it will aid materially in discovering and correcting errors, which might pass unnoticed, if merely drawn in the ordinary way of one-quarter size; and it is mainly to indorse this suggestion that I venture to add to this discussion.

I might, however, add that the practical education of the workshop, where the machine is to be daily used at its regular work, is another factor of great importance, and it is one which cannot be obtained by any system of study.

Its weak points are then seen and corrected for the future, and I can point to a system of machines long in use in very many of our American cotton-mills, and built by various makers, in which, within a few years, radical difficulties have been discovered by the workmen who had charge of them, which have led to the very extensive substitution of an English mechanism for accomplishing the same purpose, which avoided the errors. In this case both systems were old, and had been in use for years, but it was left for a keen practical workman to discover how and why it was that one did good work, while the other one failed.

*Mr. John T. Hawkins.**—Since writing thepaper I have had occasion to look over a work recently published by Dr. W. T Barnard. I believe he is assistant to the President of the Baltimore and Ohio Railroad. And I found some very pertinent references to that subject, and I make a few quotations from it as bearing out my idea, which I think is more or less understood by all the gentlemen who have spoken.

Dr. W. T. Barnard, in a recent treatise on "Technical Education," says, p. 37, referring to the tendency to ignore practica subjects:

"That this tendency is a very grave danger in technological schools generally, is very apparent from a study of those in England, where most of the institutions established purely and simply for technical instruction are already drifting into devotion for the higher mathematics, to the exclusion of drawing, applied science and mechanical teachings."

Judge McArthur says that "while we have schools for all sort of instruction in mathematics, history, literature, and philosophy i abundance, they fit nobody with either knowledge or skill in an particular branch of industry."

Again, p. 39, Dr. Barnard says: "Students in industrial classe should have greater facilities for visiting shops, factories, an mines, and for studying their operations, and should be examine with reference to their proficiency in *applying* scientific principles to the numerous processes they witness, just as students o botany visit fields and forests and study flowers and plants."

Again, pp. 105, 106:

"On the other hand, it is difficult to procure men at any pric who combine superior skill, comprehensive mechanical know edge, and general intelligence in such proportions as to mak them valuable as foremen, managers and specialists in mechanical pursuits, or in the operating branches of railway service. A appreciation of this fact, and the necessity for educating thei

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* In closing the debate.

work-people to an understanding of modern railway machinery, appliances and methods, has led a number of managers to seek the services of the graduates of technical schools as assistant foremen, assistant supervisors, assistants to engineers of roadway, master mechanics, etc. After some actual experience these young men are put in line of promotion, and inquiry shows that generally they stand well in their respective corps, but even after going through the shops, such graduates continue more theoretical than practical, and this constitutes the great objection to railroads taking into service technological school graduates, instead of educating their own young men.

On p. 11 he quotes from Prof. Huxley that "the advance of industry in all countries depends on employers being able to find to their hand persons of sufficient knowledge and sufficient flexibility of mind to be able to turn from the one thing they have been doing to something different, according to the nature of the improvement that has been made," and that "the development of industry under its present conditions is almost entirely the result of the application of science to the development of mechanical processes of complexity, requiring a great deal of attention and intelligence to carry them out."

Mr. C. D. Jamieson, of the Massachusetts Institute of Technology, in a paper on "Railroad Engineering Education," says: "In the instruction in any branch of engineering the one thing to be kept prominently before the student is economy of design and construction. It is not enough to be able to design and construct a bridge of a certain length which shall safely hold up a given load, or a station that shall accommodate a given number of passengers and trains, but this should be done at the least possible cost. In conclusion," he says, "let me say that the student should be so drilled that when he graduates he can have not only the diploma of the school, but, what is of more importance to him, can accept any position in his profession that offers, prove himself of use, and therefore a necessity to his employer, and earn a living for himself."

Replying * to Mr. Smith, I think he quite misinterprets the gist of the paper. It is not advocated that the student should be expected to become a full-fledged mechanical engineer or designer in four years, but that a good part of the more abstract mathematics of the present curriculum might be replaced by such

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* Added since the meeting, under the Rules.

development of his mechanical intuitions as to make him sufficiently far advanced as a designer to be able, at least, to commence to be one. If it should be necessary for a young man to have a thorough knowledge of the calculus, in order to become a mechanical engineer of high degree, I believe it much the more expedient that he should acquire that in after life rather than that his mechanical intuitions should be neglected so greatly as I believe is now done. I have no hesitation in taking the stand that the most superlative kind of mechanical engineer imaginable may exist, without the slighest knowledge of what "quaternions" mean or consist of; and I am equally positive that, so far as machine designing is concerned, there is so little use for analytical geometry and the differential and integral calculus that it would be vastly preferable to impart to the student nothing of these, or only sufficient of them to permit of his knowing what they were, allowing him to perfect himself in them in after life, if he found it desirable, rather than to neglect what I have tried to describe as his mechanical intuitions, for which he will have so much and such constant use.

Mr. Smith's student, who, after having studied plane geometry and recited his lessons well, failed to see the similar triangles, where the lines were to be imagined as existing in the tail stock of a lathe, serves admirably as an example of neglect of his mechanical intuitions. Shop practice, I believe, should not only be continued, but amplified in our technical schools, for which latter, I think there is plenty of room. I think technical schools may "fit young men for positions of usefulness in the department of mechanical engineering," without expecting to make them full-fledged mechanical engineers.

I cordially join Mr. Smith in saying, "Let us not denounce the technical schools," because they cannot make mechanical engineers and designers in the short time of four years; but let us improve them if we can. But neither let us, in discussions like this, put up men of straw merely to show how easily they may be knocked down again; for certainly there is nothing that can savor of denunciation of the technical schools in my paper. Nor will we "look upon the graduate with contempt," because he has not learned it all in the same brief period; but let us make him as useful as possible at the end of his school term, without detracting from, but rather adding to, his ability to reach the highest step of the ladder in due time, by giving him less of the abstract type of instruction. We do not want to equip them for astronomers, and can better omit certain of that which astronomers must have than that which is equally indispensable to them as mechanical engineers. I have no desire to find fault with the technical schools, and will yield to no one in admiration of what they are doing for the young men of our country in our profession; but I am under a very decided conviction that they may be improved, something on the lines indicated in the paper.

Gibbon says: "We have two educations, one from teachers and the other we give ourselves;" and what I wish to contend for is that the teachers should give us more of those things which we are obliged to buy, beg, borrow, steal, or dig out in some way for ourselves; and which in machine construction constitute the main requisites.

CCXLVIII.

NOTES FOR DISCUSSION IN RELATION TO THE DEVELOPMENT OF THE COMPOUND ENGINE AND THE PROBABLE LIMIT OF STEAM PRESSURE IN MARINE ENGINES AND BOILERS.

BY CHAS. E. EMERY, NEW YORK CITY.

(Member of the Society.)

FIFTY years ago, when steam navigation was in its infancy, the steam pressure employed in marine engines was as low as five to ten pounds; but as boiler construction improved, a rapid increase took place to 20 and 25 pounds, until finally a sort of stand-still, at a maximum of about 40 pounds, was established for a series of years.* This pressure was considered so high that compound engines were constructed in which to use it. Some notable examples running on the west coast of South America were familiar to marine engineers some twenty years ago, and curiously enough the reports which came from those engines corresponded closely with the reports from modern engines of certainly better economy, in that it was claimed that the power was obtained with the consumption of one and a half pounds of coal per indicated horse-power per hour. Higher pressures were in vogue on inland waters almost from the commencement of steam navigation on the same, but were introduced much more slowly on sea-going steamers. As improvements in the mechanical arts progressed, it was found that steam boilers could be safely constructed to carry higher pressures, and steam machinery for using steam at a pressure of 60 pounds

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* The well-known veteran engineer, Chas. H. Haswell, in response to an inquiry states, "In 1821, the first year of which I have a clear remembrance of the pressure of steam borne by the low-pressure boilers of the day, the general practice was from 10 to 12 pounds, as the form of boiler then, from the form of the flues and the absence of bracing, would not admit of greater pressure." He however adds, "The *Great Western*. the second steamer that reached here from England, was operated with steam at 3.5 pounds pressure. He boilers, the box flue, would not admit of a greater pressure."

and more was made from time to time. Engines of the compound type were made at an early date, but did not at first find much favor, and it was not until about the year 1870 that this engine was what may be called reintroduced and established as the marine engine of the future. The initial successes of this period were doubtless made by various constructors in Great Britain, the firm of Elder & Co. being probably in the lead. Mr. Thomas W. Lay, at about the same time, established a certain form of engine of this class on the Great Lakes. The writer also, through opportunities given as Consulting Engineer of the U. S. Coast Survey and Revenue Marine, took an active part in developing the system on the smaller government vessels,—and the information thus obtained, together with that which could be procured from abroad, was utilized by the U. S. naval officers in designs for compound engines for vessels of war.

Meanwhile, the "doubting Thomases" among marine engineers claimed that just as good results could be obtained with single engines of long stroke, and a number of vessels were built to prove this theory, many of which did very well. The practical work of the compound engines in this country soon, however, had the effect of converting the most earnest advocates of expansion in a single cylinder. Without mentioning names, it may be said that one after another the older engineers succumbed to the inevitable, and now the writer does not know of a single one, or a single firm, that adheres to former opinions and prejudices. To-day there are numbers of ocean steamers, both small and large, running with steam pressures as high as 160 pounds to the square inch. The steam is used in triple expansion engines, and the advocates of the system claim as great economy in the change from double to triple expansion as was originally claimed in that from simple engines to the ordinary compound engines. Undoubtedly overstatements are made as great as those formerly made in relation to the ordinary compound engine. The results with the latter should of course have been compared only with those from engines operating with the same steam pressure and under the best conditions for economy at that pressure: whereas they were frequently compared with those from low-pressure engines of obsolete type. The performance of the triple compound engines are not only *not* being compared with those of compound engines operating at the same pressure, but comparisons have been made with the results obtained with engines which have been allowed to run down, and in cases where the boilers have deteriorated so that the pressure originally intended is not maintained.

With the view of settling various questions of this kind in relation to the original compound engine, a series of experiments were made in the years 1874 and 1875, under the general direction of Chief-Engineer Charles H. Loring, U. S. Navy, representing the Navy Department, and the writer, representing the Treasury Department, with the machinery of various revenue steamers, designed by the writer, one having a compound engine, another a high-pressure condensing engine, another a similar engine with a jacket, and another with a low-pressure engine.

These experiments showed that at the pressure employed, viz 70 pounds, the gain due to compounding was only 12 to 15 pe cent. as compared with using the steam with an equal degree of expansion in a single cylinder. Official reports of these experiment were made to the Navy and Treasury Departments and the result discussed by the writer in several journals. Abstracts of the reports were also made by various periodicals and embodied in th current literature on the subject.* In the discussion, the write stated that the average economy of compound engines was doubtless nearer 25 per cent. than 12 to 15, on account of mechanica difficulties incident to keeping in order single engines working a a high degree of expansion and the liability of the engineer t reduce the steam pressure and follow a little further in the strok to relieve the strains on his joints and all the working parts of th engine and save himself work. The same difficulty was experienced with the first compound engines, and is supposed to be on reason why the Elders, in their original engines, set the mai valves to cut-off with full link at about one-half the stroke on th high-pressure cylinder, so that the engineer could only control th cut-off with the independent gear within that limit.

In the above experiments a horse-power was obtained in th compound engine of the U. S. Revenue Steamer *Rush* for 18.3 pounds of feed water per hour, the steam pressure being nearly 7 pounds. Now if the steam supplied to the engine in this case ha been generated at about 160 pounds pressure, and used to operat another piston until its pressure was reduced to that actually used i

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* See articles by the writer on "Compound and Non-Compound Engines,' *Transactions American Society Civil Engineers*, 1875 ; *Journal* Franklin Institute, 1875-6 ; *Engineering* (London), 1875-6 ; see also D. K. Clark's *Hand Book* Cotterill, *on the Steam Engine*, etc.

the high-pressure cylinder of the *Rush*, there would, on the basis that 80 per cent. of the total feed water was utilized in such first expansion, have been obtained about 40 pounds mean pressure, and 21.3 per cent. additional work, and the cost of the horse-power would have been reduced to 15.15 pounds of feed water per hour, which would have required for an evaporation of 8 pounds of water per pound of coal 1.89 pounds of coal per horse-power per hour. In average practice, it is believed that the evaporation would frequently be nearer 7½ pounds than 8, which would increase the coal consumed to, in round numbers, 2 pounds of coal per indicated horse-power per hour.

This indirect method of procedure probably gives about the average performance to be expected under actual conditions in modern triple expansion engines of moderate size. A better performance is claimed and undoubtedly is obtained under experimental conditions and in the larger ocean steamers. It is thought that further gain must be looked for in the performance of the boiler, as it is not believed that very much better results than 15 pounds of water per indicated horse-power per hour may be expected.

A clear gain, however, of upward of 20 per cent. is very important, and by the same method of reasoning, it would appear that steam of still higher pressure might be expanded in still another cylinder and used again and again with economy. This corresponds with the conclusion from different premises stated in another paper presented with this, on the subject of "Cylinder Condensation, and the Reduction of the same by the use of the Compound Engine."*

On the above basis, the limit of pressure would probably be fixed only by the capacity of the materials forming the steam cylinders and valve chests to resist the higher tensions and higher temperatures. It is probable that no material better than cast iron will be found for steam-engine cylinders, and this is made sufficiently dense to resist hydraulic pressure of several thousand pounds. Difficulty is, however, experienced in carrying pressures as high as 300 pounds with ordinary castings, and if the steam pressure were to be increased to or beyond that point more care would be necessary in selecting and manipulating the metal and molds.

A steam pressure of upward of 300 pounds was successfully carried on a small steamer called the *Anthracite*, which was built in England on the so-called Perkins High-Pressure System, and visited this country in 1880 to demonstrate that high pressures could

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* Trans. A. S. M. E., Vol. VIII., page 479.

be safely and efficiently utilized to furnish motive power on a ocean voyage. Her machinery was tested by a board of nava officers in New York. With an average steam pressure of 316. pounds expanded 25.7 times in triple expansion engines, there wer required 21.64 pounds of feed water per hour per indicated horse power. The engines were small, the aggregate indicated horse power developed being but 67.7. Still the cost was quite high, n lower, in fact, than has been obtained in exceptional cases wit good condensing engines, and about what ought to have been expected with an ordinary compound engine using a steam pressur no higher than 80 pounds. The same engine, tested in England b Mr. Bramwell, furnished a horse-power for 17.8 pounds feed wate per hour, the water level being then carried lower, so that the stea was superheated considerably. The superheating of the steam a these high pressures is not desirable in practical work on accoun of difficulty with the packings and lubricant. In the Dixwell experiments in Boston with superheated steam, it was considere that 450 degrees was the highest temperature which should be permitted. Proper precautions would indicate that even this temperature should not be allowed in sea-going engines involving s many responsibilities. The temperature of steam of 300 pound pressure is about 420 degrees, which, in the opinion of the writer is as high as can be carried satisfactorily in average practice eithe on sea or land. It seems certain that the highest steam pressur admissible would be limited by the temperature rather than b other conditions. In some of the marine engines using steam a 160 pounds pressure, it is found that there is a sufficient precipitation of water to permit the use of oil to be dispensed with afte the engines are fairly started from port. The temperature due t this pressure is but about 370 degrees. In the *Anthracite*, designe for a higher pressure, with some superheating, all the packing were made of a metal adapted to obviate the necessity of usin oil, and it is believed that, if the steam be kept dry so as to secur economy, a pressure even of 300 pounds will not be carried i practice without the use of some device of this character; and a any specialty always acts to limit general application, a less pressure will probably be generally adopted.

It is considered by the writer, that the proper limit of pressure has already been reached, if not exceeded, for the type of boile used in large ocean steamers. Cylindrical boiler shells 12 feet and upward in diameter, and $1\frac{1}{8}$ to $1\frac{1}{4}$ inches thick, are not recommended, although used in practice. To limit the thickness even to the figures named, it is necessary to use steel; and to procure even this of sufficient tensile strength, it is necessary, for such heavy plates, to use steel comparatively high in carbon, which is treacherous under ordinary manipulation. Not a plate of it should be used without annealing after every mechanical operation performed upon it. In fact, the whole boiler ought to be annealed after the plates have been riveted together, in order to overcome the injurious effects due to local strains produced in working it, but this is impracticable. Of course, boilers are made as heavy as this and but few fail, but the business cannot be considered on a safe and reliable basis so long as *any* fail.

As these are notes for discussion, it is considered well to state as an opinion that, since so many manufacturers have gone into the steel business, steel can no longer be considered in a commercial sense better than iron. The element of competition brings out steel which is altogether unfitted for boiler plates. Some boiler-makers recommend their customers not to use steel, and it is only when it is carefully inspected to ascertain its quality before being made up, and also carefully inspected while the boiler is being constructed, that the steel boiler can be relied upon. It is so much more homogeneous and in every way desirable when the material is right that there is no danger of every one going back to iron; at the same time, the steel industry is bound to have its ups and downs on account of the improper material furnished by many of the manufacturers. The new ones are not entirely to blame, as some of the older ones send out inferior material under the spur of competition. It may be proper to say that no steel should be used for boilers, unless it be properly inspected or furnished by a firm which is known to keep up its reputation and to send out nothing but what is suitable for the purpose.

The type of boiler used in most modern men-of-war is of the locomotive type, and has a smaller shell than the ordinary merchant marine boiler, and hence plates of proper thickness can be obtained without using steel so high in carbon. It would seem better to retain for these boilers the sizes now in vogue, rather than make larger ones requiring thicker plates, and also to retain substantially the thicknesses now in vogue, rather than to carry higher pressures requiring plates so much thicker as to necessitate the use of steel unusually high in carbon.

It is the opinion of the writer that boilers to carry the high pressures under consideration should be entirely without shells, except those of necessary separating drums with comparatively small diameters. On this system there will be no difficulty in carrying pressures as high as 400 or 500 pounds or more, if the difficulties referred to in the way of lubrication, etc., for cylinders could be overcome to permit them. Sectional boilers are so well worked out for use on land, and there has been such measure of success even in sea-going vessels, that it seems safe to conclude that the use of higher steam pressures need not be limited by difficulties in the construction of the boiler.

There is no reason why pressures as high as those in use on the Western rivers, *viz.*, from 180 as high as 200 pounds, should not be adopted in general practice to secure economy of fuel, and the considerations above expressed in relation to the lubrication, etc., indicate that the pressure may be increased to, or nearly to, 300 pounds, when commercial and economical considerations demand such a pressure. It is probable that for pressures exceeding 200 pounds, *quadruple* expansion engines should be used. The experiments with the *Anthracite* appear to sustain such an opinion. The writer has not seen a record of actual experiments, giving quantitative results with triple expansion engines other than those of the *Anthracite*, though much information of a comparative nature is available which is subject to the objections first above indicated.*

[NOTE.—*This paper was presented and discussed in connection with No. CCXLIX* (CYLINDER CONDENSATION AND ITS REDUCTION BY THE COMPOUND ENGINE), *which follows it. The Discussion is at the end of the latter paper.*]

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* In response to a request, Mr. R. H. Buel, M. E., who keeps up his index rerum, calls attention to reports of the trial of the *Anthracite*, published in the *Journal* of the Franklin Institute, February, 1881, page 81; March, 1881, page 161; October, 1881, page 260. The data given in the text were taken directly from the Government report. Reference is also made to *Engineering*, February 29, 1884, page 185, where are published some indicator diagrams from triple expansion engines of the *Isle of Dursey*, and the daily coal consumption. These data would appear to indicate lower consumption than calculated in the text, but this is the case with all such data from ocean steamers, as the single set of indicator diagrams taken daily do not represent the average performance for the entire day. Reference is also given to data in regard to triple expansion engines in *Engineering*, February 27, 1885, page 224, and this refers back to page 173, same volume, where is given a complete list, with principal dimensions, of triple expansion engines constructed prior to January 7th, 1885.

CCXLIX.

NOTES FOR DISCUSSION ON CYLINDER CONDENSATION AND THE REDUCTION OF THE SAME BY THE USE OF THE COMPOUND ENGINE.

BY CHAS. E. EMERY, NEW YORK CITY.
(Member of the Society.)

IN connection with the paper submitted herewith on "The Development of the Compound Engine and the Probable Limit of Steam Pressure in Marine Engines and Boilers," * the question will naturally be asked by those who have not particularly investigated the subject, why is it necessary to use the steam in a compound engine with a number of cylinders rather than in engines with single cylinders, the joint capacity of which, as is easily shown, need only equal the capacity of the low-pressure cylinder or cylinders of the compound engine? The reason of this was pointed out by the writer in the *American Artizan* of March 15th, 1871, and the subject of "Cylinder Condensation" was considered in an essay which was embodied as a preface to the descriptive matter of U. S. Patent No. 70,707, dated Nov. 12th, 1867, on the subject of the use of non-conducting linings in steam cylinders.

It has been thought that at this time a brief account of the experiences and considerations which led to the writing of the articles referred to will be interesting to many, and perhaps excite discussion and draw out much useful information on several important details of the subject.

The writer, from 1863 to 1869, then an Assistant Engineer in the U. S. Navy, was engaged in New York in making experiments under the general direction of Horatio Allen, Esq., and B. F. Isherwood, Engineer-in-Chief, U. S. N., designed to show whether or not there was any considerable gain in the expansion of steam—the first-named gentleman being an earnest advocate of expansion, and the one last named claiming that it had a very limited value. As is well known, Mr. Isherwood had, previous to that time, earnestly described in his writings the very considerable condensation

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* Trans. A. S. M. E., Vol. VIII., page 472.

of steam which takes place in a steam cylinder, and in a comparatively recent article in the Journal of the Franklin Institute, he claims to have been the first to call attention to that subject. Without disputing this claim, and at the same time acknowledging with gratitude valuable suggestions from him as to the proper direction of study, the writer desires to claim to have been the first to show that all of the cylinder condensation, independent of that due to the transmutation of heat into work, is fully accounted for by the heating and cooling of the metal walls of the cylinder. Mr. Isherwood clearly referred to such heating and cooling, but in an elaborate discussion went on to show that that cause alone would be insufficient, and he accounted for the greater part of the condensation on the supposition that during free expansion there is a loss of heat and consequent condensation due to what he terms "expansion *per se*," independent of the heat transmuted into the work done.* This supposed loss the writer never could understand and never expects to. As the total heat of steam at a high pressure is a little greater than at a lower, the free expansion of steam without doing work should result in superheating and not condensation, and the only heat which can be extracted during the performance of work is that due to such work at the rate of 772 foot pounds per British thermal unit. The maximum work possible is proportioned to the absolute pressure, and therefore the heat abstracted in the performance of the work is simply a function of such pressure, so that the ordinates of the adiabatic curve of expansion, or that in which

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* Extract from Mr. Isherwood's *Experimental Researches in Steam Engineering*, Vol. I., p. 131, published in 1863 :

"When steam is used without expansion, the condensation in the cylinder, exclusive of that which is due to the production of the power, probably does not exceed one or two per centum, but when it is used very expansively, the condensation, *in excess of re-evaporation*, rises to the enormous proportion of 30 and 40 per centum of the steam evaporated in the boiler, and, of course, when the re-evaporation, which in any case must be something, is added, these figures will be increased. Most of this condensation, however, I believe to be due to the expansion *per se*, particularly with moderate degrees of expansion, and not to the effect of re-evaporation, which becomes great only with large measures of expansion."

Extract from page xxviii of the Preface to Vol. II. of the above work, published in 1865 :

"The quantities in Column C are the per centum of the total heat which enters the cylinder in the steam medium of that annihilated in the production of mechanical power; there is additionally a quantity of heat annihilated by the expansion *per se* of the steam when it is used expansively in the production of internal work on the molecules of the steam."

the loss of heat due to the performance of work is considered, decrease more rapidly than in the inverse ratio of the volume, according to Mariotte's Law. Rankine states that the decrease of pressure is proportioned to the minus ten-ninths power of the volume. The actual curve of expansion of steam engines varies somewhat from this, according to differences in condition, but this general method accounts perfectly for the heat transmuted into work. This heat is derived partly from surplus sensible heat due to steam of a high pressure expanding to a lower, and the deficiency is supplied by a slight condensation of the steam; and there is no possibility of any further loss due to the "expansion *per se*" of the steam itself. To settle this and other questions, the writer, in the year 1867, constructed two small cylinders or chambers without pistons, one of glass and the other of iron, of exactly equal capacities and carefully felted, each of which could in turn be connected with a valve operated regularly by an engine to admit steam from a boiler to the cylinder, and permit its exhaust to a condensing coil. The exhaust opened slowly at first and rapidly afterwards, so that the pressure in the cylinder would approximate that in an ordinary steam cylinder. The result was, that the condensed steam from the iron cylinder was in nearly every instance fully double that from the glass cylinder. In this case there was no possible change in condition except that due to the material of which the metal walls were composed; whence the writer concluded that the losses from cylinder condensation found in practice could be attributed entirely to the heating and cooling of the metal walls, and that the "expansion *per se*" theory of Mr. Isherwood was unnecessary.*

Mr. Isherwood considered that when condensation took place in the mass of the steam, the water would be suspended in the form of a cloud, and would not be affected by the temperature of the metal surfaces. This the writer explained in 1867,—(see preliminary remarks in Letters Patent No. 70,707 †),—by reference to Tyndall's experiments, who points out in *Heat as Mode of Motion*, that the amount of moisture ordinarily contained in the atmosphere has seventy times the absorptive effect, in relation to latent heat, of dry air. Steam chilled by the performance of work or by reheating metal walls previously cooled during the exhaust stroke contains minute particles of water with a great capacity for heat,

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* Trans. A. S. M. E., Vol. VII., p. 375, No. 204—8, Top. Disc.

† And page 499 of Discussion of this paper.

and almost instantaneously absorbs that necessary for their re-evaporation from the surrounding walls, and this action is at a maximum when the exhaust takes place, when all the heat absorbed is carried to waste. Upon the above basis the writer in an article in the *American Artizan* of March 15th, 1871, explained the gain found in a compound engine on the basis that the quantity of heat which will be transferred from a radiator to an absorbent would vary as the square of the difference in temperature (the conditions being practically the same as those of force performing mechanical work in overcoming resistance). If, therefore, the metal wall of a cylinder, originally heated to a temperature of 320 degrees by live steam, were during the exhaust subjected to a temperature of only 140 degrees, the same metal surfaces would be exposed to a difference of temperature of 180 degrees; whereas, if the cylinders were so arranged that the work was done in two cylinders instead of one, with 90 degrees difference of temperature in each, the condensation per unit of surface in the compound cylinders would be, say unity, or, considering the increase of surface, say in effect twice unity, or two; whereas the rates of condensation in the single cylinder would be at the rate of two squared, or four. In other words, the double cylinder would save one-half the condensation. Now, as in practice about one-half of the total quantity of steam used is condensed in the cylinder at high degrees of expansion, the amount saved would be about one-half of this, or say 25 per cent. This rough explanation made at that time approximates the fact. On the same basis, there should be greater saving by dividing the difference of temperature into three or more parts, but the complication resulting from the multiplication of steam cylinders and their connections is not in practice warranted, without raising the steam pressure. The later developments are in the way of permitting the low-pressure engine and intermediate cylinder of a triple compound engine to do about the same work as the two cylinders of the ordinary compound engine did formerly; then to add a third cylinder to be supplied with steam at a higher pressure, to be expanded down to substantially that formerly used in the high-pressure cylinder of a double-expansion engine, which now corresponds to the intermediate cylinder of the double-expansion engine. Steam which has been reduced from a higher to a lower pressure by expansion is just as valuable for all purposes as if such lower pressure were obtained from a boiler direct. We say the *steam* is equally valuable. This does not include the water due to the expansion and to internal refrigeration. This should be removed or re-evaporated, by a system of steam-jackets, or the full benefit of the high pressure and expansion cannot be obtained. On the same basis of reasoning, evidently steam of still higher pressure could be expanded in a still smaller cylinder, and the steam from this supplied to the small cylinder of the triple expansion engine, and in this way quadruple and, by repeating the operation, quintuple expansion engines might be formed did not practical considerations interfere. This branch of the subject is discussed in the accompanying paper first above referred to.

DISCUSSION.*

Mr. Chas. T. Porter.—Disregarding at present the condensation of steam, as it enters the cylinder, we find the economy of an engine to be shown by the proportion that the mean effective pressure bears to the terminal pressure in the cylinder. The former represents the work done, the latter the steam consumed.

If in a non-condensing engine, admitting steam full stroke, an absolute pressure of 20 pounds is maintained, and a back pressure is suffered of 19 pounds, or 4.3 pounds above the atmosphere, then only one-twentieth of the steam is utilized. This may, perhaps, be taken as the most wasteful use of steam, though I have met with worse cases. It will form a good datum line—a sort of sea level—to measure our elevations from.

If in a similar engine an absolute pressure of 30 pounds is maintained, and a back pressure is suffered of 15 pounds or .3 of a pound above the atmosphere, then one-half of the steam is utilized. We have now an immense advance in economy. The same weight of steam does 10 times the work it did in the former case.

If again in such an engine an absolute pressure is maintained of 150 pounds, and the same back pressure of 15 pounds is suffered, we have a second advance in economy, almost as great as the first one. Now nine-tenths of the steam is utilized. The same weight of steam does 18 times the work it did in the first case. This would not be very bad practice. Probably a majority of non-condensing engines run with less economy than this.

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* Jointly with the paper by same author on the DEVELOPMENT OF THE COMPOUND ENGINE, AND THE LIMIT OF STEAM PRESSURE IN MARINE ENGINES AND BOILERS (No. CCXLVIII.) which precedes.

Now let us cut off our 150 pounds at ¼ of the stroke. The terminal pressure is 37.5 pounds, so we are using only ¼ as much steam as before. The mean effective pressure has fallen, however, only to 74.5 pounds, and so to twice the terminal pressure. The same steam now does twice the work it did without expansion, or 36 times the work done in the first case.

The gain made by employing high pressures is very great, especially when working expansively. This will appear by comparing with the above the inferior results got by cutting off at the same point, ¼ of the stroke, with ½ of the above pressure, or 75 pounds absolute.

The terminal pressure is now 18.75 pounds. The mean effective pressure is 30 pounds, or only 1.6 times the terminal pressure. 28.8 times as much work is now done by the same weight of steam as was done in the first case, instead of 36 times as much. The gain in this class of engines from using high pressure is due to the fact that a smaller proportion of the total pressure is wasted in overcoming the resistance of the atmosphere.

In non-condensing engines, with ordinary pressures, there is no gain made in economy by cutting off earlier than ⅓ of the stroke. In this class of engines we are absolutely limited to three expansions. If we cut off earlier than this, we merely reduce the power, and so the value of our engine, with a loss instead of gain in economy.

It is true that if the pressure be raised; as from 60 pounds to 100 pounds, and so the steam be cut off earlier to do the same work, the consumption will be reduced. This is owing to the higher pressure. If a smaller engine be substituted, in which this higher pressure must follow to ¼ of the stroke, in order to do the same work, a still further gain in economy will be made.

The following tables show the relation of the mean effective to the terminal pressure, and so the gain by expansion on the theoretical assumptions of no back pressure above the atmosphere, no waste room, and no condensation of the entering steam, under the two pressures, and for the several points of cut-off taken. The computations are made in the most simple manner, without regarding the effects of the conversion of heat into work, or of the fall of temperature during expansion. It is desired that nothing shall call attention away from the single relation which is presented.

TABLE 1.

ABSOLUTE PRESSURE, 90 LBS.

Point of Cut-off.	Terminal Pressure.	Mean Effective Pressure.	Ratio of Former to Latter.	Increase in Ratio.
1-2 stroke.	45 lbs.	61.5 lbs.	1 to 1.366	....
1-3 "	30 "	48.27 "	1 " 1.609	.243
1-4 "	22.5 "	39 "	1 " 1.733	.124
1-5 "	18 "	32.28 "	1 " 1.793	.060
1-6 "	15 "	27.18 "	1 " 1.812	.019
1-7 "	12.86 "	23.18 "	1 " 1.803	—.009
1-8 "	11.25 "	20 "	1 " 1.782	—.021

TABLE 2.

ABSOLUTE PRESSURE, 160 LBS.

Point of Cut-off.	Terminal Pressure.	Mean Effective Pressure.	Ratio of Former to Latter.	Increase in Ratio.
1-2 stroke.	80 lbs.	120.5 lbs.	1 to 1.5	....
1-3 "	53.33 "	96.75 "	1 " 1.814	.314
1-4 "	40 "	80.74 "	1 " 2.018	.204
1-5 "	32 "	68.82 "	1 " 2.15	.132
1-6 "	26.66 "	59.76 "	1 " 2.241	.091
1-7 "	22.86 "	52.64 "	1 " 2.303	.062
1-8 "	20 "	46.88 "	1 " 2.344	.041

The above tables show the possible gains from expansion in non-condensing engines under these theoretical conditions. It is obvious that the unavoidable losses from early release, incomplete exhaust, waste room, and cylinder condensation are sufficient to change into positive losses the small apparent gains from expanding below ¼ of the initial pressure with 90 pounds, or ⅕ with 160 pounds absolute pressure.

In this class of engines, when expansion is effected by the method of compounding, the limit of economic gain is reached still earlier. Here there is probably no advantage to be derived from more than two expansions, or expanding below one-third the initial pressure. The reason for this earlier limit is, the loss of pressure which is suffered when the steam is transferred from the high-pressure to the low-pressure cylinder. This loss is not compensated, except in a slight degree, by diminished condensation. The condensation in a given time varies in two ways—first, as the difference between the alternate temperatures to which the surfaces are exposed (not as the square of this difference, as erroneously stated in the paper under discussion), and second, as the area of the condensing surfaces.

Now the aggregate area of condensing surface in the two cylinders of a compound engine is greater than that in a single cylinder of the same power. The reduction in the loss from condensation which is made in the former is caused by the fact that all the internal surfaces of the two cylinders are not exposed to the highest and the lowest temperature. Suppose the alternate changes of temperature to be, as they are approximately in practice, divided equally between the two cylinders, one-half in each. Then if the aggregate condensing surfaces in the two cylinders were only equal to that in the one cylinder, one-half of the loss from condensation would be avoided, for with the same area of surface, each square inch would be exposed to only one-half the changes of temperature. If the compound cylinders presented twice the surface of the single cylinder, we would have no saving of condensation, for twice as many square inches would be exposed, each to one-half the differences of temperature. In practice, the surfaces of the compound cylinders are greater, but not twice as great, as those of the corresponding single cylinder, so there is a small, but only a small gain in this way.

The loss of pressure between the two cylinders renders compounding in this class of engines a rude mode of expansion, even within the narrow limits mentioned, and one which ought not to be used where it is practicable to cut off in a single cylinder. The large class of engines, as steam pumps, in which the steam must follow full stroke, affords the only legitimate field for compounding against the atmosphere, the resistance of which is exerted against the area of the larger piston.

It seems important that a protest should be made here against the absurd practice of compounding non-condensing engines, in which the steam can be expanded in one cylinder.

Condensing engines, when compounded with steam-jacketed cylinders, show a large gain in economy. The paper under discussion attributes all this gain to the avoidance of cylinder condensation, on the erroneous assumption that the condensation varies as the square of the difference between the temperatures of the live and the exhaust steam, and it says nothing at all about the additional work which is done by the steam when further expanded in the second, or in the second and third cylinders.

Now the fact is obvious, that this additional work is very great. While the pressure is low on these late expansions, the piston area is so much greater, that in triple expansion engines the last cylinder does the most work. This additional work accounts for the gain in economy several times over, showing that this gain must be in a large degree neutralized by losses of some kind.

It is worth while to exhibit this clearly. Let us suppose a theoretical case of 160 pounds pressure expanded 64 times, or to 2.5 pounds pressure, in three cylinders, and without loss in transmission from one cylinder to the next one, and thence to be exhausted into a condenser, in which a perfect vacuum is maintained—as compared with the same volume of steam, of the same pressure, exhausted into the same perfect vacuum, without any expansion.

In the last case we have

$160 \times 1 =$	160

In the first case, cutting off at $\frac{1}{4}$ stroke in each cylinder, we have

(1.) $\frac{160}{4} \times 2.386 = 95 - 40 = 55 \times 4 =$	220
(2.) $\frac{40}{4} \times 2.386 = 23.86 - 10 = 13.86 \times 16 =$	220
(3.) $\frac{10}{4} \times 2.386 = 5.96 \times 64 =$	380
	——
Total,	820

The final multipliers, 1, 4, 16, 64, are the product of the cylinder areas into the distances through which the pressure acts. The last cylinder, though having only 5.96 pounds mean pressure, is by far the most efficient, the area being 16 times that of the first one, and there being no back pressure to be deducted.

The gain by expansion is more than fourfold. Of course, these theoretical conditions cannot be realized. But let us see how large the margin is for practical drawbacks.

For this purpose we will compare this supposed case with one of 90 pounds pressure, cut off at $\frac{1}{4}$ stroke and expanded against the atmosphere. In that case we have already seen the theoretical gain from expansion to be 73.3 per cent. Here the theoretical gain is 412 per cent., or 5.62 times as much.

Now the former is a familiar case. It is well known that in ordinary good practice, with common losses from early release, incomplete exhaust, waste room and condensation in unjacketed cylinders, a horse-power is got in this way by the evaporation of 25 pounds of water per hour. Here, therefore, we have a good basis for comparison, and on that basis the consumption of water in the three-cylinder system should be $25 \div 5.62$, or less than $4\frac{1}{2}$

pounds of water per horse per hour. The paper does not claim a consumption to have been reached so low as three times this, or 13½ pounds.

But even supposing the expansion to have been carried down only to 10 pounds, or 16 fold, and the steam to have been exhausted from the second cylinder into the condenser against 2 pounds pressure, which is ordinary practice, then the gain from the 16 expansions should be 255 per cent., or 3.4 times as much as in the non-condensing engine. This would call for a consumption of only 7.3 pounds of water per horse per hour, or one-half the amount evaporated in practice. So it is obvious that the gain effected in compound engines is obtained wholly by the increased number of expansions, in spite of largely increased losses, due partly to loss of pressure between the cylinders and partly to increased condensation.

A few words on the assertion contained in the paper that the condensation of the steam, as it enters the cylinder, varies in amount according to the square of the difference between its temperature and that of the exhaust to which the surfaces had just been exposed. That proposition will not stand up very long to be looked squarely in the face.

It means that if this difference is 100 degrees, the condensation is 10,000 times greater than if it is one degree, or that, if in the latter case $\frac{1}{100}$ of one per cent. of the steam is condensed, then, in the former case, it is all condensed, and must be replaced, so that the indicator can account for only 50 per cent. of the water evaporated. With the difference named in the paper, of 180°, the condensation would be 32,400 times as great as it is for a difference of 1°. Common experience, in the large gain in economy, always made by using higher pressures involving greater differences of temperature, and by attaching condensers to non-condensing engines, which involves still greater differences, proves that no such ratio can exist. If it did, or anything like it, the result would be loss instead of gain, in all these cases.

Mr. Geo. H. Barrus.—I have little to say in regard to the effect of compounding upon cylinder condensation, but I would call attention to the matter of the free expansion of steam without doing work, and relate a somewhat remarkable experience which has come to me, and one which those who do not believe in condensation due to free expansion may, perhaps, find it hard to explain. I am myself an unbeliever in this cause of condensation, and I take this opportunity in the hope that discussion may make the question less obscure.

In 1877 I had the good fortune to conduct in Boston the Dixwell experiments on superheated steam. Some of the results of two of these experiments are given in the accompanying table. The two experiments referred to are those numbered 1 and 3. They are two of a series made under the direction of a board of naval engineers, of which one of our members, Chief Engineer Loring, was chairman.

In the first experiment, which was made at a cut-off of 43.9 per cent., the steam left the superheater at a temperature of 490°, and it entered the steam-chest at 441°. Arriving in the cylinder, the temperature fell to 306°, and in the exhaust pipe close to the cylinder to 210°.

In the third experiment, which was made at a cut-off of 67.2 per cent., the steam left the superheater at a temperature of 425°, and entered the steam-chest at 406°. Here the temperature in the cylinder fell to 305°, and in the exhaust pipe to 212°.

One of the objects of the Dixwell experiments was to show that in the use of superheated steam there was a great loss of temperature where the steam entered the cylinder, due to the causes which operate in producing cylinder condensation. In the discussion of the results of the experiments the question arose whether it might not be claimed that the loss of temperature could not be explained solely on the ground of the radiation of heat, which takes place from the outside of the cylinder. It was thought that if the same quantity of steam were passed through the cylinder in a given time which was consumed on these experiments, but instead of running the engine, keeping it at rest, there would be no cooling effects except radiation, and the question which was raised would in this way be settled in a most satisfactory manner. Accordingly, the two experiments referred to were supplemented by two further experiments made with the engine at rest, the four valves wide open, and the throttle-valve adjusted so as to pass the same quantity of steam through the cylinder in a given time, as was used when the engine was running. The results of these tests were given in lines 2 and 4 of the table. The last column gives the total heat of the steam discharged to the condenser, this serving at the same time as a calorimeter.

These tests show that with the shorter cut-off, the running of the engine caused the temperature at the throttle-valve to be reduced

8°, that in the cylinder 71°, and that in the exhaust pipe 148°, and there was a similar though not so marked an effect in the case of the test with the longer cut-off.

There is much here that is worthy of study, but I would direct attention only to the matter of the difference in temperature between the cylinder and the exhaust pipe in the cases where the engine was in operation. There is a difference of 96° in one case and 93° in the other case, while only 19° and 18°, respectively, occurred when the engine was not running. I can understand that the cooling action which produces cylinder condensation was the cause of the large amount of cooling which took place on the entrance of the steam to the cylinder—135° in one, and 119° in the other—but I cannot account for the large amount of cooling which took place when the steam left the cylinder, unless the free expansion into the exhaust pipe produced it.

Number for Reference.	Condition of the Engine, Running or not Running.	Boiler Pressure above Atmosphere. Lbs.	Initial Pressure above Zero. Lbs.	Pressure at Cut-off above Zero. Lbs.	Pressure at Release above Zero. Lbs.	Proportion of Stroke Completed at Cut-off.	Mean Effective Pressure. Lbs.	Average Back Pressure above Atmosphere. Lbs.	Weight of Steam Consumed per Hour. Lbs.	Temperatures of the Superheated Steam. Deg. F.				Total Heat of Steam Discharged to Condenser. Th. Un. above 0 Fahr.
										Pyrometer near Superheater.	Thermometer near Throttle Valve.	Pyrometer in the Cylinder Head.	Pyrometer in the Exhaust Pipe.	
1	Running.	50.0	66.6	58.8	26.0	.439	33.5	0.6	392.1	490	441	306	210	1159
2	Not "	54.0	....	...	...	....	...	....	409.4	490	449	377	358	1223
Reduction of Temperature or Heat due to Running Engine.										....	8	71	148	64
3	Running.	50.2	66.9	61.1	39.7	.672	43.7	0.7	560.	425	406	305	212	1171
4	Not "	50.7	....	....	....	....	....	....	550.1	425	412	355	337	1210
Reduction of Temperature or Heat due to Running Engine.										....	6	50	125	39

Mr. Geo. H. Babcock.—There is no more promising line of inquiry for securing increased economy in steam-engines than the prevention of internal condensation. All the superior economy of the compound and triple expansion type of engines seems to be based upon their effect in reducing the severe loss from this cause. In fact, all other sources of loss are apparently enhanced in the compound type, such as larger clearances, more external surface for radiation, extra friction due to increased number of working parts, and multiplied possibilities of leakage. Still, notwithstanding these apparent disadvantages of the multiple cylinder engines, there is no question but that they do secure a superior economy over the single cylinder carrying expansion to the same extent. The reason of this has come to be pretty well understood to be the less internal condensation, owing to the less differences in temperature, and also to the fact that the re-evaporation in the high-pressure cylinders becomes available steam for use in the cylinders to which it is transferred. But I am inclined to think Mr. Emery is mistaken in supposing that the internal condensation is in the ratio of the squares of the differences in temperature, for the reason that the conductivity of iron does not increase with the temperature, but decreases. According to Forbes' experiments, it would seem that the conductivity of iron at the temperature of 300° is more than seven per cent. less than at 212°. The number of heat-units therefore absorbed by a given weight of metal in a given time will not increase at a greater ratio than the increase of temperature, but probably at a lesser ratio.

Though the fact of condensation in the cylinder was not unknown to engineers, and its action at each stroke was recognized and described as long ago as 1855, by D. K. Clark, in his *Railway Machinery*, yet Mr. Emery was undoubtedly the first to suggest as a remedy a non-conducting lining to the cylinder. Could such a lining of glass or porcelain as proposed by him in 1862 be made to stand the action of the steam as well as the wear and concussion to which it would be exposed in use, it would probably reduce the losses from this cause to a minimum, and—what at first seems like an Hibernianism—the more economical the engine the greater would be the saving. It is also evident that, could the internal condensation be entirely suppressed, there would no longer be any use for the compound form of engine, as the single cylinder would be the most economical. The problem is, however, more difficult than at first it would seem. The lining must not only be a non-conductor, but must withstand the temperature and dissolving action of the steam, and must not have such a different co-efficient of expansion as to cause it to crack and come off under the extreme temperatures to which it is exposed, and must be capable of being so firmly attached to the metal as to allow no entrance of steam between, as it is evident that a mere covering which would allow the steam to come between it and the metal would magnify, rather than decrease, the losses.

Various attempts have been made to prevent internal condensation by maintaining the temperature of the metal as high as that of the steam. Steam jackets have been extensively used with some beneficial results, which have increased as the temperature of the steam in the jacket has exceeded that in the cylinder. Smoke jackets have also been used in which the products of combustion were admitted. Apparently valuable results were obtained by Sir Daniel Gooch, in locomotive cylinders, in this way, in 1850. Another method of accomplishing the same result has been to superheat the steam itself, so that it may give up sufficient heat to keep the metal up to the temperature of saturated steam of the initial pressure, without itself being cooled below the same point. This plan has succeeded well so far as lessening condensation is concerned, but it has always been found that, when the temperature was high enough to prevent condensation, lubrication was practically destroyed. Dixwell found that, by superheating so as to maintain in the cylinder a temperature of 400° with steam at a pressure of 70 lbs., the condensation could be reduced to a minimum, but it was found to be impracticable to maintain the superheating at this degree, because every variation in the cut-off required a corresponding variation in the initial superheating. This temperature he found to be the limit of possible lubrication. With a higher pressure that degree of superheating would not afford sufficient additional heat for the purpose. The present tendency to high pressures seems, therefore, to preclude the possibility of much gain through superheating, because the temperatures are already carried to very nearly the limit at which lubrication can be maintained.

The depth to which the surface is affected by the alternate heating and cooling action is very slight—much less than is usually supposed. Let us assume that we have a cylinder of usual proportions in marine engines, diameter not essential, but with 4-feet stroke, using steam at 75 lbs. pressure, and also assume that the condensation is as great as 50 per cent. of all the steam used when cutting off at ⅛, or, in other words, equal to the steam required to fill the cylinder at the point of cut-off, which would be 6 inches. Let us see how thick a film of metal will be required to be heated to absorb the latent heat of the steam so condensed. Neglecting the sides, we have for each square foot of area of the piston, and an equal area of head, ½ a cubic foot of steam to be condensed, or ¼ of a cubic foot per square foot of surface. Steam at this pressure weighs .209 pounds to the cubic foot, making .052 pounds of steam to be condensed for each square foot of metal. As there is no loss of pressure, the pressure being kept up by the admission of more steam from the boiler, each pound will lose 888 heat-units, and .052 pounds 46 heat-units. Assume, also, that the metal at the beginning was at the temperature of the condenser, 120°, and that it is heated to the temperature of the steam, 320°, a difference of 200°. The 46 heat-units will heat .023 pounds of water 200°. The specific heat of iron being .114, the same quantity of heat would elevate the temperature of .2 pounds of iron the same number of degrees, and when we spread this .2 pounds over a square foot of surface it makes a film of only .05 of an inch in thickness. But it is at the surface only that the temperature will be 320°, while each parallel plane will be less, decreasing until we arrive at a plane of constant temperature, which we have assumed to be at the temperature of the condenser. The depth of this plane of constant temperature is easily found by means of the curve of diffusion. Let the diagram Fig. 161 represent a section of surface, the horizontal scale being degrees of temperature, and the vertical the actual scale of depth. The shaded portion between 120° and 320° shows the distribution of the fluctuating heat. For heat to penetrate this distance in iron requires a little more than $\frac{1}{6}$ of a second, which would, under the conditions assumed, represent about 40 revolutions of the engine per minute. Increasing the speed will decrease the depth to which the heat will penetrate, and consequently the amount of condensation, but only in the ratio of the square root of the speed. Thus, to decrease the condensation one-half will require four times the speed, or 160 revolutions per minute. It is evident, therefore, that at any available speed the condensation would still be great at short cut-offs, and that no great saving in this respect is to be hoped for in the line of high speed.

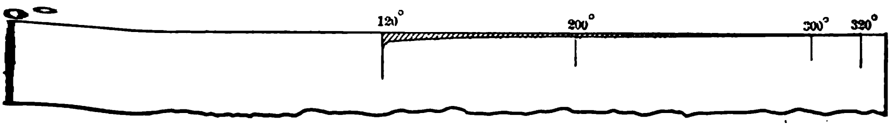

Fig. 161.

We may represent this condensation on a diagram by an added area which shows added consumption of steam, but not increased power, as in this diagram (Fig. 162), in which the shaded portion is the area added for condensation. At ⅛ cut-off the steam used would be represented by the parallelogram A, M, O, C; at ¼ by A, M, P, D; at ½ by A, M, Q, E; and at full stroke by A, M, R F, supposing the condensation to remain a constant quantity.

The loss would be a variable percentage of the steam used, at different degrees of expansion, as is well known to be the case. Thus, at ⅛ cut-off it would be 50 per cent., at ¼, 33⅓ per cent., and at ½ stroke, 20 per cent., and at full stroke, only 11 per cent. It will not, however, be constant, as the times of exposure to the steam will vary, being for the above cut-off as 1, 1⅓, 2, and 4, and, other things being equal, the percentages would be increased in the ratio of the square roots of these numbers and become 50, 38, 28, and 22, which probably represent about a maximum under the conditions of ordinary practice.

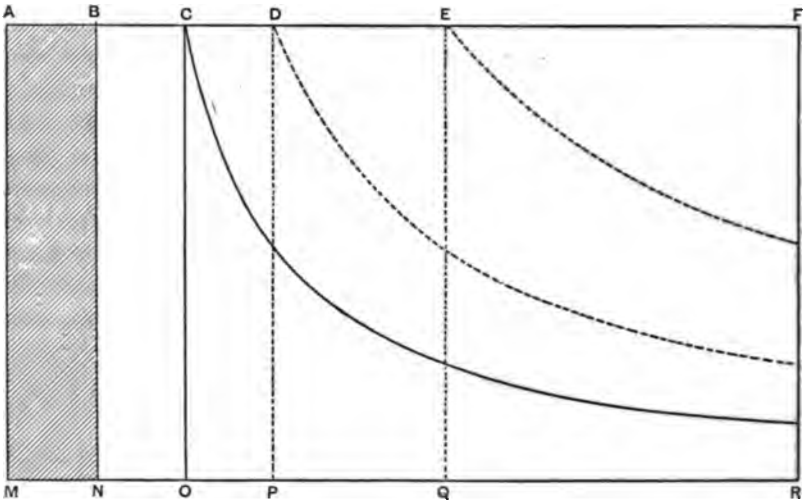

Fig. 162.

The surface film affected by the heating and cooling action in the cylinder being so very thin, it may be a question whether the ability for conducting heat has as great an influence on the result as the thermal capacity. It is supposable that the surface of even a non-conductor would condense some steam, and that the amount so condensed would depend upon the capacity of that surface for heat. This thermal capacity may be defined as specific heat by volume, and is proportioned to the product of the specific heat into the specific gravity. If time did not enter into the problem, this would be the only element determining the amount of condensation, but as time is involved and the film has had appreciable depth, that depth for equal times will depend upon the conductivity of the substance composing the surface. The relative effect, therefore, of any two surfaces in producing the condensation under discussion will be dependent upon three things: 1st, The depth to which the heat will penetrate in a given time, which is determined by the conductivity of the material; 2d, the weight of that film of material, which is determined by its specific gravity; and 3d, the amount of heat required to raise that weight of material to a given temperature, which is determined by its specific heat. The value of any given material, therefore, for the surface of a cylinder for this purpose will probably be inversely as the product of these three quantities, or what Sir William Thompson calls the "thermal diffusivity" of the substance.

The actual conductivity of substances we know very little about. In fact, Sir William Thompson, in his article on heat in the *Encyclopædia Britannica*, ventures to give it but for two metals, copper and iron. We may, however, use the comparative values obtained by Weidmann and Franz to ascertain the relative values of different metals and porcelain for this purpose:

Substance.	a Conductivity.	b Specific gravity.	c Specific heat.	Diffusivity a x b x c.	Ratio.	Rate of expansion.
Silver..............	100.	10.47	.0570	59 68	4.922	194
Copper............	73.6	8.92	.0951	62.43	5.149	186
Iron................	11.9	7.85	.1298	12.12	1.000	119
Tin.................	14.5	7.29	.0562	5.94	.489	230
Lead........... ..	8.5	11.31	.0314	8.02	.248	280
Bismuth..........	1.8	9.82	.0308	.544	.0449	133
Porcelain.....	.36	2.38	.198	.169	.0140	88

Among the metals bismuth stands by far the highest. A cylinder made of it would show but 1-22 the condensation of iron under the same conditions. Besides, it has approximately the same expansion, so that it would not tend to separate under differences of temperature, if plated upon iron. It is true that porcelain presents a higher possible saving, but it has great difficulty of application.

The evident advantages of bismuth as a lining for steam cylinders, particularly on those parts which are not exposed to wear, led me to expend considerable time and some money toward securing a practical realization of its benefits, but thus far without any success. The thickness of such a lining would not have to be great and therefore not expensive, if it could be applied; 1-64 of an inch would, I think, be ample under all circumstances. This I attempted to secure by electrical deposition, but was balked by an unforeseen difficulty—that of the extremely low electrical conductivity of the material. It was possible to make a mere surface coating of bismuth upon another metal, but immediately this arrived at an appreciable thickness it was found impossible to deposit any more of the metal thereon; subsequent deposits taking the form of non-adhesive black powder. We succeeded in covering the surface by securing thereto a sheet of the metal securely fastened, but this also proved abortive from another unforeseen difficulty. Although this metal does not melt below a temperature of 500°, it apparently becomes soft at the temperature of the steam, the impact of which broke the bismuth up into fine particles in the vicinity of the ports, and these found their way out through the exhaust pipe.

The ideal substance for this purpose is an ideal varnish which shall have a very low thermal diffusivity and be capable of standing the action of high pressure steam. I am not without hope that this may yet be discovered. Meanwhile we have to be content with what can be obtained by multiplication of cylinders with all its attendant expense and inconvenience.

I would further add, in reference to the gain from the principle of compounding, that Mr. J. P. Hall recently read a paper before the North-East Coast Institute of Engineers and Shipbuilders, giving a shipowner's view of the relative value of compound and triple expansion engines. Comparing 12 ships with compound engines with 9 others with triple-expansion engines, and reducing them to a common standard of 10 knots per hour with 1,000 tons weight carried 1,000 knots, all in "reasonably fine weather," he found an average of about 25 per cent. in favor of the triple-expansion, both on "displacement" and "dead weight" performance.

Mr. Joseph Morgan, Jr.—Mr. Emery's remarks indicate a disposition to force steel manufacturers to defend themselves. He asserts that steel can no longer be considered, in a commercial sense, better than iron. This is not the fact. In good practice, with the same attention to details of selection of stock and methods of manufacture, greater uniformity and reliability of product can be looked for in steel than in iron plates. There are twice as many well-managed concerns now making steel boiler plates as there were five years ago, and the knowledge of the properties of steel and proper methods of its manufacture is greater now than then. In our own works, in the management of several hundred boilers, many of which were built of iron in former years, we have uniformly used steel in recent construction from a conviction of its greater fitness for boiler work. All our breakages of boiler plates in actual use have been of the iron plates. Some of these breakages have been startling, though fortunately without disaster, and we unhesitatingly decide in favor of steel, with which our experience on nearly one hundred boilers has been uniformly good and so far absolutely without break or crack in use.

Our boiler makers prefer to work steel, as there are few or no wasters in flanging work. This is the experience in all boiler shops. There is no reason why manufacturers of steel boiler plate should be less conscientious than the makers of iron plates. There are fewer of the steel makers, their plants are more modern, better fitted to roll sizes now asked for, and their process is cheaper for high grades. There has been also a substantial advance in the metallurgy of steel in cheapening the material which it is necessary to put in best grades of plates. Mr. Wellman has been one of the pioneers in that way in developing the puddling business, and so getting cheaper stock than the charcoal blooms before used for best plate; and at Cambria Works we have developed another method, the Krupp washing process, of dephosphorizing pig-iron and getting cheap pure stock in that way, so that steel men are able to meet the cheaper market and be conscientious at the same time.

There is one misconception of the reason why thick plates are not as good as thin ones which seems to run through all the literature on the subject. The statement is made that to make a good thick plate it is necessary to raise the carbon to get the tensile strength. This is not the case if a proper amount of work is put on the ingot. If an attempt is made to reduce an 8-inch ingot to an inch and a half or inch and a quarter or inch plate, and get the same elastic limit and ultimate strength as if the same ingot was rolled to a quarter inch plate in the same mill, it will not succeed. But if an ingot of a proper size is reduced, and the proper amount of work is put upon it, the inch plate will certainly show as good a quality as the quarter inch plate. It is not, therefore, necessary to use higher carbon in thick plates.

Mr. Allan Stirling.—I think it is rather late in the day for Mr
Emery to say that there is any question as between iron plate
and steel plates for boilers. I think I may say that ten years ag
that question was settled completely, and that locomotive me
are using steel plates entirely for boilers. Of two or three hun
dred locomotives which were under my charge some eight year
ago, I am sure there was not an iron plate in any one of them. I
ordering some boilers some few months ago, I had no hesitation i
specifying steel from any one of three steel works, feeling confiden
that I would get steel of the quality necessary for that work. W
have with us to-night one gentleman from whom I would order stee
with the utmost assurance that I would get a material whic
would meet all the requirements of first-class boiler work.

Mr. George S. Strong.—I should like to say a word in regard t
this steel question. I was talking a short time ago in regard to i
to the mechanical engineer of one of our leading railroads, and
asked him if there was any trouble about getting steel plates t
meet their specifications. He said no, and that although thei
specifications were very rigid and very stringent in regard to th
quality of steel in every respect, as well as to the thickness of th
plates, they found no difficulty whatever in getting plates to mee
their requirements, not only from one mill, but from quite a nur
ber. I myself am now building boilers in which we require ver
thorough annealing. Our furnaces are corrugated, and the shell
are welded, and all this work requires a material which will stan
the welding, corrugating, flanging, etc., without any indications
cracking, and we use steel with the utmost assurance, which i
more than we do with any iron we can get. In fact, the Leed
company, who are the manufacturers of the corrugated fu
nace in England, when they started manufacturing these corr
gated furnaces, took iron plate and found they could not depen
on it. Then they started to roll plates of the right size, of stee
and since they adopted steel plates they have had very littl
trouble with their furnaces. Before that they never knew whe
they were going to have a blister on a furnace. We find in wel
ing to-day that we can weld with more assurance the steel tha
we can the iron.

The President.—Before calling on Mr. Emery to close the di
cussion, I will say also just a word in regard to the steel pla
matter. Mr. Stirling is undoubtedly correct in regard to boil
makers. Experience has taught them that steel is better tha

iron. Still there is a great deal of prejudice against steel and in favor of iron. I happen to know of a case in which a company building a mill for the purpose of manufacturing steel put it in their specifications for boilers that the shells must be made of wrought iron.

Mr. F. R. Hutton.—In connection with the above paper on Cylinder Condensation, etc., Mr. Walter C. Kerr, of Westinghouse, Church, Kerr & Co., sent a letter to me before the meeting, stating that the prefatory note embodied in the specifications of the United States Letters Patent referred to in the paper contained an interesting discussion of the subject of Cylinder Condensation, and the economy of steam, and suggesting that the same was worthy of reproduction in shape where it could be readily consulted. On examining the introduction it appeared to be a valuable contribution to the subject, which, though written over twenty years ago, had been practically inaccessible. It has, therefore, been decided to publish the same in full as part of the discussion on this subject.

It will be observed from the last two paragraphs that the patent was on the application of Non-Conducting Linings in Steam Cylinders, the subject heretofore discussed by Mr. Emery in connection with one of the Topical Questions. (See Transactions, Vol. VII., page 375.)

EXTRACT FROM UNITED STATES LETTERS PATENT, NO. 70707, DATED NOVEMBER 12, 1867, GRANTED TO CHARLES E. EMERY FOR IMPROVEMENT IN STEAM ENGINES.

* * * * * * * *

"It is a patent fact that the best steam-engines utilize only one-tenth to one-ninth of the heat in the steam used. Scientific men speak of the loss as unexplained, and state that the steam-engine is yet in its infancy, and that no considerable improvement has been made in it since the days of Watt. In spite of all the efforts of inventors to economize fuel, the fact still stares them in the face that nearly ninety per cent. of the steam is wasted.

Many experiments have been made with superheated steam, and that principle may undoubtedly be carried with economy as far as mechanical means will allow. Most attention, however, has probably been paid to the enormous profits promised by theory from the expansion of steam; but it is well known that the expected gains from this source have never been practically realized. Compared with full-stroke, the theoretical gain by cutting off steam at half-stroke is sixty-nine per cent. At quarter-stroke it is one hundred and thirty-eight per cent., and increases with the grade of expansion. Notwithstanding this, it is doubtful if a gain of fifty per cent. has ever been obtained in practice, which could all be attributed to expansion alone, rather than to differences in pressure, superheating, or some circumstance, often used in connection with expansion, and for which that principle has the credit, but which, in fact, increases proportionally the economy of the power nearly as much at one grade of expansion as another. Though many will deny this, the small percentage of the theoretical performance utilized in the best steam-engines, using expansion in the most approved manner, shows that there is a discrepancy somewhere. Still it is to the expansion of steam that we should look for further economy.

A steam-engine is simply a heat-engine. Water takes up the heat; steam is the result. If steam be expanded, work is done. A quantity of heat the mechanical equivalent of the work done disappears, and leaves its vehicle—the water—in its original condition. In other words, steam is condensed. Theoretically, we should be able to carry on the expansion till the heat force is all utilized and the steam all becomes water. Practically, the steam-pressure can never be economically reduced below that necessary to equilibrate the resistances incident to the engine itself. Up to this point, then, we should expect gain; still we do not in practice realize it.

Modern discoveries in science have pointed out no reason why this is so. They only show that the pressure must reduce faster than the ordinates of a hyperbolic curve, on account of condensation due to work done and varying temperature. Then, since the theory of expansion is so conclusive, and we are not able to obtain results in accordance therewith, should we not look carefully at the practical side of the question, and strive to find, in the materials used, or in their form, disposition, or movements, some fault to which may be attributed the losses? First, we must allow that a steam-engine cylinder appears to hold the steam, and that the operation of the valve-gear and piston and its connections undoubtedly permits its expansion, and transmits the force derived therefrom to the desired point. I say the cylinder appears to hold the steam. Now, what is steam? We may say water which, by the action of heat, has had work done upon its particles, separating and agitating them. Less literally, it is simply water and heat. Does the cylinder hold both? Put your hand on it and see. Ah! your flesh is not wet, but is burned—that is, the cylinder holds water, but not heat. It is, in fact, ready, at the first opportunity, to give away the very force we wish to utilize. Your hand robbed the cylinder of a little heat, the cylinder robbed the inclosed steam, leaving a little useless water. However, we expect loss from the outside, and guard against it by a covering of felt or similar material.

Let us inquire if any similar loss can take place on the inside. There is nothing but steam there, you say. Why was your hand burned? Simply because it was colder than the metal. Is the steam ever cooler than the metal? Certainly it is, at the very time it is going out of the cylinder, and can carry all the heat it receives away to waste.

The differences in temperature inside a steam-cylinder during a double stroke of the piston are very considerable. The steam enters usually at nearly the boiler pressure and temperature, and if it be expanded the temperature necessarily falls from the time the cut-off valve closes to the end of the stroke. When the exhaust-valve opens, the temperature again falls to that due to the back pressure. For example, if steam, admitted to the cylinder at a pressure of thirty-five pounds per square inch above the atmosphere, be cut off at a quarter of the stroke from the beginning, and at the end exhausted into a condenser against a back pressure of two pounds, its temperature during the first quarter of the stroke will, in round numbers, be 280° Fahrenheit, and gradually fall to 205° at the end of the stroke, when, after the exhaust, it will be suddenly reduced to 130°, remaining so during the whole return-stroke. The metal of the cylinder at first necessarily becomes nearly as hot as the boiler-steam; but when the communication with the boiler is shut off, the temperature of the confined steam at the end of the stroke becomes 75° lower than that of the metal, and the difference increases to 150° when the exhaust takes place, and remains so during the entire return-stroke.

Tyndall has shown that aqueous vapor is a very powerful absorbent of radiant heat. When, then, as we have shown, the temperature of the metal of a steam-cylinder is so much higher than that of the inclosed steam, a great deal of heat must be radiated from the metal, absorbed by the steam, and carried with it out of the cylinder. The heat radiated from the metal must be re-supplied from the incoming boiler-steam to adjust the temperatures.

When the boiler furnishes saturated steam, since that is its maximum density, as steam, at the point of saturation, no heat can be taken from it without producing condensation. Hence the heat required to reheat the cylinder and that transmuted into work both produce condensation. A portion of the steam yields up its latent heat and collapses from the volume of steam to that of water at the same temperature. So long as the steam-valve is open, new steam enters to keep up the pressure. Hence it often occurs, in practice, that twice as much steam is used as is required by calculation. There is nothing strange in this if the metal of the cylinder, at the beginning of each stroke, is cool enough to condense about half the steam that enters. With superheated steam the effect is similar, though no condensation need take place. The free superheat goes to supply that necessary to reheat the cylinder and perform the work. This decreases the volume, and while the steam valve is open more steam flows in to keep up the pressure. The density is thereby increased, and, like saturated steam, it requires a larger quantity of boiler-steam to maintain the initial pressure and temperature.

Let us now carefully follow the steam through the cylinder of an engine, explaining the changes which take place upon the principles above stated. The metal of the cylinder being somewhat cooled during a previous stroke, in the manner pointed out, sufficient steam enters not only to fill the space, but to reheat the metal and keep up the initial pressure. The steam is shut off, expansion takes place, and soon the temperature of the steam becomes lower than that of the cylinder. The metal, in turn, becomes the heater. It re-evaporates whatever water is in contact with it, and radiates its heat into the steam, and thus superheats it, or re-evaporates the suspended watery particles of condensation, as the case may be. All the heat taken from the metal during the steam-stroke is utilized, for it increases the volume or pressure of the steam, and thereby produces a dynamic effect on the piston. When the exhaust takes place, however, all the heat required to re-evaporate the water on the surfaces, all that can be radiated from the metal and absorbed by the steam, and that received by the steam by direct attrition against the heated sides of the cylinder and narrow ports during the time of the whole return-stroke, is carried away out of the cylinder, and, for the purpose of work, entirely wasted. All this heat has to be supplied at every stroke, to be again and again wasted and resupplied. The lost heat is furnished by an additional quantity of steam, which unfortunately enters and leaves the cylinder without showing its presence by an increase of pressure; wherefore calculations to determine the quantity of steam used are necessarily incorrect when founded, as is usual, upon the terminal pressures shown by indicator-diagrams.

In the famous Erie expansive experiments it was found that at the higher grades of expansion from forty to forty-five per cent. more steam entered the cylinder than was accounted for by calculation from the terminal pressure. About eight per cent. of this was condensed to furnish heat for the work; the rest was absolutely wasted. Bourne mentions that in one of the noted double-cylinder pumping-engines in England thirty-three per cent. of the steam was unaccounted for by the indicator. In small engines the discrepancy often amounts to two-thirds of the steam used.

It would seem to be impossible for an engine to work when so much of the steam is condensed in the cylinder. Now, Tyndall has shown that a good absorbent is necessarily also a good radiator. So, when steam enters a cooled cylinder, though some water may be condensed on the surfaces, like dew, the whole body of the steam is also chilled by radiation, and the condensed water left suspended in the steam in a fog or cloud. A small quantity of water may possibly be held invisibly suspended, like the aqueous vapor in the atmosphere; but larger amounts undoubtedly form a cloud, from which, when the abstraction of heat is carried sufficiently far, water probably drops like rain.

The quantity of steam necessary to reheat the cylinder should increase slightly with the grade of expansion, for the metal is exposed to a temperature lower than its own for a larger portion of each stroke. But since less steam enters the cylinder at the higher grades, not only the quantity required for reheating, but also that condensed for the work, must form a larger proportion of the whole quantity. In other words, the percentage of water condensed and unaccounted for by the indicator must increase with the grade of expansion, at least up to a point where the metal is heated and cooled all that is possible in the time of a double stroke.

In accordance with the principles above adopted to explain the losses in the steam-engine, we may account for the gains which are obtained by some forms of construction, and reconcile many statements which appear contradictory.

First, then, it is well known that large engines are more economical than small ones, even when using steam with the same degree of expansion. We can understand this from the fact that, as the cylinder is enlarged, the ratio of radiating-surface to capacity is proportionably diminished, and the metal also is farther removed from the main body of the steam.

Again, the Cornish engine is often referred to as a model of economy, and an example of the efficiency of extreme expansion. Others adopt the same pressure of steam and the same grade of expansion, but do not realize the same degree of economy. Why is this? Now, Cornish boilers are very economical; but that will not account for all the gain. On the principle above stated, we find, first, that Cornish engines are generally large, and for that reason economical. But more than this, they are single-acting. The hot boiler-steam never touches the coolest portions of the cylinder. It enters only at the top, and presses down the piston until the beam and heavy pump-rods are put in motion. The steam is then shut off, and the stroke completed by expansion. As the steam lowers in temperature, the surrounding metal is correspondingly cooled. The return-stroke is accomplished by the weight of the pump-rods, and the piston simply displaces the steam from one end of the cylinder to the other through the equilibrium-valve. The upper or hot end of the cylinder is never in connection with the condenser, and the cool end is at no time in direct communication with the boiler. Is it strange that greater economy can be obtained, using such an instrument as this, rather than a double-acting cylinder, each end of which must be heated and cooled at every stroke?

Again, the double-cylinder engine is considered very economical, and properly so, for the small cylinder is never in communication with the condenser. The heat radiated from it is utilized on the large piston, which stands like a screen to prevent its going entirely to waste. Viewing the matter in this light, it is probable that the economy of the present double-cylinder engine could be increased by making less difference in the size of the cylinders than is usual.

Great differences of opinion exist as to what is the most economical point of cut-off in the ordinary double-acting engine. There is an undoubted gain, within reasonable limits, when the higher grades of expansion have the benefit of higher pressure steam than the lower grades, which is the only practical way of testing the matter when doing the same work in the same engine and with the same piston speed. But moderate expansionists say that they are entitled to as high a pressure of steam as is used in any case, and that they can thereby employ a smaller engine with no greater loss than is compensated for by diminished first cost, and increased simplicity and reliability. The point about steam pressure is well taken; but they forget that when doing the same work with the same pressure of steam increased expansion not only necessitates a larger engine, but brings the economy due, as above explained to a large engine *per se*. It is believed, therefore, that the views of the non-expansionists are not upheld by facts. This is, however, a limit to the application of the principle of expansion in engines as at present constructed, the best, as has already been said, utilizing only one-tenth of the heat.

I have above stated my theory fully, and have given some examples of its practical application. Still, many may doubt that a cause apparently so small can produce such large and injurious effects. I would, however, call attention to the fact that Tyndall, when experimenting with the very things that we have been discussing—viz., the vapor of water and radiant heat—made discoveries still more incredible. He found that the invisible aqueous vapor present at all times in the atmosphere had seventy times the absorptive effect on radiant heat of dry air. Any person who will carefully read his remarks on this subject, in connection with a careful study of the changes of temperature that occur in a steam-cylinder, which in fact confines a kind of aqueous vapor within radiating surfaces, will, I think, agree with me as to the nature of the losses which are found in practice.

To prevent these losses, I long ago proposed to make the interior surfaces of a steam cylinder of a poor conductor or a poor radiator of heat; but I found that Tyndall had shown that a poor radiator—a polished surface, for instance—becomes a good one when a good radiating vapor like that of water is in contact with it. However, by making the interior surfaces of a poor conductor of heat, no more heat can be radiated or conducted away than is conducted to the surface. Therefore, so much cannot be lost, and less steam will be required to reheat the surface. By using a poor conductor, with low specific heat, the benefit should be still more marked. After carefully considering the subject, my convictions were so strengthened by the considerations hereinbefore expressed, and by mathematical calculations, that I tried the matter practically. After some preliminary, though encouraging, experiments with incomplete apparatus, I constructed two cylinders of like dimensions, one of glass, the other of iron, in such a manner that either could be attached to a valve which regularly admitted steam from a boiler to the cylinder, and permitted its exhaust into a condensing-coil lying in a tub of water. The capacity of the two cylinders was made exactly the same, as was shown by transferring water from one to the other. When put, in turn, in the condition of a steam-engine cylinder, the iron cylinder used twice as much steam as the glass one, shown by the fact that twice the quantity of water came through the condensing-coil for the same number of movements of the valve. Steam of the same pressure was used in both cylinders, and the experiments were many times repeated, with substantially the same results. This settled the question. The glass cylinder in that case saved half the steam. With more expansion I expect still greater gains. A large proportion of the theoretical saving promised by extreme expansion may perhaps be realized.

Whether my theories be right or wrong, I have at least demonstrated the practical value of non-conducting material for the interior surfaces of steam-cylinders.

Popularly speaking, in relation to heat, all materials are divided into two classes—" conductors " and " non-conductors." The metals constitute the first class, but differ among themselves in conducting-power. Thus, copper and compounds in which it enters freely, such as brass, conduct heat less freely than silver, but better than iron. Other metals conduct heat less freely than iron. For instance, lead and German silver have only from three-fourths to one-half its conducting-power, and bismuth has but about one-sixth. Substances such as glass, porcelain, fire-brick, marble, and stones are known as " non-conductors," and many of them have about one-thirtieth the conducting-power of iron. Cast-iron is almost universally employed in the construction of steam-cylinders, and it has lower conducting-power than brass or any other substance now practically used for the purpose.

The object of my invention is to produce economy in the steam-engine. I accomplish this by, and my invention consists in, lining or coating the interior surface of the cylinders of steam-engines with glass, porcelain, enamel, or equivalent material. I also propose to construct the whole cylinder of a poor conductor, and have made changes in the form of steam-engine cylinders and pistons, whereby certain portions of the surfaces that cannot always be conveniently lined may be protected from direct radiation.

For the purposes of my invention one material, used as above expressed, is the equivalent of another when it conducts heat less freely than cast-iron; and the application of the words "poor conductor of heat" and similar expressions, as herein used, is limited to such materials as conduct heat less freely than cast-iron."

Mr. Charles E. Emery.—The papers which we are considering were written for the express purpose of producing discussion, and it is exceedingly gratifying that the object has been so well attained.

We will first respond to the remarks of Mr. Porter, who has expressed himself most forcibly on the subject. He evidently still entertains views which we supposed had been abandoned by every prominent engineer in the country. The final suggestion in his main proposition, to the effect that additional work obtained by the additional cylinders accounts several times over for the gain in economy of the compound engine, is met simply by the fact that such engines are compared with other engines on the basis of equal work and still show economy. In reply to his protest against what is called "the absurd practice of compounding non-condensing engines," it is sufficient to say that locomotives have been successfully compounded by two independent methods in several foreign countries and have shown marked economy in fuel when using the same boilers and hauling similar trains over the same road.

I am sorry to say that both Mr. Porter and Mr. Babcock assume that we claim that the *whole* internal condensation is in the ratio of the squares of the differences in temperature, and have failed to quote our exact words on the subject, which are simply "that the quantity of heat which will be transferred from a radiator to an absorbent would vary as the square of the difference in temperature." The distinction is important, though it was not considered necessary in the original paper to go into the detail necessary to show it. There is no question but that the quantity of heat transferred by direct conduction varies directly as the differ-

[illegible]

for different portions of the length of the tubes or other heating surface, first develops a complex equation, practically that of a hyperbola with other factors involved, which is undoubtedly the true form applicable to that particular case. But on the ground that the equation is a complex one, he states that practice shows that the transfer of heat is proportioned to the square of the difference in temperature, and he then gives what is termed an approximate formula based on this particular deduction. In the case of radiation the work is not considered approximate but accurate, for the reason that there is a direct projected force acting to overcome resistance the same as that of an actual projectile overcoming the resistance of gravity. Mr. Porter has attempted to assume a set of conditions by which all the steam would be condensed upon raising the temperature, and it is not at all impossible that such would be the case under certain conditions, but in other cases the result would be much of the same character as the law of depreciation; for instance, that of the value of tools in a manufactory. If a depreciation of 10 per cent. be allowed per year, at first sight it would seem to be as if the plant would be worth nothing on the books in ten years; but when it is considered that the depreciation is 10 per cent. of the remainder in each case, instead of the original amount, it becomes evident that there will always be a remainder.

Attention has been called by Mr. Babcock to the development of cylinder condensation by Mr. D. K. Clark, as set forth in his work on Railway Machinery. In preparing the paper we had in mind only Mr. Isherwood's claim, and though ours may be correctly based upon his, we are reminded by the suggestion of the gentleman named of our early interest in Mr. Clark's unrivaled series of investigations, and feel that both claims must be modified, ours possibly only to the limited extent so kindly stated by Mr. Babcock. The experiments made by the speaker with glass and iron cylinders above recited, doubtless demonstrate, incontrovertibly, the cause of internal cylinder condensation, and it is suggested that this simple apparatus, using cylinders say one inch in diameter and 10 inches long, should form part of the philosophical apparatus of every technical school, as the results obtained are as startling as many of those customarily employed to produce strong impressions on the minds of students.

We are obliged to Mr. Barrus for introducing the facts stated in relation to the Dixwell expansion experiments. The experiments show admirably on a large scale the effect of expandin steam with and without the performance of useful work, althoug the results have apparently nothing to do with the question unde discussion. Actual loss of heat should have taken place even whe the engine was not running equivalent to the mechanical work ne cessary to displace the atmosphere, as well as that required fo external refrigeration.

In experiment 2 the temperature corresponding to (54+15= 69 lbs. is 302°. The temperature near the throttle-valve was 449 so there was 147° superheating, corresponding to about 73½ the mal units. The total heat above zero of saturated steam, at th pressure above stated, is 1174 thermal units, so (1174+73½=) 1247 thermal units entered the engine, and as 1223 thermal units we discharged, only (1247½−1223=) 24½ thermal units were lost i passing through the cylinder. This is insufficient for the mechan ical work required to displace the atmosphere, so although man interesting questions are raised by the presentation, the results no only fail to support the "expansion *per se*" theory, but, so far a they go, actually disprove it.

The discussion of the paper on the compound engine has neces sarily been combined to a large extent with that on cylinder con densation. The incidental remarks about the use of steel in th former paper appear also to have excited some attention. It wil be observed that there is no serious difference in the views of th various speakers on this subject. Mr. Morgan naturally takes th side of the steel makers, as he should, and his remarks that "i good practice, with the same attention to details and selection c stock and methods of manufacture, greater uniformity and relia bility of product can be looked for in steel than in iron plates," i only one way of putting part of the remark of the writer that stee "is so much more homogeneous and in every way desirable, whe the material is right, that there is no danger of every one goin back to iron." The qualification, "when it is right," is very im portant. If all boiler steel were as good as that manufacture under the direction of one gentleman here present, who has no favored us with remarks, there never would be any question but the number of steel makers is constantly increasing, and, un fortunately, each one has to go through exactly the same experi ence as the early manufacturers of steel.

Steel, once produced, must be disposed of, on account of th necessity of making profits in competition with others, and so stee of an improper kind for use in boilers will get on the market. Mr. Morgan's remarks in regard to putting the work upon thick plates to bring up the tensile strength, instead of increasing the carbon, are very interesting. The principle has been long known, but we must take it for granted that Mr. Morgan means that steel makers are prepared to make steel in this way. Let us, however, examine what this means. If two long plates be compared, of a width about as great as can be rolled, and one four times as thick as the other, the ingot for the thick plate would have to be four times as heavy to make the quadruple thickness, and four times as heavy again as is required for this, or sixteen times as heavy as that for the thinner plate, in order that in the process of manufacture it would receive the same work as would be obtained in rolling the thin plate. Not only this, but the thick plate, when done, would be four times as long as required. In other words, it would be necessary to make four thick plates in order to get one. Now, all this is possible, but it is left to the engineering experience of those present to judge how often mills in practical work would go to this trouble and expense instead of slightly increasing the carbon to get the necessary tensile strength. That the carbon is increased at times is shown by the failure of some of the boilers with extra-thick plates constructed abroad. These remarks are not made in an unfriendly spirit. The whole discussion shows the absolute necessity of careful inspection in the use of steel, and as these are notes for discussion, it seemed a fitting opportunity to call this subject to the attention of members, so that the young might be forewarned and the older ones reminded that the problems which antagonize the introduction of steel are still to be met, unless in an unbusinesslike way dealings are had entirely with such parties who have such a reputation that they cannot afford to send out anything not adapted for the particular purpose of the order.

During the discussion I have had thrust into my hand a note from an eminent engineer here present, which forms an amusing commentary on the situation. It reads: "You are about right on the steel question, but very unorthodox; don't mention my name, as I would about as soon attack the orthodoxy of the church as the use of steel at this day."

CCL.

THE COMPARATIVE VALUE OF STEAM AND H WATER FOR TRANSMITTING HEAT AND POWER.

BY CHAS. E. EMERY, NEW YORK CITY.
(Member of the Society.)

THE relative values of steam, water, or other vehicle for the p pose of distributing heat to be used for heating and power purpos curiously involves, in a large degree, the same elements as the tra portation of passengers and freight on railroads. With the latt the relative amount of paying and non-paying load forms one of t most important considerations, while with the former the relati values depend largely upon the percentages of their heat-carryi capacities which can be utilized in practice. In generating ste with fuel, the gases may be reduced in temperature nearly to th of the steam itself, securing fair efficiency, but in melting meta they must be rejected at a temperature higher than that of t metal, and economy secured by secondary operations to save wa heat. Similarly the efficiency of a heat-transmitting medium pends upon the amount of heat rejected or unavailable by the c ditions of the problem compared with that originally imparted such medium

If steam and hot water of 400 degrees temperature be respe ively used for some heating purpose such as cooking, requiri nearly that temperature, the steam will give up its late heat and be converted into a small quantity of water at the fir temperature, while hot water can only give up its sensible he represented practically by the difference between its original a final temperature. If the fall of temperature be from 400 to 3 degrees, the water would impart substantially one thermal unit each pound of water circulated over the surface, while the ste would impart over 800 thermal units for each pound of water c densed. If a difference of 2 degrees were allowed, the water wou impart substantially two thermal units for each pound of wa circulated, whereas the heat supplied by the condensation of o pound of steam with same limits of temperature would be but slightly changed, though the relative quantity of water required to be circulated to equal the results obtained with one pound of steam would be reduced one half. By allowing a still greater reduction of temperature the water would appear at less disadvantage. For instance, with a difference of temperature of about 11.15 degrees the water would impart 11½ thermal units for each pound of water circulated and the steam 842 thermal units for each pound condensed.* This is doubtless a greater reduction of temperature than could be allowed for cooking, and yet it would require (842.04 ÷ 11.52 =) 72.71 times as much water circulated to do the same work as would be required if steam were used. In this case, then, 72.71 pounds of water would necessarily be heated at the Station, pumped to the point where the heat was required, and then be forced back again to the Station at a lower pressure and pumped into the boiler to be reheated, for each pound of water evaporated if steam were used as the medium of transmission. The steam would be transmitted by causing a slight difference of pressure from the heating station to the point where it was used, and its surplus pressure would return the water of condensation back to the Station, where one pound would require to be pumped in the boiler for each 72.71 pounds by the water system.

As the temperature at which the heat is to be applied is reduced, the preponderance against the water system somewhat diminishes. For instance, if steam at 70 pounds pressure be required to operate engines, it may be obtained by directly expanding down the steam of 235 pounds pressure, which would result in a beneficial superheating of 25.87 thermal units per pound of steam thus expanded. If, however, the steam were supplied from hot water at 400.89 degrees temperature, corresponding to the pressure of 235 pounds, only 10.2 parts in 100 would, on reducing the pressure to 70 pounds, flash into steam at that pressure, so in that case 10.2 pounds of water would necessarily be heated at the Central Station, transmitted to the point where steam is required, and if high-

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* (A) Temperature due to 235 lbs. gauge or 250 lbs. absolute pressure, 400.89; temperature due to 205 lbs. gauge or 220 lbs. absolute, 389.74°—difference, 11.15°. Total heat above 32° in the two cases respectively, 373.75 and 362.17—difference, 11.5 thermal or heat units. Total heat of steam of 235 lbs. gauge pressure, 1204.21 heat units. Subtract 362.17 heat units due to final temperature, gives 842.04 heat units, available from condensation of steam between limits of temperature stated

pressure engines were used, 9.1 pounds would necessarily be transmitted back again, and finally 10.1 pounds pumped in the boiler for each pound weight of steam used, instead of the one pound which would be required to be evaporated at the Central Station in the case of the steam plant.*

For heating purposes the temperature could, under favorable circumstances, be reduced to 228 degrees in the coils, corresponding to a pressure of five pounds, in which case, without repeating the operations above described, there would require to be circulated from the heating Station to the point of supply and back to such Station, 5.69 pounds of water for each pound of steam utilized at the point of supply, or for the heat which would be imparted at the temperature corresponding to such pressure, for each pound of steam which in a steam system would be evaporated and sent direct from the Station.†

The above statements may be easily verified from the figures given in the foot-notes, and the great resistances found in pumping water through pipes at high velocities being well known, there would seem to be no reason why any one should think of using water rather than steam for the purposes above referred to. The subject has, however, been agitated for a number of years. Little plants to show what could be done with water heated to a high temperature have been built from time to time, but apparently did not command the capital necessary to start the business on a large scale. Another revival has recently been attempted, however, based chiefly on the favorable report of an unusually well-informed engineer of experience and acknowledged ability, to whom it is a pleasure to say the writer is personally indebted for many valu-

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* (B) Total heat steam of 70 lbs. gauge or 85 absolute pressure, 1178.34 heat units, which, subtracted from 1204.21 heat units due to 285 lbs., foot-note (A), shows 25.87 heat units for superheating. Temperature due to 70 lbs. gauge 316.08 corresponding to 286.26 heat units above 32°, which, subtracted from 878.75 heat units due to 235 lbs. (A), leaves 87.49 heat units available for making steam with water, and subtracting same from 1178.34 heat units, total heat due to 70 lbs., gives 892.08 heat units required for steam of 70 lbs. Hence there will be required 892.08 + 87.49 = 10.2 pounds of water circulated per pound of water evaporated into steam of 70 lbs. pressure.

† (C) Temperature due to 5 lbs., 227.96°, equivalent to 196.66 heat units above 32, which latter, subtracted from 878.75 heat units in water due to 285 lbs. pressure, gives 177.09 heat units per pound of water, and subtracted from 1204.21 total heat due to 285 lbs. pressure, gives 1007.55 heat units available from steam between same limits, so that there will be required (1204.21 + 177.09 =) 5.69 times as much water circulated as steam.

able suggestions as to proper courses of study at an earlier period of life. Mr. Isherwood, in forming his opinions, has evidently, however, failed to consider some of the most important elements of the problem, and occasion is thereby made for an abstract discussion on the merits of steam and hot water, so far as possible, without reference to the merits of a particular system and the details of the same.

It has been stated in the public press, quoting from the report, that a cubic foot of water at 400 degrees temperature contains $34\frac{2}{3}$ times as much heat as is contained in a cubic foot of steam at the same temperature, and it is therefore concluded that "the areas of the pipes will be in this proportion, making their diameters in the proportion of 1 for the water and ($\sqrt{34\frac{2}{3}} =$) 5.89 for the steam." Also that "the thickness of the material of the pipes for equal strength would have to be about six times greater for the larger steam-pipe than for the smaller water-pipe, even if both were lap-welded." On the supposition that larger steam-pipes would be necessary, comparisons were presented of the "greater bulk," "enormously greater cost," "extra loss of heat by conduction and radiation" due to the larger pipes, with some further remarks about the difficulty of getting rid of the water of condensation in steam-pipes, difficulties of management, etc., not at all warranted by the state of the art in relation to steam plants. Evidently the error behind these statements is to be found in the assumption that, because a given quantity of water of the temperature assumed, contains $34\frac{2}{3}$ times as much heat as that of an equal *volume* of steam, therefore the steam-pipe must be proportionably larger to that extent. It ignores entirely well-known laws of hydraulics, which teach that a fluid of much less density than another, will, with the same difference of pressure, flow at a much higher velocity. The weight of a fluid transmitted through pipes with comparatively small differences of pressure at opposite ends is proportioned to the square root of the 5th power of the diameter of the pipe, into the square root of the pressure gradient (represented by the difference of pressure between the two points divided by the length) into the square root of the weight per unit of volume of the fluid, for instance, the weight per cubic foot, called by Weisbach the "heaviness," and herein designated the "specific weight." Therefore, for the same loss of pressure in the same distance and the same size of pipe, the relative weights of water transmitted would vary as the square roots of the specific weights. The more marked. After carefully considering the subject, my convictions were so strengthened by the considerations hereinbefore expressed, and by mathematical calculations, that I tried the matter practically. After some preliminary, though encouraging, experiments with incomplete apparatus, I constructed two cylinders of like dimensions, one of glass, the other of iron, in such a manner that either could be attached to a valve which regularly admitted steam from a boiler to the cylinder, and permitted its exhaust into a condensing-coil lying in a tub of water. The capacity of the two cylinders was made exactly the same, as was shown by transferring water from one to the other. When put, in turn, in the condition of a steam-engine cylinder, the iron cylinder used twice as much steam as the glass one, shown by the fact that twice the quantity of water came through the condensing-coil for the same number of movements of the valve. Steam of the same pressure was used in both cylinders, and the experiments were many times repeated, with substantially the same results. This settled the question. The glass cylinder in that case saved half the steam. With more expansion I expect still greater gains. A large proportion of the theoretical saving promised by extreme expansion may perhaps be realized.

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Popularly speaking, in relation to heat, all materials are divided into two classes—"conductors" and "non-conductors." The metals constitute the first class, but differ among themselves in conducting-power. Thus, copper and compounds in which it enters freely, such as brass, conduct heat less freely than silver, but better than iron. Other metals conduct heat less freely than iron. For instance, lead and German silver have only from three-fourths to one-half its conducting-power, and bismuth has but about one-sixth. Substances such as glass, porcelain, fire-brick, marble, and stones are known as "non-conductors," and many of them have about one-thirtieth the conducting-power of iron. Cast-iron is almost universally employed in the construction of steam-cylinders, and it has lower conducting-power than brass or any other substance now practically used for the purpose.

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Mr. Charles E. Emery.—The papers which we are considering were written for the express purpose of producing discussion, and it is exceedingly gratifying that the object has been so well attained.

We will first respond to the remarks of Mr. Porter, who has expressed himself most forcibly on the subject. He evidently still entertains views which we supposed had been abandoned by every prominent engineer in the country. The final suggestion in his main proposition, to the effect that additional work obtained by the additional cylinders accounts several times over for the gain in economy of the compound engine, is met simply by the fact that such engines are compared with other engines on the basis of equal work and still show economy. In reply to his protest against what is called "the absurd practice of compounding non-condensing engines," it is sufficient to say that locomotives have been successfully compounded by two independent methods in several foreign countries and have shown marked economy in fuel when using the same boilers and hauling similar trains over the same road.

I am sorry to say that both Mr. Porter and Mr. Babcock assume that we claim that the *whole* internal condensation is in the ratio of the squares of the differences in temperature, and have failed to quote our exact words on the subject, which are simply "that the quantity of heat which will be transferred from a radiator to an absorbent would vary as the square of the difference in temperature." The distinction is important, though it was not considered necessary in the original paper to go into the detail necessary to show it. There is no question but that the quantity of heat transferred by direct conduction varies directly as the difference of temperature, and the same is true of heat transferred by direct circulation or convection. In transmitting heat through a plate, the fall of temperature necessary to produce the transfer through each successive layer is the same, or, as it may be expressed mathematically, the differential of the temperature divided by the differential of the thickness is constant, so the resistance would vary as the thickness. If a fluid were circulated against the cooler side, each particle would take away the same quantity of heat as the next, and the total quantity received by each particle would be proportioned to the difference of temperature modified by a co-efficient of resistance based on the kind of contact which the particular fluid had with the particular material of the plate. In a steam-cylinder all the moisture deposited on the surfaces is undoubtedly re-evaporated by direct contact with the steam, and the quantity of heat transmitted is therefore proportioned or nearly proportioned to the difference of temperature. As, however, the whole body of steam is chilled and is in the condition of a cloud, the greater portion of the transfer of heat takes place by radiation from the hotter metal surfaces and absorption by the suspended watery particles. It is this action which it is claimed is proportioned to the square of the difference in temperature. With falling bodies we know that the rate of acceleration is constant and proportioned to the acceleratrix of gravity (g). Hence the velocity is proportioned to this acceleratrix multiplied by the time, but the space passed over, as well as the energy absorbed, and which can be afterwards utilized, is proportioned to the square of the velocity. Hence the application of the short explanation embodied in parenthesis in the original paper after the quotation above given, viz.: (" the conditions being practically the same as those of force performing mechanical work in overcoming resistance "). Now, in throwing up a projectile it is continually retarded by the force of gravity, which gradually reduces the stored-up energy until the velocity becomes naught. So, in projecting heat from a radiator to an absorbent through vapor, the energy transmitted from the heated surface is continually dissipated by evaporating water, in the form of mist, met with on the way, until finally all the heat energy is absorbed. The parallel of the two cases is complete.

The proposition is, moreover, supported by authority. Rankine, in developing a formula to represent the quantity of heat transferred to the water from the heated gases passing through a boiler, for different portions of the length of the tubes or other heating surface, first develops a complex equation, practically that of a hyperbola with other factors involved, which is undoubtedly the true form applicable to that particular case. But on the ground that the equation is a complex one, he states that practice shows that the transfer of heat is proportioned to the square of the difference in temperature, and he then gives what is termed an approximate formula based on this particular deduction. In the case of radiation the work is not considered approximate but accurate, for the reason that there is a direct projected force acting to overcome resistance the same as that of an actual projectile overcoming the resistance of gravity. Mr. Porter has attempted to assume a set of conditions by which all the steam would be condensed upon raising the temperature, and it is not at all impossible that such would be the case under certain conditions, but in other cases the result would be much of the same character as the law of depreciation; for instance, that of the value of tools in a manufactory. If a depreciation of 10 per cent. be allowed per year, at first sight it would seem to be as if the plant would be worth nothing on the books in ten years; but when it is considered that the depreciation is 10 per cent. of the remainder in each case, instead of the original amount, it becomes evident that there will always be a remainder.

Attention has been called by Mr. Babcock to the development of cylinder condensation by Mr. D. K. Clark, as set forth in his work on Railway Machinery. In preparing the paper we had in mind only Mr. Isherwood's claim, and though ours may be correctly based upon his, we are reminded by the suggestion of the gentleman named of our early interest in Mr. Clark's unrivaled series of investigations, and feel that both claims must be modified, ours possibly only to the limited extent so kindly stated by Mr. Babcock. The experiments made by the speaker with glass and iron cylinders above recited, doubtless demonstrate, incontrovertibly, the cause of internal cylinder condensation, and it is suggested that this simple apparatus, using cylinders say one inch in diameter and 10 inches long, should form part of the philosophical apparatus of every technical school, as the results obtained are as startling as many of those customarily employed to produce strong impressions on the minds of students.

We are obliged to Mr. Barrus for introducing the facts stated in relation to the Dixwell expansion experiments. The experi-

ments show admirably on a large scale the effect of expandi
steam with and without the performance of useful work, althou
the results have apparently nothing to do with the question und
discussion. Actual loss of heat should have taken place even wh
the engine was not running equivalent to the mechanical work n
cessary to displace the atmosphere, as well as that required f
external refrigeration.

In experiment 2 the temperature corresponding to (54+15=
69 lbs. is 302°. The temperature near the throttle-valve was 449
so there was 147° superheating, corresponding to about 73½ the
mal units. The total heat above zero of saturated steam, at t
pressure above stated, is 1174 thermal units, so (1174+73½=) 124
thermal units entered the engine, and as 1223 thermal units we
discharged, only (1247½−1223=) 24½ thermal units were lost
passing through the cylinder. This is insufficient for the mecha
ical work required to displace the atmosphere, so although ma
interesting questions are raised by the presentation, the results n
only fail to support the "expansion *per se*" theory, but, so far
they go, actually disprove it.

The discussion of the paper on the compound engine has nece
sarily been combined to a large extent with that on cylinder co
densation. The incidental remarks about the use of steel in t
former paper appear also to have excited some attention. It w
be observed that there is no serious difference in the views of t
various speakers on this subject. Mr. Morgan naturally takes t
side of the steel makers, as he should, and his remarks that "
good practice, with the same attention to details and selection
stock and methods of manufacture, greater uniformity and reli
bility of product can be looked for in steel than in iron plates,"
only one way of putting part of the remark of the writer that ste
"is so much more homogeneous and in every way desirable, wh
the material is right, that there is no danger of every one goi
back to iron." The qualification, "when it is right," is very in
portant. If all boiler steel were as good as that manufacture
under the direction of one gentleman here present, who has n
favored us with remarks, there never would be any questio
but the number of steel makers is constantly increasing, and, u
fortunately, each one has to go through exactly the same exper
ence as the early manufacturers of steel.

Steel, once produced, must be disposed of, on account of th
necessity of making profits in competition with others, and so ste

of an improper kind for use in boilers will get on the market. Mr. Morgan's remarks in regard to putting the work upon thick plates to bring up the tensile strength, instead of increasing the carbon, are very interesting. The principle has been long known, but we must take it for granted that Mr. Morgan means that steel makers are prepared to make steel in this way. Let us, however, examine what this means. If two long plates be compared, of a width about as great as can be rolled, and one four times as thick as the other, the ingot for the thick plate would have to be four times as heavy to make the quadruple thickness, and four times as heavy again as is required for this, or sixteen times as heavy as that for the thinner plate, in order that in the process of manufacture it would receive the same work as would be obtained in rolling the thin plate. Not only this, but the thick plate, when done, would be four times as long as required. In other words, it would be necessary to make four thick plates in order to get one. Now, all this is possible, but it is left to the engineering experience of those present to judge how often mills in practical work would go to this trouble and expense instead of slightly increasing the carbon to get the necessary tensile strength. That the carbon is increased at times is shown by the failure of some of the boilers with extra-thick plates constructed abroad. These remarks are not made in an unfriendly spirit. The whole discussion shows the absolute necessity of careful inspection in the use of steel, and as these are notes for discussion, it seemed a fitting opportunity to call this subject to the attention of members, so that the young might be forewarned and the older ones reminded that the problems which antagonize the introduction of steel are still to be met, unless in an unbusinesslike way dealings are had entirely with such parties who have such a reputation that they cannot afford to send out anything not adapted for the particular purpose of the order.

During the discussion I have had thrust into my hand a note from an eminent engineer here present, which forms an amusing commentary on the situation. It reads: "You are about right on the steel question, but very unorthodox; don't mention my name, as I would about as soon attack the orthodoxy of the church as the use of steel at this day."

CCL.

THE COMPARATIVE VALUE OF STEAM AND HOT WATER FOR TRANSMITTING HEAT AND POWER.

BY CHAS. E. EMERY, NEW YORK CITY.

(Member of the Society.)

The relative values of steam, water, or other vehicle for the purpose of distributing heat to be used for heating and power purposes, curiously involves, in a large degree, the same elements as the transportation of passengers and freight on railroads. With the latter, the relative amount of paying and non-paying load forms one of the most important considerations, while with the former the relative values depend largely upon the percentages of their heat-carrying capacities which can be utilized in practice. In generating steam with fuel, the gases may be reduced in temperature nearly to that of the steam itself, securing fair efficiency, but in melting metals, they must be rejected at a temperature higher than that of the metal, and economy secured by secondary operations to save waste heat. Similarly the efficiency of a heat-transmitting medium depends upon the amount of heat rejected or unavailable by the conditions of the problem compared with that originally imparted to such medium

If steam and hot water of 400 degrees temperature be respectively used for some heating purpose such as cooking, requiring nearly that temperature, the steam will give up its latent heat and be converted into a small quantity of water at the final temperature, while hot water can only give up its sensible heat, represented practically by the difference between its original and final temperature. If the fall of temperature be from 400 to 399 degrees, the water would impart substantially one thermal unit for each pound of water circulated over the surface, while the steam would impart over 800 thermal units for each pound of water condensed. If a difference of 2 degrees were allowed, the water would impart substantially two thermal units for each pound of water circulated, whereas the heat supplied by the condensation of one

nd of steam with same limits of temperature would be but
itly changed, though the relative quantity of water required to
irculated to equal the results obtained with one pound of steam
ld be reduced one half. By allowing a still greater reduction
emperature the water would appear at less disadvantage. For
ance, with a difference of temperature of about 11.15 degrees
water would impart 11½ thermal units for each pound of water
ulated and the steam 842 thermal units for each pound con-
sed.* This is doubtless a greater reduction of temperature
could be allowed for cooking, and yet it would require
.04 ÷ 11.52 =) 72.71 times as much water circulated to do the
e work as would be required if steam were used. In this case,
, 72.71 pounds of water would necessarily be heated at the Sta-
, pumped to the point where the heat was required, and then be
ed back again to the Station at a lower pressure and pumped
the boiler to be reheated, for each pound of water evaporated
team were used as the medium of transmission. The steam
ld be transmitted by causing a slight difference of pressure
the heating station to the point where it was used, and its
lus pressure would return the water of condensation back to
Station, where one pound would require to be pumped in the
er for each 72.71 pounds by the water system.

s the temperature at which the heat is to be applied is reduced,
preponderance against the water system somewhat diminishes.
instance, if steam at 70 pounds pressure be required to operate
ines, it may be obtained by directly expanding down the steam
35 pounds pressure, which would result in a beneficial super-
ting of 25.87 thermal units per pound of steam thus expanded.
however, the steam were supplied from hot water at 400.89
rees temperature, corresponding to the pressure of 235 pounds,
y 10.2 parts in 100 would, on reducing the pressure to 70
nds, flash into steam at that pressure, so in that case 10.2
nds of water would necessarily be heated at the Central Sta-
, transmitted to the point where steam is required, and if high-

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(A) Temperature due to 235 lbs. gauge or 250 lbs. absolute pressure, 400.89 ;
perature due to 205 lbs. gauge or 220 lbs. absolute, 389.74°—difference, 11.15°.
l heat above 32° in the two cases respectively, 373.75 and 362.17—differ-
, 11.5 thermal or heat units. Total heat of steam of 235 lbs. gauge pressure,
.21 heat units. Subtract 362.17 heat units due to final temperature, gives
.04 heat units, available from condensation of steam between limits of tem-
ature stated.

pressure engines were used, 9.1 pounds would necessarily be transmitted back again, and finally 10.1 pounds pumped in the boiler for each pound weight of steam used, instead of the one pound which would be required to be evaporated at the Central Station in the case of the steam plant.*

For heating purposes the temperature could, under favorable circumstances, be reduced to 228 degrees in the coils, corresponding to a pressure of five pounds, in which case, without repeating the operations above described, there would require to be circulated from the heating Station to the point of supply and back to such Station, 5.69 pounds of water for each pound of steam utilized at the point of supply, or for the heat which would be imparted at the temperature corresponding to such pressure, for each pound of steam which in a steam system would be evaporated and sent direct from the Station.†

The above statements may be easily verified from the figures given in the foot-notes, and the great resistances found in pumping water through pipes at high velocities being well known, there would seem to be no reason why any one should think of using water rather than steam for the purposes above referred to. The subject has, however, been agitated for a number of years. Little plants to show what could be done with water heated to a high temperature have been built from time to time, but apparently did not command the capital necessary to start the business on a large scale. Another revival has recently been attempted, however, based chiefly on the favorable report of an unusually well-informed engineer of experience and acknowledged ability, to whom it is a pleasure to say the writer is personally indebted for many valu-

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* (B) Total heat steam of 70 lbs. gauge or 85 absolute pressure, 1178.34 heat units, which, subtracted from 1204.21 heat units due to 285 lbs., foot-note (A), shows 25.87 heat units for superheating. Temperature due to 70 lbs. gauge 816.08 corresponding to 286.26 heat units above 32°, which, subtracted from 878.75 heat units due to 235 lbs. (A), leaves 87.49 heat units available for making steam with water, and subtracting same from 1178.34 heat units, total heat due to 70 lbs., gives 892.08 heat units required for steam of 70 lbs. Hence there will be required $892.08 + 87.49 = 10.2$ pounds of water circulated per pound of water evavorated into steam of 70 lbs. pressure.

† (C) Temperature due to 5 lbs., 227.96°, equivalent to 196.66 heat units above 32, which latter, subtracted from 878.75 heat units in water due to 285 lbs. pressure, gives 177.09 heat units per pound of water, and subtracted from 1204.21 total heat due to 285 lbs. pressure, gives 1007.55 heat units available from steam between same limits, so that there will be required $(1204.21 + 177.09 =)$ 5.69 times as much water circulated as steam.

able suggestions as to proper courses of study at an earlier period of life. Mr. Isherwood, in forming his opinions, has evidently, however, failed to consider some of the most important elements of the problem, and occasion is thereby made for an abstract discussion on the merits of steam and hot water, so far as possible, without reference to the merits of a particular system and the details of the same.

It has been stated in the public press, quoting from the report, that a cubic foot of water at 400 degrees temperature contains $34\frac{2}{3}$ times as much heat as is contained in a cubic foot of steam at the same temperature, and it is therefore concluded that "the areas of the pipes will be in this proportion, making their diameters in the proportion of 1 for the water and $(\sqrt{34\frac{2}{3}} =)$ 5.89 for the steam." Also that "the thickness of the material of the pipes for equal strength would have to be about six times greater for the larger steam-pipe than for the smaller water-pipe, even if both were lap-welded." On the supposition that larger steam-pipes would be necessary, comparisons were presented of the "greater bulk," "enormously greater cost," "extra loss of heat by conduction and radiation" due to the larger pipes, with some further remarks about the difficulty of getting rid of the water of condensation in steam-pipes, difficulties of management, etc., not at all warranted by the state of the art in relation to steam plants. Evidently the error behind these statements is to be found in the assumption that, because a given quantity of water of the temperature assumed, contains $34\frac{2}{3}$ times as much heat as that of an equal *volume* of steam, therefore the steam-pipe must be proportionably larger to that extent. It ignores entirely well-known laws of hydraulics, which teach that a fluid of much less density than another, will, with the same difference of pressure, flow at a much higher velocity. The weight of a fluid transmitted through pipes with comparatively small differences of pressure at opposite ends is proportioned to the square root of the 5th power of the diameter of the pipe, into the square root of the pressure gradient (represented by the difference of pressure between the two points divided by the length) into the square root of the weight per unit of volume of the fluid, for instance, the weight per cubic foot, called by Weisbach the "heaviness," and herein designated the "specific weight." Therefore, for the same loss of pressure in the same distance and the same size of pipe, the relative weights of water transmitted would vary as the square roots of the specific weights. The weight of a cubic foot of water at 400 degrees is approximately 53 pounds, and a cubic foot of steam at the pressure of 235 pounds due to such temperature is 0.5478 pounds The relative weights of the steam and water are therefore as 1 to 96.36. The weights transmitted under like conditions as above referred to, would therefore be as the square roots of these numbers, or as 1 to 9.816. Therefore, if the steam and water be compared on the basis of use for heating buildings exclusively, which, as has been shown, is most advantageous to the water system, there would, as has been stated, be required a circulation of 5.694 times as many pounds of hot water as of steam, but 9.816 pounds of water would, under like conditions, be circulated to 1 of steam. The relative capacities of the pipes required to convey the steam and water under like conditions would then be for the steam 1, and for the water, the increased weight required, viz., 5.694 divided by the increased weight conveyed, viz., 9.816, or as 1 to 0.5796, or as 1.7253 to 1. But the carrying capacities of the pipes are not as the areas or the squares of the diameters, but, on account of the friction element, as the square root of the fifth power of the diameters, on which basis, under this most favorable condition for the water-pipe, the diameter of the steam-pipe would require to be but 24.38 per cent. in excess of that of the water-pipe. This does not, however, represent the relative cost of the system. For heat taken the same distance, the return-pipe of the water system must be as large as the direct pipe, whereas that of the steam system, which has to do but about one-sixth of the work, could, on merely theoretical conditions, have a carrying capacity that much smaller. For practical reasons, which, as will be shown hereafter, will have greater force with the water system, this pipe is made somewhat larger, or on the average about one-half of the diameter of the steam-pipe. On the basis that the costs are proportioned to the lengths and diameters, which is not far from correct when the two pipes are laid together in the same trench, the cost of the steam-pipe of 1.2438 diameter should be increased one-half to allow for the return-pipe, making, in the case of the steam system, 1.8657 compared with 2 as the cost of the full-size double pipes of the water system, which numbers are as 1 to 1.072. That is, even under the most favorable conditions for the water-pipes, they would cost at least 7 per cent. more than the steam-pipe system, and even this result is obtained by favoring the water system in the calculations, for the reason that the water has to be pumped double the distance that the steam is conveyed, and therefore requires double the difference of the pressure. However, as this pressure is produced with a pump, for simplicity the comparison has been allowed to stand as above.

If the water-pipes were designed to furnish power at a distance by generating steam to be used at 70 pounds pressure, it would be necessary, as stated, to circulate 10.2 times as much water as would require to be evaporated for steam used directly, when, on the same basis previously discussed, the water-pipes would require to have 3.9 per cent. greater carrying capacity under like conditions than the steam-pipe, that is, would require to be of 1.55 per cent. greater diameter, when the cost of both the direct and return water-pipes would be 35.4 per cent. greater than that of the steam-pipe and its smaller return-pipe.

If, however, the water-plant were designed to furnish water for cooking purposes, and the temperature were maintained in the stoves at 400 degrees by circulating, as claimed, water of only 400 degrees, there would be required the circulation of an infinite quantity of water to fulfill this condition. If, however, the temperature in the stoves were allowed to fall one degree below that of the water, there would require to be circulated, as first stated, something over eight hundred times as much water as would be required to be evaporated and conveyed if the work were done by steam. Without stopping to calculate the size of the enormous pipe required on this basis, we may assume, as before, that in practical work a loss of say 11.15 degrees would be permitted. On this basis, as stated, the water required to be circulated would be 72.21 times the weight of steam required to do the work, so the water-pipe would necessarily have 7.407 times the carrying capacity of the steam-pipe, or 2.228 times the diameter, and the cost of the two systems of piping on the basis above explained would be as one for the steam to 2.97 for the water. We thus see that in doing exclusively the work for which these high pressures are principally to be carried, to wit, cooking, instead of the steam-pipes requiring to be 46¾ times the area, or 5.8 times the diameter of the water-pipe as claimed, the water-pipes must have 7½ times the carrying capacity, be of about 2¼ times the diameter, and about 3 times the cost of the steam-pipes. The relative cost of the pipes by no means represents the cost of operating the two systems. The water-system would always be at a disadvantage in this respect, on account of the high cost of pumping.

It should be stated that it is proposed to use steam for power at only 20 pounds pressure, but it is unnecessary to say that this would involve a very extravagant use of steam, and the size of the pipes would only take an intermediate position between those given for heating and power respectively. It may also be claimed that the fall of pressure available to transmit steam is limited, whereas the pressure available by pumping to force the water is comparatively unlimited. This will not sustain investigation. With an initial steam pressure of 80 pounds, a loss of pressure of but ten pounds will give, in a steam-pipe 12 inches in diameter and one-half mile long, a velocity of fully 80 feet per second, so that there will be readily transmitted, through such pipe, nearly 1,700 horse-power of 30 pounds of feed-water per hour for that entire distance. The most unfavorable conditions for the transmission of steam are when used for cooking, where a high temperature is to be maintained; but even in this case, unless the assumption be made that the water will maintain the ovens at 400 degrees with steam at 400 degrees temperature, which, as has been stated, will require an infinite quantity of water circulated, there must be some loss of temperature, and as soon as it is permitted to drop, so that instead of fabulous quantities only 72 times as many pounds of water are required to be circulated as of steam, the loss of temperature of about 11 degrees entails a loss of pressure of 30 pounds, and but a portion of this difference of pressure will circulate the steam as fast as would be safe for the permanence of the pipes. With water the velocity would need to be kept down in the inverse proportion of its density compared with that of steam, for a similar reason. If the necessary loss of temperature for cooking be made up by increasing the temperature of the water, this would also, in a much greater ratio, increase the pressure of the steam and still keep it at an advantage.

An average presentation of this branch of the subject may be had by examining the pressure available when the hot water and steam are used to furnish steam for power. In the case of the hot water, in order to evaporate about ten per cent. of its volume into steam, the reduction in temperature will be that due to a fall in pressure of 165 pounds, or from 235 down to 70 pounds. In a steam system this entire difference of pressure may be used as the energy which transports the steam to the point where it is used, and as the pumping pressure, on the principles above expressed, must be double this, the circulating pump would require to work against a pressure of 330 pounds to compete with steam, and 10.2

times as much water must be pumped with the water-plant as would be required by the steam-plant; also the water for the water-plant must be pumped twice:—once at the high-pressure of 330 pounds, to circulate it in the pipes, and again at 235 pounds, to pump it into the boiler; whereas, with the steam-plant one-tenth of the quantity of water would be pumped, and but once: viz., into the boiler. It may, however, be claimed that the steam-plant must be charged with the power required to return the water of condensation. The water is returned in practice by the pressure in the heating systems or by steam operating pumps, or pump traps which exhaust into the heating systems, so that no heat is wasted, and the losses are too inconsiderable to mention in comparison with the handicaps of the water system.

The hot water circulated has been called "superheated water," because it is hotter than 212°, but, of course, water cannot be superheated in the scientific sense that its temperature exceeds that due to its pressure. Steam may be superheated and must always have as high a temperature as that due to its pressure. Water cannot be superheated, but may, of course, have a pressure greater than is due to its temperature; in other words, be *sub*-heated, which is the condition that the so-called superheated water would be in when maintained at constant pressure the moment it imparted any heat to another object.

Reference must finally be had to one point, which has been made to appear very important on paper. The following quotation may be made:

"The fuel cost of the power developed by the steam-engines employed in [a hot water] system for circulating the superheated water in the hot-water pipe, for pumping the used water from the return-pipe into the boiler, for driving the blowers, if a mechanical supply of air is needed for the combustion of the coal, and for hoisting coal and its refuse, will, owing to the peculiarity of the system, be not over one-twelfth of the similar cost per horse-power developed by the most economical steam-engines employed in other work. In fact, the only coal required to work these circulating, pumping, blowing, and hoisting steam-engines is what furnishes the heat actually transformed into work according to the thermodynamical theory, and to supply the loss of heat by conduction and radiation from the external surfaces of these engines. The cooled water from the return-pipe will be in such excessive quantity compared with the feed-water required for generating the steam used in the engines, that it will be enormously more than sufficient to condense all the steam worked through the engines, the condensed steam and the water condensing it will be wholly pumped back into the boiler, and there will be no rejected heat, as in the case of other steam-engines, which rejected heat averages about eleven-twelfths of the total heat of the vaporization of water. If the cost of the indicated horse-power in the best engines be taken at about 2⅜ pounds of ordinary coal per hour, that cost, with the engines of [a hot water] system, will be only one-tenth of a pound of coal per hour. The steam taken from the boilers at the temperature of 400 degrees Fahrenheit (pressure 250 pounds per square inch above zero) for working the engines, will be condensed by the water of the return-pipe at the temperature of, say, 160 degrees Fahrenheit, and both the water of condensation and the condensing water will be pumped into the boiler, so that the total quantity of water in the boiler and in the hot water-pipe and in the return water-pipe will always remain constant."

With all the hot water used for power purposes rejected at a temperature of 316 degrees and that for cooking at 390 degrees or upward, how is the very large quantity of heat still remaining in the water to be reduced to the temperature of 160 degrees, as stated in the above extract? It may be said it will be used for heating water, boiling articles of food, heating buildings, and such like uses. But what can be done with it in summer when there is no heating to do, and even in winter or at any other time, in fact, how is the surplus heat in the hot water from cooking and power apparatus to be exactly that required for some other culinary operation or for heating some particular building? The slightest calculation will show that the surplus heat will be so great that it cannot in practice be reduced to the temperature stated. The low temperature of the return water could only be secured in individual instances in buildings provided with specially large heating coils arranged to receive the water as it was about to escape to the street. Houses and public buildings already provided with heating apparatus would necessarily have connections made to the apparatus in place, and the heat would be rejected at the temperature of the steam used for heating, say at the temperature due to five pounds, as has been provided for in the previous calculation. In no case, as has been intimated, could it be assured that the surplus heat from the cooking apparatus would not exceed that required for other culinary operations and heating the house. In seasons when no heat was required, the only economical way to dispose of the hot water at 390 degrees rejected from the cooking apparatus would be to pump it back to the station at that temperature and at the pressure due thereto. The result would only be worse were it allowed to expand down to atmospheric pressure, for then a large portion would fly into steam and the return-pipes be filled with a mixture of steam and water. If the hot water were used to generate steam for power, the surplus heat would be so great that it would be impracticable to dispose of it in the same or adjacent buildings even during the heating season. Few factories can use all the exhaust steam from their engines, whereas with the water system there would be about five times as much heat in the rejected water as would be used in the engine. If part of the latter be used for heating, the heat in the exhaust steam must be absolutely wasted. In fact, at all times a very large quantity of hot water must be rejected at the temperature of 316 degrees due to the pressure, and as in the case of cooking, the only economical way would be to return it to the station at a pressure of 70 pounds. If it were permitted to expand down to the pressure of the atmosphere, there would be 2.89 cubic feet of steam per pound of water circulated, or 29.4 cubic feet of steam at atmospheric pressure in the returns for each pound of water evaporated into steam for use in the engines, and the volume of steam in the return pipes would be about 60 times as large as that of the water contained in the same. Of course, in a small plant for exhibition purposes radiators may be arranged to keep down the temperature rejected from cooking and power systems; but a slight study of the problem will, as above indicated, show that the demands for different purposes cannot be adjusted, even in winter, so as to prevent the rejection of a great deal of heat, and that in summer the heat in the water can practically only be utilized through a small range of the higher temperatures, and much the greater part of the heat must be rejected, though it may be returned to the station at great cost and be saved if practical means are found for the purpose.

The writer has thus far discussed the subject in the abstract without comparison with other work. At this point it may be of interest to state that precisely the feed-water apparatus described above has been used from the first in the plant of the New York Steam Company designed by the writer, and that we are barely able to condense the steam which comes back in the returns *when half the feed-water is supplied directly from the Croton mains* to make up the loss due to the escape steam from high-pressure engines supplied on the lines. At times a portion of the steam from the pumping-engines can also be condensed in the tank, but at other a portion of this escapes. It is utterly hopeless to do better or eve as well with a very much larger proportion of hot water supplie from the returns.

If the present proposed system, to return the water at high pressure, be changed, then, without helping the feed-water question, a the old complications of the former developments of the system wi be necessary in every house, and under some circumstances boiler would necessarily be used on the premises arranged to be heate by hot water instead of fuel. On the other hand, if steam be use merely, the full range of temperature is available for *every* operation and the heat rejected due to the smaller quantity of water require be readily returned to the station by the surplus pressure in th pipes.

It will naturally be asked what the probable cost of pumping th hot water will be. This requires the assumption of a certain set o conditions. Previous discussion has been based on allowing the ho water a difference of pressure at the two ends of the line of twic that allowed to the steam between the station and the point of us On this basis, with a comparatively low pumping pressure, say a difference of twenty pounds between the extremes of the line, the ne power required for pumping would be somewhat more than one pe cent. for each volume of water pumped compared with that require to be pumped in the boiler for a steam system. Reckoning th efficiency of steam-pumps at 50 per cent. on the basis of one horse power for the heat required to evaporate 30 pounds of water fro 70 pounds pressure per horse-power, there would be required fo circulating water for heating fully 11.4 per cent. of the power transmitted through the pipes: for power there would be required full 20.4 per cent., and for cooking fully 145.4 per cent. Higher pumping pressures would of course entail higher losses. For the stea plant, on the contrary, there would be required on the same basis fo pumping the water in the boiler, a little less than 2 per cent. of th power transmitted, and this cost would be independent of the los of pressure in transmission. The water in the returns would b forced back, as has been stated, by surplus pressure. It will be seen therefore, that the water plant will not only be more expensive t construct originally, as well as more difficult to operate, but tha the actual cost of the operation would be greater in the proportion stated, independent of many other considerations which cannot here be discussed, which would make the cost still greater on account of the indirect method of doing the work.

The resistance to explosion of the steam and of the water pipes could be made the same originally by increasing the thickness of the water-pipes proportionally to their increased diameters; but if high capacities were attempted by pumping water at very high velocities, the pipes would be rapidly scoured out so as in time to become dangerously thin. In case of a break in the steam-pipe, the steam dissipates at once and is not dangerous. The writer has known a case where, through carelessness of workmen, a man was struck full in the body at a distance of only a few feet by a jet of steam two inches in diameter, issuing from a pipe at 80 pounds pressure, but no injury to his person whatever resulted. Evidently, however, a single quart of hot water, projected in the same way, would have caused fearful scalds, and anything like the same quantity of water as of steam would have caused a lingering death. Hot water is also very destructive when the pressure is suddenly released, and the flying particles would scald persons and do other injuries, even when projected long distances.

It is interesting to see all the operations of cooking performed by hot water of high temperature, but evidently every one of these operations could be performed equally well by steam with the pressure due to such temperature, and all the operations would be much more simple and economical. In other words, the advantages due simply to high pressure are claimed for hot water. It may be said that the hot water at the high temperature ought to be compared with steam at the pressures ordinarily carried, but the steam can be supplied at the high pressure much more readily than the water. There is, however, a separate question as to the relative advantage of transmitting steam at the high pressure of 235 pounds referred to above, compared with a transmission at a pressure of 80 or 90 pounds corresponding to that ordinarily used in practice. Evidently the lower pressure will supply all the steam which is required for heat and power purposes quite as well as if generated at the very high pressure. The only possible object in increasing the pressure would be to do some kinds of cooking which cannot be done with the lower pressure, and it may be claimed to save something in the size of pipes. So far as the latter is concerned, the increased thickness must also be taken into consideration. The culinary operations which require the greatest amount of heat are the heating of water and the boiling of meat and vegetables. This can be done with perfect satisfaction with a steam pressure but little above the atmosphere, say at the ordinary heating pressure of 5 pounds or under. Meat may be roasted and browned satisfactorily if put in steam-jacketed vessels directly on the metal with a steam pressure of 40 pounds. This is done every day in many saloons. Of course, steaks and chops can be cooked in the same way if desired, though without the aroma of slightly scorched meat as in broiling. Cake and bread can be cooked but not browned with a steam pressure of 80 pounds. The higher pressure of 235 pounds is only required for such operations as broiling meat, and for the baking and satisfactory browning of bread and cake. But little bread is baked in private houses, and that required may be sent either to the baker's or be done in the house by customary methods on a particular day of the week. The other operations stated as requiring a high temperature can readily be performed at very slight expenses with gas-stoves. When the comparatively small income to be derived from this particular work is considered in connection with the enormous cost required to do such work, particularly with hot water and also to some extent with steam, it appears to the writer that it will not pay to go to the extra expense and risk necessary to carry steam or the more expensive hot water at such high pressures for this purpose alone, and it is hoped that this presentation will enable others who are required to assume responsibilities in this direction to judge for themselves what limit, everything considered, is the best to adopt.

DISCUSSION.

Mr. C. T. Porter.—I have great pleasure in agreeing with the conclusion of the paper, and the only thing that I propose to say, is to tell a little story, which looks very much at first as if it would help the other side. It is interesting in itself, however. Quite a number of years ago, in the city of New York, we had a very severe winter, which froze our water-pipes very seriously indeed. We had more trouble that winter than ever before. Mr. Samuel Sloane, who is not so well known to the present generation as he was to the last, but who was one of the most gifted men we have had (he was the inventor of the gimlet-pointed wood screw, and the machinery for making it), applied his ingenuity to meeting that difficulty of freezing pipes, and the next fall, at the Fair of the American Institute, he exhibited a very admirable apparatus by which he thawed out an ordinary lead pipe which was laid in a long trough filled with a freezing mixture, and while the pipe remained imbedded in this freezing mixture, he thawed the ice through it at the rate of a foot a minute. Before he put his apparatus in operation there, he asked me, "Can you tell me which contains the most heat, a cubic foot of steam or a cubic foot of water, at 212 degrees?" I told him water contained about three hundred times as much heat as an equal volume of steam at atmospheric pressure. "That explains it," he said; "I could not do anything with steam, but as soon as I tried hot water I was successful."

The hot water was discharged from the end of the internal tube against the ice, and flowed back through the annular space, and so thawed out the pipe, and kept it thawed out right through this freezing mixture. Mr. Sloane expected to make a fortune out of that invention, but I doubt if he ever sold one apparatus.

In this case water impinged against a very small surface, and it would part with all of its heat at that point, and it was only necessary that it should remain warm enough not to freeze in flowing out. The advantage of water over steam there shows that a universal rule cannot be made to apply to all cases. But the cases in which water has an advantage over steam are very few. Steam takes itself anywhere upward, and returns in water to the boiler, while water requires to be pumped. Steam will rush along at a great rate when wanted, and although the heat in a cubic foot of steam is so much less than that in a cubic foot of water, still, if there is condensing surface enough, it is quite as efficient.

Mr. G. H. Babcock.—If the question before us were one of the quantity of heat which could be carried by a given sized pipe, of a given length, in a given time, the problem would be a simple one. The approximate weight of any fluid which will flow in one minute through any given pipe with a given head or pressure may be found by the following formula:

$$W = 300\, d^2 \sqrt{\frac{D\,[p_1 - p_2]}{L\,[1 + \frac{3.6}{d}]}}$$

in which W = Weight per minute in pounds avoirdupois,

d = Diameter of pipe in inches,

D = Density or weight per cubic foot,
$p_1 - p_2$ = Pressure per square inch tending to produce flo and
L = Length of pipe in feet.

Where the pipe and pressure are the same, the only variable the density, so that

$$W \propto \sqrt{D}$$

Now, the quantity of heat transmitted is equal to the total he per pound multiplied by the number of pounds.

Let Q = quantity of heat per minute, and
H = total heat per pound of the fluid,

then

$$Q \propto H\sqrt{D}.$$

With steam H increases gradually but slightly with the ter perature, while the density increases in rapid ratio. With wat H increases nearly in the ratio of the temperature, while D d creases slightly. It will therefore be seen that with both the fluids Q will increase with the temperature, but most rapidly wi steam. Assuming a constant motive pressure, the curve f water is practically a straight line, while for steam it rises rapidl so that it is evident that the values of H and D are approachi each other, and that where they cross the value of Q will be t same for both fluids. This will occur at about 1200° temper ture, when the pressure will be about 21,000 pounds to the inc and the total heat per pound would be about 1,400 units. Th diagram (Fig. 163) shows the two curves intersecting at th point.

But this is not a solution of the problem, because it is not question of how much total heat may be transmitted, but of h much available heat. This will depend upon the temperature which the water from each system is rejected at the point of u This will, of course, be dependent upon circumstances; b assuming that it is the same in each case, and putting it 200 as the minimum, this amount must be taken from the val of H before it is multiplied by the square root of the density. the case of water this gives us a line nearly parallel with b gradually approaching the curve of total heat, while the stea curves are divergent. The two show, however, a nearer appro mation to equality. A higher temperature for the discharg

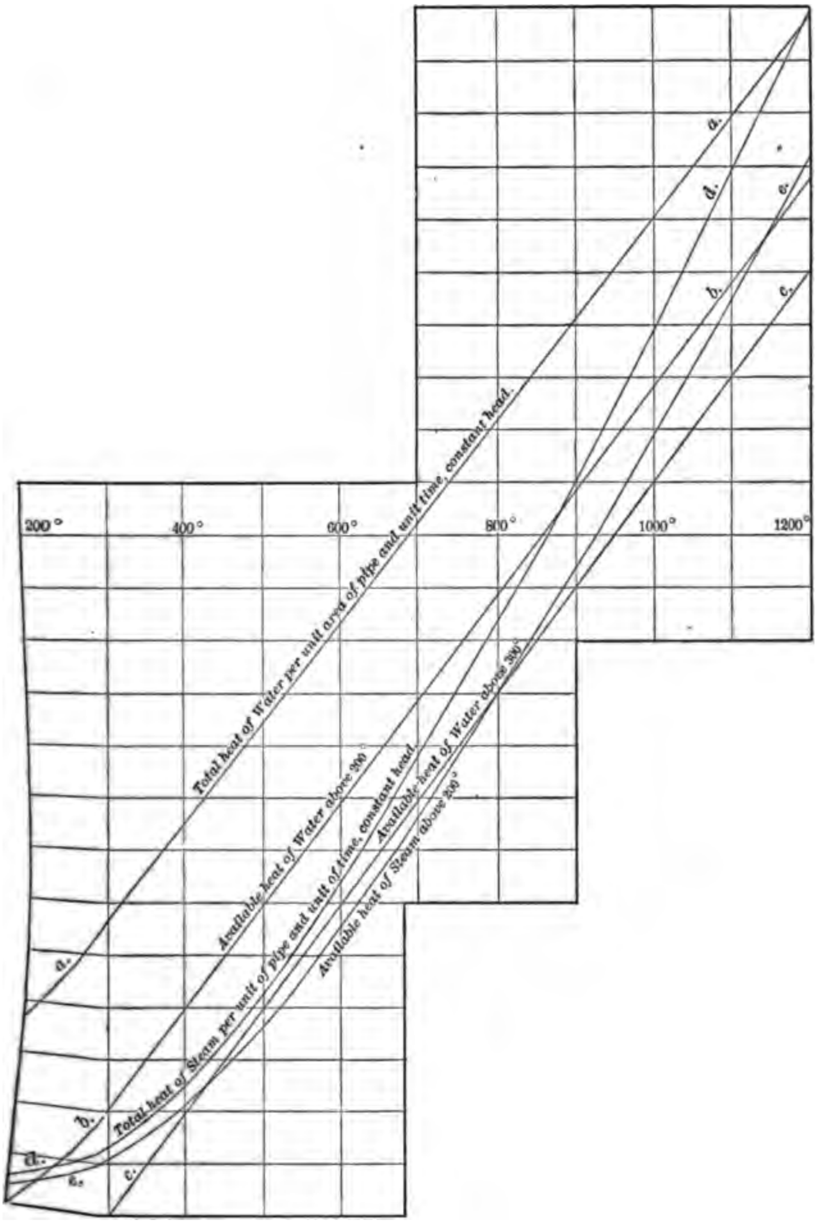

FIG. 163.

water will show still more to the disadvantage of the water system as compared with steam, and it needs to be carried to only about

300° to overcome the apparent advantages of water over stean and reverse their relative positions. This will occur when th system is used for power.

But an equal amount of surface will not dispose of the sam amount of heat in equal times, when supplied with water, as i will when supplied with steam. The experiments of Tredgol show that a square foot of cast-iron surface exposed to hot wate upon the one side and air upon the other will give off 1.85 Britis thermal units, while a square foot of sheet-iron will give 1.24 It is fair to suppose that the wrought-iron pipes ordinarily used wil be a mean between these two, giving only 1½ units per hour fo each degree of difference in the temperature. With steam-heate pipes, however, 3 British thermal units for each degree of difference in temperature is the usual allowance. It will therefor require twice as much surface to heat a building to the same degree with the water as with the steam system, both being at th same temperature, while at the same areas of heating surface th water will part with one-half of the heat in a given time. Th reason for this difference is not difficult to see. When steam i cooled below the normal temperature of the pressure, it instantl condenses and makes room for another supply of steam to tak its place, so that the heating surface is always supplied witl steam at full temperature. With water, however, the film o water lying next to the surface parts with its heat and is cooled after which it does not give way for hotter water to take its place but remains as an impediment to the transmission of the hea from the hotter water within. As a practical means, therefore of heating buildings from a central station, it would seem that th water, although capable of transmitting, under usual conditions a greater amount of heat in a given time, will not be able to utilize the heat thus transmitted without a much greater increasec expenditure for heating surface.

Mr. Geo. M. Bond.—I would say that in the manufacture of pape barrels, which has been perfected by a company having built thei machinery in Hartford, the use of hot water is found to be fa superior to steam in drying the pulp of which the barrels are made. Steam was first tried, but was found to act too slowly fo the operation. With hot water the barrels are dried in about seven minutes almost as hard as a piece of seasoned lumber.

CCLI.

TESTS OF THE COMPARATIVE VALUE OF DIFFERENT KINDS OF BELTING.

BY SAMUEL WEBBER, CHARLESTOWN, N. H.
(Member of the Society.)

THE arrangements for the different experiments, of which the results are shown in the accompanying tables, were made early in the summer of 1886, but various circumstances prevented their execution until December of that year, when they were taken up and continued during a period of several weeks.

When the tests were begun, the writer was unaware of the experiments made by Messrs. William Sellers & Co., which were described in a paper read by Mr. Wilfred Lewis* in May, 1886; he might then have so designed his apparatus as to have been able to contribute some farther information as to the relative value of T^1 and T^2, or the tension on the driving and slack sides of the belt, and he would probably have adopted the arrangement described by Mr. Lewis for determining the *slip*, as more accurate and positive than the one which he thought of and used for that purpose.

As his intention was, however, rather to determine the relative driving capacity of various kinds of belting than to establish any formula by which such belting might be applied, the apparatus which he used answered the desired purpose very satisfactorily, and the results obtained, when compared with those furnished by Mr. Lewis and Professor Lanza in a previous paper,† may aid in establishing some such definite formula for practical use.

The apparatus employed, as shown in the accompanying plan and elevation (Figs. 109 and 110), was as follows: *A* is the main shaft in the room, driven direct from the engine which furnished the necessary power for the belt factory where the experiments were made, by the main belt *D*, which led from an 8 ft. pulley on the engine to one of 4 ft. on the main shaft. The engine had a "Wheel-

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* No. CCXIII. Trans. A. S. M. E., Vol. VII., p. 549.

† No. CCII. Trans. A. S. M. E., Vol. VII., p. 347.

Fig. 109

PLAN OF BELT TESTING APPARATUS USED AT TOLLAND, CONN.

½ in. to the foot

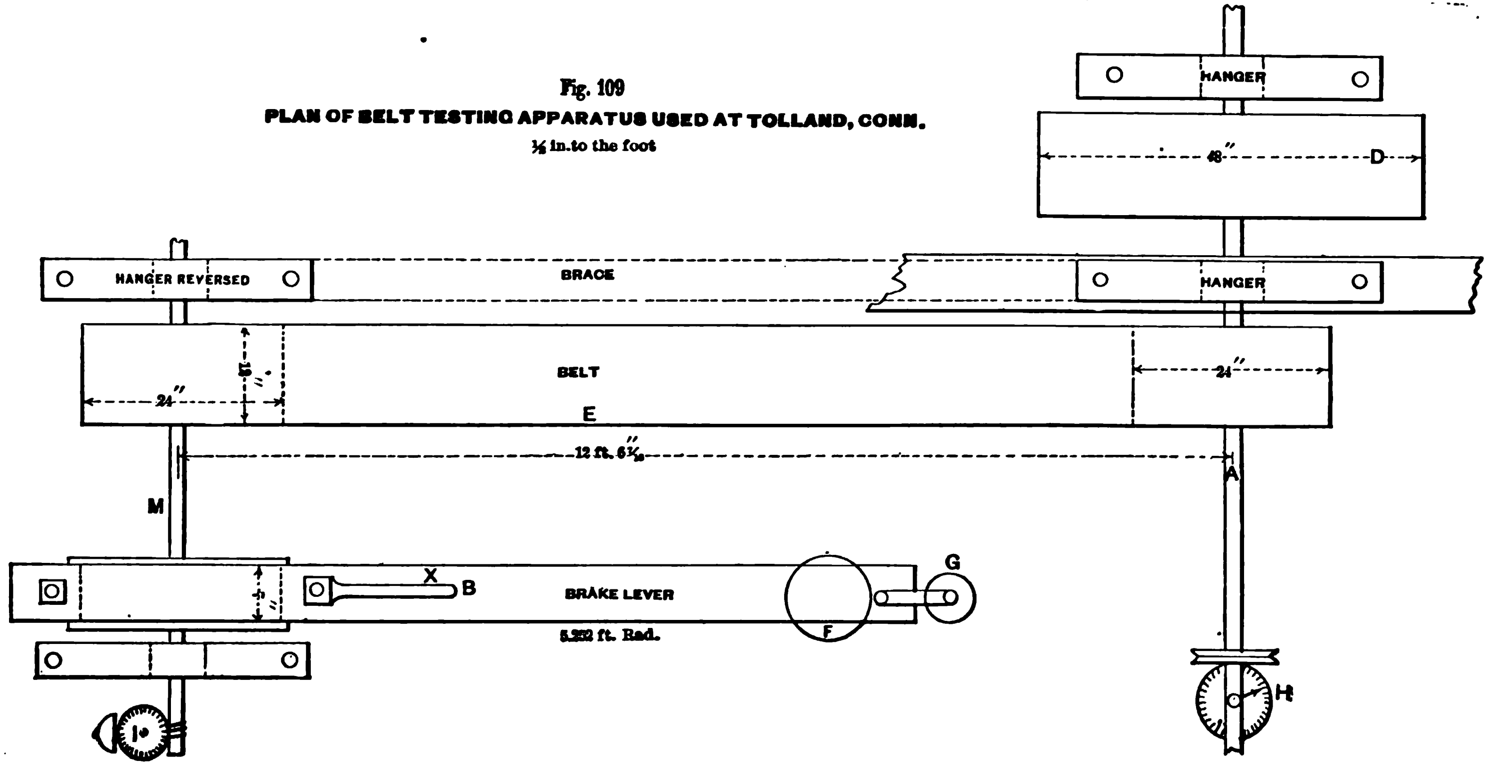
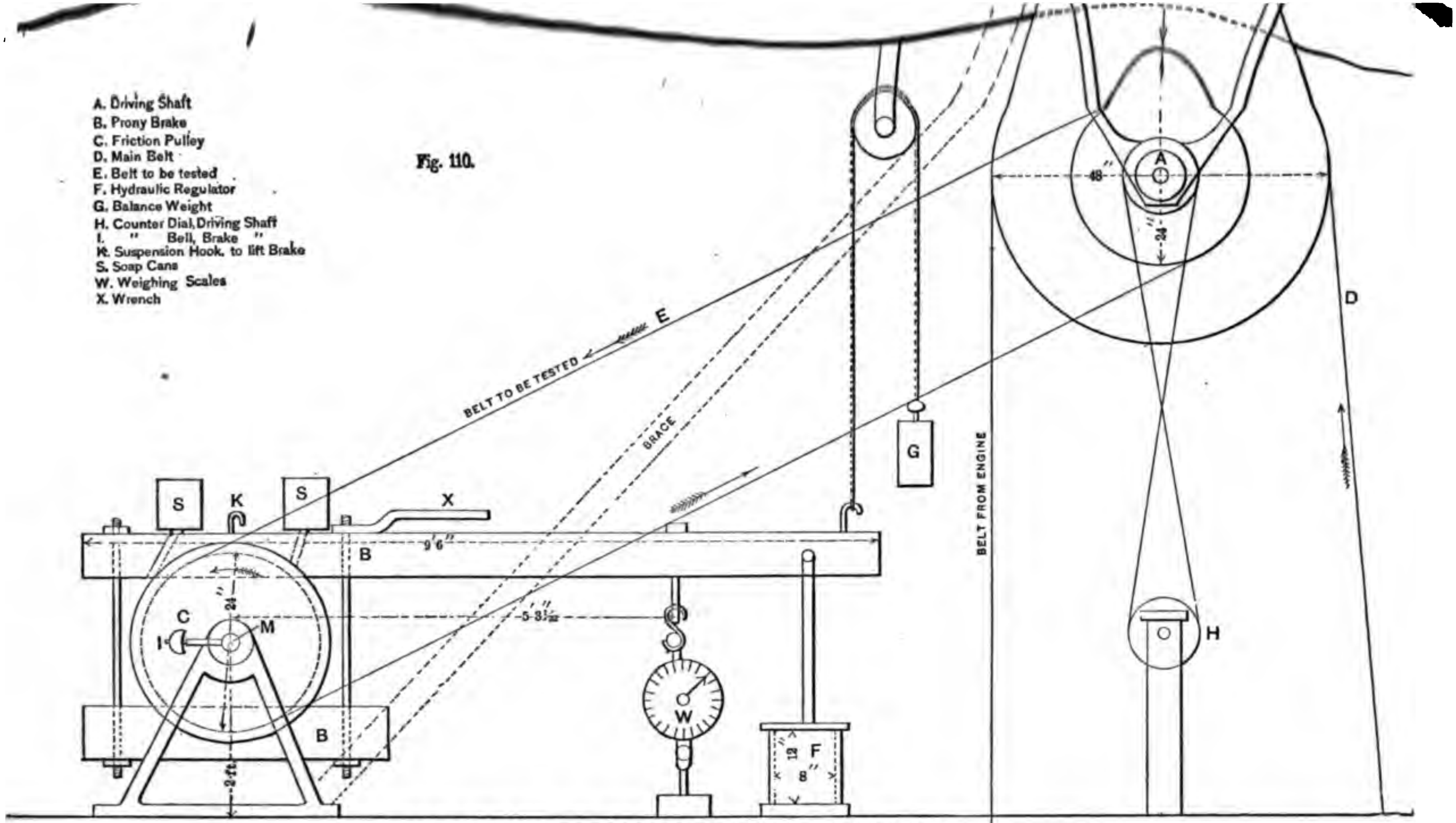


Fig. 110.



ELEVATION OF BELT TESTING APPARATUS USED AT TOLLAND, CONN.
½ in. to the foot

ock" cylinder, 10″ × 30″, was making 60 revolutions per minute, and was ordinarily used with a boiler pressure of 60 lbs. per inch. Shaft *A*, therefore, made 120 revolutions per minute, and from a 24″ pulley on it, the belts to be tested were taken to a similar pulley on the brake-shaft *M*, which was supported from the floor, as shown in the elevation, at a height of 2 ft. above it, and carried the friction pulley *C*, 24″ diam., 7″ face, with flanges 1″ deep, which kept the brake *B* in position. The two shafts were 12 ft. $6\frac{7}{16}''$ from center to center, having a horizontal distance of 11 ft. and a vertical of 5 ft. 6″, and the direction of the belt was such as to bring the strain on the under side, leaving the slack side on top. The brake was made of two pieces of spruce timber 7 ft. square and 9 ft. 6″ and 3 ft. 6″ long respectively. These were scooped out to a depth of 3″, with a radius of a 2-foot circle, and grooved in "herring-bone" grooves around the curve to admit of the free passage of the lubricant, which was simply strong soapsuds, and was fed from the cans *S S* on the top of the brake.

The vibrations of the brake were controlled by the "dash-pot" *F*, which was $8\frac{1}{2}''$ interior diameter and 12″ deep, with a piston having a clearance of rather less than $\frac{1}{16}$ inch.

The radius of the brake circle was 5 ft. $3_{4}''$, *scant*, being as near as possible to 5.252 ft., or the radius of a 33 ft. circle.

This dimension was adopted to facilitate the calculation of the horse-power attained on the spot, as it was only necessary to multiply the weight shown on the scale by the *Rp m* of the pulley to get the power at once by striking off decimals. The number of revolutions of the brake-shaft was ascertained by counting the seconds between the strokes of the bell *I*, which, by means of a worm and gear, was rung every 100 revolutions of the shaft.

A worm gear and dial *H* were driven from the main shaft by a cross-belt on grooved pulleys, and an observer at that dial counted the revolutions of the main shaft corresponding to 100 of the brake-shaft, by letting the pointer on the dial, which was an easy fit on the shaft, start when the bell rang on the brake-shaft, and noticing the point it had reached at the next ring. This apparatus was all so light that there was no possibility of slip or lost motion in it.

Before commencing the tests, the brake was perfectly balanced by suspending it by means of the hook *K*, placed directly over the center of the pulley, with the scales attached, and the dash-pot filled with water and the piston in place. After the weight *G* was ascertained and adjusted, it was never changed or removed, and the brake was never known to move from any nearly level position in which it was left, after releasing it from the pulley.

To facilitate the operation of weighing, a spring balance was used, obtained of Charles Forschner, of New York, having a dial graduated to 250 lbs. This was found to be capable of weighing up to 30 lbs. more, after describing the circle, but was not used to that extent, although occasionally strained to register 10 lbs. over, or 260 lbs.

This was carefully tested and found to be entirely accurate, and, with a companion of the same size, was used in getting the strain applied to the belts, when, later on, the two, by the use of levers, being made to register 1,000 lbs.

The dash-pot checked the vibrations of the brake-arm so that we were able to read the scale very accurately, and it proved a very rapid and easy method of determining the load on the brake. Having provided ample surface for the friction, in the brake and pulley, the whole apparatus worked smoothly and perfectly, and the only trouble encountered was the inability of the engine to furnish sufficient power to test fairly a 12″ belt.

One attendant was employed at the brake lever to screw up the nuts of the clamp, and another to keep the soap cans filled, and Mr. F. H. Underwood assisted me in the count of revolutions, checking each other at every experiment.

The hook to which the scale was attached had a sharp knife-edge, which was set as accurately as possible to the exact distance from the center of the shaft required for the radius of the 33 foot circle. I have been thus full in my description of the apparatus used, because I have found the want of it, in reading other papers, not being able to follow all the results clearly.

The engine was supposed by Mr. Underwood, when the tests were proposed, to be of 35 HP., which is higher than I should rate t, and we found it impossible to make the more important tests while the factory was in operation, and had to do them at dinner-ime and in the evening. Each test occupied from half an hour to an hour and a half; and another belt was then put on the pulleys and allowed to run without any load but that of the brake-shaft and pulleys for half a day before testing, in order to let it get fitted to the pulleys as much as possible.

The insufficiency of the engine is clearly seen in the loss of speed n the driving-shaft, as the load was increased, and as the main belt was taken up 3 inches after the first preliminary tests, and no sign of slip in that noticed afterward, this loss of speed can only be due to the engine. In one or two tests, slight variations can be noticed due to forcing the fires so as to get 75 lbs. of steam on the boilers.

Table 1 gives the result of the tests of an ordinary leather belt, in good working order, taken off one of the machines in the factory, and of a 2-ply leather-lined canvas belt of the same length, width, and weight, put on merely as preliminary experiments, before the scales for weighing the tension were ready; to test the operation of the brake.

Tables 2 and 3 contain the records of the tests of a heavier "cotton-leather" belt, as it is designated by the makers, the details of which are fully explained in the tables. It will be seen that it was tested under two different tensions, and under the heavier tension, at two different velocities.

Tables 4 and 5 give the results, with one of the well-known best single leather belts, entirely new, as were all the others, but being somewhat rigid, not giving quite as good results as it would have done after six months' use. Changing the side to the pulley showed no essential difference. Not getting entirely satisfactory results from it, at the width of 12 inches, it was, together with the 12″ raw-hide and the cotton and leather belt of the same width, split down to 8½ inches, and tested again, as shown in *Table* 6. This belt did not appear to have stretched any more, after the strain of 1,000 lbs. was first put on, as noted in *Table* 5, but did not even then appear to have fairly fitted itself to the pulley.

Table 7 contains the results of the test of a cotton duck belt which requires no farther explanation than is given in the table.

Table 8 gives the record of the tests of a remarkable structure of leather and iron, known as a "link belt." No farther comment is needed than that given in the table to show the incorrect principle on which it is constructed.

Table 9 shows the tests of a woven canvas belt, woven 4-ply, not stretched like the belt in *Table* 7. When received, it smelled very strongly of linseed-oil, and appeared to have been dusted over with powdered soapstone, which caused it to slip very badly on the first trial with a light strain, but on the application of a strain of 1,000 lbs., gave very good results, and but for want of time and an unwillingness to spoil the belt, would have been cut down to 8½ inches, and tested farther, and on other pulleys.

Table 10 is that of a first-class rubber belt, which is fully explained. It will be noticed that it showed no signs of slip in any test until it went off finally, and on the last one was found to be somewhat "demoralized," the rubber chafing in places. The last tension of 83⅓ lbs. per inch was probably greater than should have been applied to the belt.

Table 11 gives the first series of tests of what was called a "raw-hide belt," which I suppose to have been the same material as that mentioned by Mr. Lewis, in his paper, under the same name. Although this could not be called strictly "raw-hide," it was evidently prepared in some manner which was not ordinary tanning. It possessed great softness of surface and flexibility, and hugged the pulleys in a wonderful manner, and, as is shown, slipped very little, until the final test.

Table 12 shows the result of trials of the same belt, on wooden pulleys, when the power of the engine was practically exhausted, and the belt was then cut down to 8½ inches, and the test repeated, as shown in

Table 13. Here the belt was simply laced as before, in the old holes, but the original strain of 1,000 lbs. now gave 117.7 lbs. per inch, and while giving a higher power per inch of width than before, the strain stretched the belt permanently, as it was found to be much looser when laced on to the paper pulleys, and the tension was measured at the end of the trials on them, and found to have shrunk to 660 lbs., while the belt, when taken off and measured by the 3½" strip which had been split off, was found to have stretched 1¼ inches. This may possibly have been partially done in the trial on the wooden pulleys, while of the full width of 12 inches, but was, I think, mainly due to the strain of the test after reduction of width.

It will be seen that this final test on the paper pulleys varies but very little from the results obtained from the new leather belt, which did not seem to have stretched any more than noted in the tables.

Table 14 gives the results obtained with a plain canvas belt, such as used by the Underwood Manufacturing Company, as the basis for their cotton-leather belt, the first trial of it being made just as it came from the factory, the second one after it had been *sized*, and the third of the same belt under double tension.

These are mainly interesting as showing the impossibility of obtaining any great friction from simple woven canvas.

Tables 15 and 16 show the results obtained with a 12″ cotton-leather belt, under different tensions on iron pulleys, and under the highest tension on wooden pulleys, and *Table* 17 gives the records of the same belt cut down to 8½ inches on the wooden and paper pulleys.

It will be seen that this belt did not stretch, but retained its tension after the last test, and it will also be noticed that the slip on the wooden and paper pulleys was very slight to the end.

This belt consists of a firmly woven duck or canvas, which is first stretched by running it at a high speed over pulleys, which are adjustable by means of screws to any required tension, and after the stretch seems to be thoroughly taken out of it, a thin and soft leather lining is cemented on to one side, under heavy pressure, so as to make a holding surface to be run next the pulleys. The canvas is woven two, three, four, or more "plies" in thickness, and of any desired width; and a sample of this, and of the other belts tested, are exhibited herewith.

It will be seen, by examination of these tables, that they fully corroborate the conclusion arrived at by Mr. Lewis, as to extent of slip with which a belt will remain on the pulleys, and which, as shown by his experiments, seems to be about 20 per cent.

In determining the co-efficient of effect, or friction, which I understand to be the same thing, I have simply divided the power shown by the brake by the sum of the tensions, $T^1 + T^2$, or twice the strain with which the belt was laced on, multiplied by the speed of belt in ft. per minute.

This is the only way in which I can look at it, and although I have not succeeded in obtaining any co-efficients of over 100 per cent., these results only agree with the old axiom, that "a part cannot be greater than the whole."

It will be seen that the "rawhide" belt, as well as the cotton and leather belt, gave co-efficients of over 50 per cent., and the writer is of the opinion that the new leather belt would have reached 50 per cent. had it been well oiled and flexible, as it might become after six months' use. It had been kept some time in a warm dry room, and was rather stiff, and does not seem to have given the power which it ought. It certainly was in no wise stretched or injured in the experiments.

In a little volume which the writer published a few years since,[*] he endeavored to harmonize a number of rules given by

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[*] *Manual of Power for Machines, Shafts, and Belts.* D. Appleton & Co., N. Y., 1879.

different parties, for determining the width and horse-power of belts, all using as a factor the amount of surface contact. Subsequent experiments of his own, which were reported at the Hartford meeting of this Society,* in the spring of 1881, finally convinced him that the extent of surface had nothing to do with the matter, and that the "*arc of contact*" was the only factor to be considered, and that with an assumed co-efficient of friction of 50 per cent., the following formula might be safely adopted.

This only requires the initial knowledge of the tension with which the belt is put on, and which may vary from 330 to 350 lbs. per sq. in. of belt section. With the exception of this change in formula, the writer sees no reason to change any of the views or tables expressed or published in the aforesaid "Manual of Power," as these tests fully confirm them and show a much greater power obtainable from a single belt than that deduced by Mr. Lewis from Mr. Nagle's theoretical formula.

The proposed formula is as follows:

$$\text{Width in inches} = \frac{\text{No. HP.} \times 33000 \times 180^\circ}{\left\{\begin{matrix}\text{vel. in ft.}\\ \text{per min.}\end{matrix}\right\} \times \left\{\begin{matrix}\text{strain in lbs.}\\ \text{per inch width}\end{matrix}\right\} \times \text{arc of cont.}}$$

and

$$\text{HP.} = \frac{\text{vel. in ft.} \times \text{strain per in.} \times \text{width} \times \text{arc of contact}}{33000 \times 180^\circ}.$$

With the strain known, or assumed, these results are easily attainable.

Agreeing fully with the views expressed by various gentlemen at the Chicago meeting, that there is yet a good deal more to be learned about this matter, I have made this paper as full as possible of the facts regarding the experiments, on which Mr. Underwood and I have spent many weeks, leaving the matter open for any one to deduce as many theories as possible. The results fairly prove the great driving capacity of that style of belt to test which these trials were planned.

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* Trans. A. S. M. E., Vol. II., p. 224.

TABLE I.

Record of Belt Tests at Underwood M'f'g Co.'s Shop, Tolland, Conn., December 15, 1886. One old leather belt in good order, 3 in. wide, $\frac{1}{8}$ in. thick, 31 ft. 2 in. long. Weight, 6$\frac{3}{4}$ lbs.; strain on belt not taken; stretched to 31 ft. 4$\frac{1}{4}$"; iron pulleys, crowned $\frac{1}{16}$ inch.

Lbs. on brake.	Rpm.	HP.	Rpm. driver.	Slip.	Ft. per m. speed belt.	$T^1 + T^2$ Total strain.	Coeff. effect or friction.	Ft. per m. to 1 HP. per in.
15	120	1.80	120	0	754	Undetermined.	Undetermined.	1,256
20	117.6	2.36	"	2 %	754 ?			988
25	117.6	2.95	"	"	754 ?			766
30	115.4	3.45	"	3.83	754 ?			655
35	100	3.50	"	16.66	754 ?			600
40	Belt slipped off entirely.							

One new 2-ply cotton leather belt, 3 in. wide, 31 ft. 2 in. long; weight, 7$\frac{3}{4}$ lbs.; $\frac{1}{8}$" thick, put on with strain (not taken); stretched to 31 ft. 4$\frac{1}{4}$". Iron pulleys.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. per m. speed belt.	$T^1 + T^2$ Total strain.	Coeff. effect or friction.	Ft. per m. to 1 HP. per in.
30	117.6	3.52	117.6	0	739	Undetermined.	Undetermined.	630
35	117.6	4.13	"	0	739 ?			537
40	111	4.46	"	5.62 %	739 ?			504
45	100	4.95	"	7.31	739 ?			448
50	109.2	5.35	"	8.85	739 ?			414
55	101.7	5.61	"	13.7	739 ?			395
70	98	6.86	"	20 %	739 ?			323
75	Belt slipped off pulleys. Main belt probably slipped also. Speed of driving-shaft uncertain. These two belts were put on to try the working of the brake, were cut the same length, and thrown on to pulleys without weighing tension.							

TABLE II.

Record of Belt Tests at Underwood M'f'g Co., Tolland, Conn., December 16, 1886. One 5-ply cotton leather belt, 31 ft. 2" long, 5" wide, $\frac{5}{16}$" thick, 19 lbs., stretched to 31' 4$\frac{1}{4}$", with strain of 380 lbs., 76 lbs. per in. Iron pulleys, crowned $\frac{1}{16}$ inch.

Lbs. on brake.	Rpm.	HP.	Rpm. driver.	Slip.	Ft. belt per min.	Total strain $T^1 + T^2$.	Coeff. effect or friction.	Belt Ft. per m. to HP. per in.
50	120	6	120	0	754	760	.345	628
60	120	7.20	120	0	754	760	.416	524
70	115.5	8.08	117.6	1.30	739	760	.465	466
75	115.5	8.77	117.6	1.80	739 ?	760	.516	421
80	115.5	9.20	117.6	1.30	739	760	.541	402
85	113.2	9.60	115	1.70	725	760	.575	377
90	109	9.81	113	3.54	697	760	.613	355
100	98.3	9.83	109	9.80	684	760	.624	348
110	82.2	9.02	105	21.5	662	760	.595	344
115	Belt slipped off pulleys; main belt found to slip also, and the speed of driving-shaft uncertain.							

December 30, 1886. Same belt with same tension, on Dodge's wooden pulleys crowned ⅛ inch.

Lbs. on brake.	Rpm.	H.P.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$	Coeff. effect or friction.	Belt Ft. per m. to HP. per in.
30	120	3.60	120	0	754	760	.207	1,047
40	120	4.80	120	0	754	760	.276	785
50	120	6	120	0	754	760	.345	628
60	117.6	7.05	117.6	0	739	760	.415	524
70	117.6	8.21	117.6	0	739	760	.483	450
80	117.6	9.32	117.6	0	739	760	.548	396
90	113.2	10.19	113.2	0	711	760	.622	348
100	100	10	115.4	13%	725	760	.600	362

Belt still on pulleys, but test stopped on account of evident loss of power by slip.

TABLE III.

Record of Belt Tests at Underwood M'f'g Co. Same 5" belt reported in Table II. on iron pulleys, with strain of 584 lbs. = 117 lbs. per inch, January 6, 1887.

Lbs. on brake.	Rpm.	HP.	Rpm. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$.	Coeff. effect.	Ft. belt per min. per HP. per 1 inch.
50	120	6	120	0	754	1,168	.225	628
60	120	7.20	120	0	754	1,168	.266	524
70	117.6	8.23	117.6	0	739	1,168	.315	449
80	117.6	9.41	117.6	0	739	1,168	.360	392
90	114.3	10.29	114.3	0	718	1,168	.405	348
100	111	11.10	112.2	1.08	704	1,168	.444	317
110	109	12	112.2	2.85	704	1,168	.480	293
120	105.3	12.63	111.6	5.65	698	1,168	.511	277
125	100	12.50	112	10.75	704	1,168	.500	280

Stopped test in good order from loss of power.

Same belt. January 5, 1887. Same strain, increased speed from 46" iron pulley on driving-shaft.

Lbs. on brake.	Rpm.	HP.	Rpm. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$.	Coeff effect.	Ft. belt per min. per HP. per 1 inch.
40	230.8	9.23	120	0	1,445	1,168	.180	783
50	226.4	11.32	117.7	0	1,417	1,168	.224	626
55	222.2	12.22	115.5	0	1,391	1,168	.248	569
60	214.3	12.86	111.5	0	1,343	1,168	.270	522
70	214.3	15	111.5	0	1,343	1,168	.315	448
80	210.5	16.84	109.7	0	1,321	1,168	.360	392
90*	206.9	18.62	108	0	1,301	1,168	.404	349
100	200	20	106	1.50	1,276	1,168	.441	319
110	200	22	106	1.50	1,276	1,168	.487	290
115	200	23	106	1.50	1,276	1,168	.509	277
120	200	24	106	1.50	1,276	1,168	.531	266
125	196.7	24.59	106	3.10	1,276	1,168	.544	255
130	184.6	24.	101.5	5.50	1,222	1,168	.555	254

Stopped in good order, but power reduced beyond maximum, from over-loaded engine.

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* Raised higher steam in boilers.

TABLE IV.

Record of Belt Tests continued on iron pulleys. Best quality single Leather Belt, 12" wide, ¼" thick, weight 34¼ lbs., cut 31 ft. long, stretched 2¼ inches, with 520 lbs. strain; 2 inches added to make length 31 ft. 4¼". Strain = 43⅓ lbs. per inch. December 16, 1886.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Belt. Ft. per min.	$T^1 + T^2$.	Coeff. effect.	Belt ft. per min. per 1 HP.
50	113.2	5.65	120?	?	754?	1,040	.285?	1,600?
60	111	6.66	120?	?	754?	1,040	.336?	1,360?
70	101.7	7.14	120?	?	754?	1,040	.423?	1,267?
80	81	6.96	120?	?	754?	1,040	.351	1,300?
85	Belt slipped off. Tension too light to give any result of value. Main belt also slipped, and 8 inches were cut out of that.							

December 21, 1886. Same belt repeated with strain increased to 750 lbs., or 62½ lbs. per inch. 2" previously laced in, and 2¾ in. more, cut out.

Lbs. on brake.	Rpm.	HP.	Driver rev. per min.	Slip.	Belt ft. per min.	$T^1 + T^2$.	Coeff. effect.	Ft. belt per HP. per in.
50	112	5.60	112	0	704	1,500	.175	1.508
60	112	6.72	112	0	704	1,500	.210	1,257
70	111	7.77	111	0	697	1,500	.245	1,076
80	109	8.72	111	1.80%	697	1,500	.275	959
90	107.2	9.65	110	2.55	691	1,500	.307	859
100	103.4	10.34	109	5.14	685	1,500	.332	795
110	101.2	11.12	108.8	7	684	1,500	.357	738
120	97	11.64	108.4	10.5	680	1,500	.377	700
125	87	10.87	105.2	17.3	662	1,500	.361	730
130	Belt slipped off pulleys. Strain evidently too light to get power of bel							

TABLE V.

Record of Belt Tests continued, December 21, 1886. 12" Belt. continued, wi strain of 1,000 lbs. = 83⅓ lbs. per inch, 2½ inches cut out to butt, grain side pulley.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$.	Coeff. effect.	Belt ft.p min. p HP. 1
50	115.4	5.75	115.4	0	725	2,000	.131	1.513
60	115.4	6.90	115.4	0	725	2,000	.157	1,260
70	113.2	7.92	113.2	0	711	2,000	.161	1,077
80	111	8.88	111	0	697	2,000	.214	942
90	160	9.90	111	1.00%	697	2,000	.234	845
100	109	10.90	111	1.96	697	2,000	.258	768
110	107.2	11.79	111	2.91	697	2,000	.280	709
120	106.2	12.50	110.4	3.85	693	2,000	.300	665
130	102	13.26	108	5.66	679	2,000	.322	614
140	100	14.00	108	7.41	679	2,000	.340	582
145	98.4	14.27	107	8.25	672	2,000	.350	565
150	95.25	14.29	105.7	10.00	663	2,000	.355	557
160	92.3	14.77	105	13.00	660	2,000	.369	538
165	87	14.85	114	17.3	717	2,000	.330	600

Stopped on account of extreme slip.

December 29, 1886. Same belt reversed, flesh side to pulley.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$.	Coeff. effect.	Belt ft. per min. per HP. 1 in.
75	115.4	8.62	115.4	0	725	2,000	.196	1,009
80	114.3	9.14	114.3	0	718	2,000	.210	942
85	113.2	9.62	113.2	0	711	2,000	.223	887
90	112.1	10.09	112.1	0	704	2,000	.236	837
100	111	11.11	111	0	697	2,000	.263	753
110	110	12.10	110	0	691	2,000	.290	685
120	109	13.08	109	0	685	2,000	.315	627
130	107.1	13.92	107.1	0	672	2,000	.342	580
140	106.2	14.87	106	0	667	2,000	.368	538
150	106.2	15.93	106	0	667	2,000	.394	502
160	105.3	16.85	105.3	0	662	2,000	.420	472
170	Belt slipped from pulleys without previous warning of slip.							

December 31, 1886. Same belt, same tension on wooden pulleys, crowned ⅛ in. Grain side to pulleys.

Lbs. on brake.	Rpm.	HP.	Rev driver.	Slip.	Ft. belt per min.	$T^1 + T^2$.	Coeff. effect.	Belt ft. per min. per HP. 1 in.
80	120	9.60	120	0	754	2,000	.210	942
90	120	10.80	120	0	754	2,000	.236	838
100	118.8	11.88	118.8	0	746	2,000	.263	753
110	117.6	12.94	117.6	0	739	2,000	.289	685
120	117.6	14.01	117.6	0	739	2,000	.313	633
130	117.6	15.29	117.6	0	739	2,000	.341	580
140	113.2	15.85	118.8	4.61	746	2,000	.351	565
150	105.3	15.80	118.0	10.76	740	2,000	.353	562
160	93	14.88	112.5	17.33	706	2,000	.348	569

Stopped on account of loss of power from slip, belt still on pulleys.

TABLE VI.

Belt Tests continued, January 3, 1887. Same Belt, cut down to 8½″ width, but laced in same holes as last test, giving apparent strain of 1,000 lbs. or 117.6 lbs. per inch. Dodge's wooden pulleys, crowned ⅛ inch.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$.	Coeff. effect.	Ft. belt per min. to 1 HP. per in.
50	117.6	5.88	117.6	0	739	2,000	.131	1,508
60	117.6	7.15	117.6	0	739	2,000	.160	1,240
70	117.6	8.23	117.6	0	739	2,000	.184	1,077
80	114.3	9.14	114.3	0	718	2,000	.210	942
90	113.2	10.19	113.2	0	711	2,000	.237	837
100	109	10.90	110.2	1.09	692	2,000	.260	762
110	107.2	11.79	111.4	3.77	699	2,000	.280	711
120	Belt slipped off pulleys.							

January 4: Same belt, same strain, repeated on paper pulleys, from Westinghouse, Church, Kerr & Co., crowned ⅛ inch.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$.	Coeff. effect.	Ft. belt per min. to HP. per 1
50	120	6	120	0	754	2,000	.131	1,508
60	120	7.20	120	0	754	2,000	.158	1,256
70	118.8	8.32	118.8	0	746	2,000	.184	1,076
80	117.6	9.41	117.6	0	739	2,000	.210	942
90	116.5	10.48	116.5	0	732	2,000	.236	838
100	115.4	11.54	115.4	0	725	2,000	.262	754
110	115.4	12.69	115.4	0	725	2,000	.290	685
120	114.3	13.72	114.3	0	718	2,000	.315	628
130	113.2	14.72	113.2	0	711	2,000	.341	580
140	112.1	15.69	112.1	0	704	2,000	.367	538
150	110	16.5	111.2	1.09	697	2,000	.391	507
160	108	17.28	112.5	4.00	707	2,000	.404	491
170	Slipped off pulleys.							

TABLE VII.

Belt Tests continued, December 18, 1886. 4-ply Belt, 12" wide, $\frac{3}{16}$" thick, weight 41½ lbs., 31 ft. long. Stretched 13¼ inches, with strain of 520 lbs. or 4[illegible] lbs. per inch. Cut out 9" to butt at 31 ft. 4¼". Iron pulleys.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$.	Coeff. effect.	Ft. belt per min. to HP. per 1
60	120	7.20	120	0	754	1,040	.303	1,256
70	116.4	8.05	116	0.5	729	1,040	.350	1,087
75	113.2	8.49	116	2.40	729	1,040	.369	1,030
80	105.4	8.43	115	8.43	722	1,040	.370	1,028
85	92.3	7.85	106	12.9	667	1,040	.374	1,020
90	Belt slipped off pulleys.							

December 24. Same Belt continued, with 750 lbs. strain, or 62.5 lbs. per inch. Stretched 2" more to butt.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$.	Coeff. effect.	Ft. belt per min. to HP. per 1
60	117.6	7.06	117.6	0	739	1,500	.210	1,256
70	115.4	8.08	115.4	0	725	1,500	.245	1,077
80	109	8.72	115.0	5.22	722	1,500	.266	993
90	105.3	9.48	111.3	8.70	698	1,500	.300	883
100	Belt slipped off pulleys.							

ember 24, 1886. Same belt, 1,000 lbs. strain = 83$\frac{1}{3}$ lbs. per in. 3 in. cut butt.

ke.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$.	Coeff. effect.	Ft. belt per min. to 1 HP. per in.
	120	7.20	120	0	754	2,000	.157	1,256
	120	8.40	120	0	754	2,000	.184	1,077
	117.6	9.41	117.6	0	739	2,000	.210	932
	116.5	10.48	116.5	0	732	2,000	.236	838
	115.4	11.54	116.5	1%	732	2,000	.260	761
	111.0	12.21	115.5	3.90	725	2,000	.277	712
	109.0	13.08	114.5	4.80	718	2,000	.300	659
	101.7	13.22	113	10.00	711	2,000	.307	645

Belt slipped off pulleys.

ving stretched this belt from the beginning 18$\frac{1}{4}$ in., the tests were carried no er.

TABLE VIII.

t Tests continued, December 18, 1886. ——— Link Belt. 12" wide, ally 30 ft. 10" long. Weighed 196 lbs. Shrunk in shop to 178 lbs. and ich in length that 8 inches had to be added to make it meet, at 31 ft. 4$\frac{1}{4}$", strain of 750 lbs., $\frac{15}{16}$ in. thick. Iron pulleys.

on e.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$.	Coeff. effect.	Ft. belt per min. per HP. per in.
	120	6.0	120	0	754	1,500	.175	1,508
	115.4	6.90	118	2.20	740	1,500	.205	1,287
	111	7.77	117	5.18	735	1,500	.232	1,135
	107.2	8.01	116	7.59	729	1,500	.242	1,092
	104.5	8.84	116	9.91	729	1,500	.251	1,049
	88	7.48	116	24	729	1,500	.222	1,170

pped from excess of slip, and fear of shaking down the main shaft, which fterward strongly braced, as shown in illustration, and all its bolts tightened.

cember 29, 1886. Same belt repeated with 1,000 lbs. strain, or 83$\frac{1}{3}$ lbs. per n putting on which a link was taken out, or 1$\frac{1}{16}$ inch.

on e.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$	Coeff. effect.	Ft. belt per min. per HP. per in.
	120	6	120	0	754	2,000	.131	1,508
	117.6	7.05	117.6	0	739	2,000	.157	1,258
	116.5	8.15	116.5	0	732	2,000	.183	1,077
	115.4	9.23	115.4	0	725	2,000	.210	942
	113.2	10.19	113.2	0	711	2,000	.236	887
	112.1	11.20	112.1		704	2,000	.262	754
	109.0	11.99	110.2	1%	692	2,000	.285	693
	108.0	12.97	110.0	1.73	691	2,000	.309	639
	105.2	13.71	109.5	3.93	687	2,000	.330	601
	100.0	14	107.0	6.54	672	2,000	.344	576
	92.0	13.80	106.0	13.3	667	2,000	.341	580

lt slipped very badly, but could not slip off, but burnt against beam orting hanger.

TABLE IX.

Belt Tests continued, December 20, 1886. ——— Belt, canvas, painted slate color, 12″ wide, 3/16 thick, 31 lbs., cut 31 ft. 2″ long, 4-ply, stretched 7¾ inches with 520 lbs., or 43⅓ lbs. per in. 5½ inches cut out to butt at 31 ft. 4¼″. Iron pulleys.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$	Coeff. effect.	Ft. belt per min. per HP. for 1 in.
20	120	2.40	120	0	754	1,040	.101	3,770
25	117.6	2.94	119	1.17%	747	1,040	.125	3,048
30	115.4	3.46	119	3.00	747	1,040	.147	2,590
35	113.2	3.96	119	4.86	747	1,040	.168	2,263
40	113.2	4.53	119	4.86	747	1,040	.192	1,980
45	111	5.00	119	6.72	747	1,040	.212	1,795
50	Slipped off pulleys.							

December 25, 1886. Same belt strained to 750 lbs., or 62.5 lbs. per inch. 3 inches more cut out.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$	Coeff. effect.	Ft. belt per min. per HP. for 1 in.
40	125	5.00	125.0	0	791	1,500	.139	1,898
50	122.45	6.225	122.45	0	769	1,500	.178	1,482
60	122.45	7.35	122.45	0	769	1,500	.210	1.255
70	122.45	8.57	122.45	0	769	1,500	.245	1,102
80	120.00	9.60	120.0	0	754	1,500	.280	942
90	120.00	10.80	120.0	0	754	1,500	.315	838
100	117.6	11.76	117.6	0	739	1,500	.350	754
110	117.6	12.94	117.6	0	739	1,500	.385	685
115	115.4	13.27	115.4	0	725	1,500	.402	656
120	112.00	13.25	115.4	2.68	725	1,500	.401	656
125	Slipped off pulleys.							

December 25, 1886. Same belt strained to 1,000 lbs., or 83⅓ lbs. per inch. 2 inches more cut out.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$	Coeff. effect.	Ft. belt per min. per HP. for 1 in.
80	120	9.60	120	0	754	2,000	.210	942
90	120	10.80	120	0	754	2,000	.236	838
100	117.6	11.76	117.6	0	739	2,000	.262	754
110	115.4	12.65	115.4	0	725	2,000	.287	688
120	114.3	13.72	114.3	0	718	2,000	.315	628
130	113.2	14.72	113.2	0	711	2,000	.341	580
140	112	15.70	112	0	704	2,000	.367	538
150	111	16.65	111	0	697	2,000	.394	502
155	110	17.05	110.5	0.49%	694	2,000	.406	488
160	108	17.28	114.5	5.71	719	2,000	.399	500
165	Slipped off pulleys.							

Discontinued tests with this belt for want of time to try on other pulleys.

TABLE X.

Belt Tests continued, December 27, 1886. 3-ply rubber belt, 12" wide, $\frac{3}{16}$" thick, weight 37 lbs., 31 feet long originally. Cut off 11½ inches to meet at 31' 4¼'', 520 lbs. strain, or 43⅓ lbs. per inch. Iron pulleys.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt. per min.	$T^1 + T^2$	Coeff. effect.	Ft. belt per min. per 1 HP. per in.
30	120	3.60	120	0	754	1,040	.151	2,513
40	120	4.80	120	0	754	1,040	.202	1,885
50	120	6.00	120	0	754	1,040	.252	1,508
60	117.6	7.05	117.5	0	739	1,040	.303	1,258
70	115.4	8.07	115.4	0	725	1,040	.354	1,078
75	Belt slipped off pulleys.							

December 28, 1886. Same belt, 750 lbs. strain, or 62.5 lbs. per in., 6 inches cut out to butt.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$	Coeff. effect.	Ft. belt per min. per 1 HP. per in.
50	120	6	120	0	754	1,500	.175	1,508
60	117.6	7.05	117.6	0	739	1,500	.210	1,258
70	116.5	8.15	116.5	0	732	1,500	.245	1,078
80	115.4	9.23	115.4	0	725	1,500	.280	942
90	115.4	10.38	115.4	0	725	1,500	.315	837
100	115	11.54	115	0	722	1,500	.352	751
110	113.2	12.45	113.2	0	711	1,500	.386	685
120	111	13.32	111	0	697	1,500	.426	627
130	105	13.68	105	0	660	1,500	.456	579
140	Belt slipped off pulleys.							

December 29, 1886. Same belt, 1,000 lbs. strain = 83⅓ lbs. per inch, 4 inches cut out to butt.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$	Coeff. effect.	Ft. belt per min. per 1 HP. per in.
50	120	6	120	0	754	2,000	.121	1,508
60	117.6	7.05	117.6	0	739	2,000	.157	1,258
70	115.4	8.06	115.4	0	725	2,000	.183	1,078
80	114.3	9.14	114.3	0	718	2,000	.210	944
90	114.3	10.29	114.3	0	718	2,000	.237	837
100	114.3	11.43	114.3'	0	718	2,000	.263	754
110	113.2	12.45	113.2	0	711	2,000	.290	685
120	113.2	13.58	113.2	0	711	2,000	.315	628
130	112.1	14.57	112.1	0	704	2,000	.341	580
140	111	15.54	111	0	697	2,000	.368	538
150	109	16.35	109	0	685	2,000	.387	503
160	109	17.44	109	0	685	2,000	.420	471
170	106.7	18.14	106.7	0	670	2,000	.447	443
180	103.4	18.61	103.4	0	650	2,000	.472	419
190	Belt slipped off pulleys. Rubber partially chafed off, from evident over-strain. No slip noticed until belt flew off finally.							

TABLE XI.

Belt Tests continued, December 16, 1886. Rawhide belt, 31 feet long $\frac{3}{16}$" thick. Weighed 29 lbs. 2 inches added to butt, with strain of 520 lbs., or 43" lbs. per inch, at 31 feet 4¼". Iron pulleys.

Lbs. on Brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$.	Coeff. effect.	Ft. belt per min. to 1 HP. per in.
50	120	6.00	120	0	754	1,040	.252	1,508
60	117.6	7.06	119	1.18%	747	1,040	.300	1,270
70	115.4	8.08	118	2.22	740	1,040	.342	1,100
80	111	8.88	116	4.31	730	1,040	.386	986
85	109	9.25	115	5.22	725	1,040	.405	810
90	105.3	9.48	115	8.43	725	1,040	.416	790
95	Slipped off pulleys.							

December 22, 1886. Same belt. 750 lbs. strain = 62.5 lbs. per inch. Lap 2" + cut 2¼" = 4¼ inches cut out to make ends butt

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$.	Coeff. effect.	Ft. belt per min. to 1 HP. per in.
50	113.2	5.65	113.2	0	711	1,500	.175	1,510
60	113.2	6.79	113.2	0	711	1,500	.210	1,256
70	111	7.77	111	0	697	1,500	.248	1,076
80	111	8.88	111	0	697	1,500	.283	942
90	111	10	111	0	697	1,500	.314	836
100	111	11.10	111	0	697	1,500	.353	753
110	109	12	111	1.80	697	1,500	.382	697
120	107.2	12.84	110	2.73	691	1,500	.409	646
130	105.4	13.69	109.4	3.74	685	1,500	.440	600
140	104.5	14.50	109.4	4.38	685	1,500	.466	567
150	103.5	15.52	108.8	4.70	681	1,500	.502	527
160	101.7	16.27	107.8	5.66	675	1,500	.530	500
165	Slipped off pulleys. Speed very slow at start.							

December 22, 1886. Same belt repeated with 1,000 lbs. strain = $83\frac{1}{3}$ lbs. per inch, 8½ inches cut out.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$.	Coeff. effect.	Ft. belt per min. to 1 HP. per in.
70	113.2	7.92	113.2	0	711	2,000	.184	477
80	111	8.88	111	0	697	2,000	.212	942
90	111	10	111	0	697	2,000	.237	836
100	111	11.10	111	0	697	2,000	.263	753
110	110	12.10	110	0	691	2,000	.296	685
120	109	13.08	109	0	685	2,000	.315	628
130	107.1	13.92	107.1	0	673	2,000	.341	580
140	105.4	14.74	105.4	0	662	2,000	.367	540
150	105.4	15.79	105.4	0	662	2,000	.394	503
*160	104.3	16.69	106.4	2%	667	2,000	.413	479
170	103.4	17.58	106.4	2.82	667	2,000	.435	455
180	101.7	18.31	106.4	4.42	667	2,000	.458	437
190	100	19	106.4	5.66	667	2,000	.470	421
200	90	18	113	20%	711	2,000	.417	474
205	Slipped off instantly.							

Speed very slow through this trial as well as last.

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* Steam raised to 75 lbs. at boilers.

TABLE XII.

Belt Tests continued, January 1, 1887. Same raw-hide belt 12" wide, laced in old holes, on wooden pulleys.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$	Coeff. effect.	Ft. belt per min. per 1 HP. per in.
80	114.8	9.14	114.8	0	718	2,000	.210	942
90	111	10	111	0	697	2,000	.236	836
100	115.4	11.54	115.4	0	725	2,000	.262	767
110	115.4	12.69	115.4	0	725	2,000	.288	685
120	114.8	13.72	114.3	0	718	2,000	.315	628
130	113.2	14.72	113.2	0	711	2,000	.341	580
140	113.2	15.85	113.2	0	711	2,000	.368	538
150	113.2	16.98	113.2	0	711	2,000	.394	502
160	113	18.08	113	0	710	2,000	.420	471
170	111	18.88	111	0	697	2,000	.445	443
180	110	19.80	110	0	691	2,000	.471	419
*180	118.2	20.37	116.6	2.91%	732	2,000	.459	431
190	111	21.09	114.4	3	718	2,000	.485	409
200	109	21.80	112.36	3	705	2,000	.510	388
210	109	22.89	112.36	3	705	2,000	.536	369
220	109	23.98	112.36	3	705	2,000	.561	352
230	109	25.07	112.36	3	705	2,000	.587	337
240	109	26.16	112.36	3	705	2,000	.612	323
250	107.2	26.87	110.36	4.59	693	2,000	.640	309
260	Belt slipped off pulleys, and exhausted the power of the engine to such an extent that it was reduced to 8½ inches wide for further trials.							

TABLE XIII.

Record of Belt Tests continued, January 8, 1887. Same belt cut down to 8½ in. wide, re-tested on wooden pulleys. Laced in old holes. Strain = 117.7 lbs. per inch.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$	Coeff. effect.	Ft. belt per min. per 1 HP. per in.
80	120	9.60	120	0	754	2,000	.210	942
90	120	10.80	120	0	754	2,000	.240	898
100	117.6	11.76	117.6	0	739	2,000	.262	754
110	115.4	12.69	115.4	0	725	2,000	.289	685
120	115.4	13.75	115.4	0	725	2,000	.312	633
130	114.8	14.86	114.8	0	718	2,000	.342	580
140	114.8	16	114.8	0	718	2,000	.368	538
150	113.2	16.98	113.2	0	711	2,000	.394	502
160	113	18.08	113	0	710	2,000	.420	470
170	111	18.87	111	0	697	2,000	.446	443
180	111	19.98	111	0	697	2,000	.473	419
190	111	21.09	111	0	697	2,000	.500	396
200	110	22	110	0	691	2,000	.525	377
210	Slipped off pulleys, probably *over-strained* in this test, as it gave less power on the next one, and when weighed at end of that test, the strain was found to be reduced to 660 lbs., or 77.7 lbs. per inch, and to have stretched 1¾ inches.							

* Got up higher steam.

January 4, 1887. Same belt repeated on paper pulleys.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$	Coeff. effect.	Ft. belt per min. per 1 HP. per in.
90	122.4	11.02	122.4	0	769	1,320 ?	.358 ?	837
100	121.2	12.12	121.2	0	761	1,320	.398	753
110	120	13.20	120	0	754	1,320	.438	686
120	120	14.40	120	0	754	1,320	.477	628
130	118.8	15.44	118.8	0	746	1,320	.517	580
140	117.6	16.46	117.6	0	739	1,320	.557	539
150	115.4	17.31	117.6	1.87%	739	1,320	.585	512
160	Belt slipped off pulleys. Tension taken as weighed at end of test, 660 lbs., or 77.7 lbs. per inch.							

TABLE XIV.

Belt Tests continued, December 18, 1886. One plain cotton canvas belt, just as received, 12" wide, 21½ lbs., $\frac{3}{16}$" thick. Iron pulleys, cut 30 feet 3" long. Stretched 13¼" with 520 lbs. strain, or 43⅓ lbs. per inch.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$	Coeff. effect.	Ft. belt per min. per HP. per 1 in.
30	120	3.60	120	0	754	1,040	.151	2,514
35	120	4.20	120	0	754	1,040	.177	2,154
40	120	4.80	120	0	754	1,040	.202	1,885
45	115.4	5.17	120	3.83	754	1,040	.217	1,750
50	Belt slipped off pulleys.							

December 20, 1886. Same belt, sized and prepared for lining.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$	Coeff. effect.	Ft. belt per min. per 1 HP. per in.
20	120	2.40	120	0	754	1,040	.101	3,770
25	117.6	2.95	120	1.66	754	1,040	.124	3,067
30	115.4	3.45	120	4	754	1,040	.145	2,620
35	115	3.69	120	4	754	1,040	.155	2,400
40	109	4.44	120	9.17	754	1,040	.187	2,037
45	Belt slipped off pulleys.							

December 21. Same belt, 1,000 lbs. strain. 2½" cut out.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$	Coeff. effect.	Ft. belt per min. per 1 HP. per in.
30	120	3.60	120	0	754	2,000	.079	2,514
40	117.6	4.72	117.6	0	739	2,000	.104	1,880
50	115	5.75	117.6	0	739	2,000	.128	1,542
55	114.5	6.30	117	2.13%	735	2,000	.142	1,400
60	113.6	6.79	115.5	1.64	725	2,000	.154	1,281
70	113.2	7.38	115.5	2%	725	2,000	.168	1,179
75	112	8.40	115.5	3	725	2,000	.191	1,036
80	111	8.88	115.5	3.89	725	2,000	.202	980
85	110	9.35	115	4.35	722	2,000	.214	926
90	109	9.81	114.5	4.80	718	2,000	.225	879
95	106.7	10.14	114.5	6.83	718	2,000	.233	849
100	103.5	10.35	113.8	9.05	711	2,000	.240	827
105	90.3	9.16	112.6	17.38	708	2,000	.213	927
110	Belt slipped off pulleys.							

TABLE XV.

Belt Te-ts continued, December 17, 1886. One 12" cotton-leather belt, 4-ply, 37¾ lbs., cut 31 feet, 2" long. Stretched with 520 lbs. strain, or 43⅓ lbs. per inch, 2¼ inches, or to 31 feet 4¼", on iron pulleys.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$	Coeff. effect.	Ft. per min. per HP. for 1 in.
80	120	9.60	120	0	754	1,040	.404	942
90	120	10.80	120	0	754	1,040	.454	838
100	113.2	11.32	116.6	2.90%	732	1,040	.490	776
105	111	11.65	115.5	3 90	725	1,040	.511	747
110	107.2	11.79	111.6	5.63	725	1,040	.517	738
115	105.3	12.10	115.8	9.07	725	1,040	.530	719
120	103.4	12.4	119	12.94	747	1,040	.528	723
125	Belt slipped off pulleys.							

December 23, 1886. Same belt, 750 lbs. strain, or 62½ lbs. per inch. Cut out 2¾ inches to butt.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$	Coeff. effect.	Ft. per min per HP. per in.
80	120	9.60	120	0	754	1,500	.280	942
90	120	10.80	120	0	754	1,500	.314	838
100	118.8	11.88	119	0	747	1,500	.350	754
110	117.6	12.94	119	1.10	747	1,500	.380	698
120	116.5	13.98	118.8	1.10	746	1,500	.412	640
130	115.4	15	117.7	1.95	739	1,500	.446	590
140	113.2	15.85	116.6	2.91	732	1,500	.479	554
150	111.1	16.67	115.5	3.81	725	1,500	.505	522
160	109	17.44	114.5	4.80	718	1,500	.535	494
170	105	17.9	112.6	6.48	708	1,500	.556	474
180	102.5	18 45	110.8	7.49	695	1,500	.584	452
190	100	19	114	12.30	717	1,500	.583	453
200	97	19.40	113.2	14.3	711	1,500	.600	440
210	89	18.79	111	19.37	697	1,500	.593	444

Stopped for excess in slip and loss of speed. Belt still on pulleys.

TABLE XVI.

Belt Tests continued, December 23, 1886. Same 12″ cotton-leather belt on iron pulleys. 1,000 lbs. strain, or 83⅓ lbs. per inch, ¼ inch cut out to butt.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$	Coeff. effect.	Ft. belt per min. for 1 HP. per in.
100	120	12.	120	0	754	2,000	.262	754
110	117.6	12.94	117 6	0	739	2,000	.289	695
120	116.5	13.98	116.5	0	732	2,000	.315	628
130	115.4	15	115.4	0	725	2,000	.341	580
140	114.3	16	114.3	0	718	2,000	.368	538
150	113.2	16.98	113.2	0	711	2,000	.394	502
160	111	17.77	112.5	1%	707	2,000	.415	477
170	110	18.70	112 5	2.22	707	2,000	.436	453
180	109	19.62	112.5	3.11	707	2,000	.458	432
190	107.1	20.35	112.5	4.80	707	2,000	.475	418
200	105.3	21.06	112.5	6.40	707	2,000	.491	403
210	100	21.00	110	9.09	691	2,000	.500	394
220	95.2	20.95	109.5	15	688	2,000	.506	394
230	88.2	20.29	104.1	15	659	2,000	.507	390

Stopped in good order (belt on), from loss of power.

January 1, 1887. Same belt repeated on wood pulleys, laced in old holes.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$	Coeff. effect.	Ft. belt per min. for 1 HP per in.
120	120	14.40	120	No slip whatever in this test.	754	2,000	.315	628
130	118.8	15.44	118.8		746	2,000	.342	580
140	118.8	16.63	118.8		746	2,000	.368	538
150	117.6	17.64	117.6		739	2,000	.394	503
160	116.5	18.64	116.5		732	2,000	.420	471
170	115.4	19.55	115.4		725	2,000	.444	444
180	115.4	20.70	115.4		725	2,000	.470	420
190	114.3	21.70	114.3		718	2,000	.499	397
200	113.2	22.64	113.2		711	2,000	.525	377
210	113.2	23.77	113.2		711	2,000	.551	359
220	112.1	24.66	112.1		704	2,000	.579	342
230	112.1	25.78	112.1		704	2,000	.605	328
240	111	26.64	111		697	2,000	.634	314
250	111	27.75	111		697	2,000	.660	301
260	107.2	27.87	107.2		673	2,000	.684	290
270	Stalled the engine. Belt in place and no sign of slip.							

TABLE XVII.

January 3, 1887. Belt Tests, continued. Same cotton-leather belt, split down to 8½ inches. Laced in old holes. 1,000 lbs. strain, or 117 lbs. per inch.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$	Coeff. effect.	Ft. belt per min. to 1 HP. per in.
130	120	15.6	120	0	754	2,000	.341	608
140	120	16.8	120	0	754	2,000	.367	540
150	117.6	17.64	117.6	0	739	2,000	.394	503
160	116.5	18.64	116.5	0	732	2,000	.420	471
170	116.5	19.72	116.5	0	732	2,000	.444	445
180	115.4	20.77	115.4	0	725	2,000	.472	419
190	114.3	21.71	114.3	0	718	2,000	.500	397
200	113.2	22.64	113.2	0	711	2,000	.525	377
210	113.2	23.77	113.2	0	711	2,000	.551	359
220	111	24.42	112.2	1.08%	705	2,000	.572	346
230	111	25.53	112.2	1.08%	705	2,000	.600	331
240	105.3	25.27	106.31	0.95%	668	2,000	.624	313
250	Belt slipped off driver from engine slowing down.							

January 4, 1887. Same belt, on paper pulleys. Laced in old holes.

Lbs. on brake.	Rpm.	HP.	Rev. driver.	Slip.	Ft. belt per min.	$T^1 + T^2$	Coeff. effect.	Ft. belt per min. to 1 HP. per in.
100	122.4	12.24	122.4	0	769	2,000	.264	754
110	121.2	13.33	121.2	0	761	2,000	.289	685
120	120	14.40	120	0	754	2,000	.315	628
130	120	15.64	120	0	754	2,000	.342	585
140	120	16.80	120	0	754	2,000	.368	539
150	118.8	17.82	118.8	0	746	2,000	.394	502
160	118.8	19.00	118.8	0	746	2,000	.423	470
170	117.6	20.00	117.6	0	739	2,000	.446	443
180	116.5	20.97	116.5	0	732	2,000	.472	419
190	115.4	21.93	115.4	0	725	2,000	.500	400
200	114.3	22.86	114.3	0	718	2,000	.523	377
210	113.1	23.73	113.1	0	711	2,000	.550	359
220	111	24.42	111	0	697	2,000	.578	343
230	110	25.30	110	0	691	2,000	.602	328
240	101.4	24.40	107.8	6.30%	675	2,000	.595	335
250	Belt slipped off from loss of speed. Scales applied, and tension found to be same as at first = 1,000 lbs.							

It might be noted here, that the power to drive the brake shaft is not included in these tests, but would be very slight, probably not over $\frac{1}{10}$ H. P.

DISCUSSION.

Mr. C. Seymour Dutton.—The report is rather severe on the link belt, and perhaps justly, from the point of view from which the experiments were made. But it happened that in the shops with which I am connected we had occasion to try a link belt for a specified purpose, and it was so satisfactory in that case that I wish to say a word in regard to it. We had occasion to run a cross-shaft some distance to drive a 60-inch lathe. The pulleys were about 36 inches in diameter and the shafts perhaps 15 feet apart. The ordinary belt used for this purpose strained out very rapidly, so that we had great difficulty in driving it. A representative of a link belt came along and recommended it for that purpose, agreeing to put it in on trial, which he did. It was a 5-inch belt which we used, and the belt has proved very satisfactory. I think two sets of links were taken off after it was run two or three weeks. But now for two or three months the belt has run with entire satisfaction and has not required any further attention, so that I can say that for that purpose the same width of belt that we used before has been entirely satisfactory, while the ordinary belting was very unsatisfactory. Those of us who are interested in motive power, I think, would be glad to see experiments on larger belts and considerably higher speed than these. Of course, this was not undertaken by Mr. Webber's experiments at all. There are a number of things which seem to need consideration in that. In running at very high speeds with very small pulleys, there seems to be a special difficulty. I remember one instance in which we ran a 12-inch double belt over a 60-inch pulley below running 300 revolutions, and over a 30-inch pulley perhaps twelve or fifteen feet from it and directly above it. In this case, for a large portion of the way around the pulley, by holding a light on the farther side of it, you could see the light between the pulley and the belt, notwithstanding that the belt was drawn up pretty tight before commencing to run it. It may have been partly due to the stiffness of the belt. It may have been partly the centrifugal force of it—it undoubtedly was; and it is also claimed that a wide belt will carry a film of air between the belt and the pulley. It is a thing about which I would be glad to know more.

Mr. H. R. Towne.—The questions involved in the testing of belting are so numerous and complex that it is very difficult to cover them all in any ordinary system of experiments, and I think that this report shows rather more than the usual deficiency in this respect. The set of experiments on belting which I made twenty years ago showed one thing conclusively, namely, that the condition of the atmosphere is a large factor in the transmitting energy of leather belts. Leather belting is a capital hygrometer;

it measures the dryness or the moisture of the atmosphere with great delicacy, and the same belt tested under identical conditions in all other respects will give a very different result on a day with the atmosphere moist or "muggy" from what it will on another day with the atmosphere dry and capable of absorbing moisture freely and rapidly from objects with which it comes in contact. I think that any critical and conclusive series of tests of belting, at least of leather belting, must be continued through a long enough period of time to cover the average range of variation in the humidity of the atmosphere to give perfect results. These variations are of less consequence, and possibly of little or no consequence, with belting in which rubber is the chief component. What their effects are on belting in which linen or other vegetable fiber is the chief component, I do not know. Conceivably, however, they would also be affected a good deal by the moisture of the atmosphere. I think that the tests reported in this case are also deceptive so far as they pretend to make a comparison between leather belting and other kinds, by reason of the very unfavorable condition of the leather belt. A well-made leather belt certainly holds very high rank, if not the highest rank, as a belt transmitter, and yet here the results would indicate rather the contrary. A perfectly new leather belt is stiff, inflexible, does not readily adapt itself to the pulley, and has a peculiar quality of surface which we all recognize by the touch, and which is much less conducive to adhesion than the surface of the same belt after it has been run for some time and has been treated with the ordinary dressing. Therefore, to institute a comparison between a *new* leather belt, which requires this preparatory use and treatment in order to bring it to its full efficiency, and another belt, like a rubber belt, which requires no such preliminary preparation and has its highest efficiency when first put on, is deceptive. The leather belt should first be run long enough to bring it up to its maximum efficiency.

Again, the adoption of the tension on the belt, per inch of width, as a determining factor in all cases, is, in my judgment, erroneous. The initial tension put upon a belt is rarely maintained. The ordinary belting material is elastic to a certain point; but is usually worked under conditions which carry it past its elastic limit, and induce permanent stretch. We all know this as indicated by the frequent taking up of belting and cutting out a portion of its length. Variation in length, again, is dependent on the matter I have already alluded to—namely, the condition of the atmosphere as to moisture.

Finally, no formula yet offered can, in my judgment, be accepted as universally applicable to belts, whether horizontal or vertical. Stretching becomes of much more consequence in a vertical belt than in the case of a horizontal belt. Generally, therefore, I think that the conclusions of the paper, while useful in some respects as giving some additional light on this subject, cannot be accepted without modification and the exercise of judgment.

Mr. Olin Scott.—It seems to me that each belt-maker should publish tests which are reliable in regard to the belts he makes, having special regard to these different conditions which have been suggested. We should know whether a set of figures refer to the belting as used wet or dry, vertical or horizontal, and whether they apply to a crossed belt or to a belt with a quarter-twist driving by one edge only, each man stating his own case.

Mr. Daniel Ashworth.—We have had the various points connected with this belt problem touched upon very forcibly, but in our experience during many years in putting up belts, especially high-speed belts, as applied to dynamos and fans, the main trouble has not been from the conditions spoken of in the paper or the debate, but it has arisen from the different kinds of belt furnished from the same house, and almost from the same roll of belting. We start out with the belting which is represented as possessing all the merits of adhesiveness, lightness, and flexibility. We are enthusiastic with the result in this case, but upon the next application, which is decidedly a parallel case under all conditions, we find that the result is entirely negative or disappointing. I know of no more uncertain factor than the matter of belting as applied to motive power. While we recognize the fact of the condition of the atmosphere varying, yet we find that while we have one first-class belt which is eminently satisfactory, we have a dozen that are disappointing, and from the same house and under the same conditions. It seems an exceedingly difficult problem, not so much from the conditions of use, but from the condition of the material placed in our hands at different times from the same houses, having a pre-eminent reputation.

Mr. F. H. Underwood.—In the experiments described by Col. Webber there seems to have been no detailed description given of how the belts which he tested were prepared or made. And as the new leather belt described by Col. Webber was cut from what we call oak-tanned side-stock, that is, after taking out a strip, say, 24 inches in width, from the hide, measuring 12 inches each way from the center, this 12-inch belt was then cut out, one strip from each side-piece; this would make the belt heavier and more elastic than if cut from the center of the same hide, as the fiber is coarser and not so dense along the sides as it is in the center. But from the side stock you have an uneven belt in thickness, and one which is apt to stretch more on one side than the other, unless great care is used in so cutting the 12-inch strip that the lines are equally taut on each side. In this case the belt was uneven in thickness and not cut properly, and therefore imparted a swaying motion to the belt when running, and part of its contact was lost by running over the edge of the pulley, first on one side and then on the other. This belt, if put to a high rate of speed, say 5,000 feet per minute, would impart a lateral motion to the shaft, and cause an unequal wear and strain upon the bearings. The belt mentioned in table 7 was made from cotton duck, folded to make the requisite number of plies, and then fastened longitudinally with rows of stitches one-fourth of an inch apart, the belt then being filled with composition of boiled linseed oil and red lead, which soon comes off when put to use, and nothing but the plain cotton is left. This belt was quite stiff, and when first run rode upon the stitches and prevented a perfect contact. The "link belt" mentioned is made in the following manner: The links are cut out of leather, about 2 inches in length and 1 inch in width; holes are then punched in this link about 1 inch from center to center; the link so made is then compressed as much on each end as the succeeding link will lap over on it, so as to bring as much edge surface next to the pulley as it is possible to do. The requisite number of links to make the width required are placed side by side, with an iron rod running through them, about $\frac{3}{16}$ of an inch in diameter. In the belt tested there were 33 of these links in width to make the belt 12 inches wide. The rod has a head on one end, and after being put through the links is riveted on the other. The belt does not lie perfectly flat on the pulleys, standing as it does on the edge, as the pieces of leather do not conform readily to the circle of the pulley touching only here and there. Owing to its great weight, stiffness, and the great loss of contact surface, by reason of the spaces between the links and the places where the belt does not touch, this belt could not be made to show very much of a result in horse-power.

The canvas belt, 4-ply, mentioned, was made of solid woven cotton, and a mixture of linseed oil, plumbago, etc., had been worked in and dried under pressure. Powdered soap-stone is then used over the surface of the belt on both sides, to prevent its sticking while standing in the roll or coil. It drives well for a time, but stretches a great deal. The rubber belt mentioned was made of cotton duck, folded and cemented together with rubber in the usual manner. The proportion of pure rubber was found to be very small, and under the severe test to which it was put, the rubber peeled off from the canvas. The raw-hide belt mentioned is tanned on the surfaces only, the intermediate portion being as near raw-hide as it is possible to have it. The hides are left in the bark liquor a few days only, just long enough to allow the liquor to penetrate a sufficient distance into the hide to prevent its surfaces from cracking; it is then put into a machine and fulled with a preparation of oil, tallow, tar, etc., to make it pliable and soft, and at the same time to preserve it. After taking the hide from this machine, it is smoothed and set out in the usual manner, and the belt cut out, stretched, cemented, and finished. This belt was cut, as was the first belt, from the side of the hide, and in running under the tensions mentioned, stretched very unevenly—so much so that it ran over the edges of the pulley. The preparation of tar used makes the belt hug the pulley very closely when first run; but as soon as this preparation wears off the belt will not transmit as much power in proportion as it would when first put on. This belt is very flexible and soft, but is liable to stretch a great deal, and during the above test the belt tore apart at the lace-holes. This belt is not tanned in the same manner as the one described by Mr. Lewis in his paper read at the May meeting in Chicago, 1886. By reason of its great pliability, this belt kept on the pulleys until the final test, and the liability to stretch and lose its grip is shown by the difference of tests in tables 12 and 13. Col. Webber has already fairly described the process of making the cotton-leather belt; it is simply a combination of two old, tried, and well-known principles, both of which have been in use for a great many years. By this combination many of the difficulties heretofore experienced with leather, cotton, and rubber belting are overcome. The cotton is woven solid any number of plies while the warp is under a steady tension; producing a belt which is of great tensile strength, equal in quality, and of uniform thickness and strength throughout, without laps, and is balanced from end to end.

A recent test of the tensile strength of an 8-inch 4-ply cotton belt showed that it would withstand a strain of 10,790 pounds. These tests by Col. Webber were made for the purpose of getting at data sufficient to formulate a rule whereby results could be guaranteed under different arcs of contact, tensions, and belt speeds, and I think the proposed formula given by Col. Webber a good and sufficient one. Each and every manufacturer of belting ought to publish his own formula, and then, with a more thorough knowledge of how the belt should be made, it would be a simple matter for a purchaser of belting to judge where it would be best for him to procure the belt adapted to his needs. The action of centrifugal force upon a belt running at different rates of speed is clearly shown in the difference between the result shown in table 3, where you will find a record-test of a belt 5 inches wide, with a strain of 117 pounds per inch of width running over 24-inch iron pulleys. The driving pulley was then increased to 46 inches, the belt lengthened out, and put on under the same strain. I do not know what the result would have been with the other belts which were tested running under the above conditions, as they varied a great deal in thickness and weight. It would not have been expedient to have tried the link belt running at any such rate of speed, as it was considered unsafe while it was running on the 24-inch pulleys, and the shafting had to be braced up as shown in the cut. Where a belt is of a flexible, soft nature, I do not think that the centrifugal force will make very much difference with its running; and I am inclined to think that the reason why a belt lengthened out as described by Mr. John T. Hawkins in his discussion of Mr. Lewis's paper,* where he referred to an experiment made on a wood-turning lathe, is not so much as he imagines because of the centrifugal action, but rather of the heat developed by the belt slipping or creeping over the small pulley, which, together with the working up of the fiber, tends, after a few minutes' run, to heat and extend the fiber of the material of which the belt is composed and lengthen it enough to reduce the strain upon the bearings, so that it will run comparatively cool at a high rate of speed. The point I wish to make is, that the belt lengthened out, as described above, by running at a high rate of speed, will, as soon as it is stopped, contract to its original tension and length, and upon starting again the same conditions are observed. Another point was clearly established by Col. Webber's test, and that was,

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* Vol. VII., Trans. A.S.M.E., page 583.

that a belt will drive fully as much, if not more, with the flesh side next to the pulley, as it will with the grain; that is, with the same amount of work put upon the flesh, it will drive more than the grain side. It is almost impossible to select a belt which has to be made from so many and varying qualities of leather as may be found in the different hides which will have a perfect contact upon the pulley, no matter which side is used. And it would only be possible to procure a belt of uniform thickness made of leather by splitting it down in thickness from end to end, and immediately you have done that, you materially reduce the strength of the belt. The more elastic and pliable the leather is made, the better it will hug the pulley; and I think the reason why the new leather belt did not show a better result was on account of its great solidity and harshness. It is almost impossible, at the present time, to sell to a consumer or user of belting a soft, pliable piece of leather, in the shape of belting, for immediately he comes to purchase he wants to see the belt of the width required, and no matter how fine a piece of leather may have been used in its manufacture, if it is soft he complains that the belt was cut from the flank portion of the hide, and any amount of argument will not make him think otherwise. The result is, that going across the street to a neighboring establishment, where they use a different class of leather, which feels hard and solid, no matter if it had been cut from the flanky portion of the hide, he purchases the belt, takes it home, and hands it to his foreman with the remark: "There's a belt that is solid and substantial, and I guess will last a long time." When the foreman comes to put on this belt, which, by the way, is to run at a high rate of speed, he is unable to make the belt drive the machine intended, and sends for his employer to know if he can tell him the reason why; but he cannot inform him, and it is only when they call in a practical belt-maker that they find out that the belt is so hard and stiff that it will not readily bend around the small pulleys, and that the centrifugal force will immediately throw it off the pulley. A thin, pliable belt is immediately substituted, and found to do the work in a perfectly satisfactory manner, and the purchaser just commences to realize the fact that he knows very little about belting, and is thereafter ready to listen to what is told him in regard to the matter. An instance similar to that above described came under the writer's notice only a short time since, and it goes to show clearly that to produce a belt which will give the best results as a transmitter of power, the material used must be of a flexible nature, in order to produce perfect contact with pulleys. The belt must lead straight on to the pulleys, no matter how much it is elongated, and when finished must be evenly balanced, straight and true in its running, and transmit the required power with the least strain upon the belt and bearings.

Another point established by Col. Webber's test is, that as you increase tension and speed, so you increase the driving capacity of the belt.

Mr. John T. Hawkins.—Mr. Underwood is a little disposed to undervalue my idea of the question of centrifugal force in the action of high-speed belts, and I would like to emphasize this fact: that in making the tests or experiments referred to, with the lower or driven spindles 1 inch in diameter, carrying 4 pulleys, and the overhead or drivers 36 inches, the 4-inch belts were tightened, by taking up, until, at 2,000 revolutions per minute of the spindles, the bearing could not be kept cool by any means known to me; while, without any other change than to increase the speed to 5,000 per minute, they would not only cool down from their previously heated condition, but continue to run cool indefinitely. I think this proves conclusively that the centrifugal action, at such speeds, releases the bearing from a very great part of the pressure due to the tension of the belt.

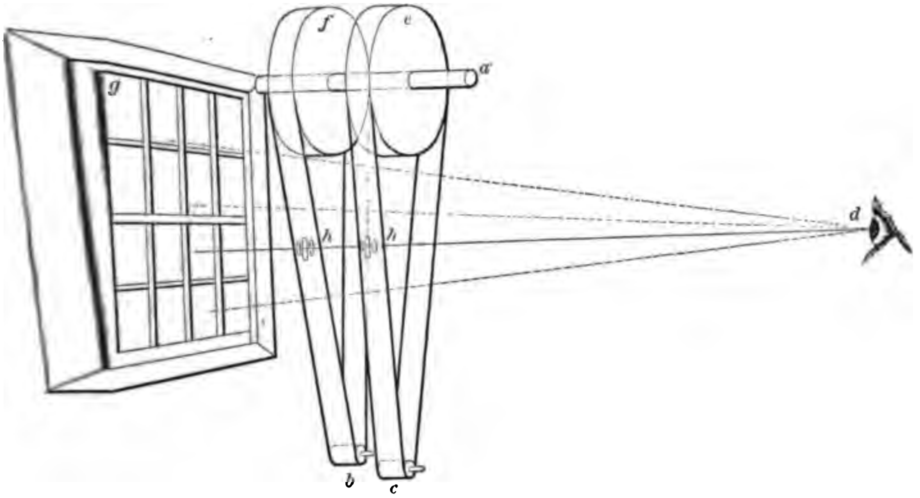

FIG. 164.

I would like to call the attention of the meeting to a method, arrived at in prosecuting these experiments, for determining the amount of creeping or slip. I have done nothing of a very close character in these experiments, but indirectly fell upon this method of determining how much a belt may slip, or creep, from the difference of tensions, upon a pulley, under given conditions.

The illustration (Fig. 164) represents an overhead countershaft *a*, carrying two 36-inch pulleys *e*, *f*, the belts therefrom running on to two separate 4-inch pulleys *b*, *c* below, the upper ones being the drivers. Applying a brake to one of the lower spindles would tend to make the belt creep upon the upper one, by increasing the difference of tensions in the belt; and I happened to discover one day, standing in a position *d*, such that the line of sight would pass through both rows of belt-lacing holes *h*, *h* (these holes being not entirely filled by the lacing), and terminating in a window *g* opposite, that when both lower spindles were unloaded, owing to a slight difference of diameter in the upper drivers, the pencil of light passing through the lacing-holes would have a slow motion downward, as indicated by the lower dotted line, until it disappeared at the lower pulleys, returning again, after a proper lapse of time, at the tops. By applying the brake to one of the lower spindles the motion of the light pencil might be made to remain stationary as projected on the window, or to move upward or downward at will. I merely mention the experiment as something that may be of value in other future experiments upon belting, in which it might be desirable to know the amount of slip or creeping of a belt under given conditions.

Mr. H. R. Towne.—Before we drop this subject I want briefly to comment on one point which has been mentioned, namely: the carrying of air between the belt and the pulley at high velocities. In a paper by the late Robert Briggs, published in the *Journal of the Franklin Institute* for February, 1868, in reporting the experiments to which I previously referred, Mr. Briggs threw out the suggestion that the entrainment of air between belt and pulley might be a factor of importance in the transmitting power of belts at high velocities, and suggested that the fact could be easily ascertained and a remedy applied, in part at least, by putting a stripper close to the pulley, and between the pulley and the belt, of such form as to strip any air carried along by the belt from it as it passed on to the pulley. I think it would be interesting and useful for any others who may make experiments in this field to apply such a stripping device as Mr. Briggs suggested in order to determine whether or not there is anything important in it.

Mr. C. E. Emery.—I wish to take issue with the form of the expressions used to obtain the width in inches and horse-power. The correct form of an equation expressing the relations referred to is well settled, and it would add very much to the value of any series of experiments and prevent constantly recurring confusion in attempting to compare results if all writers would publish their results in the form of constants applicable to the established formulæ on the subject. As a matter of course the driving side of the belt must be under greater tension than the other, and there must be a gradual reduction of the extra tension from the point of contact on the pulley on the driving side to the point of contact where the slack side of the belt leaves the pulley. This reduction is not, however, in an arithmetical ratio. The friction of the belt upon the pulley is in all cases proportioned to the pressure of one surface upon the other. This pressure is necessarily greater on the strained side and the tension of the belt transmitted to the next section to produce frictional contact is reduced and so on throughout the entire arc of contact at a rapidly reducing rate expressed by the ordinates of a hyperbolic curve readily integrated by hyperbolic logarithms. The subject was very exhaustively discussed by Professor, now President, Morton, when he was editor of the *Journal of the Franklin Institute.* (See that *Journal* for January, 1868.) He discusses a long series of experiments made by Mr. J. H. Cooper and others by Mr. Henry R. Towne and finally concludes that for ordinary belts of average thickness (say 0.22 inch), a maximum working strain of 66⅔ pounds per inch in width could be permitted. On this basis Rankine's formula (Applied Mechanics) takes the following form:

$$W=66\tfrac{2}{3}\,(1-10^{-0.003206a})$$

in which W=the working strain transmitted per inch of width in pounds.

a=the arc of the contact in degrees.

This investigation was accepted by Mr. D. K. Clark, and he presents, on page 750 of his well-known *Manual,* a table showing the driving power of leather belting calculated therefrom. A statement of these various considerations and a copy of this table will be found at page 56 in the general report of the Judges of Group XX., International Exhibition, written by the speaker.

Mr. F. H. Richards.—It would seem there is no one detail of machine construction which has in its application a greater variety of circumstances than belting. It seems to me impossible, for that reason, that any rule can give the correct relative value of different kinds of belting for all purposes. Let me give you an instance of that. Some years ago, on shafts 4 feet apart, a belt 1½ inches wide, and running from a driving pulley 12 inches diameter on to a driven pulley 1½ inches diameter, was run at a speed of over a mile per minute (6,000 feet). Owing to the centrifugal force, as it was supposed, the belt seemed to touch the small pulley only about one-half inch on the circumference. The durability of this belt was about a week, and under the circumstances that was considered quite satisfactory. I could give you many other instances of belts operated under special conditions, tending to show that any rule which will apply to the ordinary transmission of power (as from one line shaft to another) is totally valueless for universal use.

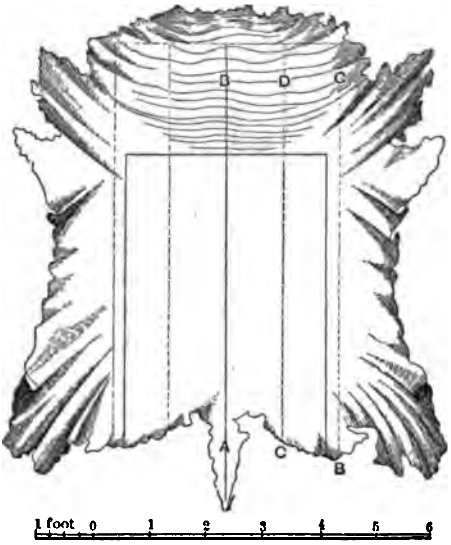

FIG. 165.

Mr. F. H. Underwood.—A good deal has been said about belts which show different results being from the same grade of material and of the same manufacture.

That is easily explained. In Fig. 165 we have a drawing of a hide showing the different portions ; A-B is the tight line running through the hide in the center. There is also a tight line or side running where the flank comes up and stops, as shown by the dotted lines B-C. Now, a great many belt-makers have an idea that if you want a four-inch belt, you must split the hide right through the middle, at A-B, and cut off as many strips as you like. That is not right. The only place you can take it from and have it run perfectly true is 2″ each side of the center. No manufacturer could afford to make belting and sell it at the price they have to, and cut it all from that portion. To produce a straight belt we must work equidistant from this line A-B, and that line B-C. You put a lot of strips together cut from A-B and C-D and it will form a circle. But if you want to cut this into belts, you must strip it into something very narrow. If you want a 36″ belt, you must go 18″ each way from the center. If we want a belt which is wide, say 48″, we commence at the center here and measure out 24″, and putting this side A-B, and the flank portion B-C together, we make a double belt which ought to draw on each side alike. The reason why you have trouble in getting the same kind from one manufacturer is that you cannot afford to pay him enough. In making these tests Col. Webber suggests taking each belt as it comes from the manufacturer and running it two or three hours before making the test. Now, the atmosphere will have a great effect upon leather, or, in fact, upon any material, I do not care what; even if it is iron it will have more or less influence on it. And you cannot get a belt but what will be influenced by the atmosphere more or less, so that on a dry day it will run differently from what it will on a wet one.

CCLII.

ON A NEW METHOD OF MAKING TUBES FROM SOLID BARS.

BY GEO. H. BABCOCK, NEW YORK CITY.
(Member of the Society.)

WE have all heard of the Irishman's method of making a cannon by " taking a hole and pouring melted iron around it," but it has been reserved for a German actually to do a similar, or apparently, an even more difficult thing—to take a hole and force a bar of wrought iron or steel around it! We are familiar with the process of drilling and punching for perforating metals, but here comes a man who, ignoring all such makeshifts, by "external applications only"—as a skillful physician treats an internal congestion—rolls a hole into the middle of a solid rod, thus forming it into a tube! What makes the hole? Apparently, like the boy's whistle, it "does itself."

Seriously, this is no joke. The specimens which I have the privilege of exhibiting to the Society tell their own story, and scarcely need the evidence of the eye witness who saw them made, and who loaned them for this purpose. As yet the process has not been worked in this country, but it is in practical operation in Germany. It is the invention of two brothers named Mannesmann, of Remscheid, and the *modus operandi* is as difficult to understand and explain as was Gifford's injector, or Bohnenberger's gyroscope.

The apparatus necessary to effect the result consists of two rollers slightly conical, the axes of which are in different planes—or form two lines in a twisted surface—their nearest approach being at or near the bases of the cones. The surface of the cones may be threaded in such a way that they tend to draw a body rolling between them towards their larger ends. The bar to be operated upon should be approximately round, and its end is to be inserted, while hot, between the cones, its axis being intermediate at all points to the axes of the rollers. The action of the cones is to draw out and twist the bar, during which operation a hollow forms in its axis, and when the bar emerges it is a tube with a somewhat rough but approximately cylindrical and concentric bore, the surface of which shows a decided twist.

Among the exhibits is a bar which was drawn down at each end before going through the mill, so that no action took place at these ends. This bar, after cooling, was broken, and shows conclusively, by the color and character of the bore, that no tool and not even the air touched it during the operation, the interior having the same appearance as the fracture.

The tubes thus formed are applicable directly for some purposes, but by a proper formation of the rolls behind the bases of the cones, or additional pairs of rolls, with suitable mandrel or mandrels, this tube may, at the same heat, be expanded and finished into a regular weldless boiler tube, or gas pipe, as some of the specimens shown; or this may be done at a separate operation.

That the metal is not harmed by this rather rough handling may be inferred from several specimens shown, of tubes which have undergone operations of expanding, flanging, flattening, etc., which would try the temper and quality of any respectable tube. Specimens are also shown of brass and copper tubes; made by the same process.

DISCUSSION.

Mr. William Hewitt.—This matter has a peculiar interest to me, as it reminds me of a little experiment which I tried about twelve years ago. I devised a machine similar in some respects to Mr. Mannesmans' apparatus with the idea of rolling in a single pass a bar or billet of perhaps one inch in diameter, to a wire rod of about quarter-inch in diameter. By the present method this requires about 13 or 14 passes. It was one of those things which was going to revolutionize the art, but this has since been done in quite a different way. The way in which I proposed to do it was to pass the bar axially between two revolving hyperboloids in a direction intermediate to that of their axes, the latter being so arranged that the generating lines would fall into the same plane where the rolls came together, converging or rather tapering slightly from the small end of the rolls to the larger end; the degree of taper depending of course upon the amount of reduction which I proposed to give the metal. The rolls had the appearance shown in Fig. 166.

Suitable guides were provided for retaining the metal in its proper position; the action of these rolls it is obvious was such as to draw the metal in and through the apparatus, reducing it with a spiral motion. I constructed a small hand model with which I experimented upon some lead bars. The model was a very rude affair, and hardly adequate for a fair experiment. It was too light; the rolls would spring slightly from the guides, and the metal got jammed up between the rolls and the guides and was cut to pieces against the sharp edges of the latter. I did not succeed in getting a piece entirely through the rolls, but managed to make some fair points. I therefore abandoned the idea of drawing the metal out into a rod in this way, and devised another machine for the purpose of simply pointing; this being a necessary operation before the rod can be drawn down into wire. I attempted to do this by introducing the end of the rod between the inclined faces of two vertical dies moving alternately and very rapidly up and down in slides. I first tried some iron rods, but these were mashed into no shape, the fibers of the material being actually torn asunder; I then tried some Bessemer steel and managed to get some very good points. I noticed, however, that the tips all had a deep conical depression or cavity, and I presume if the experiment could have been carried farther, forcing the metal through the dies and over a mandrel, that it would have been rolled into a pipe or tube, but as I was not particularly interested in the manufacture of tubes, and the apparatus as a pointing machine was so uncertain in its operation, I abandoned it.

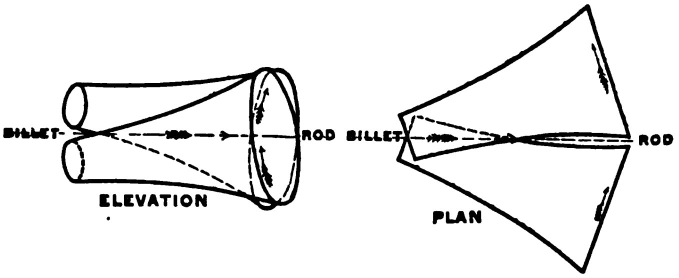

Fig. 166.

I am also reminded of the experiment in the rolling of rivets which my father, Mr. Charles Hewitt, tried many years ago,—indeed it was before my time,—and I cannot, therefore, give you an exact description of the apparatus which he employed, but I believe he attempted to roll them in a manner similar to that in which a puddle ball is rolled into a bloom in the ordinary rotary squeezer, the blanks being introduced in one end in a continuous stream and dumped out at the other end in finished rivets. The rivets all had that peculiar conical depression or cavity in the end, and the thing was abandoned on that account.

Mr. Francis H. Stillman.—Having received information that there were parties in New York City who had made tubes in this method described by Mr. Babcock, I endeavored to get a meeting with them and was finally referred to a lawyer who proved to be an attorney for one of the Mannesmann Bros. From papers which he has allowed me to examine the following points are taken.

Mr. Mannesmann states that the rollers should have abrupt conical faces placed like skew rolls as used for polishing surfaces and I believe for straightening bars. The working faces of the rolls should be provided with spiral ribs or corrugations which give a more effective hold upon the blank, and owing to the fact that they gradually become coarser in pitch, the spiral ribs have the effect of progressively drawing the metal from the outer portion of bar, inward and forward. A later invention allows the tube to be expanded after drawing to even larger than original bar. Iron and steel should be very highly heated preparatory to being subjected to the action of the diagonal rolls.

The employment of a so-called "holding back bar," which is placed with one end against the center of the heated blank at the point where it is being acted on by the rolls (Fig. 167), renders it unnecessary to reduce the diameter so much as when the holding-back effect is obtained entirely by the convergent faces of the rolls, so that the blank suffers comparatively little or no reduction in diameter.

The pointed mandrel, the shape of which is shown in Fig. 167, is to be used on brass or soft and ductile metals which cannot be so highly heated.

The headed mandrel also serves the purpose of a plug (see page 571) to prevent the entrance of air into the tube and interfering with the welding, or drawing, by the formation of oxide. Mr. Mannesmann has made further inventions by which the same results are to be performed between revolving hemispheroidal rolls.

Mr. Thos. S. Crane.—I would like to ask if any one present can tell us the speed at which this tube is formed or the rod is traversed through the rolls. I suppose the friction would generate more or less heat. I would like also to ask whether the tube as i is formed is projected upon the point of the mandrel—whether that is the understanding from the sketches.

Mr. George Schuhmann.—From what I have learned about this new method I understand that the tube is not projected against the point of the mandrel. The metal parts before it reaches the mandrel. In fact, tubes can be made without any mandrel at all, and the latter is only used to smooth the inside of the tubes and to enlarge them. Although there does not seem to be any wear on the point, I should think there must be a considerable amount of friction on the bearing part of the mandrel.

Mr. Wm. Kent.—The rolling hyperboloids are in constant use at the National Tube Works at McKeesport, Pa. I saw them in Scotland five years ago at the tube works of A. & J. Stewart, Coatbridge, and the invention was credited to Mr. Matheson, then of the National Tube Works. It is curious that a process should be used for finishing tubes which should later be discovered as a process which would make a tube.

Mr. F. R. Hutton.—I might also add a point which has come to my own knowledge. Prof. Reuleaux has taken up anew the subject of transmitting power by means of compressed air, and proposes that for that purpose compressed air should be applied of a great deal higher tension than we are in the habit of using. I think he proposes that a pressure of fifteen hundred pounds to the square inch should be used at Frankfort which will enable him to carry his pressure through very small pipes. His hope and expectation is that a Mannesmann tube thirty to thirty-five feet in length would enable him to use the compressed air in these small pipes with very much fewer joints in his conducting pipes than would be required with a shorter length of the ordinary lap-welded tube.

Dr. Robert Grimshaw.—Prof. Sweet called attention yesterday to the fact that some one whom he knew had made a machine for straightening spindles, using those small rolls, but that the machine was not a success, because if you were not careful with it you would roll a hole in the spindle.

Mr. Geo. M. Bond.—I believe the Medart Patent Pulley Company of St. Louis have a straightening machine on that principle.

So far as I could judge from their circular, the wheels were conical instead of hyperboloids, the action of the straightening rolls drawing the shaft from one end of the machine to the other. I have never seen the machine in operation—simply having had its operation described to me.

Mr. J. F. Wilcox.—A machine similar to the one Mr. Bond speaks of was patented about 1878 or 1879 by Mr. Seaman of Pittsburgh and is now in use on a very large scale at Akron, Ohio, for making hot rolled shafting. The rolls are conical—not hyperboloids, and it has been used there as a commercial success for the last four or five years. It is also used by Park Brothers in Pittsburgh, and they had this same trouble in making finger bars for harvesters where the end had to be cut off, leaving four or six inches of waste. It has been used and used successfully. We have used a number of feet of it where we require high speed. It is far superior to the old Jones & Laughlin cold rolled shaft. I believe Jones & Laughlin have put in two of these machines in the last two years.

Mr. Wm. Kent.—I remember seeing Seaman's straightening machines in Pittsburgh, Akron, and other places, and my recollection is that it was a series of cylindrical rolls with collars. I would ask Mr. Wilcox if there are any modifications of that machine. I never saw the conical one.

Mr. Wilcox.—The earlier machines were made with collars. The later ones are turned conically but with such a slight cone you would think by the eye that they were cylindrical. Unless you put a calliper on, they do not appear conical. They are turned up—I should hazard a guess upon it—about fourteen inches in diameter, tapering down probably a quarter of an inch in a length of thirty inches.

Mr. Kent.—They have abandoned the collar system, then?

Mr. Wilcox.—They have abandoned the collar system.

Mr. Allan Stirling.—When I mention the name of Mr. Henry Burden, of Troy, the members will recognize the name of one of the great inventors of the country, and particularly in the iron trade. I think that his horseshoe machine as used at present is one of the most wonderful machines in existence. Mr. Burden is also the inventor of the Burden Squeezer, and in experiments which he made in connection with squeezing puddle balls it has come to my knowledge that he used three vertical conical rolls. The ball was put in at the top (I believe at the small end of the cones) and gradually worked through and came out at the bottom in the shape of a billet ready to be rolled. I am glad to speak of Mr. Burden on this occasion and to pay a tribute to his memory as one of the great inventors in this field.

The President.—Mr. Stirling cannot tell us if those rolls were placed on parallel axes or otherwise?

Mr. Stirling.—I am not sufficiently familiar with them to say.

Mr. F. H. Richards.—The reference by Prof. Hutton to the opinion of Prof. Reuleaux respecting compressed air for transmission of power recalls some experiments once made in this country in that direction. The air was compressed at two steps, first to about 150 lbs., and next to upwards of 2,000 lbs. pressure per sq. inch. The first compression generated a high temperature in the machinery, but during the last compression no trouble was experienced from that cause. The air was conveyed several hundred feet in wrought-iron pipes, coupled with straight threads and copper washers between the ends; and was stored in a series of wrought-iron reservoirs similarly connected. The maximum pressure was maintained for months at a time without any leakage. There is no serious difficulty in putting together the piping and apparatus, provided a few simple precautions are always observed. Fine workmanship is not required; but the metal must be sound, and ample force must be applied,—sufficient, in fact, always firmly to imbed metal to metal. The "fits" (joints) must be made tight by pressure, not by hand fitting. This principle was successfully applied not only to the piping, but also to the valves and cocks.

The results attained warrant the opinion that air at high pressures is a most perfect spring, giving out very nearly the whole power put into it. Between the pressures of 300 and 1,500 lbs. the changes of temperature are slight; and allowing 200 lbs. for friction and other losses, there is 1,000 lbs. available for use. Of course, a return circuit should be used, in order to avoid the freezing of the engines, and excessive heating in compression. In our experiments, however, the air was not returned.

The engine which gave the best results had a stroke 5 times the diameter of cylinder, this being about 3 inches; and the highest pressure used on the piston was about 1,200 lbs. My own belief is that the air should be both compressed and expanded on the top of fluid columns, which lie on the piston of the compressor or engine, as the case may be. Besides this, there should be a surplus of fluid, a little passing through the valves at each stroke, to pack them. Oil, notwithstanding its tendency to foam, is probably the best, as it effectually disposes of the problem of lubrication, and is taken up and carried through the pipes by the air, in considerable quantity.

It is probable that the loss by friction in pipes will be against

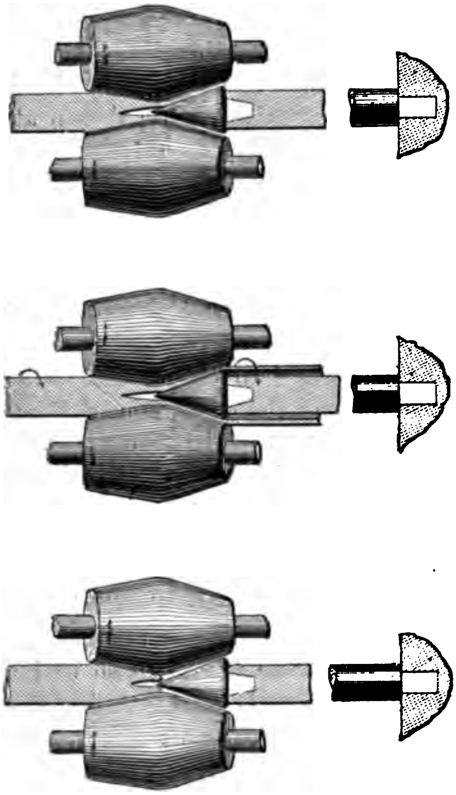

FIG. 167.

the use of air for transmission through long distances, since at such high pressures it is a comparatively heavy body. In this respect, and as to fluidity, steam will, I think, prove the superior. But in some situations, air offers important advantages, and these will become much more available, if, as the paper indicates, perfectly sound pipes can be supplied by the new method at a much lower cost.

Mr. Geo. H. Babcock.—I would say in reference to several remarks that have been made, that Mr. Mannesmann, who is now in New York, promised that he would be present at this meeting and be prepared to answer any question which might be asked in regard to the process.

Mr. Crane inquires as to the speed. Mr. Mannesmann tells me that they have rolled tubes 2 inches in diameter and 24 feet long, I think, at one heat, and he thinks that they could make them still longer if they could conveniently hold their mandrel; that the speed with which the tube comes out is a question entirely of the velocity of the rollers, and that he does not see any limit to the length of tube which can be drawn at one operation excepting the question of holding the mandrel properly. I understand that this mandrel is socketed in a screw and is allowed to revolve, but that it does not revolve at the same speed with the tubes at all points, causing a twist in the tube. You will notice that these samples all show a sort of spiral—not exactly fiber but something like fiber. The mandrel is held firmly by resisting points. (See Fig. 167.)

It is reduced as it passes into the conical rolls; and that reduction, in connection with the continuous shifting of the point of action, is what is supposed to explain the peculiar effect in making this opening. If no mandrel is used the opening will be made as is shown in the specimens, through the bar. I have seen a tube about the size of my finger and 12 feet long with an opening the whole length of it made in that way.

Mr. Kent refers to Mr. Matheson's straightening device for tubes. I am familiar with that and its operation. Mr. Matheson is, I think, now in Europe for the purpose of investigating this method. His rolls are hyperboloids, bearing the whole length and operating upon tubes with no reduction in diameter. Of course he found no interior action.

With regard to the shafting machine: Mr. Wilcox has explained why it does not make the shafting hollow, because the rolls are nearly straight, very slightly inclined, and there is no material reduction in diameter of the shafting; therefore no such action can take place on the interior as is found in the Mannesmann machine. The inventor explains the action thus (Fig. 168): The lines of force radiating from the two compressing points tend to produce a rupture at the

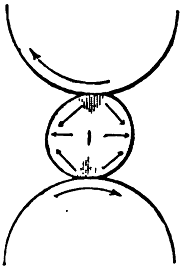
FIG. 168.

center just the same as occurs in hammering large shafting which is frequently found to be either porous or ruptured in the center by the action of the hammer on the exterior. As the roll and bar revolve, the rupture, so to speak, revolves also, and produces a continuous bore. There is also the drawing forward action, mentioned by Mr. Stillman, particularly at the end of the bar, and after it has reached the mandrel. But that alone does not explain why a tube is made when no mandrel is used, nor how the bore can start in the interior of a bar, as in some specimens shown.

CCLIII.

WHAT ARE THE NEEDS OF OUR NAVY?

BY H. ASHTON RAMSAY, BALTIMORE, MD.

(Member of the Society.)

It is now more than two decades since the great shock of arms, which the nations of the old world viewed at first with only half-concealed pleasure, in the hope that it would dismember the young republic. Later on their feelings changed to awe, surprise, and forced admiration at the sight of the vast armies and navies called into existence as if by magic, as they read of the great battles which were fought ashore and afloat, quite equaling, if not eclipsing, anything which the world had before seen in the way of practical military operations, although conducted by a professedly unmilitary people, the genius of whose country was opposed to large standing armies and navies. Now that our country is reunited, with all her people working together for the common weal, and keeping abreast of the world in progress and improvements in all peaceful arts and industries, in turning our eyes to the east, we cannot help observing the unusual activity among the great naval powers of Europe in constructing the most powerful engines of destruction. We see iron-clads, torpedoes, and great guns with capacities to hurl tons of metal through the air, with ranges and penetrative power before supposed to be impossible. Does it not behoove us, under these circumstances, to look to our own defenses, as these great iron and steel-clad monsters may at any time be turned against us; and whereas it is opposed to the established principles on which our great civil republic is founded to keep up large standing armies and navies, it is of vital importance that we should provide ourselves with all the most improved engines of war, and do what we can to foster and build up the necessary engineering establishments for their production. This question has to be met technically by the mechanical engineer, and I have thought it would be a suitable subject to be discussed by this society, and especially appropriate at the present meeting in the capital of the country.

A great many opinions have been advanced of recent years, in view of the rapid improvements made in the power of guns and the effectiveness of torpedoes, as to the kind of vessel to be built for the navy. Now, it would occupy too much time to discuss this question in all its bearings, and besides, it has been done over and over again by much abler heads than mine, and my object is simply to bring the subject before the society so as to invite discussion on it, believing it to be one that comes peculiarly within the province of its discussions. All the modern instruments of war are of a mechanical nature, and present some of the most difficult problems in mechanics, metallurgy, hydraulics, chemistry, and pneumatics to be solved by those who would be successful in grasping this subject in all its details. Take a modern Moncrieff mounted gun, for instance, with its various mechanical appliances. There must be, first, the machines to construct the gun and carriage and manipulate the parts; then the several devices for lifting and loading the projectile, to elevate and depress the gun; to trail or submerge it altogether; to take up the recoil after the fire; bringing in play all the mechanical powers and the application of hydraulics and pneumatics; and the same can be said, on a still larger scale, of the great engine of war called an "iron-clad" or a "torpedo-boat," brimful of mechanical devices.

But to come to the question, What are the present needs of our navy? As the nations across the water have a greater stimulus than we on this side to provide themselves with the most powerful naval appliances, let us watch them and keep pace with them, at least to the extent of having some examples of every variety of war vessel, gun, or torpedo, but let us produce them ourselves, and improve on them if possible, and there is ample engineering skill and ability in this country to accomplish it, if given the opportunity. It has been said in Congress that it was useless to appropriate money to build vessels, when more and more powerful guns were being invented, capable of penetrating the heaviest iron-clads that could float. In answer to this, could not cannon-balls always penetrate wooden war-ships, where the splinters ensuing would do more damage than the missile itself? Ships were never invulnerable to shot, but, like a soldier in the field, must take their chances.

What we seem to need in the way of a navy in peace times is one or two of each established type of vessel and gun, to act as a nucleus—in addition some of these should be constantly under construction at private works, in order to stimulate this line of engineering, and enable us to be prepared not only to build for ourselves, but, some fine day, when the tariff is removed from iron and steel, to take the place of our old mother England, and build vessels for our South American neighbors; yes, and even for England.

The so-called new navy seems to be starting right. We want, first of all, fast cruisers or commerce-destroyers, which will have the heels to run away from iron-clads. Some of this class of vessel have been recently constructed. Even the much-abused *Dolphin* seems to be a tolerable success, and, considering the bad name she has been given, one is quite surprised to see such a fine-looking vessel, and formidable, although only intended for a dispatch-boat. The *Atlanta* is quite up to the type she represents on the other side. By the way, as we are always comparing the speeds of our vessels with the English, we should have a measured mile and try our vessels over this, just as the English do, in order to make the comparison fair. Of course, we know this forced trial, under the best and most favorable circumstances, is two to three knots better than the vessel will do on an average at sea. Still, it is a standard, and when we know what a vessel will do at her best, it is easy to compute what allowances are to be made for less favorable conditions. The other cruisers recently contracted for by Mr. Whitney appear to be pretty close copies of the *Namiva Kan* and her class, built by the English, representing the most successful specimen of this type of vessel, which is also in the right direction. The smaller light-draught gunboats also seem to meet our needs. But while this commencement is all right, we are losing time in not having under way several, say at least three, of the most powerful steel-armored vessels for the defense of our coast from the attack of foreign iron-clads. These vessels take many years to build, and until we have them, our coast and valuable harbors may be said to be defenseless. These vessels should be quite equal to the *Italia* and *Lepanto*, of 14,000 tons, or the English *Trafalgar*. Great attention should also be paid to torpedo-boats and torpedo-boat destroyers. The power of the heaviest gun carried in war-ships is unquestionably greater in comparison with the strongest armor in use for resistance than ever before, if we are to accept the experiments at Shoeburyness and other places, where all the advantage is on the side of the gun; but it is well known that fighting guns at sea against a moving enemy is very different from target practice. Speaking of target practice, I should like to see experiments instituted with a view to ascertaining the effect of firing at shields inclined at different angles. From my observation in the *Merrimac-Monitor* fight, I am convinced that the inclination of the resisting armor will greatly diminish the effect of the shot. On the *Merrimac* we had only 4″ of armor, yet the eleven-inch shot from the *Monitor* all glanced, with one exception, and this shot struck us, point-blank, between wind and water, right on the knuckle, where for a short distance the shield was vertical, and this shot, although it did not get through into the vessel, broke the iron, and sent off large splinters from the wood backing, and forced down one of the deck carlines into the engine-room. It is very desirable to know whether all iron-clads should not have inclined shields, and if so, what is the best angle to give them. To me it looks as if the inclined sided shield or citadel, with a submerged barbette battery, will prove to be the outcome of all the present experiments as the best and most effective type of iron-clad. The English iron-clad *Ben Bow* is a near approach to this type. In addition to the several classes of vessels before mentioned, I should think it would be advisable to have some armored vessels without batteries, constructed with a special view to ramming and discharging torpedoes. These vessels would be most effective for harbor defense against iron-clads or other vessels attempting to force their way in our harbors.

My own practical experience in these matters goes back twenty odd years ago, and as from my constant business occupation in other channels since that period I have not had time to keep as fully posted as I would have liked in these matters, I therefore feel somewhat diffident in venturing these remarks, but I have purposely dealt only with generalities, and tried to avoid questions which are in dispute. But we are all interested in building up the navy of this country, and in doing it with our own people. If these remarks shall have the effect of arousing some further interest in the subject among our members, I will feel as if something had been accomplished.

DISCUSSION.

Mr. Thos. S. Crane.—Will Mr. Ramsay kindly tell us if the shield of the *Merrimac* was of railroad iron, as I have been informed?

Mr. Ramsay.—No, sir; plate iron.

Mr. Crane.—Plates fastened on to an iron or wooden frame?

Mr. Ramsay.—Fastened on to a wooden backing. The timbers were twelve inches square. They were inclined from the extreme beam of the vessel at an angle of thirty degrees and these timbers were laid in a transverse direction on that angle. Then there were oak planks which were three to four inches thick at right angles to the direction of the square timbers, and then the iron was placed at right angles to the direction of the oak planking, and the whole was bolted through and through. Oak plank and southern pine were used.

Mr. Crane.—Did the glancing shot have the effect of tearing off any of these planks?

Mr. Ramsay.—No, sir; one of the shots which struck the vessel on an angle broke through the plates. The plates, I must tell you, were made of different thicknesses. We could not get them all of one thickness; but in some places there were four plates each an inch thick; in other places there were two sets of plates each two inches thick.

Mr. E. P. Stratton.—I would say for the information of the gentleman who preceded me that the projectile which made such destruction in the case of the *Merrimac* was a shell, not a solid shot, and at the present time we have guns which shoot with over twice the velocity and fully three times the penetrative force of the guns which we had at that date, 1861. Under these circumstances the penetrative power of guns seems to have surpassed the wielding power of ships to carry sufficient thickness of armor to resist the penetrative power of the largest cannon. If there are any doubts in this matter at the present time it certainly seems to be on the side of the guns.

Mr. Ramsay.—I am aware of that; but the suggestion which I make, it seems to me, holds just as well with shot as with shell. Only shells were used in that action.

Mr. Wm. Kent.—It seems to be the tendency of the times on the other side of the ocean, if not on this, to seek for three things in combination in a war vessel—first, the most powerful gun that can be made; second, the most impenetrable armor—so impenetrable that the most powerful gun cannot pierce it; and third, the highest speed. Now, I think that in the present stage of engineering it is almost impossible to produce a vessel having that combination. I will not say that it is impossible, but to produce a vessel to-day which should carry an armament of the most powerful guns in existence, should have the most impenetrable armor in existence, and should be of higher speed than any English vessel afloat, would cost not less than five millions of dollars; and then that vessel, if so built, would probably have too great draught to enter any of our harbors. Therefore, I think the solution of this problem is to abandon the attempt to combine all those elements in one vessel. Mr. Ramsay, I think, has very well brought out the idea of dividing those elements—having commerce destroyers as one class of vessel, and heavily armored vessels for ramming as another. The defense of the Port of New York should be first considered. There is a larger amount of property to be laid under contribution there than in any other spot on this continent. To New York the immediate danger is that some long range gun may be stationed at some point where we cannot drive her away, and lay New York under contribution. It seems to me that the most practical way to proceed would be to build a vessel, leaving out one of those three elements—say leave out speed. It is the immense weight of engine and boiler and coal that adds to the draught of the large vessels at the present time, and if we sacrifice the speed, having it say five miles an hour, and build a floating fort that should have the largest guns and the most impenetrable armor, but not much speed, it could float around in the shallow water near Sandy Hook, and no vessel belonging to an enemy would dare approach it for fear of its guns. None of their guns at far range could penetrate its armor. The other vessels, of course, would be the light cruisers—commerce destroyers, which would have a different task. It would be their business to run away when they came near a big iron-clad, but the others could stay anchored like a fort, except that they could float around and change their place. A fort which could change its location around a harbor is the idea. I think such an idea was in the mind of Edwin A. Stevens some thirty years ago when he designed the Stevens Battery.

Mr. E. P. Stratton.—There is one statement I desire to make regarding the "needs of our navy." The nation to which we have to look most to as a probable adversary is Great Britain. In the event of war with her, or, in fact, with any of the first-class powers of the earth, the United States would require as a prime necessity a full fleet of fast cruisers, to prey upon the commerce of our adversary. England to-day controls over one-half of the carrying trade of the world, and she is vulnerable to a first-class power having a navy, to the extent of her commerce at least. A fleet of fast cruising naval vessels, in the event of war with Great Britain would soon accomplish results, to which none other of the grea powers of Europe are liable. England and Ireland have increase so rapidly in population that for a number of years these island have not produced sufficient food to feed their people, and thei support is only maintained by a close commercial intercourse wit other nations. Hence, with a powerful fleet of American cruiser in time of war, we could soon produce an effect on the populatio of England and Ireland which would be fearful to contemplate.

Mr. C. E. Emery.—An officer of the navy, whose position prevents him from taking part in the public discussion, suggests tha the key-note of the matter has not yet been touched. It is th apathy of Congress to the necessity of improving our navy. W all know that the average granger believes that there should be n navy. He would rather expend what little money can be obtaine in improving harbors in inaccessible places, on public buildings i his district, or in encouraging new railroads, or something of tha kind. Every member of the Society can aid in overcoming thi apathy and be a missionary of good in explaining to his Congressman the necessity of encouraging the officers of the Governmen by putting means at their disposal which will enable them to utiliz the information they have already obtained, and apply Yanke genius and improvements to the advances made by others at so much labor and expense abroad and thereby secure still better results.

Mr. Allan Stirling.—This discussion has, so far, mainly been about armor—the best methods of constructing armor for defense. Now, those who have looked into this matter know that the English Government is doing a great deal more than simply improving their armor; they are attending to the construction of the great merchant steamships which are built in the United Kingdom, and are having them built on a system which will make them very useful in case of war. The advantage of a fast unarmored ship is very great. In the first place it can choose the vessel with which it will deal. If it should meet a heavy armored vessel it can simply say, "Good day, sir; I don't care about having anything to do with you," and steam away, the armored vessel being unable to run as fast as this one. But should it meet a merchantman or a less formidable adversary, it at once destroys it or brings it into port. One of the important things for our nation to do is to encourage the construction of very large and very fast ocean ships which in case of emergency could be used as the English Government is systematically intending to use their merchantmen.

*Mr. Ramsay.**—In closing the debate, I have only to answer the gentleman who made the statement in regard to shell being used on the *Monitor* instead of solid shot. I am aware that it has been so stated. Still it seems to me that the idea is pertinent to solid shot (as well as to shell) to make experiments in firing at inclined shields, instead of vertical shields, because if we can neutralize to some extent the effect of the shot by inclining the shield, of course we save that much weight in the vessel. It is not necessary to have the metal so thick. The observations made by myself referred to establishing experiments where the shields were to be inclined instead of vertical. Such experiments may have been instituted, and it may have been apparent that they would not be as beneficial as I would think. All experiments which I have noticed, principally in England, were with vertical targets instead of inclined ones.

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* The discussion of this paper bore so close a relation to the matters touched upon in the discussion which follows of No. CCLIV. that readers are also referred to that paper, entitled "Our Coast Defense, its Cost and its Mechanical Problems;" by Jos. Morgan, Jr.

CCLIV.

OUR COAST DEFENSE, ITS COST AND ITS MECHANICAL PROBLEMS.

BY JOSEPH MORGAN, JR., JOHNSTOWN, PA.
(Member of the Society.)

MUCH has been said upon the question of coast defense, and little new matter can be presented, but the subject is of such importance and the engineering problems are so largely mechanical that they are worthy the attention of this Society.

The work to be done may be classified as follows:

1st. Construction of foundations and other masonry and earth works for protection of guns and appurtenances.
2d. Armor protection for same.
3d. The structural metal and machinery for mounting and working the turrets and guns.
4th. The construction of guns and carriages.
5th. The building of armor-clad vessels.
6th and 7th. The installation of submarine mines.
8th. Torpedo-boat defense.

This paper is intended to open a discussion on coast defense and to call attention to important facts which should be familiar to every one, or about which there appears to be misinformation. Upon the general subject there have been many and able official reports, embracing in the list those of the Chiefs of Engineers and Ordnance of the army.

The Army Armament Board, December, 1884.
The Gun Foundry Board, December, 1884.
The Fortification Board, January, 1886.
The Senate Committee, Ordnance and War Ships, January, 1886.
The House Committee, Ordnance and War Ships, March, 1886.

These are all full of interest, and are no doubt in the hands of many members of the Society. There is little difference in the conclusions reached. The reports treat the various subjects involved in national defense exhaustively, and much of the matter of this paper is taken from them, particularly from that of the Fortification Board, with which the writer is most familiar.

A very able paper is that of Lieutenant Griffin, U. S. A. Engineer Corps, in the Journal of the Military Service Institution of the United States, which gives in tabular form the cost of defenses of 8 principal sea-ports, in which ports there is destructible property to the value of 4,500 million dollars, and shows "the total cost of adequate defense is about 1.3 per cent. on the value of the property protected." The estimate of the Fortification Board, page 28 of report, for defenses for these 8 ports is somewhat greater, as floating batteries were added to the defense. This increased estimate is, however, barely 2 per cent. of the value of destructible property, or about the premium paid for ordinary fire insurance on a good risk for a three years' policy.

The whole estimate of Fortification Board for twenty-seven ports is as follows:

Land Defenses,	Masonry and Earth Works........	$31,863,000
	Armor..........................	20,300,000
	Structural Metal................	3,320,000
Armament,	Guns and Mortars...............	28,554,000
	Carriages......................	9,411,800
Floating Batteries and Armament	....	18,875,000
Submarine Mines and Adjuncts	....	4,334,000
	Torpedo Boats..................	9,720,000
		$126,377,800,

this amount to be expended in annual appropriations of $11,000,000. The total sum required for defense to be spread over a time of twelve years is less than the payments made in three years for extravagant pensions. For our population of fifty millions, it is less than $2.52 per head. The total cost is little over twice the estimated cost of coast defense to beat off a navy in 1840, when the most powerful ship cost $550,000. To-day the most powerful war ships cost "$5,000,000. Ships have increased in cost nine fold and cost of defenses only two to three-fold."

The question—"What shall we do with our surplus?" should be answered with a little common sense. Have we ceased to think our national honor is worth anything? Have the people who spent four thousand millions to keep our country united so far changed that a hundred millions is too high a price to pay for safety? And is not present and future protection the most important duty which the Government owes its people? Why should our defenses remain obsolete?

Apply to this question the judgment which a manufacturing concern applies to its business. Would a management with good sense neglect to put in new plant and allow its factories to remain filled with machinery of capacity unequal to competition with other plants springing up around it? Such a concern would soon go out of business. We should have defenses complete, with all preparations necessary to defend our shores from any force, land or naval, which may be brought against us. Proper means being provided, we would be able at short notice to beat off an enemy with little cost to ourselves, with great damage to him and small chance of success on his part.

What are these means? And why must they be provided in advance?

It cannot be done after war is declared, as it will take years to do. There are dozens of great steamers in the possession of England, France, and Germany capacious enough to carry a regiment each, and swift enough to land a force on our shores a week after a declaration of war.

The question of a national organization of militia with provision for prompt mobilization is, therefore, an important one.

It is perfectly feasible to-day to land a force on our shores. It would, however, be impossible to maintain such force except in close communication with some convenient port occupied as a base of supplies. To prevent such occupation and the levying tribute upon our great coast cities is the object of our coast defense, or to quote Bernard and Totten's report, 1826:

"Fortifications must—

"1st. Close all important harbors against an enemy and secure them to our military and commercial marine.

"2d. Deprive an enemy of all strong positions where, protected by naval superiority, he might fix permanent quarters in our territory, maintain himself during the war, and keep the whole frontier in perpetual alarm.

"3d. Cover the great cities from attack.

"4th. Prevent, as far as practicable, the great avenues of interior navigation from being blockaded at their entrance into the ocean.

"5th. Cover the coastwise and interior navigation by closing the harbors and the several inlets from the sea which intersect the lines of communication and thereby further aid the navy in protecting the navigation of the country.

"6th. Protect the great naval establishments."

It is taken for granted the proper line of impassable defense is not an off-shore fleet. Although a naval officer is quoted as saying, "We need no forts, but our coast defense should be intrusted to a navy," his position is erroneous. A navy to protect our extended shore of over 4,000 miles, and ready to meet a squadron of the enemy anywhere on the coast, means a powerful fleet at many points, and involves an outlay immensely greater than coast fortifications. To meet a possibly superior naval force and join it in battle with no alternatives but victory or destruction afloat, followed by surrender ashore, is not the task to be given to our naval force.

"The navy should be free to seek the high seas," to accept the issue of battle when victory is possible, to destroy commerce of the enemy, to harass the flanks of a superior force, cut off its supplies, and, when close pressed, find, under powerful guns ashore, an efficient shelter. It should be a flying column, not tied to a harbor defense line, but free to go even to the enemy's own shores and thus keep his fleet at home. It should be powerful enough, when concentrated, to break up a blockade of our important ports.

To permit this concentration of the naval force, the defense of any port must be complete without a navy. A navy may assist, although it should not be essential to the defense. It can follow up crippled vessels, destroy them after repulse by fortifications, and make defeat in attack a total ruin to the enemy.

The defense of harbor entrances by guns afloat, exposed to destruction from shot and torpedoes, and mounted upon that most costly, complicated, and delicate of gun carriages, a steamship, is an expedient only to be adopted when other location for the guns is not available.

This is not the age of chivalry, when one offers an enemy equal terms of battle, but the age of science and mechanism, when results are calculated, and we offer an adversary destruction if he accepts battle on our prepared ground. The defense, therefore, should be, when practicable, at the harbor mouth by permanent works. To be complete and impregnable there must be obstructions in the channel, to prevent a run by, and powerful guns ashore to keep the enemy's fleet from removing the obstructions or quietly anchoring and bombarding cities near the shore without danger to them selves. There must be obstructions because "modern ships of war with high speed and heavy armor can pass any shore battery if the channel be clear." There must be guns, otherwise the enemy will destroy the torpedo obstructions by countermining. The guns must be heavy, for while light guns would prevent countermining by unarmored vessels, the heavy armored vessels would proceed with this work, unless prevented by armor-piercing guns on shore.

It is a popular delusion that a good torpedo defense can be made by a few old barrels filled with powder or dynamite, and anchored in the channels. As a matter of fact, such an improvised defense would amount to very little. Efficient obstruction requires careful preparation and study of the ground. It is not enough to have a single line of mines, "but the whole area swept by the guns of defense" must be rendered dangerous. The mine most used is a water-tight spherical welded steel case containing about 100 pounds of gun-cotton, either sunk to bottom in shallow water, or anchored and floating below surface in deep water. It has an electric fuse and is connected by cable to the shore, and can either be exploded from shore at will of operator, or be made dangerous at his pleasure and exploded by the contact of a vessel. It can, again, at any time be made non-explosive by the operator ashore, who also can determine if its electric circuit is complete, or if it is being tampered with. This electric circuit can be made to sound an alarm or fire a gun directed at the location of the torpedo in case an attempt is made to fish it up under cover of a fog or darkness. These electric cables must be carried ashore, under water, to prevent their being cut off by shot, to casemates suitably secure provided for the operator and his electrical apparatus.

What a good torpedo defense should be is treated by General Abbott, an authority on that subject, in the supplemental report to Fortification Board. He says:

"For such defense there must be in hand before the outbreak of war—

"1st. The mines and their accessories, cables, electrical instruments, etc.

"2d. Submarine galleries and operating casemates.

"3d. Instructed corps of operators. Without them failure would be certain.

"4th. Flanking guns and electric search lights."

The total sum to provide about 5,000 necessary mines, 50 operating casemates, 200 electric search lights, is estimated at about four million dollars, a very small sum. Congress has so far provided about 1,400 torpedoes and very little of the other apparatus.

The casemates and under water protected galleries for cables require time to build and should be provided at once. The whole expense of complete apparatus for torpedo protection of all our important harbors is smaller than the amount often spent on a single government building.

For flanking guns to prevent unarmored vessels and small boats fishing up or countermining torpedo obstruction, our present forts with present guns and a few machine guns will answer. A fleet of harbor torpedo boats should also be available to assist in making the torpedo defense dangerous, should the enemy at night or in fogs attempt countermining or removal of torpedoes. These boats would also make blockades dangerous, or compel the enemy's squadron to haul off coast at night. They may be—say, boats of 100 feet length, speed, 21 knots per hour. There should be enough of them built at once to train our engine and boat-builders, to bring out the special knowledge required and to have the plans and details well developed. Then, in case of necessity, these small engines could be turned out in numbers from our numerous shops, trained to do good high speed engine work, and the hulls could be built anywhere and transported by rail or canals to our lakes and harbors. The Fortification Board recommended the building of about 160 of these torpedo boats, to be distributed to various ports, New York, San Francisco, Boston, Hampton Roads, New London, Portland, Oregon, to have 18 each. New Orleans, 12, Philadelphia, Portland, Maine, Narragansett Bay, Key West, and Charleston 6 each, and lake ports 12 torpedo gun-boats.

Another item in the scheme of coast defense is floating batteries, for which five ships, three at San Francisco and two at New Orleans, are estimated at a cost of $19,000,000. At San Francisco the depth of entrance and strength of currents prevent the use of torpedoes. At New Orleans, near the heads of passes where torpedoes must be, no foundation can be had for heavy gun protection on shore. The vessels designed for this are superior in armor protection and offensive power of guns to any now afloat.

The report of Commander Sampson, United States Navy, of Fortification Board, upon the proper use of navy and floating batteries, is a very interesting and able one. Of the whole $126,000,000, however, estimated as necessary to defense, over $94,000,000 are for forts and for armament of heavy guns. Torpedoes alone will not suffice, as before shown. It is necessary there should be powerful guns properly mounted and properly protected. Sir Andrew Clarke, Inspector General of Fortifications of Great Britain, defines coast defense as "the science of mounting heavy guns to best advantage." And, notwithstanding the use of torpedoes, it is still so. In the estimate of the Fortification Board of a total of $94,000,000 for forts, etc., $32,000,000 were for masonry and earth works, $23,000,000 for armor and metallic structures, $29,000,000 for guns, $10,000,000 for gun carriages. Another popular delusion is that, if we only had the guns, we could improvise earth work cover in a few days, and send the guns when needed to be mounted forthwith, or perhaps fired from the shoulder like muskets.

The question of how much cover is necessary comes in here.

The penetration of largest guns on shipboard in 1860 was less than 4″ of iron and less than 3 feet of granite. It is to-day 30″ of iron, 10 to 20 feet of granite, and 75 feet of earth.

The Board of Engineers for fortifications says of late designs: "In designing a battery to resist a naval attack at one mile, it has been assumed that the heaviest shot at close range may be stopped by a bank of compact sand 70 feet thick; that rubble masonry will offer three times and concrete twice the resistance of sand.

"The parapet of the proposed battery consists of a wall of concrete 5 feet thick; a mass of stone imbedded in concrete 12 feet thick; and 27 feet of sand; equivalent, according to the above estimate, of 73 feet of sand. The traverses are 30 feet thick at top and rise 10 feet above the crest of the parapet, and but one gun is mounted between two adjacent traverses.

"The service magazine is placed under the traverses, and when circumstances will permit, is sunk to such a depth that a shot must pass through at least 70 feet of sand before reaching it. When this depth cannot be attained, the requisite protection must be supplied by rubble masonry." This gives a measure of the amount of work to be done on a seacoast battery.

The use of rapid firing guns has increased the necessity of thoroughly covering the gun detachments, while the increase of penetration has added largely to thickness of work necessary to cover guns and magazines. Embrasures in masonry or earth work no longer afford much protection without greatly limiting the angle of fire. Guns must either be mounted in barbette on disappearing carriages or in armor-clad casemates or turrets. A high military officer is quoted as saying, "We need no forts, but the guns can be mounted in holes in the ground on disappearing carriages." He is probably not correctly quoted, for while mounting upon disappearing carriages of guns in elevated position will no doubt be largely adopted as recommended by the Fortification Board, for guns of 12" caliber and under there are many positions so low down and close to the channel that some armor-clad protection for guns used there is absolutely necessary. The few guns of 16" caliber will be such an expensive part of the artillery defense, their manipulation will be so slow and require so much important machinery, which must be fully protected, that a revolving turret clad with armor seems the only expedient applicable for their cover.

The great increase of weight of guns from old smooth-bores of 10" caliber, of 7 tons weight, with 2,600 foot-tons energy, and 15" caliber, of 20 tons weight, with 9,000 foot-tons energy, to 12", 50 tons weight, 24,000 foot-tons energy, and 16", 115 tons weight, 54,000 foot-tons energy, makes other changes necessary. The mass of masonry required to take the strain of recoil is greatly increased; hand power is no longer applicable to handling of guns and ammunition, but steam power applied to shafting, to hydraulic or to pneumatic machinery is necessary for working guns. Guns of 10 or 12-inch caliber, with pivot fastening, covering for the boilers, machinery, magazines, gunners, and relief detachments, require much preparation of masonry and earth work, even when mounted in barbette on disappearing carriages.

As an example, each 10" gun in barbette is estimated to require 2,300 cubic yards of concrete masonry and about 2,400 cubic yards of earth work, and the expenditure of about $13,000 to prepare to receive each gun carriage and gun. Again, to mount four 16" guns in two revolving turrets is estimated to require about 34,000 cubic yards of concrete and rubble work, and about 4,900 cubic yards of dressed granite work, or a structure of masonry about 10 yards deep, 90 yards long, 50 yards wide. To lay such a mass of masonry and give the concrete time to harden properly, will require several years, and is estimated to cost $400,000. The selection and acquisition of sites for batteries in the neighborhood of large towns without paying exorbitant prices for land require time and care. Sites selected for military reasons may not be readily accessible by rail or water transport, and the getting together of necessary material and mounting guns of 25 to 35 tons' weight requires weeks.

Nor are we prepared with either guns or armor protection for casemates and turrets. The estimate of the Fortification Board includes guns and armor, as follows:

44	16″	Guns	110	Tons	=	4,840
6	14″	“	80	“	=	480
203	12″	“	50	“	=	10,150
222	10″	“	27	“	=	5,994
102	8″	“	13	“	=	1,326
						22,790 Tons guns.

700 12″ Mortars }
24 10″ “ } of cast-iron hooped.

68,000 tons of armor.
12,000 “ “ substructure.
44 Guns or all the 16″ are to be in turret.
80 Guns of 12″ and 10″ are to be in casemates.
These 124 are to be protected by armor.
54 Guns 12″ to be mounted on lifts.
394 Guns 12″, 10″ and in barbette on disappearing or non-disappearing carriages.

The rifled mortars of 10″ and 12″ caliber are a very important and efficient part of the artillery defense, although least costly and requiring no protection but a sunken pit in the ground. These pieces are of modern development; they throw large shells with heavy bursting charges, which, falling vertically, can pierce the decks of ships of war and will have a most damaging effect. Experiments have shown them capable of such accuracy of fire, that at two miles it is possible to drop 50 per cent. of shells fired inside the horizontal area of a large ship. Their position will be entirely invisible from off-shore, and no effective reply can be made to their fire. With a number of such pieces commanding an anchorage, a hostile fleet will be slow to anchor or take position for bombardment. These pieces can be cheaply made of cast-iron hooped with steel, although it is likely steel forgings will also be used.

Of the various details of defense none have been the cause of so much debate as the construction of the guns and the material of which they shall be made. The Congress committee man has the lobbyist always with him, and the steel casting Rodman advocates, the cast-iron gun contractors, the patent-gun inventors, and other interested parties have been sufficiently numerous and active to counteract the wisest counsels of our ordnance officers, so that to-day all work upon army ordnance is at a stand-still.

There are symptoms, however, that the cast-iron men are getting tired, and we shall hereafter hear little of the possibility of making a large, high-powered rifle gun of cast-iron. The great 12″ cast-iron rifle at Sandy Hook is practically *hors de combat*, from erosion of the barrel, although only about one hundred and forty rounds have been fired from it. With 54 tons' weight, it has only the same muzzle energy as a 10¼″ steel gun of 32 tons' weight.

The multicharge gun is an ingenious but now useless device. It is constructionally bad, and a modern rifle gun, with progressive powder, is practically a gun with a great number of charges exploding in rapid succession. The Haskell 6″ multicharge gun had a weight of 25 tons for an energy of 2,885 foot-tons. The army 10″ steel gun, with a weight of 27 tons, has an energy of 13,642 foot-tons. The advocates of gun steel cast and annealed, not forged, or guns cast in steel of one piece, not built up, but treated by internal cooling, on the Rodman principle, are yet to be disposed of.

If guns or gun steel can be made equal to the present specification by casting, let them be accepted, but the standard must not be lowered to reach material of a lower grade. Nor can casting makers ask the Government to demand higher qualities for forging than for casting.

No American engineers can put themselves on record as asking for material of low ductility to be put in guns. The engineering work of this country has never been done on a lower standard of excellence than that of English, French, or German engineers. The makers of steel castings have, so far, failed to make the quality of steel found in forgings, and the task is, apparently, hopeless.

The beneficial effect of mechanical work upon a cast-steel ingot, in making it higher in elastic limit, higher in ultimate strength, and much more ductile, cannot be denied. This improvement in quality has not yet been obtained with certainty in any other way than by forging. Of a dozen of steel-casting makers doing a commercial business in this country to-day, I do not believe one will undertake to produce a 6″ gun tube, unforged, of quality equal to United States ordnance specification. They will say the ductility and elongation asked for are not necessary in gun material. If this be so, then raise the powder pressure and the elastic limit, and so increase the efficiency of a given weight of gun and projectile.

There is no place for inferior metal in gun construction. The best that can be had is always the cheapest. The steel castings gentlemen say they can make guns much cheaper by casting than by forging. This is important, if true. It will hereafter be shown in this country, as has already been proved abroad, that the cheapest way to produce gun material is to forge from an ingot of the simplest possible shape. Meanwhile, the armament of this country is delayed, while steel casting enthusiasts search for a cheap way over ground where "No thoroughfare" has already been written.

Whitworth's hydraulic forging had its origin in a signal failure to produce castings equal to forgings. He spent £40,000 in the endeavor to make good steel castings, and only retrieved his fortunes by the grand success of the hydraulic forge.

His establishment has also been through the experiment of casting ingots with a central core, and abandoned the method, as it was found cheaper to bore out a hole than to core it, and also because by boring a porous central portion of inferior material is taken out. The cored casting always had an inferior part near the center of its thickness, and this is the trouble with all attempts to cast guns hollow. It was so with Rodman heavy gun castings of iron.

The very intelligent and persistent efforts made at Terre Noire to produce castings for gun material have not made headway in competition with forges. Nor do I see how they can. Gun hoops, for instance, are made at Terre Noire from a large, solid ingot. The top is cut off as a waster, and the center is bored out. The hoops so made cost more in waste material than all the labor of forging.

The whole cost of forging labor on a large gun, with good plant, can be made less than the cost of special molds and preparations to cast guns into shape. The advocates of cheap and nasty casting methods are deceived by the apparent ease of making castings, and overestimate the cost of forging, which, of itself, is a small fraction of the cost of production of gun material.

Referring to remarks of Mr. Davenport, of Midvale Works, in the Naval Institute discussion, we find that some causes of the high cost of forged gun steel not generally known are:

1st. Sink head scrap.

2d. Machine work and tool chip loss.

3d. Oil tempering cost.

4th. Extra length of forgings for tests, and cost of preparing these tests.

5th. Rejection of submitted material.

In making gun steel by casting alone, none of these elements of cost can be avoided. In fact, in casting methods the primary losses from the first and second causes will be greater on castings, and the certain increase of rejections again will carry with it an increase on every other element of cost in production.

A forge will also have one very decided advantage over any casting plant which attempts to make guns in one casting. The largest ingot required to make a 12" built-up gun by forging will be of about 40 tons weight. The 12" gun, being of 50 tons' weight when complete, will require, if made in one casting, an ingot of 80 tons' weight, and apparatus very heavy and large to handle it. If such a piece is rejected, it will be worth less than pig-iron for remelting; but in a forge rejected pieces and sink heads can be worked down to small size for commercial uses, and, from the excellence of the stock and its thorough working, will bring the highest market prices. There should be very good profit for a forge in this item alone. Another great objection to a method that requires enlargement of ingots is the fact that the larger the ingot the greater the tendency of the iron and other elements to separate partially, and the difference in analysis between top and bottom and outside and inside is greater, and the variation in quality of steel is greater as the size of the ingot increases. The irregularity of quality will be much greater than should be found in good forging from the small ingots. Mr. Metcalf, in his very interesting paper lately read before the Society of Civil Engineers, calls attention to this, although in the same paper he advocates a method of manufacture leading to the bad results he deprecates.

The question whether we should forge or cast gun material can be left to the commercial test of cost of manufacture, with every chance that the forge will live and grow fat, whilst the foundry will die of starvation.

Bridge makers may as reasonably expect the steel manufacturers to cast their steel compression members as the ordnance manufacturers to get their gun material by casting. The reasons for and against such practice hold with equal force in both cases.

The difficulty of getting a good quality of gun steel from unforged metal is great, and increases with the size. Slow cooling from a high heat means coarse crystallization and brittleness. This is particularly the case when the cooling has been from melted metal and, in less degree, with forgings.

A very fair example of what may be expected from castings is found in the Report of Chief of Ordnance, 1884, of tests of a rial cylinder made under the most skillful superintendence, and by metallurgists of acknowledged ability. This cylinder—24 inches in diameter and 5 feet long—was the lower half of an ingot of 17,000 pounds weight. A 6-inch hole was bored in it, and it was then carefully annealed, and, while still hot in the furnace, was cooled from the interior with an air blast. It will be sufficient to give results of six tangential specimens, three top and three bottom of cylinder:

		Elastic Limit.	Ultimate Strength.	Elongation.	Red. Area.
Top....	Outside...........	39,000	60,880	2	5
	Middle...........	43,000	73,120	2	5
	Inside............	48,000	97,440	3.8	11.8
Bottom.	Outside...........	35,000	86,400	14.8	27.6
	Middle...........	35,000	86,000	14	21.4
	Inside............	37,000	88,800	16	21.4

The elongation and reduction of top specimens is exceedingly poor, and for 55 carbon steel the ultimate strength and elastic limit are far below results obtained with great certainty in good forgings. To show this, of 55 tangential tests of gun hoops made at Cambria Works, in which the elastic limit was 55,000 pounds, the minimum elongation and reduction in the 55 tests was 9.67 and 14.4, and this in case of a flaw.

The average elongation and reduction was 13.4 and 29.9.

The first fifteen tests on the list are as follows:

Elongation.....................	14.7	14.4	12.9	13.7	13.4	13.0	12.8	13.4	13.0	14.2	13.6	13.2	11.7	14.2	14.6
Reduction of Area..............	35.3	28.0	33.7	42.5	38.2	32.4	32.7	42.8	39.5	39.5	28.0	30.4	30.7	33.7	16.7

The others run about the same.

The reason of the inferiority of castings is that all untreated ingots show a coarse crystallization difficult to remove. It can be made fine mechanically by continuous forging work upon the metal whilst it is cooling down to the critical temperature, and considerably below it, or by the other method of allowing the steel to cool to low temperature, then slowly to heat it up to, and not above the critical temperature. At this heat, somewhat variable with the character of the steel, it becomes, if the metal is rapidly cooled, fine-grained, tough, and ductile. This whole subject is very ably treated by Mr. J. A. Brinell, whose paper is translated in Ordnance Construction Notes, No. 37.

A further treatment of the steel is to anneal it at a lower temperature to relieve strains from sudden cooling. The larger a mass of steel the greater the difficulty of cooling with sufficient rapidity to keep it from becoming coarse-grained and weak.

Assuming that a steel gun equal to the present designs would be strong enough if it was made in one piece, and that we can succeed in getting a casting free from small blow holes or large cavities, which I admit is possible, we should find a specimen cut out of the mass anywhere to be coarse and unfit for gun steel. Therefore, the casting must be heated and suddenly cooled to change the structure.

In the 16-inch built-up gun the tube to be tempered is about 7 inches thick, but if the gun were one piece, the metal to be tempered is 24 inches thick, and, as experience has shown, the result with pieces 6 inches to 7 inches thick is not as good as with pieces of smaller guns, and therefore thinner metal; the results of tempering will be certainly very poor on so great a thickness as 24 inches. Admitting it were accomplished, and the steel gun was a homogeneous mass of good quality, it would be a much weaker gun than the built-up gun. Mr. Virgile, quoted in Ordnance Construction Notes, No. 10, gives resistance limits of the two guns, as follows:

Limits of internal pressure of steel gun, simple...24,179 pounds per square inch
Limits of internal pressure of steel gun, compound.49,780 pounds per square inch

To remedy this difficulty, the steel casting advocates the present Rodman panacea, and propose to cast the gun with a water-cooled core, and put the outer metal under initial tension. This, with a casting of 150 tons' weight varying say from 24 inches thick to 6 inches thick, with 50 to 60 feet length, and a corresponding static pressure of fluid metal upon the mold, is a very interesting problem of exceeding difficulty, and the result depends largely upon chance. In four attempts to make a Rodman cooled 12-inch rifle gun of cast-iron, two were failures from castings parting by tension in the mass, one was lost by accident to the flask. This was, too, in a works where many Rodman guns had been made. The records of the manufacture of these guns during the late war, at a time when skill of manufacture was supposed to be good, show that many guns parted by internal strains either in the pit or in the lathe. If this was so with iron, and a shrinkage of $\frac{1}{8}$ inch per foot, will there be any less trouble with steel and shrinkage $\frac{1}{4}$ inch per foot? The difficulties of the Rodman people do not end in getting a solid casting free from blow holes. Their casting being unannealed will be coarse-grained and weak. It must be heated and annealed, and it should be tempered and afterward annealed. This, any steel casting manufacturer will admit. Mr. Metcalf says there is no known way of getting improper strains out of high steel, except by annealing.

When this mass of steel, cooled from the interior and with the exterior under initial tension, is again heated and annealed, will any of the initial tension remain? From my considerable experience in annealing steel rolls up to 40 inches diameter and 20 tons' weight, I am sure that if any large mass of steel is heated hot enough to break up properly its coarse crystallization, no part of it can remain under such initial tension as the Rodman method requires. Coarse crystallization and weakness, or no initial tension, must be accepted with a cast steel gun made on the Rodman plan.*

Having made a 16-inch gun 42 feet long, what can we know about the metal in it?

We can test pieces from the ends, but the position of the base of the shot and the front end of the powder chamber is 11 feet from the test ring cut off at the breech, and I submit that aside from the difficulty of making the large casting and treating it, the impossibility of knowing anything about the interior condition of the thick mass of metal in and around the powder-chamber is a sufficient cause for condemning any such steel gun.

To so treacherous a weapon, however, the advocates of the Rodman methods would commit us, because it is cheap. To mount and protect a 16-inch gun will cost $500,000, the best 16-inch gun we can buy will cost $100,000. If we saved the whole cost of the gun it would be false economy to do it at the expense of safety or power of the gun.

Having shown how difficult is the application of the Rodman idea to a material like steel, let us consider whether building up a gun is not, so far as shrinkage is concerned, a good and practically an exact method of putting the parts under initial strains.

The theory of construction of guns built up by shrinkage is, that whether the system is at rest or subject to powder pressure, no part of the system shall be subject to strains equal to the elastic limit of the material, and that when in action all parts shall work

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*There is no reason why methods necessary for cast-iron should be used for steel, and it seems to me the only promising method is that adopted by Mr. Wellman, viz., cast the interior solid, bore out the center, treat the metal to correct crystallization of structure, and then cool from the interior by air, or water, or oil.

together and assist each other in resisting the powder pressure, and this can be accomplished.

In No. 33, Ordnance Construction Notes, the shrinkage of outer hoops of 8-inch B. L. Steel Rifles is given .034 inch on a diameter of 26.3 inches.

The tensile strain in the hoop due to a variation of $\frac{1}{1000}$ inch is about $\frac{30,000,000}{1,000 \times 26.3} = 1140$ pounds per square inch. As the difference of $\frac{1}{1000}$ can be readily measured on this diameter by proper instruments, it will be seen that error of measurement will cause variation of strain less than the smallest variations of elastic limit and ultimate strength which we could hope to obtain in the most perfect material. The formulæ by which the shrinkages are calculated are proved for each type of gun by an experimental cylinder representing a section of the gun through the powder chambers. The results of experiment agree closely with the formulæ. Full discussion of these experiments may be found in Report of Chief of Ordnance, October, 1885, appendixes 26, 27, 28. And a study of these papers will, I believe, convince an unbiased engineer that our ordnance officers have brought to the question great practical good sense as well as high theoretical attainments.

In addition to the fact that they are the strongest and lightest, the most powerful argument in favor of built-up guns is the fact that, with proper tests applied to each piece after tempering, we have such knowledge of the condition of the metal in all important parts of the gun as gives confidence in the weapon.

The few guns made by our ordnance officers of material manufactured as first efforts by steel works not previously engaged in heavy forging work show excellent results, which should inspire trust in the material, the construction and the constructors.

Of building up guns by shrinkage, it may be said truly to be "simple, easy, and sure." It does not depend upon the uncertain chances of the lottery combination of casting a metal of unknown temperature in a mold of varying radiating power surrounded by a fire of irregular temperature, cooled inside by a fluid flowing in unmeasured quantities and temperature. Success does not hinge upon the combination of a dozen varying conditions acting to influence final results. On the contrary, the method of shrinkage is as certain as the ability of a mechanic to calliper the diameter of a bored or turned cylinder.

The question of armor also opens a wide field for debate.

Shall it be soft wrought iron, iron and steel compounded, all steel, or chilled cast iron? If a forged material is adopted, the reasons for preferring steel are:

1st. From the small number of times the material is worked, it is certain steel will be cheaper to manufacture than piled wrought-iron material, which, taken from the muck bar, cannot be properly massed together with less than 5 or 6 reheatings and weldings.

2d. There are great possibilities of improvement in the manufacture of steel armor which has so far only been made for a few years, and by one concern, and there is reason to hope, with experience of many concerns and a longer time applied to the manufacture, there will be great improvement in its quality. That there can be much improvement in that of wrought-iron armor seems improbable.

3d. That it is likely soft and hard steels can be compounded in the same plate successfully, which so far has not been done.

4th. That steel armor, as now made, is at least equal to iron and steel compounded.

At the time the report of the Fortification Board was made, a trial of chilled cast-iron armor with very heavy guns had not been made, but the trials of Gruson chilled armor at Spezzia, about a year ago, was an event of the greatest interest to the military engineer. A plate of 87 tons weight and 3 to 4 feet thick was subjected to 4 point blank shots of the 100 ton 17 inch gun with 826 lbs. powder, and projectile of 2,205 lbs., striking velocity 1,765 ft., and energy 47,500 ft. tons. The angles of incidence of projectiles was about 40°. The shots were broken up into small fragments. Any one of the shots would have pierced 31 inches of wrought-iron plate; the fourth heavy shot struck nearly in the same place as the second. The plate was, of course, somewhat cracked, but small pieces only having dropped off from interior, it was considered to have passed the test condition, which was that no fragment should be detached from inside by blow of shot. Full description of the trial is found in the London *Engineer*. The Krupp steel shot that were fired on this trial are said to be equal to perforation of steel plates of 1¼ to 1½ calibers without deformation. These experiments settle the possibility of defending guns in casemates and turrets, with a material much cheaper than forged iron or steel. The influence of mass is so great in constructions to resist large shot that a future seems opened for the use of chilled armor for land defenses. And to it I commend the attention of cast-iron-gun

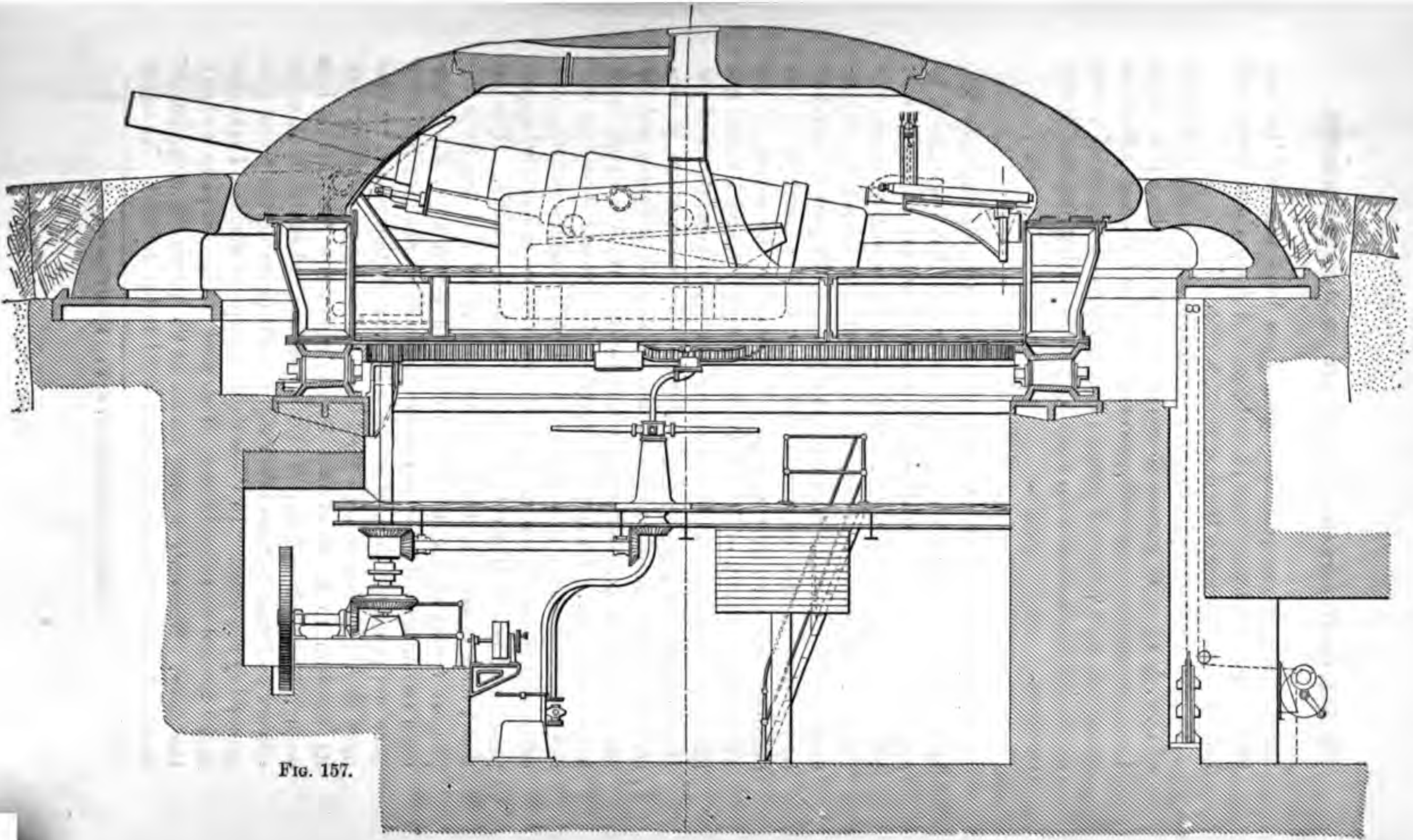

FIG. 157.

foundrymen, whose plant the steel guns have rendered usel Sixty-eight thousand tons of castings will keep them busy a leave the gun forges a fraction of work also.

To give some idea of size and construction of proposed guns, t of U. S. A. 12"x16" guns is shown in Fig. 154 and Fig. 155. F 156 shows relative size and power of old guns for coast defense compared with proposed guns. Fig. 157 gives a section of tu of Gruson chilled cast-iron, showing gun mounting and machin of turret.

DISCUSSION.

*Mr. F. M. Barber.**—My first idea in hastily reading over Morgan's admirable and suggestive paper was that I was going approve wholly and unreservedly each view that he express but I find, on re-reading, that in defense of my branch of the serv I ought, before touching upon the mechanical problems, to critic some few points which he mentions regarding the part to be tak by the navy in the defense of the country. I aware that his view is thoroughly indorsed by the report of Fortification Board, of which he was a member, and I would like to be understood as undervaluing the recommendations of Board in so far as the *kind* of defenses they recommend is c cerned; but in regard to the quantity of floating defenses and part that must necessarily be taken by the navy, their position not a strong one. I am particularly anxious that I should be c rectly understood in this matter, for I well understand the e effect of officers criticising each other's reports where the app priation of money is involved, as it confuses the public. Nothi that I have to say would alter the specific use of a dollar from purpose for which the Fortification Board would apply it. Th have recommended none too many defenses at anchor; but th have not protected those at anchor with enough that move. "T navy should be free to seek the high seas," as the paper says; b its freedom to do so should depend upon the fact that it h enough iron-clads and torpedo boats (surface and submarine), wi officers and men to man them still remaining to do their share protecting the coast. In these days of torpedo warfare, I take t broad ground that the defense of no port can be complete witho the navy. Not only it MAY assist, as the paper suggests, but *must* assist, and it is absolutely essential to the defense. No co

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can be properly defended, according to the ideas which I believe are most prevalent in Europe, without an outer line of iron-clads, an inner one of gun-boats and torpedo boats, and lastly, the fixed defenses at the bottom of the harbor and on the shore. The officers and men of the navy have unquestionably the particular kind of education which adapts them to the two lines afloat, and torpedoes fixed and moveable have caused such a gradual crawling down of all the live defenses of a port, off the land and into the water, that more and more aquatic knowledge is necessary, and in Europe this whole matter is gradually drifting bodily into the navy, forts and all. In Germany it is already done. In France, coast defenses are in an amphibious state, and in England, judging from the discussion of the paper of Colonel Schaw, of the Royal Engineers (Deputy Director of Fortifications), read before the Royal United Service Institution in December last, the necessity of some kind of combination of the army and navy is recognized and its consummation is probably a matter of the near future in England. One of our most prominent army torpedo experts once told me that he could see plainly the desirability of some such arrangement in order to obtain a more perfect knowledge of the value of fixed mines with reference to their location. Said he: "I know what *I* think a navy-man would do if he had to attack any harbor; but I would very much like to know what *he* thinks he would do."

As Viollet le Duc argues in his *Annals of a Fortress*, the proper place for defense is at the outworks, to harass, discourage, and demoralize your enemy, and prevent work on his system of approaches." But this only enhances the value and necessity of a stronghold which you know is always in your rear, and to which you can retreat from time to time, but which could scarcely be made so formidable that a modern fleet could not pass it eventually, if the approaches were once clear and could be kept so. Guns and electric lights on shore would not sufficiently protect the submarine mines at the bottom of the harbor. Co-operation of army and navy will probably be the solution of this problem in our country, and the horse marine will become a living actuality.

The Fortification Board recommended but 160 torpedo boats for our enormous stretch of coast (it is 12,000 miles inside of 4,000, if we include the lakes and Alaska), while Germany is building to-day 150 torpedo boats for her exceedingly limited coast line. The Board recommends but five armor clads, three for San Francisco and two for New Orleans. It is true, as the paper says, that they are the most expensive gun-carriages in the world; but they are movable, and while the Board provides fixed protection for the principal ports on our coast, it makes scarcely any provision for the coast between the ports. To fortify strongly only important places is an open invitation to an active enemy to effect a landing at the unimportant ones, and it should be remembered that, unlike Europe, we have no inland fortifications whatever in our country. Cost what they may, movable defenses must be had, and their power of concentration on any point attacked renders them far cheaper than a continuous line of strong-fixed defenses.*

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* The following information, from reliable sources, shows the growing importance of the Navies of Europe in connection with the problem of coast defenses.

Germany.—The sea-coast defenses, torpedo boats, torpedoes, submarine mines, etc., of Germany, have been placed in charge of the Navy. The Naval Budgets of 1885-86 and 1886-87 provide for the expense of maintaining torpedoes, torpedo boats, and submarine mines, and of the necessary personnel to manipulate them.

The fortifications have recently been transferred from the Army to the Navy and the following reasons have been assigned for this transfer:

The forts at the mouth of the Elbe, in addition to those at Kiel and Wilhelmhaven, have been transferred to the Navy and are garrisoned by the third battalion of Marine Artillery, which has been added to the existing force for this purpose. These battalions are officered by line officers of the Navy, and the non-commissioned officers are seamen-gunners. Three-quarters of the privates are gunners from the Navy, and the rest come from the conscription district in the neighborhood. The change was made on the recommendation of the Minister of War, approved by the General Staff and Military Cabinet of the Emperor for the following reasons:

First—Because the guns and carriages are similar to those used in the Navy. *Second*—As this defense is chiefly against attacks from ships and naval landing parties, seamen would more readily appreciate the points of weakness, objects of manœuvers by the movement of the ships, and recognize the probable designs of the enemy from the preparations. *Third*—That, as the defenses were considerably made up of turrets moved by steam or hydraulic power, a class of men became necessary for the manipulation of the machinery which did not exist in the army. *Fourth*—That seamen were better adapted to care for works situated in the mouth of rivers, and would co-operate with the submarine defenses more advantageously.

Submarine defenses of all kinds have been placed entirely in the hands of the Navy. They formerly belonged to the Engineer Corps of the Army, but were transferred to the Navy for similar reasons to those given for the transfer of the coast defenses.

An Imperial order of March 16, 1886, prescribes the duties of the Board of Inspection for Torpedo Affairs of the German Navy; and all defenses of ports and harbors are to be arranged with the approval of the Chief of the Admiralty.

The submarine coast defense, as organized by the German Admiralty, distributes the available torpedo boats to different districts on the German coast;

I thoroughly agree with Mr. Morgan in his opinions regarding the impossibility of manufacturing steel-cast guns of large caliber, and he has gone over the ground so thoroughly that nothing remains to be said. The fact that $20,000 in the last Navy Bill is to be devoted to paying for the cast six-inch guns, in case they pass the statutory test, will probably settle the matter to the satisfaction of the public, although it would have been better to have made the amount $100,000, as it would have offered greater inducements to experimenters.

I agree also with the opinion that the question of armor opens a wide field for debate; but I do not think that the case for chilled cast-iron is quite as favorable as the Spezzia experiments would indicate. I think it is the general opinion among engineer officers, both in this country and in England, that the future possibilities of gun-fire are so difficult to estimate that any metallic protection should consist of a material whose thickness can be readily increased if necessity should require it. The very nature of the chilled cast-iron prevents the possibility of bolting to the outer

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each district has a torpedo depot, and the amount of torpedo armament of each district depends on the extent, importance, and vulnerability of the district to be defended. The last fortifications remaining in charge of the Army were the forts at the mouth of the Weser, and, by the Imperial order of November 25, 1886, these were transferred to the Navy on April 1, 1887.

France.—On the 6th of March, 1886, a Ministerial decree created an additional department in the Ministry of Marine, called the Direction (Bureau) of Submarine Defense. This Direction has control of all torpedoes, mines, and apparatus used in connection with them, and all torpedo boats. The Director of Submarine Defense is a Rear Admiral, and the service in each port is divided into two sections. 1st. The defense mobile, consisting of all torpedo boats and torpedo store ships. 2d. The defense fixé, having in charge all submarine mines, electric search lights and torpedoes operated from the shore, harbor boats carrying spar torpedoes, care of material, mine boats, etc. The commandant of the defense mobile in each fort is a commander in the Navy; that of the defense fixé is either a commander or lieutenant in the Navy.

A French "Fortification Board," organized in December, 1886, consists of five Naval officers and five Army officers.

England.—In England the fortifications and submarine mine defenses are under the control of the Army. The torpedo boats are all under Naval control. The question of turning over the command of the sea-coast fortresses to the Navy, and making the latter completely responsible for the defense of the coast, is under the very serious consideration of the British authorities, and the matter is now being discussed between the War Office and the Admiralty. It is proposed by the former that the latter shall have entire control, it having come to the German and French view of the subject of coast defense.

Italy.—The Direction General of Artillery and Torpedoes of the Ministry of surface, and since a part of its virtue is due to the mass, the plates are large and weighty, and would be expensive to move if one wished to get at the inner surface, and once there, it is questionable if cast-iron could be well held by bolting anyway.

The plate fired at at Spezzia was supported on each side by a 40-ton casting, and the whole mass was solidly wedged into a cutting in the side of a huge rock cliff; the conditions of the mass to absorb the energy of the projectile were admirably obtained; but these conditions would not have been the same had this plate been in its place in a revolving turret, of which it forms about one-fifteenth the weight. The great curvature of the plate, of course, greatly assisted it; but this is an advantage to be obtained with any metal; and it is quite possible to suppose that a turret of 1,400 tons might have at least had its revolving gear seriously damaged, because the turret is obliged to absorb all the energy that does not glance off in consequence of the hardness and angle of the surface, or is not dissipated by the breaking of the projectile.

A plausible objection to the Gruson turret, and the first to catch

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Marine (chief a Rear Admiral), in addition to the manufacture and purchase of material, is also charged with the submarine defense of the coast. The Navy is exclusively charged with all that relates to Naval and submarine features of coast defense, including light batteries of guns which may be erected to command lines of fixed torpedoes and prevent the operations of the boats of the enemy, and with the surveillance of the coast and its electric lighting for military purposes.

The general plans of coast defense are studied and drawn up by a mixed commission of Army and Navy officers. In the projected scheme of coast defense it is intended to assign from three to eighteen torpedo boats to each of the ports of importance, twelve to fifteen in number. Palermo and Naples will be dependent on the fleet for defense, as they can be bombarded from the open sea (so can New York). The general staff, whose chief is the President of the Superior Council of the Ministry of Marine, forms a separate bureau without administrative functions. It is charged with the study of all that relates to the organization of the maritime forces, the plans of campaign, the defense of the coast and questions of naval tactics and strategy.

It is proposed to organize a special corps for coast defense, to be officered by the older men of the several grades of the line officers of the Navy; men whose age entitles them to be withdrawn from active service at sea, and who, for the same reason, can best be spared. The corps will have all or a part of the coast defense batteries.

This project is advocated for two reasons: first, to enlarge the sphere of the Navy in the defense of the coast; second, to stimulate promotion. In the event of its creation the corps of coast defense will be charged with the duties of the maritime conscription for recruiting the Navy.

It is also proposed to form a naval reserve of men under forty years of age to take part in the defense of the coast.

the eye of the military man, is the apparent weakness of the turret between the two guns. As the guns are parallel to each other and close together, that part of the turret which is exposed to the enemy when you are in the act of firing is the weakest part of the whole turret. This is true of all turrets with guns mounted in this way; but cast-iron would appear to be less reliable than steel in such a situation.

The projectiles all broke up, which diminishes the value of the results to a degree which it is impossible to estimate. They were undoubtedly the very best that could be obtained; but steel projectiles are still in their infancy, and will be extraordinarily developed in the future. They will undoubtedly be made so that nothing can break them up or deform them. Given an indeformible projectile, more wonderful results are yet to be obtained in shaping the head so that it will take hold of an inclined surface. In this particular case at Spezzia, it is possible that a softer projectile would have given better results in spite of inevitable deformity, on the theory that those used did not hold together long enough to get their work in.

This whole matter of the proper material for land armor was left in an unfortunate position by the Fortification Board.

They state that in their opinion "forged steel plates" should be used for land fortifications; but they also recommend that "immediate experiments be undertaken to determine the most suitable armor for turrets and casemates." It appears to me that this is a kind of a Bunsby opinion, whose bearing and application is difficult to discern. It affords little satisfaction to those who contemplate the manufacture of steel plates, and no encouragement whatever for those who would wish to venture on any other material, while at the same time the experiments recommended are known to be utterly impossible with the present attitude of Congress regarding the importation of foreign armor and guns for the purpose. I think that a special report should have been made on these experiments, specifying distinctly the kind, the cost, and where the material to make them with is to be obtained. The Board's recommendation of *steel* for ship armor and guns was specific, and has resulted in the establishment of the necessary manufactories. The recommendation for land armor should have been equally so, or, in default of it, a tangible method of solving the difficulty of decision pointed out. Engineer officers generally wish for money to commence at once the purchase of land sites, construction of casemates, earthworks, etc., and think that they would be fully occupied for a couple of years without any reference to armor whatever. This is true enough; but the manufacturing interests of the country are concerned in the contracts for armor which is to be applied to these works, and they should have an authoritative decision as to the kind, in order to know what to go in for. If these experiments are to be made, it cannot be done too soon.

I think it was the general belief that the Gruson target at Spezzia would be knocked out at the first round, and yet the results were marvelous. On the other hand, it has been discovered, in the use of forged steel armor, that by increasing the number of bolts which retain it to the backing, instead of weakening the plate from the greater number of bolt holes in the back, you vastly increase its power of standing up against continuous pounding, because a multitude of cracks does not appear to diminish the resisting power, so long as the plate remains on the backing. It has also been found that in deck plates exposed to oblique fire—say 20° to 25°—with ordinary ogival-headed projectiles, the soft steel presents rather unexpected advantages over a hard steel surface or over a softer wrought-iron surface, because, although it scores badly, it does not present the kind of a depression or crack which enables the projectile to turn and go through as the hard surface does, and at the same time it is hard enough not to score so deeply as to let the projectile get hold for that reason, as the softer surface does. It is the popular theory that a compound steel plate—*i. e.*, a soft-steel back and a hard-steel surface welded together—will be exactly what is wanted. Wilson, of Sheffield, has lately patented such a plate, with a layer of fibrous iron between the two steels, and a sample of it is said to have stood well at a recent trial at Shoeburyness; but I am somewhat inclined to doubt the correctness of this theory, certainly for the armor of anything that must be moved. If we can get a homogeneous mass of steel which will combine hardness and tenacity to such a degree that, while keeping out a moderate blow, it will next bend locally, and afterward, if the projectile enters the outer skin, offer it a sufficient uniform resistance without cracking, we will then present such conditions that the energy of the projectile will be gradually absorbed without getting through the backing; the injury will be local, and we will not rack our whole turret or the framing behind the backing. The extraordinary results that they are now obtaining in France with shields made of chrome steel to resist musketry appear to suggest that heavier armor can be improved by working in that direction. Plates of very great curvature are excessively difficult, if not impossible, in compound armor, and I should think that the cost would always be in excess of that of homogeneous metal.

The cost of steel armor would probably be in this country about 100 per cent. greater per ton than that of chilled cast-iron, say as $540 per ton to $270; at any rate, for the first that is turned out, though a Gruson plant for casting 100-ton ingots of iron is not a cheap affair. The European prices are $400 per ton for steel and $200 for chilled iron. The number of tons required, however, is much less for the steel. Twenty-five inches of steel would have been equal to the 4 feet of chilled iron used at Spezzia, and with the same curvature would probably have kept out the shot. This difference in thickness would not, probably, equal the difference of first cost; but difference of cost of transportation, cost of foundations, etc., are all in favor of the steel.

*Mr. Rogers Birnie, Jr.**—Mr. Morgan has very kindly commended the work of the officers of the Ordnance Department of the Army in practical investigation upon the construction of built-up guns, and has truly stated that the manufacture of the several experimental guns of new types made in recent years for sea-coast defense has been preceded by the construction of compound cylinders representing actual sections of the guns through the powder chamber.

These experiments have included not only the construction of the full section by shrinking together the several elementary cylinders composing it, but also the subsequent dismantling of the section. Their object has been two-fold—on the one hand is the practical object and on the other the theoretical.

In the first place the metal of all the cylinders or hoops used in these experiments was carefully tested by detaching specimens from each for determining the physical qualities of the metals under free tension and compression tests. The data furnished by these tests afforded means for regulating the strains to which the several cylinders in the structure should be subjected in the building-up process.

In building up a gun the object must be to utilize as fully as possible the elastic strength of all the parts when the powder-gas pressure acts, but the strains due to construction, that is, for the

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* Capt. Ordn. Dept. U. S. N. By invitation.

state of rest, must be so regulated as to leave a proper margin of elastic strength in the most exposed parts to give play for the added strains due to the pressure of the powder gases, yet still preserve these strains within safe limits. The most direct way of estimating these strains is to base them upon measured displacements of the cylinders when under stress, and this method was adopted in the experiments.

In a practical point of view the tests enabled us to determine in general the *behavior* of the cylinders under stress and when relieved from stress, and to make a comparison between the displacements of the elementary cylinders and the displacements of the same metal under free tests. Also, by free tests of the metal after the subjection of the cylinders to the shrinkage tests, to determine the effect of the whole operation, including the necessary heating for the shrinkage, upon the physical qualities of the metal.

The results of numerous experiments have shown that the values for physical qualities of the metals determined by the free tests can be fully relied upon to indicate the behavior of the cylinder as a whole subjected to shrinkage. It will be remarked that in the case of a built-up gun the successive cylinders are subjected to peculiarly homogeneous strains in the direction of the tangent.

The effect of the heating and shrinkage upon the physical qualities of the metal has been made a subject of careful examination. It is, upon the whole, very little injurious, and in the case of the tube or inner cylinder the qualities of the metal have been found improved by the compression to which this cylinder had been subjected. One who attempts to question the utility of the principles involved in the construction of built-up guns upon the plea that either the heat used for the assemblage , or that which will be developed in the firing, destroys or even materially vitiates the construction, can have no good ground to stand upon.

The investigation of theories constituted, in these experiments, quite as important an item as the practical observations. It will be readily admitted that if the truth of the theories be once established for a given structure their application to similar designs follows as a matter of course.

The results of the tests have established in a remarkable manner the truth of the theories upon which the formulas of application are based. The elementary cylinders used embraced forged and oil-tempered steel cylinders or hoops and cylinders of cast-iron made and cast hollow upon the Rodman plan to introduce initial tension strains. The applicability of the formulas to the changes of dimensions which occur in a thick cast-iron cylinder, having initial tension, when subjected to an exterior pressure by wire winding has been demonstrated.

This leads to a few remarks upon cast-iron guns made on the Rodman plan. If it were possible to regulate the tensions in cooling so that exactly the right state of tension would be induced in the casting to produce a uniform tension, T, throughout the wall of the gun under the action of an interior pressure, P, (per sq. inch),—then the value of P would be derived from the formula,

$$P=T.\frac{R_1-R_0}{R_0}$$

in which R stands for the exterior radius, and R_0 for the radius of the bore.

In the 12-inch cast-iron rifle to which Mr. Morgan refers the dimensions about the chamber are, $R_1=4\ R_0$ nearly. Then, in the cases supposed, would

$$P=3.\ T.$$

The average tenacity of cast gun-iron may be taken at 30,000 pounds per sq. inch and 13,000 pounds as a fair elastic limit: whence we would have $P=3\times13{,}000=39{,}000$, or say 40,000 pounds per sq. inch, as the safe pressure for this gun, on the assumption that it possesses a perfectly regulated initial tension as above. It is safe to say that it would be the merest accident if this state of things were realized in practice.

By cutting out rings from initial tension disks of Rodman castings it has been found that the curve of initial tension is quite different from what it should be to bring about this uniformity of strain. The actual initial compression at the surface of the bore is less than it should be and the curve through the wall is irregular and shows in some cases an actual compression at the exterior of the wall instead of the considerable tension which should exist there. The interior part of the wall at say from ¼ to ⅓ the thickness from the exterior is in a state of abnormal tension. And it seems probable that in those cases where the castings have burst spontaneously, the rupture started about that place rather than at the exterior.

I do not think that Gen. Rodman ever claimed to produce the theoretical state of initial tension, but his claim was in effect to relieve the gun from the injurious strains apt to be induced by exterior cooling. It appears, however, that even this much was a somewhat uncertain claim in dealing with the metal cast-iron.

From all this it is apparent that the value of 40,000 pounds deduced above for the interior pressure which the 12 inch gun would repeatedly support must be considerably reduced. What shall we say then to the claim of the cast-iron gun men that the factor of safety is 3, that is, that the gun would not be ruptured with a pressure of about 90,000 pounds per sq. inch in the chamber. We are much nearer the truth in saying that for repeated pressures 30,000 pounds is quite as much as this gun can bear. And in general we may say that a cast-iron gun is strained to near the safety limit at every discharge, under what is now considered a low pressure, *i. e.*, 30,000 pounds; hence the factor of safety is practically nil for elastic strength.

I think this helps to explain the unreliability of cast-iron guns, independent of their low power. The present entire lack of confidence in cast-iron guns is due, mainly, it is true, to their failures under actual trials, and if our artillery is compelled to use them (I refer especially to rifles) the men will be subject to the moral ill effect of expecting the gun to burst at any time. There is certainly no need of further experiments with cast-iron rifles, their reputation for unreliability is too well established, and there seems to be no excuse for our entering upon their manufacture in time of peace. The only excuse for their production would be in a period of national danger when they might be procured, provided any time could be saved over the production of steel guns.

The question of a steel-cast gun is in such an inchoate state in our country that there seems little to be said about it. Considering all the uncertainties of the question the manufacture of these guns ought at least to be preceded by a system of carefully conducted and extended experiments. If the curve of initial tension in a Rodman cast-iron gun is so irregular and varies from gun to gun, how much more irregularity must be anticipated in a steel-cast gun made in this way? Or, if this casting is to be annealed and the initial tension afterwards re-introduced, is it not probable that unsoundness in the interior wall of the original casting will more than counterbalance any probable gain of soundness at the bore over a gun which might be produced by solid casting?

The firing proof of a limited number of steel-cast guns would not establish the reliability of the system. We have now reached a stage in gun construction where we cannot justly accept hap-hazard affairs. It will be necessary first to establish that the operation of producing initial tension in a steel-cast gun can be conducted with certainty and can be regulated to a proper degree by the manufacturer. When this has been shown and sound guns so made have been produced, the steel-cast gun will have a very respectable standing, but at the present date it remains to be shown what can be done.

As regards the built-up guns I have little to add to what Mr. Morgan has so ably stated. In these guns we have a certain and sure means of producing, by the exterior pressure of the applied cylinders, the exact state of initial tension desired at the bore. Independent of the careful tests and detailed examination of the material which is permitted in making the gun of parts, we have, in the effect produced by the very assemblage of the pieces, an excellent means of measuring their soundness and elastic strength. In fine, the built-up gun made of oil-tempered steel forgings stands to-day a pre-eminent success in comparison with any other system. A most thorough study of this system has been made in detail and the practice and theory of the construction is understood.

Mr. Oberlin Smith.—Regarding this question of enormous ironclad vessels, to act partially as forts, in the way mentioned by Mr. Kent in the previous discussion; and also our land forts requiring to be heavily ironclad, to meet such attacks as we may expect in future from modern war vessels, it seems to me that the solving of the enormous expense problem incident to such protection, may lie in the direction pointed out by Sir Henry Bessemer. He has proposed to cast a fort whole. Of course the expense of building a fort sufficiently large and sufficiently strong in the ordinary way is very great; but by this new method, if practicable, as he seems to think, and as seems to me very likely, the expense would be greatly reduced. His idea was to build a number of temporary converting plants and make molds in the sand or other earth at hand, and then run the metal from the converters directly into the walls of the fort, so as to make them one solid piece of any desired thickness of steel, thus having the whole fort one great steel casting. It has since been suggested that the same method might be applied to great ironclads. I do not see why it cannot be done with forts, and, perhaps, as has been suggested, for large vessels of the type spoken of by Mr. Kent—very heavy and thickly clad ones. Of course, with such, great speed would be impossible.

Dr. Robert Grimshaw.—Such forts as are just referred to are not chimerical. They have been cast in 1878 and 1879. I was at Buckau, in Germany, and Mr. Gruson, whose establishment is subsidized by the German Government, was casting chilled iron forts. The sections were twenty-two inches in thickness at the bottom and fourteen to eighteen inches in thickness at the top, the chill extending to a considerable depth.

At the works at Buckau, they made a special mixed iron. Mr. Gruson uses immense chilling molds. There were two classes of such castings made; one was for turrets and the other for stationary forts. Some of the stationary forts were built with turrets and some had straight walls. I believe there is now one of those straight forts at Antwerp. The masses of the castings were so heavy that it was not necessary to make any great connection between the sections which composed the turrets or the walls of the fort. In 1880 I gave to the Franklin Institute an album containing photographs of all the sizes of sections that had been cast, and I think that in the Bureau of Naval Intelligence they are probably better provided than that. The reference made by one of the Naval officers this evening to the Gruson apparatus shows that they are well informed as to what has been done and what is proposed to be done. While on my feet I should like to call attention to the desirability of one or two sets of experiments that might be made. It seems that too much attention has been paid to getting high tensile strength and high ultimate strength, and not enough to getting a high elastic limit. It is more desirable to have a plate or any piece of steel of which the tensile strength is low but the elastic limit high and sure, than to get a high tensile strength and never knew where your elastic limit is going to be. There is no use in going beyond your elastic limit. As to the means of getting such high elastic limit I would suggest that the experiments and practice of Whitworth be turned upside down. Whitworth is compressing liquid steel, but he is generally compressing it from the bottom. In compressing the ingot the liquid chills against the walls of the mold, and the piston being pressed in from the bottom is compressed against a chilled section of steel. You get a tremendous pressure in your accumulator but you do not get much in your ingot. Whatever pressure is put on the ingot in the mold should be from above, so that the liquid steel should be what is forced into the skin, exactly the same as in stuffing a sausage. As soon as the ingot commences to be bright the steel chills, and we have that skin which is to be filled with liquid steel. Experiments made at the Joliet Steel Works show that the specific gravity can be increased from fifteen to sixteen per cent. by such pressure from above. The elastic limit is very much raised and the gases are pressed out so that the walls of the blow-holes are actually welded together. Those experiments point to the desirability of carrying them out with such facilities as the Government can give, and with such talent as our Navy and Ordnance Department can apply

Mr. Oberlin Smith.—It seems to me, Mr. President, that the method suggested by Sir Henry Bessemer, of casting a large fort complete, may prove to be a good thing when it is tried, for one reason especially, because no great care need to be taken to get the very best material. It is not necessary to use accumulators to compress the metal, and thus improve its density, because the method of constructing is in principle a very cheap one, and on account of this cheapness of construction a great deal of metal may be used and enormously heavy walls made of a comparatively common quality of steel. The idea looks so promising that I sincerely hope it will be tried in this country as well as in England.

*Mr. W. H. Beehler.**—I thoroughly agree with what Mr. Morgan said in his paper. A great deal has been said about steel. There is no doubt but what steel is gradually displacing cast iron and other forms of iron for ship-building. There is one form of armor of which nothing has been said. I refer to cellulose armor invented in France. It has been applied to making what is called a non-sinkable ship. It seems that we have in this country very few plants for making steel armor or Gruson armor, but we might be able to utilize this cellulose system. This cellulose is a product from the husks of cocoanuts. The value of it consists in its elasticity. A projectile will penetrate the side of a ship and the cellulose will follow right in the wake of the projectile, and close up the breach and keep out the water, so that the hull of the ship may be thoroughly riddled with shot and yet this cellulose will keep the water out and maintain the ship afloat. That is a form of armor which we might use in this country without the expense of great steel plants. The French claim that this cellulose will not deteriorate. The patent consists in a process of extracting the glucose. It has been claimed by its opponents that the glucose is not thoroughly extracted, that it is liable to decay and that it gives off odors, which might make the ship a very unhealthy place. The

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* Lieut. U. S. N. By invitation.

cellulose is being applied in the Italian and French navies; and English shipbuilders have applied it to some of their ships. It is a question whether it is really serviceable, but those Goverments are putting it in.

In order to emphasize the fact that the cellulose system of protection is being adopted, I submit the following list of vessels in which it has been introduced:

First Class Steel Cruisers—France.

Name.	*Displacement tons.*	*Name.*	*Displacement tons.*
Sfax	4,488	Dupuy-de-Lome	4,162
Tage	7,045	Alger	4,123
Amiral Cecille	5,706	Isly	4,123
Jean Bart	4,162	Mogador	4,325

Third Class Steel Cruisers—France.

Forbin	1,848	Lalande	1,877
Surcouf	1,848	Cosmas	1,848
Troude	1,877	Coetlogon	1,848

Torpedo Vessels—France.

Condor	1,272
Epervier	1,272
Faucon	1,272
Vautor	1,272

Torpedo-Hunters.

Bombe	321	tons
Couleuvrine	321	"
Dague	321	"
Dragonne	321	"
Fleche	321	"
Lance	321	"
Saint Barbe	321	"
Salve	321	"

Italian Vessels.

Cruisers.	*Tons.*	
Giovanni Bausan	3,008	—Built at Elswick, England.
Vesuvio	3,530	" in Italy.
Etna	3,530	" "
Stromboli	3,530	" "
Fiermosca	3,745	" "
Angelo-Emo	2,700	—Built at Elswick, England.

There are no heavily armored battle ships in this list, but it is contemplated in new designs to have the extremities protected by cellulose armor.

The cellulose is packed in the cells and the inventor has applied the term "cofferdam" to designate the material because it serves to keep the water out of a breach caused by a projectile. Its specific gravity is only .08, and it is, therefore, one of the lightest substances known. In packing the cells a fine dust arises which causes the men to bleed at the nose, and which is so disagreeable that the men can only work in it for a short space of time.

With the exception of an occasional newspaper note, very little has been mentioned about this cellulose armor, and as the naval service depends upon the ingenuity of our mechanical engineers to provide floating fighting machines, I submit these few remarks with the hope that it may lead to further development, and, perhaps, at some critical period rescue the country from disaster.

Mr. Jos. Morgan, Jr.—Commander Barber's principal criticism of the estimate of the fortification board which I quoted is that not enough ships and torpedo boats were provided for. Doubtless there should be outer lines of defense, and ships and torpedo boats in addition to those estimated for by the fortification board should be provided, but the fortification board was directed by Congress to report "at what ports fortification or other defenses are most urgently required," etc. Their business, therefore, was not to report upon naval fleets or their armament, except places where floating defenses were absolutely essential. I think the board did not attempt to more than indicate what was most urgently required. In presenting this estimate only, they judiciously refrained from increasing the cost by that of a navy of armored vessels. To have done so would have been to distract the attention of Congress from the defenses most urgently required, and by an enormous estimate have defeated all appropriations for reasonable and immediate provision for coast defenses. If Commander Barber's argument that more torpedo boats are necessary, be admitted, it may be said they are an element of the defense readily provided at a few months' notice, and so great a number as 160 boats would be sufficient to afford experience to a large corps of officers and men. Examples being ready for duplication our engine and small ship building shops could turn out these boats rapidly. They could be transported by canal or rail to all ports when needed. The expense of doubling or trebling the number of boats is comparatively small.

The position of the fortification board on the subject of armor is a point which Commander Barber possibly criticizes justly. They did not make their views very clear. In my opinion there is but one line of progress open, and that is the development of forged steel armor by proper appropriations and experiments. I do not want to be understood as advocating cast iron armor for our coast defense. It seems feasible in some places and does not involve so much outlay or preparation for its manufacture as for steel, nor so much time. As to steel forts cast in place, any plan or suggestion of Sir Henry Bessemer commands the respectful attention of engineers. The evident objection to such construction or any other involving large steel castings, unforged, untempered, and unannealed, is that from large size and slow cooling the steel will be a coarse-grained mass of large crystals with little coherence and strength to resist impact, little, if any, superior to cast iron. It is suggested that a high elastic limit is the desideratum for armor and other steel. This is true, if accompanied by toughness or ductility, and tempering in oil has that object in view. By it the elastic limit can be raised to the highest point attainable with the given analysis of steel. The elastic limit of tempered steel is far above that of untempered. The raising of the specific gravity is accomplished best by forging. The Whitworth compression can scarcely be described as compression from the bottom of the ingot. In the apparatus I have seen and which is described by Whitworth in his patent, the steel is poured from the ladle direct into a metal mold lined with refractory material. This mold is transported sideways under a cap and above a hydraulic press. The cap is lowered into the mold upon the top of the steel and fastened by tensile connections to the base of the press below, which, being put in action, lifts the mold up so that the cap connected to the base is pressed upon the top of the metal. This operation I have never seen, but it may be useful in preventing the formation or increase of size of blow holes in the steel. Whether gas, after it has once formed, can be driven out of the steel through the chilled outer skin of metal is a mooted point, but the end compression of the ingot must assist the work of longitudinal shrinkage and prevent cracking or tearing of the outer tender shell of partly cooled metal, and also by shortening the cylindrical ingot, assist the flow of the liquid metal into the cavity tending to form in the center. It is likely that an ingot so made will have a smaller center cavity than one cast without pressure. The pressure, if applied to the steel while fluid, is doubtless distributed, in accordance with laws of fluid pressure, equally throughout the mass. While so applied, in my opinion, it prevents blow-holes of appreciable size, and during later stages of cooling prevents cracks from shrinkage.

CCLV.

STEAM AND POWER; THE COMMERCIAL DETERMINATION OF COSTS.

BY HENRY R. TOWNE, STAMFORD, CONN.
(Member of the Society.)

THE accurate determination of boiler and engine efficiency under the varying conditions of actual use has been the subject of study and discussion by engineers ever since the early days of the steam engine. Although authorities differ in regard to matters of detail, there is a general consensus of views as to the proper methods for making accurate tests, both of boilers and engines. In all cases it is a good plan to have accurate tests of this kind made occasionally, as conducive to the discovery of unsuspected sources of loss, and as tending to promote further economy in the management of steam plant.

Tests of the kind above referred to, being both troublesome to arrange for and expensive to conduct, are necessarily resorted to only occasionally. In all large establishments, however, it is desirable to obtain, by some continuous method, reasonably accurate indications concerning the operation and management of steam plant. Desiring to effect this in the establishment under his management, the writer has arranged and adopted the system described below. It has now been in use for some fifteen months, and having proved satisfactory and useful, it is thought that a description of it may be of interest.

The conditions of the case in question may be briefly summarized as follows: The steam plant in the establishment referred to,* consists of a battery of 6 tubular boilers, each rated commercially at 60 horse-power, of which usually only 4 are in use during the summer, and 5 during the winter months; of a Harris-Corliss steam engine, having a cylinder 20″ diameter and 42″ stroke, running 72 revolutions per minute, with about 60 pounds boiler pressure, and developing 120 horse-power; also of a pump, connected

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* The works of The Yale & Towne Manufacturing Company, Stamford, Conn.

with an artesian well and operated continuously during working hours. The steam generated is utilized for power, for pumping, and for heating, and its cost is, therefore, to be divided between these three items. The measuring and observing instruments consist of an indicator, which is connected to the engine on one day of each week, during which day 6 or 7 cards are taken at about equal intervals of time; of a water meter, for indicating the amount of feed water supplied to the boilers; and of a counter attached to the deep well pump by which the amount of water raised is computed. The weight of coal consumed is computed by counting the barrows as wheeled in, they being always loaded equally, as nearly as can be determined by the eye. The only important assumption in the calculations is that relating to the amount of water evaporated per horse-power per hour, which is taken at 30 pounds.

Appended hereto is a transcript of the blank form of report arranged by the writer, and in use at the works referred to, necessarily condensed in size for convenience in printing.

Page 1. This comprises so much of the record as relates to the boilers and fuel. The latter consists, in this case, of bituminous slack and anthracite dust, mixed in the proportion of about one to three or four. Each lot of coal as delivered in the yard has a small placard placed on it bearing a number by which the lot may be identified. The purpose of this is to enable the purchasing agent to be correctly informed as to the quality of each lot purchased, as a guide to him in regard to values in future transactions, and also as a means of checking the weight of fuel burned as reported by the engineer. A small amount of cinders and coke screenings is also burned, in order to get rid of them, the proportion of these varying from 3 to 10 per cent. of the total fuel. The value of this latter fuel is inserted at an arbitrary price per ton. The value of the two kinds of coal is inserted at the actual cost of each. Finally, a considerable quantity of chips and shavings is burned (in order to get rid of them), the value of which, as a fuel, is determined by assuming that during each hour in which it is used under a boiler, its value is equal to the value of the coal fuel required to run the same boiler an equal length of time. Columns are provided for indicating the number of hours during which the boilers are in use, whether under fire or banked.

Page 2. This contains the record indicating the use of steam. It shows the number of hours run by the main engine; the number of strokes of the deep well pump (from which its duty is calculated); the number of hours during which the steam is admitted to the buildings for heating purposes, and the amount of opening of the valves. A record is also kept of the temperature, wind, and weather.

Page 3. This contains, first, a record of the indicated horse-power of the engine. To ascertain this, the indicator is connected early each Saturday, and cards are taken, during running hours, at 9, 10, and 11 A.M., and at 2, 3, and 4 P.M., thus giving 6 observations each Saturday, or a total of 24 per month. The mean of these is assumed to indicate the average horse-power developed during working hours. Two other cards are also taken each Saturday, just after the close of work at noon and in the evening, in order to ascertain the horse-power required when all the machine tools are disconnected. The purpose of this is to enable any unusual increase in the amount of power required to operate the line shafting, and the machinery permanently connected therewith, to be detected, and the cause investigated and if possible corrected. The large amount of power required "after working hours" in this instance, is due to the fact that some 10 or 12 rotary fans, several of them being of large size, are in constant use, and are permanently connected with the line shafting. The works employ from six to seven hundred hands, and as the buildings are large, and are separated by yards of considerable size, the transmission of power to the several buildings involves a very considerable length of shafting, and the use of numerous jack-shafts, mule-pulleys, etc.

On page 3 is given also a table showing the cost of water pumped from the artesian well. The total number of gallons pumped is ascertained accurately. The cost of raising this is assumed at five cents per thousand gallons, this arbitrary figure being based upon well-established facts relating to the cost of raising water by power to a given height.

On page 3 is also given a summary of the cost of fuel, showing the percentage of each kind of fuel used, and the cost thereof per ton, and finally the total cost of fuel burned.

Page 4. This contains an analysis of the use of steam, showing the average weight of fuel burned per square foot of grate per hour, the average evaporation per square foot of grate per hour, and also per pound of fuel burned. The aggregate horse-power developed during the month, is then determined, and the evaporation being assumed at thirty pounds of water per horse-power per hour, the total gallons of water chargeable to the engine is thus ascertained. The number of gallons of water pumped from the well being stated, the amount of water evaporated in the boilers in order to supply the steam needed for lifting the water pumped from the deep well is ascertained by multiplying the former figure by an arbitrary multiplier, or equivalent, indicating the proportion of a gallon of water of evaporation required to raise one gallon of water from the well under the stated conditions. Finally, the amount of water evaporated which is chargeable to heating purposes is assumed to be that remaining out of the total, after deducting from the latter the amount chargeable to the engine and to pumping. These figures are then utilized in the table which follows, and which indicates the percentage of steam chargeable to power, to pumping, and to heating.

At the foot of page 4 is a similar table, in which are given the actual amounts of all other expenditures for steam and for power. The total cost of steam being thus ascertained, it is distributed between power, pumping, and heating in the ratios previously determined, as explained above. The total cost of power is then ascertained by adding to its portion of the charges for steam the expenditures for wages and supplies.

The information thus obtained is utilized to enable an exact distribution of charges for power, water, and heat, to be made among the several departments of the works in which this system is now in use. For this purpose a careful determination was made of the *proportionate part* of the power, which, under average conditions, is required in each of the several departments of the works. In like manner, by carefully ascertaining the cubic contents of each room, the amount of window surface, etc., the *proportionate amount* of steam required for heating each has been determined. The further use of steam for manufacturing purposes (for heating kettles, etc.), being known, it thus becomes possible at the end of each month to make an accurate distribution to each department of the charges against it for power, for heating, and for steam for manufacturing purposes. By providing water and gas meters for each of the principal departments of the works, an accurate determination of the charges for these accounts is also arrived at. Finally, a summary of all the charges above referred to, is made on a separate form, as shown below, by means of which each department can be charged monthly with the actual cost of these several items, ascertained and distributed with a degree of accuracy quite

MONTHLY CHARGES FOR POWER, HEAT, STEAM, WATER, AND GAS.

For January, 1887.

DEPARTMENT.	POWER.	HEAT.	LIVE STEAM.	WATER.	GAS.
A..........	$126 53	$62 08	$44 20	$27 00	$78 17
B..........	56 24	103 46	......	12 60	7 92
B..........	11 25	12 07	......	6 80	2 16
C..........	9 84	6 90	......	4 50	18 49
D..........	14 06	24 14	2 60	5 40	1 80
E..........	37 96	43 11	3 90	7 65	3 24
F..........	8 44	32 76	......	6 30	9 95
F..........	4 22	25 36	......	6 30	12 80
G..........	8 44	6 90	78 00	3 15	1 26
H..........	2 81	8 62	1 80	2 70	2 84
J..........	1 40	1 72	......	1 35	2 84
K..........	......	5 17	......	2 70	9 40
L..........	......	3 45	......	2 25	6 27
M..........	......	1 72	......	......	......
N..........	......	6 90	......	......	......
P..........	......	......	......	1 80	3 24
Q..........	......	......	......	......	5 76
Totals....	$281 19	$344 86	$130 00	$90 00	$166 14

sufficient for all ordinary commercial purposes, and obtained without resort to any special or expensive methods of observation.

To enable the operation of this system to be more easily understood, the two blank forms which are appended are given with the ctual figures inserted as taken from the records for the month of anuary, 1887. The blanks required in other establishments, ould doubtless require some modification from those here given, order to adapt them to altered conditions. In the main, hower, it is believed that the method herein shown is substantially rect, and that it offers an inexpensive and reliable basis for the urate distribution of an important group of expense accounts in business using a steam plant, and in which it is desirable to rtion the cost thereof among different shops or departments.

[First page of Blank Form.]

ENGINE ROOM REPORT.

For January, 1887.

BOILERS.

Date.	Fuel.						No. of Boilers in Use. 5.				Amount of Feed Water, Gallons.	Remarks as to Fuel.
	Bituminous Coal, Lbs.	Number of Lot.	Anthracite Coal, Lbs.	Number of Lot.	Cinders and Coke, Lbs.	Hours Burning Wood, etc., One Boiler.	Day.		Night.			
							Banked.	Fired.	Banked.	Fired.		
							Hours for One Boiler.					
1	....	....	....	....	....	....	....	33	9	30	....	No. 21 was a bad lot of anthracite dust, and necessitated using equal parts of the two kinds of coal.
2S	....	....	....	....	....	...	25	25	40	30	....	
3	20,700	3	20,700	21	....	...	....	55	30	35	....	
4	10,800	3	10,800	21	....	....	....	55	30	35	. .	
5	8,700	3	7,800	21	1,200	....	....	55	35	30	....	
	9,300	3	9,300	21	....	1	....	55	40	25	....	
	7,200	3	7,200	21	....	....	....	55	20	45	....	
8	11,100	3	11.100	21	....	....	....	50	15	25	....	
9S	....	....	....	....	....	....	20	25	40	25	....	
10	12,900	3	15,900	22	....	....	....	55	40	25	....	On lot No. 22 again, using 75 per cent. of same.
11	4,200	3	12,600	22	....	....	....	55	50	15	....	
12	4,500	3	13,500	22	....	1	....	55	50	15	....	
13	4.200	Cumberland.	12,600	22	....	....	....	55	50	15	....	Using one car load of Cumberland. It mixes and burns well.
14	3,000	3	9,000	22	....	1	....	55	50	15	....	
15	2,400	3	7,200	22	....	....	....	50	25	15	....	
16S	....	....	....	....	....	....	20	15	15	20	....	
17	4,800	3	14,400	22	....	1	....	55	45	20	...	
18	8,900	3	11,700	22	....	....	....	55	15	50	....	
19	5,400	3	16,200	22	....	1	....	55	50	15	....	
20	3,900	3	11,700	22	....	....	....	55	50	15	....	Feed water record not used this month owing to meter being out of order.
21	2,100	3	6,300	22	....	1	....	55	50	15	....	
22	3,600	3	10,800	22	....	1	...	50	26	....	....	
23S	....	....	....	....	....	....	20	15	24	15	....	
24	3,000	3	9,000	22	...	....	....	55	35	30	...	
25	3,300	3	9.900	22	....	....	....	55	50	15	...	Evaporation assumed at the average of previous month, viz., 155.93 gals. per boiler per hour.
26	2,700	3	8,100	22	....	....	....	55	45	20	....	
27	3,600	3	10,800	22	....	....	....	55	40	25	....	
28	3,300	3	9.900	22	....	....	...	55	50	15	....	
29	3,000	3	5,400	22	1,800	....	..	50	....	....	....	
30S	....	....	....	....	....	....	20	10	15	30	....	
31	3,600	3	10,800	22	....	....	....	55	50	15	...	

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USE OF STEAM.

Date.	Main Engine Running Hours.	Pump. Number of Strokes.	Heating Buildings.							Thermometer.			Wind Average.		Remarks: as to Weather, etc.
			Exhaust.		Back Pressure. Lbs.	Live Steam.									
			Hours.	Opening.		Day.		Night.		7 A.M.	12 M.	6 P.M.	Direction.	Force.	
						Hours.	Opening.	Hours.	Opening.						
1	....	......	....	....	..	....	....	....	....	....	....	....	....	....	
2S	....	......	....	....	..	....	....	10	⅓	14	24	12	....	....	
3	10	4,366	10	½	3	10	½	8	⅓	10	20	14	N. W.	S.	
4	10	3,990	10	½	3	10	½	7	⅓	10	20	24	E.	L.	
5	10	5,228	10	½	3	10	½	6	⅓	18	40	36	E.	S.	
6	10	3,275	10	½	3	10	½	5	⅓	31	46	34	N.	M.	
7	10	3,517	10	⅓	3	10	⅓	9	⅓	26	40	30	W.	M.	
8	9	4,155	9	⅓	3	9	⅓	5	⅓	10	20	14	N.	S.	
9S	....	......	....	....	..	....	....	5	....	....	....	....	....	....	
10	10	5,185	10	⅓	3	10	⅓	3	⅓	22	30	22	N. W.	S.	
11	10	3.953	10	⅓	3	10	⅓	3	⅓	12	20	18	N. W.	S.	
12	10	4,107	10	⅓	3	10	⅓	3	⅓	22	40	30	W.	M.	
13	10	3,851	10	⅓	3	10	⅓	3	½	24	40	34	S. W.	M.	
14	10	2,982	10	⅓	3	10	⅓	3	½	36	40	36	N. E.	M.	Rain.
15	9	3,569	9	⅓	3	9	⅓	3	⅓	30	32	28	S. E.	L.	
16S	....	......	....	....	..	....	....	4	½	....	....	....	....	.	
17	10	2,948	10	⅓	3	10	½	4	⅓	36	44	40	E.	M.	Rain.
18	10	5.938	10	⅓	3	10	½	10	⅓	20	21	14	N. W.	S.	
19	10	10,054	10	⅓	3	10	⅓	3	⅓	8	16	26	S. W	S.	
20	10	5,880	10	⅓	3	10	⅓	3	⅓	30	36	34	S.	L.	
21	10	4,756	10	⅓	3	2	⅓	3	⅓	36	44	40	S. W.	L.	
22	9	3,084	9	⅓	3	6	⅓	....	....	26	48	36	E.	L.	
23S	....	......	....	....	..	....	....	3	⅓	....	....	....	....	....	
24	10	......	10	½	3	2	⅓	3	½	36	50	36	N. W.	S.	Rain.
25	10	5,515	10	½	3	3	⅓	3	⅓	28	40	44	S.	M.	
26	10	3,282	10	½	3	10	½	4	⅓	36	40	28	N.	S.	Rain and Snow.
27	10	9,343	10	⅓	3	10	½	5	⅓	12	20	26	S. W.	M.	
28	10	1,967	10	½	3	10	½	3	½	28	40	38	S. W.	M.	
29	9	......	9	½	3	2	½	....	⅓	36	48	40	S. W.	L.	Rain.
30S	....	......	....	....	..	....	....	6	⅓	....	....	....	....	....	
31	10	......	10	½	..	10	½	3	½	32	38	32	N. W.	M.	
Totals....	246	100,945	246	....	....	231	....	130	....	62.9	890	766	....	....	
Average..	9.46	4,588	9.46	½	....	8.1	½	5	⅓	24.1	34.2	29.4	....	....	

[Third page of Blank Form.]

INDICATED HORSE-POWER.

To be taken every Saturday.

Date.	9 A.M.	10 A.M.	11 A.M.	12.03 M.	2 P.M.	3 P.M.	4 P.M.	4.50 P.M.	Remarks.
8	139.7	142.5	141.8	......	134.87	136.50	134.00	71.5	
15	146.2	143.6	142.18	......	130.00	121.87	126.25	66.62	
22	141.37	141.37	141.37	......	125.98	125.12	136.62	67.37	
29	133.24	138.10	131.62	......	121.87	130.00	135.12	61.75	
Totals........	560.51	565.57	556.97	......	512.72	513.49	531.99	267.24	
Means	140.13	141.39	139.24	......	128.18	128.37	132.99	66.81	

MAIN ENGINE.

Cylinder, 20 Inches · Stroke, 42 Inches.

Average revolutions per minute............ 72
Average pressure of steam in Boilers, in lbs.................. 62

Average Horse-power, as above, during working hours..... .... 135.05
Average Horse-power, as above, after working hours 66.81

WATER.

Total number of strokes of Pumps........................... 100,945
Total number of gallons pumped, at 3 gallons per stroke............................ 576,735
Assumed cost per 1,000 gallons. .... .05

Total cost for month.................................. .... 28.84

February 3d, 1887. I. H. B., Engineer.

Remarks.... .. ..

SUMMARY OF CONSUMPTION OF FUEL.

Kind of Fuel.	Lbs.	Proportion. Per Cent.	Tons of 2,240 Lbs.	Average Price Per Ton.	Cost.	Remarks.
Bituminous Coal.............	145,200	34.39	64.82	$3 90	$252 80	
Anthracite Coal.........	272,700	64.58	121.74	2 37	288 85	
Cinders, Coke, etc............	3,000	.71	1.34	60	80	
Wood, Shavings, etc.........	....	*.32	....	....	†1 76	
Totals	420,900	100.00	187.90	Mean, $2 89	$544 21	

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* To ascertain proportion of wood, find (1) total number of hours *all* the boilers are fired, and (2) total number of hours burning wood : then divide (2) by (1) and result will be proportion desired.

† To ascertain value of wood burned, divide total number of hours run of boilers burning coal and cinders into cost of latter. Then assume *value per hour* thus ascertained as the value of the wood fuel, and multiply this by the number of hours burning wood.

[Fourth page of Blank Form.]

STEAM.

Average weight of fuel consumed per square foot of grate, per hour		9.10 lbs.		
Average evaporation of water per square foot of grate, per hour		7.25 gals.		
Average evaporation of water per lb. of fuel burned		.799 gals. = 6.66 lbs.		
Total gallons of water evaporated		336,510.		
MAIN ENGINE, Number of hours run	246			
Average Horse-power	135.05			
Aggregate Horse-power for month	3,376.25			
Gallons water per Horse-power per hour	3.60 (= 30 lbs.)			
TOTAL GALLONS CHARGEABLE TO ENGINE		119,600		
WELL PUMPS, Number gallons pumped	576,735			
Assumed equivalent	.00393			
TOTAL GALLONS CHARGEABLE TO PUMPS		2,266		
HEATING, Balance of water evaporated:				
TOTAL GALLONS CHARGEABLE TO HEATING		214,664		
TOTAL GALLONS		336,510		
Percentage of steam chargeable to POWER			35.54	$238 25
Percentage of steam chargeable to PUMPING			.68	4 55
Percentage of steam chargeable to HEATING			63.78	427 55
			100	$670 35
HEATING, Cost of:				
Steam, as above	$427 55			
Wages	47 81			
	$474 85.			

SUMMARY.

BOILERS.	
Cost of fuel for month	$544 21
Wages of firemen	119 06
Wages of night firemen	
Water, 15 per cent. of evaporation, say 50,476 gallons at .05 per 1,000	2 52
Gas, 32 per cent. of total 1,600 cubic feet at $1.80	2 88
Repairs during month	$1 68
Sundry supplies	
TOTAL COST OF STEAM	$670 35

POWER.	
Steam, 35.54 per cent. of total cost	$238 25
Wages	31 11
Oil, 9 gallons cylinder oil at 80 cents	7 20
Oil, 2 gallons machinery oil at 40 cents	80
Waste, 10 lbs. at 9½ cents	95
Gas, 32 per cent. of total 1,600 cubic feet at $1 80	2 88
Repairs during month	
Sundry charges	
TOTAL COST OF POWER	$281 19

DISCUSSION.

Mr. James E. Denton.—I would like to ask the author if the amount of water used by the engine and the amount of evaporation of the boiler per pound of fuel and water per horse-power of engine are given by this report. I presume for his purposes these data are not of any particular interest, but to one situated as I am, desirous of collecting reliable data about the duty of engines and boilers, it seems to me that a record of this kind is likely to give us information of a great deal more value than isolated tests do, and I should be interested in knowing on this account. I should also like to know if Mr. Towne can use his meter for a long time without testing its accuracy. Is it a meter of approved type?

Mr. Towne.—Yes, sir.

Mr. Denton.—We have been using one for some time and I find it is necessary to make a little by-pass to test this meter from hour to hour. We recently fitted up a tug-boat for some thesis work and we found that the meter gave 115 pounds to the cubic foot and, on opening it, we found the slide valve was sprung. I should think it very necessary to check the meter by arranging it so that at any moment the meter might stop the feeding of the boiler and deliver a known number of cubic feet on a platform scale. I want to say in this connection that I think the only figures of any value practically for distinguishing between the duties of engines and boilers must come from records of this kind which go on from day to day. I believe that isolated tests of boilers, ten hours at a time, are prone to errors which constantly arise and keep our records dancing up and down without our being able to get any satisfactory deductions from them. I should therefore like to see figures resulting from such arrangements as this put in the scientific units. Mr. Towne speaks of the cost of maintaining this system being a trifle. I should like to know how much that was. It strikes me that such a detail as this, kept up perfectly, must be a strain on some one, and would take quite a fraction of one man's time to attend to it. I believe that any record in a manufacturing establishment which gives units for comparison, if the work is at all uniform and the comparison can be made to stimulate the attendants from day to day by getting closer and closer to desirable results, is bound to be an important feature in manufacturing. Where mill-work is uniform, competition is very close as to coal, and the practice of using scales for weighing coal is becoming more general every day, and I have seen very useful results come from that. I attempted, in some tunneling operations with which I was connected last year, to apply a system of this kind to the cost of powder, drilling, mucking, hoisting, etc., to greater extent regarding detail than is usual, and I found we could not find results to offset the cost except while a number of gangs were working under similar circumstances, so that our data could stimulate one against the other. Then it appeared to have a usefulness which paid for its cost. If competition can be brought to bear, I think such elaborate systems of records always have a money value as profit.

Mr. George H. Babcock.—I notice a point of some interest to us as giving a partial answer to a topical inquiry at our last meeting, which was, "How much power have you found it to take to drive machine tools?" We have here a machine shop of some size, and we have the indicated horse-power given. We find on the eighth page that the total horse-power required to drive the shop is 135.05, while that taken to drive shafting and blowers is 66.81, leaving 68.24 horse-power to drive the machine tools. It is also stated that there are 700 men employed. You will see that this is a little less than a horse-power for ten men, which was the estimate I gave roughly at that time as a fair approximation.

Mr. H. R. Towne.—In regard to measuring the feed water I may say that that was done by a meter made of brass, especially for the purpose by the manufacturers, and that we have had a great deal of trouble with it. The meter has been returned two or three times for repairs, and I think has just recently come back again from the makers. The complaint at first on their part was that our water must have sand or grit of some kind in it, but such is not the case. We have changed its position twice to meet their views, but with no benefit yet, and I am unable to account for this constant trouble, unless it is inherent in the system of meter construction. Water passes through the meter at a temperature of about 212.

The cost of keeping this record is correctly stated in the paper as being trifling. All of the original entries are made by the engineer, the man who has charge of the engine and boilers, and are entered by him on a report blank, just as a marine engineer keeps his log. The boiler entries occupy daily probably five or ten minutes, spread over the day, and practically give the engineer so slight an addition to his duties that he does not notice it.

The other work, namely, of digesting the record and reducing it to the form needed for use is attended to in the office and requires to be done by an intelligent and careful person, but it does not occupy more than about one hour of his time once a month, and justifies, therefore, I think, the statement that it is trifling in cost.

Referring to the blank forms which are printed in the paper, I might explain further that the first page contains the fuel record. It is a blank large enough to give a line for each day of the month with space for the entries to be made comfortably, in pencil or ink, by the engineer. In this he enters every day the amount of coal burned and certain other particulars which are shown. On the next page, which is also kept by him, is the steam record of each day, showing the number of hours during which the engine is run, the amount of back-pressure on the exhaust in winter for heating purposes, the amount of opening of the different valves for heating and for passing steam for manufacturing purposes, and also a record of the strokes made by the supply pumps. On the third page the engineer's record is completed by noting the indicated horse-power of the engine, which is taken on one day of each week at intervals of two hours, or a little less. We get an average of six or eight cards during the day on one day in each week. The engineer, by the way, is provided not only with an indicator but with a planimeter for computing the result of the cards, and these entries are made by him, and also the record of the total number of strokes of the supply pumps. The feed-water record is kept on the opposite page, and, having these data, it is a very simple matter to deduce from them the other items which are of interest. In the first place we make up the commercial account, showing the amount of coal of each kind burned, and ascertain the actual cost of that coal from the bills, thus showing each month the total cost of fuel, the average cost per ton, and the ratio between the two kinds of coal used, which consist, in this case, of anthracite dust and bituminous slack, burned in the ratio of three or four to one. On the fourth page are deduced from the preceding record such items as the average weight of fuel consumed per square foot of grate per hour, average evaporation of water per square foot of grate per hour, average evaporation of water per pound of fuel burned, and the total number of gallons evaporated. We then take the horse-power of the engine, as given by the indicator, determine its aggregate for the month and deduce the total amount of water which has been pumped and used for power. In like manner the counter on the supply pumps indicates the amount of water raised for all purposes through the works, and an arbitrary coefficient is taken as representing the cost of raising that water, since it is simply so many foot-pounds of work done. This is easy to do and is sufficiently exact. The amounts of water used for pumping and for furnishing steam for power are put together and their sum subtracted from the total amount of water pumped into the boilers, the remainder being assumed to have been used for generating steam for other purposes, namely, heating and manufacturing. The total cost of steam is thus divided into three proportionate parts. Its actual cost in money is ascertained by adding together the charges for fuel, wages, water, and repairs, as shown at the foot of page four, amounting in this case to $670. Now, the figures already obtained give the division of that into three heads, viz., power, pumping, and steam used for other purposes than power or pumping; and taking these proportionate parts, you can at once divide the money value into three items. Having thus ascertained the cost of steam used for power, it is entered in the last column of the fourth page. To it is added the wages chargeable to power, the cost of oil, water, gas, sundry repairs, etc., the sum of these items giving the total cost of power.

I think the use of the tables will be clear from an examination of them by any one accustomed to the use of steam.

Mr. Denton.—Does it not take a great deal of time to figure that out?

Mr. Towne.—One hour a month. The entries are easily made by the engineer from day to day, which he does at intervals during his work when he would otherwise have nothing to do, and it costs nothing.

CCLVI.

A PROBLEM IN PROFIT-SHARING.

BY WILLIAM KENT, NEW YORK CITY,
(Member of the Society.)

If we admit that sharing of profits among the workmen in a manufacturing establishment will be of benefit both to employers and employed, and so in some degree tend toward a solution of the labor question, on what basis should profits be divided among workmen in a business in which the profits or losses depend rather upon the efficiency of the selling department of the business than upon that of the manufacturing department?

Suppose the following hypothetical case: Three manufacturing companies A, B, and C, each produce per year 100,000 of an article which under average conditions costs $1 for labor, material, and shop expenses. It is sold by expensive advertising, the employment of agents on high salaries and commissions, and the giving of large discounts. These selling expenses cost $100,000 per year, and the selling price of the article is $2.10. All three concerns being on the same footing, each makes 10 cents profit on each article, or $10,000 in one year. Suppose that the next year, through competition or other cause, the average selling price is reduced to $1.50.

A, in the hope of cheapening production, introduces the profit-sharing system, and in the expectation of sharing the profits of the manufacture, the workmen became so much more efficient that the cost of production, for material, labor, and shop expenses, is reduced to 75 cents, the daily stipulated wages of the men being reduced 5 per cent, and the amount of production being increased 25 per cent. The selling organization and expense remains the same as the preceding year, costing $100,000, or 80 cents on each of the 125,000 articles. The total cost for production and sale is therefore $1.55, or five cents more than the selling price, making a loss on the year's business of $6,250, and no profits to be divided among the workmen, although their wages were reduced 5 per cent in expectation of such profits.

B puts in better machinery, runs overtime, pays its workmen the same wages as before, and doubles its product. The greater efficiency of the machinery, and the dividing of shop expenses by a larger product, reduces the cost of production to 80 cents. More liberal advertising and more agents increase the selling expense to $120,000 per year, which, divided by 200,000 articles sold, is 60 cents each. The profit and loss account then shows:

200,000	manufacturing cost at 80c	$160,000
do	selling expense cost at 60c	120,000
		$280,000
do	sold at $1.50	300,000
	Profit	$20,000

C thinks its selling department costs too much, cuts down salaries, commissions, and advertising to such an extent that the yearly expense of the selling department is reduced to $40,000. It consequently does not increase the quantity of its business as A and B did, but sells the same number as before, 100,000. No change being made in the manufacturing department the article costs, as before, $1.00 each. Result:

100,000	manufacturing cost at $1.00	$100,000
do.	selling expenses at 40c	40,000
		$140,000
do.	sold at $1.50	150,000
	Profit	$10,000

Comparing the results briefly, A has the most efficient workmen, who give up five per cent. of their wages, and reduce the cost of production 25 per cent., yet these workmen, through no fault of their own, get no profits, and less wages than the workmen of B and C. B and C both pay their workmen as before, and both make a profit through the adoption of two exactly opposite lines of policy, B spending money more liberally, C cutting down expenses.

Does not this hypothetical case, which is not at all unlike cases which continually happen in actual business, show that if profit-sharing be adopted in a manufacturing business, in which the selling of the articles produced is entirely separate from the making of them, that the share of the workmen should be calculated not on the profits of the whole business, but on the savings in the manufacturing department alone?

The workmen of A were entitled to a share in the saving of 25 per cent. which they made in the cost of manufacturing, and if the selling department had been managed like those of either B or C they would have obtained it. Had B and C adopted the profit-sharing system in addition to their change of policy in the selling departments, their profits would have been still greater, for their cost of production would have been reduced.

The following is suggested as a fair basis for profit-sharing: From the statistics of a year's production in the establishment make an estimate of the cost for labor and shop expenses of the articles manufactured. Call this the maximum allowable cost for the next year. Pay the men the same daily wages as before, and at the end of the year pay them in addition a certain percentage, which was agreed on in the beginning of the year, of the difference between the maximum allowable cost and the actual cost, if there has been a saving. It might be fair to deduct from the maximum cost in this calculation any saving which is clearly due to the introduction of new machinery or to the expenditure of capital, and not to increased efficiency of workmen.

The following is an example under this method:

No. of pieces made.	Material.	Labor.	Shop Expenses.	Total.	Labor and s. ex. only
1st year..100,000	$20,000	$60,000	$20.000	$100,000	$80,000
Each piece costs	20c.	60c.	20c.	$1.00	80c.
2d year...150,000	30,000	70,000	20,000	120,000	90,000
Each piece costs	20c.	46⅔c.	13⅓c.	80c.	60c.

Maximum allowable cost for labor and shop expenses 150,000 at 80c. $120,000
Actual cost.......... ....150,000 at 60c. 90,000

Saving..$30,000

This saving of $30,000 is to be divided between the employers and employees in proportions previously agreed upon. The shop expenses, including rent, taxes, insurance, gas, fuel, wages of superintendents and clerks in manufacturing department, and wear and tear of machinery, are properly included in the amount upon which savings should be calculated, since if the total cost of these is a fixed sum per year, the cost per piece produced depends upon the number of pieces made, and hence to a great extent upon the efficiency of the workmen. The cost of raw material is not included, because this cost depends upon fluctuations of the market, and to some extent upon the foresight and judgment of the purchasing department, and generally not upon the efficiency of the workmen. Material spoiled by the workmen, or excessive waste of material, might, however, be charged as part of the cost of labor.

By this method of profit-sharing, the profits to be divided among the workmen are the profits which they make themselves by more rapid work, by carefulness in avoiding waste of time and material, and by general increase of skill. It removes the most serious objection to the general principle of profit-sharing, that it is easy for the capitalist to share profits with his workmen, but impossible for the workmen to share in the losses which the capitalist must bear himself. It makes the workmen share the losses if there are any in the manufacturing department, in which department alone the workman is concerned. He shares neither the profits nor the losses of the purchasing and the selling departments, with which he has nothing to do.

The writer is not aware that the plan herein suggested has ever been tried, but thinks it likely, in view of the numerous experiments which have been made with profit-sharing in various forms, that it may have been tried somewhere. If so, he would be glad to learn what were its results in actual experience.

DISCUSSION.

Mr. Wm. Hewitt.—This is certainly a common-sense and concise way of presenting this important problem, and Mr. Kent's suggestion as to the basis on which such a division of the profits should be made is no doubt very fair and just, but the question arises as to the practicability of such a scheme. Who is to determine what the material costs? If such a system were adopted, the employees of course would naturally wish to be satisfied as to the correctness of the estimates, and this might be a difficult thing to do in certain cases. Where a concern is manufacturing a solitary article, the cost of which can be determined with accuracy, the scheme would no doubt be feasible; but where a concern manufactures a variety of articles, would it not lead to as many disputes as now occur under the existing relations between capital and labor? Whatever the result might be, a scheme of this kind should certainly not be applied with any idea of obtaining labor at lower rates than the market commands, but solely for the purpose of increasing its efficiency; and this is what I understand to be the purport of Mr. Kent's suggestion. To a man who is paid by the day, of course it is an incentive for him to be more energetic and less wasteful; but for a man who is paid by the piece, the matter does not possess the same significance. Such men usually do their best anyhow, not to mention frequent spurts; and all the incentives which could be offered them would produce no better results, as far as quantity is concerned. The only incentive which the profit-sharing system would have to this class is to do better work or decrease the amount of waste. The natural inference, therefore, is that it is a better policy to pay by the day, and give the workmen the benefit of the difference between the actual and maximum allowed cost. The determination of the latter would open as many questions for discussion between employers and employees as the determination of the schedule of wages. Would the advantages be worth the effort and would not the tendency be for the employers to make the difference between the actual and maximum cost as small, and the employees to make it as great as possible? With the increased efficiency in the men which such a system of profit-sharing might effect, the old estimates on which the original maximum cost was based would lose much of their significance in the lapse of time, and the general tendency would be, I think, for this figure to decrease. The conditions which would govern such decrease would depend largely on the skill of the men, and the improvements in mechanical appliances which may be introduced; and the question arises, would the decrease in the actual cost be proportionately as great? In other words, would not the actual and maximum costs—like water—tend to a common level, the level of good management? So that the only profits to be considered, after all, would be what could be realized on sales. If this scheme is to become the means of improving the management of a concern, it would seem to have more force if those to whom the management is intrusted should receive the benefit of any share of the profits which may be set aside for such a purpose.

Mr. Jno. T. Hawkins.—I do not believe that any equitable system for profit-sharing has yet been or can be suggested, nor that the basis suggested by Mr. Kent, on the third page of his paper, can be considered as fair to the manufacturer or employer. It can be shown, however, I think, to be a decidedly one-sided arrangement. The fundamental idea conveyed by Mr. Kent's proposition is, as I understand it, that, whatever diminution in the cost of product shall come from increased efficiency on the part of the so-called workman only, shall accrue to them alone as a share of the profits, no matter what conditions may obtain in those parts of the business over which they have no control, or which their operations do not affect. If this is not, in principle, what he aims at, what he proposes is merely ordinary profit-sharing, except that some portion of the *personnel* engaged in the selling part is left out in the cold. But he thinks that his hypothetical cases show "that the share of the workmen should be *calculated*, not on the profits of the whole business, but on the savings of the manufacturing department alone," and says, further on, that the saving of $30,000 in his example in the second over the first year "is to be divided between the employers and employees *in proportions previously agreed upon*." As the saving, however, so far as the workman's interest is concerned, hinges on the meaning of the word "calculated" in the first and on "in proportions previously agreed upon" in the second quotation, what the workman would get additionally to his regular wages remains very indefinite, particularly if he accepted a reduction in the latter, as in one of the hypotheses. If the workmen, so-called, are to share in the profits of a given business, it can only be done by their sharing, also, in all the responsibilities, as well as in the losses, when the latter obtain; and I do not think that the basis proposed provides any better means for doing this than ordinary profit-sharing, where all may share in the entire profit. Articles of manufacture are as good as not made until they are sold; and their sale, as well as the purchase of the materials of which they are made, through and by means of the machinery of commerce, is as much to be regarded as a part of their production as the mere labor expended upon them in the factory; and this constant effort to draw a line of marked distinction between the men who sell and buy and plan and originate and conduct and convey the articles made in the factory and those who labor at their actual construction, regarding the latter as the only workers in the case, is most mischievous, and goes a very long way toward encouragement of the present unsatisfactory state of things in the world of labor.

A steam-engine or a printing-press or a mousetrap is not a perfected production, occupying its allotted and designed place in the world's economy, until it is sold, transported to its destination, erected and ready for operation; and every person employed in any capacity whatever, including the employer and his subordinates who assist him in the conduct of the whole business, such as superintendent, salesmen, clerks, book-keepers, foreman, buyers, etc., are as much workmen, and contribute their share to the placing of the pig-iron or ore or trees of which the article is primarily constructed in its place in the world as an operative machine or product, as the man who makes the patterns or the drawings or the castings, or he who applies his labor to the various processes which are necessary to its mere construction as a machine or product. It would profit the workman, so-called, little, be he never so assiduous and productive in his part of the performance, if the salesman does not sell his product, if the book-keeper does not perform his duties properly, if the designer causes imperfect machines to be built, if the draughtsman makes errors such as to offset a workman's greater efficiency, or if the proprietor adopts such methods throughout that his machines or product cannot compete successfully with others; and there would be no more equity in confining the so-called workman's emoluments to and basing them upon the net results of his particular branch of the business than there would be in the proposition that the employer and his other assistants—salesmen, buyers, etc.—should be accorded a certain profit independently entirely of the so-called workmen, leaving to the latter the losses or gains, as the case might be, after the proposed profits were paid to the others. It would be quite as proper and equitable to suggest that, in order to encourage the salesman to sell more machines, the buyer to make better bargains, the railroad company to transport more quickly, safely, or cheaply; the superintendent so to arrange the details of the business as to save more at every point, the book-keeper to dispense with a part of his assistance and do more himself, and the proprietor to manage his part of the business better and more economically—to allow them all, in addition to their fixed salaries or rates of commission, a share of the profits, leaving the so-called workers the responsibilities and the losses which might ensue, as to insure the workmen a profit based upon their efforts, to the exclusion of the rest of what goes to make up the entire *personnel* of the business.

Profit-sharing, to be equitable, means co-operation; and co-operation fails, because men cannot do well what they have not learned to do or had experience in doing. A blacksmith cannot be a book-keeper nor a salesman. His voice, therefore, in such a concern, is good only so far as it applies to blacksmithing. And so it is with every branch of such a concern.

As the world has gone on supplying the multitudinous demands of civilization, it has been, and, in my opinion, will be, best done by a man or set of men fitted by education and experience as proprietors, with the necessary money capital, carrying on and conducting the business for the production of a given article ; and he or they who supply this capital and experience and ability to carry it on, if done successfully, must have the privilege of deciding what that part of his or their business, which is so often mistakenly distinguished as "labor," alone shall cost, quite as much as how much he or they will pay for advertising or selling or conveying or buying; and of obtaining it in the open market just as he does his materials and all the other items which go to make up his total cost. The only other equitable way is to make every one concerned in a business a proprietor—which, in other words, is co-operation ; and, so far, the latter has failed, and inevitably will, I think, always fail, for the reason that it calls upon men to exercise forces not at their command. To make co-operation successful, every member of such an association must be capable of performing successfully the duties of a proprietor. The actual capital employed in any business consists of two kinds—money and services rendered. Every person contributing his services to the prosecution of any business invests it in a portion of the total capital employed, which is in proportion to his ability to perform, and the relative proportion of his services as existing in the sold product. The money capital invested is used to enable the proprietors of personal capital to obtain a fair interest on their investment ; and the workmen hires this capital and makes proportional use of it just as much as the proprietor or any other person engaged in the business. The money capital must, of course, earn its interest and be protected, so as to be always prepared for the exigencies of business which call for its use; but there is no kind of capital invested in business which obtains so much or so sure an interest upon the value invested as that contributed by the *personnel* of a business in services rendered. Let us, however, see how the proposed scheme would work, if what Mr. Kent regards as the workman were to receive all that he saved by his extra efforts—which are the only conditions under which he may be said to be the recipient of all the savings he may effect. Let us apply this in the light of Mr. Kent's own hypothetical cases. Assuming that he would divide the total cost of mere production among the items, labor, material, and expenses, in the four hypothetical cases, in about the same ratio as in his example on his third page—*i.e.*, 20 per cent. material, 60 per cent. labor, and 20 per cent. expenses,—this would give, in his first case, for labor, 30 per cent. of the total cost of producing and selling; and I have no doubt that, in many manufactures, this proportion would hold good. In such a case, the employer, with his money capital, together with all those that take part in the business who are generally distinguished as apart from labor, and exist under the elevated title of expenses, would supply 70 per cent. of what constitutes the total cost, and the so-called workman, 30 per cent. in his labor. Now, if the profit is 5 per cent. on the total cost, as in case 1, it seems fair, without any other change whatever, that $\frac{3}{10}$ of this should be divided among the so-called workmen, and it would be, provided that, in case of loss, the latter could be similarly divided. Three-tenths of this 5 per cent. profit on the total cost would be equal to 1½ per cent. of the total cost; but this, if paid him as a share of the profit, would be equivalent to an advance of 5 per cent. in his wages. But in his second hypothesis, Mr. Kent suggests that the so-called workmen consent to a reduction in fixed wages to this amount, expecting that, by dint of doing what was clearly his duty to do in any case, *i.e.*, produce as much in the allotted hours as he could do, without undue exertion (and certainly he could not expect to do any more than this, in any case), he could produce so much more that he would cheapen the cost of production by 25 per cent., or the total cost of production and sale by 12½ per cent. But the selling price, owing to competition, has gone down 25 per cent., everything else remaining as efficient as before, and unchanged. Now, in this case, who is to bear the loss? Is it fair that the results of the so-called workman's efforts—which are strictly only what he was in duty bound to perform in the first year—shall be rendered to him as profit, when the results of the remainder of the *personnel* of the establishment, as well as of the money capital employed must be less, because of the reduction in price? Should not the workman bear his proportion in reduction in price as well as the proprietor or any other person taking part therein? Under the hypothesis, if the workman is to receive all that he has added to the value of the product, he will receive 12½ per cent. of the total cost, because he saved that much by his superior assiduity over the preceding year, and loses 5 per cent. of his wages, equals

1¼ per cent. of the total cost, which he voluntarily submits to in reduction of his fixed wages, and would be in pocket 11 per cent. of the first year's total cost, or would have received what is equivalent to an advance of 36⅔ per cent. in his wages, while the employer would suffer a loss equal to over 3 per cent. on the total cost, to say nothing of the value of his personal services. As in this case the proprietor could scarcely be held responsible for the drop in prices, this would be equitable profit-sharing with a vengeance! And if the line is to be drawn somewhere below the proprietor, where the profit-sharing man ends and the non-sharing salesman, superintendent, or other person begins, all on the same side of that line with him would share with the proprietor his loss. But if the workman is not to receive all this because *he* produces it, to be equitable, he must share it with all the rest in some proportion previously agreed upon--and the scheme becomes ordinary profit-sharing, in which all participate. It is either one or the other, or some one engaged in the business is treated unfairly. And we have only to ask, if the fall in price more than equals the gain by the workman's extra efforts, how can a proper share of the net loss be borne by the workman?

In Mr. Kent's third case, B first puts in better machinery. Well, if he does, he probably has to advance or borrow from some one who will accept his signature as collateral an additional amount of capital, in order to buy this improved machinery; and he will be lucky if, by means of it, for several years, he realizes more than the interest on the additional outlay necessary to buy it. Then he runs overtime. Mr. Kent may have found out how this may be made to contribute to greater product per dollar expended; and, if he has, I have no doubt the majority of manufacturers would like to be let into the secret. My experience has been that it causes three distinct series of losses when it is resorted to: First, all workmen expect to be, are, and should be, paid higher rates for overtime; second, the work is done at night, under indifferent light, and after the operative is comparatively exhausted with his daily endeavors, and therefore he does not produce so much in a given time; and, third, being worked more hours per day than when not working overtime, he is not able to accomplish so much in his regular hours the next day. Instead of gain, therefore, this process involves three distinct sources of loss. If the average manufacturer could, by putting in better machinery and working overtime, paying the same regular rates of wages for regular hours and extra rates for overtime, reduce the cost of his product 20 per cent., after paying interest on the additional capital required for the improved machinery, we ought, all of us, to be able to retire from business at a very early age. But Mr. Kent supposes the product to be doubled by the means taken, while he has reduced the cost 20 per cent. Therefore he either works his men a good deal more time or employs more men; and, as the capital employed will be somewhere in proportion to the amount of wages paid out, there will be another little item of interest due to the proprietor or to the bank or to the party from whom he may borrow—all of which constitute a part of the current expenses, and, therefore, of total cost to him; and, instead of his finding he could devote 10 per cent. additional to the cost of advertising and selling, and having 7 per cent. profit left, his interest account and loss from non-efficiency of workmen—due to working overtime—would bring him out at the small end of the horn.

In the third hypothesis, C furnishes an equally singular piece of ratiocination. Here we have the manufacturer cutting down salaries, commissions, and advertising (in which case, certainly, the poor devils engaged in those pursuits do not see much prospect of profit-sharing), but continues to sell the same quantity of goods. Well, who would not reduce these items, under such circumstances? One would only have to repeat this process for a year or two to make the goods sell themselves! The first factor in this hypothesis is, therefore, so far from probable that it will be hardly necessary to follow it further. But let us take his example.

As I have said above, the first hypothetical case will no doubt approximate quite closely to what is experienced every day, *i.e.*, that which Mr. Kent designates as the cost of selling, will equal the cost of production at the factory. Under such conditions, the total cost of the article manufactured, for the first year's product, will be $2 each; and, if, by dint of superior efficiency in the workmen, he reduces the labor and shop expenses from 80 to 60 cents each, for the second year, everything else remaining the same, the total cost of each piece will be reduced 10 per cent.; and, if the so-called workman were to get all that his efforts have added, it would all be paid to him, under the arrangement, as additional profit, because he alone made the saving. Under such circumstances, suppose that during the first year the employer made a profit of 5 per cent., and that during the second prices had fallen, for reasons alike beyond the control of employer and employee, 15 per cent., while all not included under the name of "workmen" had been as assiduous as they — a by no means unusual case. Would it be fair that the employer and those on his side of the line should suffer a loss of 5 per cent. this second year, while the workman should receive an addition of 10 per cent. of the total cost, equal to 33⅓ per cent. advance in his wages? And if so, and this state of things should continue for several years, how long would it take to wreck an ordinary business? And, as, when prices go down in one business, they generally do in all, would there not be a plentiful crop of failures, and an equally plentiful harvest of unemployed workmen, eager to work at the original figure, rather than not at all? And if the saving made by the workmen alone is to be divided among all, in some preconcerted ratio, does it not become merely ordinary profit-sharing, in which no one has as yet succeeded in formulating a means of the workman's sharing in the losses, when they occur? Or, if only the selling department is to be excluded from this arrangement, why should the employer be included, so long as he contributes no more toward the superior results than the selling department? But one of the worst features in any such scheme is that every workman in a manufacturing establishment would share *pro rata* with his wages in any profit coming from their superior total efficiency—which would be proper enough, if every man would consent to have his fixed wages rated just in proportion to his ability and industry, as determined by his employer or superior officer, or the results of his efforts. But we all know that it is not, and cannot be, in human nature, that any such equality of effort or ability as is involved, can exist in any considerable body of men. In a machine shop, for instance, where the general determination might be to increase the product with a view of correspondingly increased income, the more competent and industrious would contribute much more largely to the general results than the naturally inefficient and idle; and every shop *personnel* is made up more or less of the various grades of men, both as to ability and energy. Such a scheme would be a leveling engine, contrary to the first principles of equity; and one of the first fruits would be quarrels among the workmen themselves, because some were not contributing their *pro rata* share to the increased product, where others were doing more than this. Any scheme whatever, touching this labor question, of a leveling nature, and which tends to reward the idle and naturally inefficient man equally with his competent and industrious brother, must fail just as this very thing is to-day disintegrating labor organizations throughout the country, which have for their object the equalizing or leveling of wages.

Referring to loss-sharing, the following from a recent editorial in one of our New York dailies expresses it admirably:

" Yet, before profit-sharing or co-operation can become an effective agency for the solution of labor problems, this side of the question will have to be faced and dealt with practically. The truth is that it is not a simple question, but goes to the root of nearly all the difficulties which swarm about the relations between capital and labor. For it involves a just comprehension of the value, utility, and necessity of those personal qualities which distinguish successful from unsuccessful men everywhere and under all conditions."

And again, referring to the responsibilities of workmen:

"The second" [workman] "has no anxieties or requirements outside the narrow range of his mechanical duties. His income is steady and safe. He is far less sensitive to the fluctuations of the markets than his employer. He, as a rule, is so little in touch with the industry by which he lives that when crises happen and it becomes necessary to reduce wages, nine times out of ten he jumps to the conclusion that the reduction is the result of parsimony instead of necessity, and so he drifts into calamitous strikes. If workingmen undertake to co-operate or to adopt profit-sharing, they must, to some extent, change places with their employers. They must learn the absolute necessity for accepting the downs as well as the ups of business. They must be prepared to share the losses as well as the profits."

" In no other way can they develop the qualities which are required for success. In no other way can they acquire habits of steady thrift, ceaseless industry, patient application, vigilant supervision, prescience, and grasp of the situation—and all those habits are indispensable."

There is no royal road to wages or profits any more than to learning; and the true profit-sharing workman will inevitably be he who makes himself most valuable. He can always command a share in the profits through higher comparative wages; and if in any establishment a given production were obtained in one year at a saving of 33⅓ per cent. in labor and shop expenses over the previous year, as in the example of Mr. Kent, due solely to superior efficiency of the workmen, the main point that would be established would be that in the first year the workmen had failed of their plainest duty. And herein lies the gist of the whole matter. The problem of the so-called profit-sharing assumes, to start with, that the wage-earner does not participate in the profits; while, under a proper state of things, in every establishment in which the proprietors give recognition in wages to the workman's individual efficiency (and they must do this, if they consult their own interests, just as they must in buying material or in transacting any other branch of the business), and the workman strives to and does make himself the most efficient possible, he will command the higher pay, just as the best qualities of material will bring the higher price; and he will invariably receive his share of the profits in his wages, and in a more equitable way than by any scheme of profit-sharing that can be proposed. In my opinion, the manufacturer who experiments with profit-sharing—on Mr. Kent's basis, particularly—will find himself on the station platform, wondering when the next train leaves. Edward Atkinson says that there is always plenty of room on the front seats in every profession, trade, art, or industry; and I cheerfully join him in the following quotation:

> "Honor to him who, self-complete and brave,
> In scorn can carve his pathway to the grave;
> And, caring not for what men think or say,
> Makes his own heart his world upon the way."

Mr. H. R. Towne.—Mr. Kent closes his paper with an intimation that the method he outlines, which, in brief, is that the workman's share in profits should be limited to the things which he can control and should not take cognizance of things he cannot control, is a novel idea. I must dissent from that opinion for the reason that many writers have for a long time past discussed this question. Moreover, I am impelled by the statement of Mr. Kent to say—what I had not intended to say until a later meeting—that on the first of January last I put into practical operation a system embodying precisely the feature alluded to, which system is now in force as affecting, perhaps, one or two hundred men, and which I hope to have in operation by the end of this year as affecting two or three time that number.

The points which have been touched upon in discussion are most of them pertinent, and yet I think that some of the statements need modification. No one rule will apply to all kinds of manufacturing or production. Take, for example, the business of sugar-making, in which, if I am correctly informed, the labor item is a fraction of a cent per pound on the cost of the finished product, or from ten per cent to five per cent of the value of the finished result. Obviously the system of co-operation or profit-sharing which is possible in a business of that kind, where labor is a minute fraction of the whole cost, and where the skill with which the buying and selling are conducted is the major part or larger factor in determining the result, would not apply equally to a business of the kind usual in the average manufacturing establishment, where labor amounts to approximately one-half of the total cost, the other half being composed of material and expenses.

Objection has been made that a system of profit-sharing interferes with the adoption of piece work. We all admit the theoretical correctness of the latter system, under which every man is working for himself; that is the ideal condition in any industrial establishment. But it is entirely feasible, as I know by actual experience, to have the one system coexisting with the other; the men being employed and paid by the piece for the work produced, but also having some further interest in the economy of their work.

The objection is made that profit-sharing tends to the leveling of wages. Again I answer that experience denies the statement. It is just as possible, with a system of profit-sharing, to regulate the wages of each man by his value as a workman as it is to do it if you do not have the profit-sharing system in force also. I believe, however, that at the present time, if not permanently, a system of profit-sharing based on the labor cost of goods must be coupled with a periodic revision of the basis rate which affords the means of comparison between the original cost of the goods and the reduced cost accomplished by the increased efficiency of labor; that you cannot, in most businesses at least, adopt an original basis of comparison and adhere to that permanently, but that in the course of time, say every five or ten years, or some other interval of time, that basis must be revised and readjusted to the changed condition of things.

The point is made that no profit-sharing is equitable unless it includes loss-sharing. Broadly stated, I think that perhaps this is true; but there are modifications in profit-sharing which eliminate this difficulty. The one which I have ventured upon in a somewhat large way, after much study and consideration, may be more correctly designated as *savings sharing* than as profit-sharing. It is based upon ascertaining the present cost of a given product in labor, or in the things that labor can control, and then agreeing to divide any reduction that may be effected in the labor item below that cost with the producers; not giving them all of it by any means, possibly only one-half, but giving them some fraction which is equitable under all the circumstances, and making an arrangement whereby these conditions will last for such a length of time as will give the producers an inducement to exert themselves to reduce the cost of production by earnest effort on their part.

As I stated at the outset, I am not ready, at present, to submit in detail the plan I alluded to, but I hope to do so at a later meeting, after it has been longer in operation, and when I am assured that it is a success. During the four months it has been in operation, however, I may say that it has operated very satisfactorily in reducing the cost of goods. Although pitched, as to piece rates, upon a basis just as low as previous records indicated it should be, the workmen continue to receive precisely the same day wages or piece rates as previously. The results indicate that giving the workmen the stipulated fractional part of the savings effected will yield them, at the end of the year, an increase of from 5 to 8 per cent. on their wages, a bonus which is a handsome inducement to any workman who has industry and desire to get on in the world to exert himself.

Mr. W. H. Doane.—I would ask whether that increase was in the increased quantity of production or in the saving of the manufacturer?

Mr. Towne.—The gain results almost entirely from increased efficiency of production. But the basis is such that the men are interested also in the economy of the materials which are consumed in the shops. I do not mean by that the raw material which enters into the product, but all other material, such as oil and waste, files and cutters—all incidental materials necessary for the operation of the shop. And there, by the by, is one of the points of difference between a system of profit or savings-sharing like this, and a system of piece work. In piece work, the workman is interested in making his own efficiency as high as possible, but has no interest whatever in the economy with which he uses the plant. On the contrary, his interests and those of the manufacturer are opposed to one another. If a workman, by the wasteful use of tools, can increase his product and wages, he naturally does it. There is obviously a point where the loss from this wastefulness will more than offset the increased product. Piece work, pure and simple, has, therefore, this element of antagonism between workman and employer. If you superadd to piece work, however, the inducement of profit-sharing, or, to call it by its better name, "savings-sharing," the workman ceases to have this incentive to be wasteful in his use of shop appliances, and becomes at once identified with his employer in seeking to promote efficiency of production and economy of production in all directions.

I have already exceeded my time, I am afraid, but I wish, with the consent of the meeting, to make one further comment, and it is this: It is argued in all discussions of this kind, that profit-sharing should always include loss-sharing. I do not think that this is a correct view. The difference in the conditions of the classes of men must be taken into account. The employer, as a rule, has reserve capital. He can stand loss for awhile, and still not suffer in his domestic conditions; whereas the workman generally has but little, if any, reserve of that kind. Loss to him, therefore, means immediate suffering for himself and family. A prudent householder or head of a family should not incur risk of that kind if he can avoid it, and rightly; therefore, with reference to their present responsibility, and to those who are dependent upon them, the workmen should decline, or, at least, be very cautious about entering into relations which involve a possible sharing of losses on their part. Employers, on the other hand, having some reserve to fall back upon, can consistently take risks which may result in loss, but which they hope will result in profit.

I think, therefore, that, as society exists at the present day, better relations of labor and capital must be sought by systems of co-operation or profit-sharing, tending to identify the two interests, under which the employer gives a guarantee of fair wages to the workman, and also arranges that he may have, beyond that, a reasonable and equitable interest in the economy and increased efficiency with which he does his work, some portion of the gain resulting from that increased efficiency coming back to the employer as his fair return for the time, trouble, and cost which he has given to establishing conditions under which labor may gain for itself this enlarged privilege and reward.

Mr. Allan Stirling.—I did not intend to make any remarks on this paper, but it appears to me fitting to call attention to Mr. Towne's expressions of his kindly feeling to his workmen. I believe that when employers have those kindly feelings to their workmen it makes very little difference whether the system adopted is the co-operative or the profit-sharing system, or whether it be high wages. I have occasion to come in contact with a company who have little occasion to consider this question at all. They never had any trouble with their workmen. They all live right among their men. They are well known to their people. They have kindly feelings toward their workmen, and, as I say, a strike is unknown to them ; they do not know what it is, and I think that, given the kindly sentiments such as Mr. Towne has expressed, in the hearts and minds of the employers towards those whom they employ, the method of arranging any difficulties that may arise is quite immaterial. They can be arranged in some satisfactory way.

Mr. W. H. Doane.—I am much pleased with the remarks of Mr. Towne, and shall be greatly interested to learn the result of the profit-sharing co-operative experiment he is making. I am aware that somewhat similar efforts have been made by others in different parts of the country, but, as far as I am advised, they have not been successful. I have not much confidence in any co-operative plan.

I believe in paying workmen all they are really worth, even if it be very large wages. The problem in my mind seems to have resolved itself into something like this: If small wages are paid and a share in the profits divided with the workmen, based on what they are worth, in lieu of what their services should command, or even more than they could command, it acts as a stimulus to greater efforts, and they will feel that the value of their labor is appreciated, and manifest a deeper interest in their work, insuring, I think, closer application, which alone, of itself, will secure increased production.

One of the great questions of the hour with manufacturers is, how shall they increase the product of their works without enlarging their plant or pay-roll, and any system which will bring the employer and employee into closer relation with each other, it seems to me, will tend to secure this result, whether by piece work, co-operation, or both.

My past experience, somehow, has not led me to feel much confidence in co-operative plans, and, as far as my observation has gone, they have not proven a success. One reason may be the general impression among workmen that one is as good and can do as much work in a given time as another. Select a hundred mechanics out of any shop, and you will find the degree of intelligence and mechanical ability among them will vary greatly, and yet none of them will concede that he is worth less, or can do less than any other. To question this generates trouble at once, and yet we all know it to be true.

For example, suppose we select two establishments, each regarded as good shops; that one having the best tool equipment, and is the best organized and managed, will turn out its work for less than the other possibly can. Now, taking the cost of production in this shop as the standard, and using a co-operative plan, something after Mr. Towne's suggestion, the contrast in the reduction of the cost in manufacturing as against the shop not so well organized and with poorer equipments, even with equally good workmen, would quickly be apparent. Therefore, it seems to me, this question of profit-sharing is likely to prove a hard matter to define.

I am a believer in paying all a man is worth, and, by so doing, secure his very best efforts. I feel quite confident that an examination of the fact will disclose that the best and most successful shops are those that employ the highest grades of intelligent and skilled labor. Such workmen are diligent, faithful, devoted, and the establishment in whose employ they labor generally does a profitable business. I am profoundly interested in Mr. Towne's new plan of profit-sharing, and sincerely trust it may be so successful that he will kindly unfold it to us at our next meeting.

Mr. Jno. T. Hawkins. I cannot agree with Mr. Towne, that, in case of disaster to a manufacturer, the workman loses his all, while the manufacturer has something on which to fall back. I take it that the reverse of that is rather true than otherwise, because the capital the laborer puts into his business is simply his ability to work, while the manufacturer has a large money value at stake besides, which he has to manage and provide for; and it is liable to be lost. Now, I think there is one way by which profit-sharing or co-operation might be made successful, provided the workmen themselves would agree to it; that is to say, a joint stock company, in which a workman desiring to become a profit-sharer might be a stockholder. Throughout the United States there are a great many millions of dollars in savings banks, to the credit of working or laboring men. It is particularly the case in the New England States, where the men are more generally thrifty than in more southern localities. They have a considerable amount of money saved and laid up. Suppose, for example, one man has a thousand dollars invested at 4 per cent. in a savings bank; you could go to him, and say: Well, if you want to share in our profits, we will sell you as many shares of our stock, at their value as shown by our books, as your $1,000 will buy; if at the end of the year we have not earned anything, we will assess you in proportion to the loss—if there is no surplus; or you may make a little, or lose it all in time. Now, I will venture to say that there is not one out of twenty that would take those chances. As a matter of ethics, it is as near right for a workman to take the money chances with the proprietor, in proportion to his investment, as for the proprietor to take them; and, if the workman insists upon the employer taking the biggest or all the risk of loss, he must expect that, when times are flush, the latter will—and it is right that he should—fortify himself against the risks of losses which obtain when the balance comes upon the wrong side of the ledger.

[NOTE.—*The remaining discussion bore more closely upon the Topical Query, No.* 259—49:

"What system of regulating the wages of labor in our manufacturing establishments will tend to make that labor most efficient and produce the largest returns both to employer and employee. Give especially data from actual experience with effects of piece-work, premiums, participation in profits, graduation of wages with terms of service, etc., etc.

It is printed here from its close relation to what has preceded.]

Mr. C. E. Emery.—It appears to me this discussion has not reached the root of the present labor difficulties. We should endeavor to induce the workmen to organize on different principles, so that methods used by business men in conducting their business may be employed in dealing with labor. A manufacturer wishes to know for a reasonable time in advance at what rate he can procure his labor as well as his materials, in order that he may make contracts and send out a definite product in a definite time. Therefore, if organizations must be had—and I think it is the intention of the labor organizations to organize all labor as fast as they can—it is the business duty of all business men to try and get their labor on a business basis. With the present organizations the interests of the employer and employed are antagonistic; and no matter if the former strive to put themselves on good terms with the latter, as suggested by Mr· Stirling, the labor organizers and walking delegates immediately introduce discordant elements, all reason disappears, and the selfish side of human nature is put in play. On the present system each working man feels that he must stand by his fellows, right or wrong. If one employer underpays or misuses employees a strike is ordered in all similar establishments, and the kindness which others have lavished upon employees is all lost, and the good must suffer with the evil. Strong parties may win by show of force, but the system of the present labor organizations is to push the weaker parties to the wall and squeeze all they can out of them. In these contests the workmen suffer with the employers, and the solution seems to be for the latter to capture the leading men of the organizations and try and show them their true interests so that they may all be respected as much as the Association of Locomotive Engineers is now. The present system will degrade them by forcing employers to pay all at the same rate, thus suppressing all honest emulation to improve one's self and causing all to relapse a step towards barbarism. The difficulties are greatest in large cities. Many establishments have been forced to move into the country in self-protection, while others, not having experienced the difficulties in the country, are preparing to move in the city to gain some business advantages by having their shops nearer their business offices. It is not believed that the co-operation system will solve the problem permanently. It will aid in particular cases and under particular circumstances. But where the present movement has once got a firm foothold, success can only be secured by inducing the labor leaders to co-operate with employers so that the latter can make their calculation in a business way. If the organization can be induced to make the object of their societies to secure permanent employment rather than intermittent work at high wages, and command sufficient respect with the mass of employees to enable them to give up their views of perfect equality, the labor question will be solved whether the men be paid regular wages, as is customary, or by the piece, either with or without co-operation. I really see very little difference between the piece system and the co-operative system as Mr. Towne has developed it, although he has not told his whole plan. The piece system makes it for the interest of a man to do more work than his fellows in order to obtain more pay, so that if good prices are offered by the employers it practically amounts to co-operation. The success of this plan on the Pennsylvania Railroad apparently confirms this view.

Mr. O. C. Woolson.—I want to extend to Mr. Emery my thanks for saying what I have had in my thoughts to express. I came from Newark with this particular paper in my mind because of all the subjects before the meeting I think this is one of the most important. The one great trouble I find—and I presume it is found by the majority of the members, with this co-operative or profit-sharing scheme, is that we have got all kinds of people to deal with. Now, if I am going to organize a company, if I am going to get around me a board of directors, I want a board of directors such as I can harmoniously associate with. I want men who are in sympathy with me in my enterprise all the way through. I do not want to have them only partially interested and with an idea that there is some other man—in this case we will call him a walking delegate—who claims by by-laws and oaths his sympathies and prejudices, who can come around at certain periods and influence the men in the slightest degree. Now, that is the case. You may organize a most perfect co-operative system. You can pick out all the best points that have ever been devised and put them together, and I do not believe the time has come yet nor the scheme has been devised yet when you can put a sufficient number of those points together and have them practical. I am a workingman myself, and I have a great deal of sympathy with those men who have to get up early in the morning and work until late at night, and at best only earn enough for a very plain living; but there is one thing you must consider: wages are established in different branches of business all over the country. They all have a level somewhere; it is pretty well understood just what that level is. Now, if my company is so generous as to say, here, we will pay these regular wages that are acknowledged to be the proper wages everywhere, and at the end of the year we will give you men a certain percentage of our profits, if there should be any, and you all shall be profit-sharers. but not loss-sustainers. Now, there is a point—we must not assume to pay a percentage to a certain class of our men, but we must take in everybody. The consequence is we get men who are scarcely worth a dollar a day and we are getting men who are worth ten dollars a day. We will assume that the men getting ten dollars get it because they are worth it, and we can reason with them concerning percentages and loss and profits, so that it will be mutually satisfactory, although there are exceptions to this rule; but you come to a man who is getting his dollar a day and you can't reason with him, as a rule, about percentages and profits; take such men, as a body, understand me, when the time comes to pay that certain percentage I undertake to say that seventy-five per cent. will not be satisfied, and if by good luck they should be the first year, long before the next dividend day comes round, the chances are the labor organization to which he belongs will take action and inform you that your methods are demoralizing the men in other factories, and you find yourself in hot water.

I will further venture one remark which, in my opinion, is a practical one, and that is, where you have a man that is valuable and whom you appreciate, give him an opportunity to understand and believe that you do appreciate him, and if you want to express it in dollars and cents there is not one of us who would not know just exactly how to do it if we have the will.

Mr. Olin Scott.—You may have the will to do that, but you may not have the money. I have been thinking, while listening to the discussion, that this body here is a representative body, perhaps such as cannot be found in the United States or any other country. I know a great many of the men here who have grown up from poor boys: they have worked their way up; if they have any money they have earned it. They are a class of men, who, if organized as these laboring men organize, and should go on a strike, might make a great deal of trouble. There does not seem to be any disposition to do anything of the kind, and I don't know that anyone but myself has ever thought of it. Now, I know that the meeting of this body is going to be looked upon, throughout the whole length and breadth of the land, as a body that represents the labor question, because they are laboring men; they have worked from the lowest wages up to the highest. Their places cannot be filled by men who can be picked up in the country for a long time, and they have a right to speak and be heard. Now these questions that come up here before this meeting are becoming so numerous that it seems to me they ought to be formulated in some way; that an expression like that of the House of Representatives, or something of the kind, should be adopted, by which a man could record his vote without scrutiny, if desired, and let it be known that what is voted is the expression of this organization on this question. Now, this labor question is before this meeting to-day, and takes precedence of all others. We have heard the expression of one gentlemen here, who has, in a kindly, good-natured way, consented to make an experiment with his operatives and see how it works. That has been done a great many times before. He shows his good-nature by doing that. Perhaps his good-will toward the workingman is not greater than that of others here, but when he says the workingman should not take the risks of the capitalist, where is he going to put the men who have just a little capital, and who start in manufacturing?

Three or four men go in together; they borrow their grandmother's money and their sister's money, and they get a little money to start, and employ a dozen more workmen; is it fair to say to those workmen: You go and do the best you can, and if there is any money made you shall have so much extra, no matter if it does take grandmother's money and sister's money. Now, there are thousands of shops in this country in this condition. A man will work industriously and save his money, and just as soon as another man hears the chink of it he wants to get it; and that is so with these labor organizations. I have been a workingman and I worked for the smallest wages, and I always saved money. Now, you might just as well take this bull by the horns, and say what you mean. I do not care if the whole afternoon is devoted to this question; if anyone has anything to say, I would like to hear it. I feel as though I would like to see this thing fought out on a square basis.

Mr. George L. Fowler.—For a long time there has been an undercurrent in the minds of some car builders and master mechanics that tended toward the advocacy of the contract system of repairing cars and locomotives, but, from the faint-hearted, or rather let me say from the timid way in which the case has been presented, it is evident that these gentlemen, though firm in their convictions that the contract is the correct system of doing the work, have had no data, taken from actual practice, to support the claims which they have made for their hobby. They say that it is all right, that it can be made to work, and that if time were taken, the plan could be fully elaborated. Then their opponents come down upon them like an avalanche; they show the tremendous amount of detail necessary; they picture the expense of the army of clerks, foremen, and inspectors that will be required, and the impossibility of getting the system established on a firm and satisfactory basis; marching out of the discussion with flying colors.

Meanwhile the Pennsylvania Railroad has been building up a system that answers every requirement that the most bitter opponent could demand. They have put into actual practice a system which, for the simplicity of its detail, the minuteness of its specifications, the effectiveness of its action, and the general satisfaction that it gives to all of the parties concerned, exceeds the bounds of what would once have been considered possible, and demonstrates, in the clearest manner, its perfect practicability and desirability.

To perfect and develop such a system two things are required: honesty and a strict attention to the cost under the system of daily wages. The last must receive the first attention. For years the rate of wages must be studied, and a system of time-keeping established which is minute in its detail. The men must be required to give in, each day, the time occupied in every move that they make. Not a bolt should be driven nor a nut tightened that does not receive the due charge of time that belongs to it. The bookkeepers must separate each item, and classify and re-classify until some order seems to appear out of the chaos of data that flows in upon the office. Waste of time must be watched and the actual time expended in doing the work determined. I am speaking, it will be understood, more particularly of the repairs of railroad work or of a general job shop than of those places where there is a special line of manufacture; in these cases the scheduling of the prices for piece work will be comparatively easy.

This careful watching was carried into every detail by the Pennsylvania Railroad. They learned not only how long it took to make the rocker arm of an engine as a whole, but the expense for each and every step was noted; the time required to forge, to center, to lay off, to turn, to mill the edges, to chip or mill the bosses, to bore the pin holes, to ream them out, to bore the bushing, to fit and to case-harden the same, to put it in position and to connect the rods and links. Car details were kept with an equal strictness. The sills were watched with careful scrutiny. A memorandum was taken of the time required by the men to carry the stick from the lumber pile to the planer, also that occupied in facing and dressing, laying out, cutting the tenons, boring the holes, and putting in position.

Are you frightened by these few examples that are given? Well, the same detail and attention must be given not only to the sills and rockers, but to posts, carlines, plates, sheathing, rods, transoms, trucks, to guides, connections, links, eccentrics, cylinders and pins, to every bolt and screw in all of the complication of a car or engine. When all this had been done, there was a mass of matter which had to be sifted, sorted, and arranged until the whole was in that perfect order that was demanded before it could be put into use.

The time required to turn the crank pins on a switcher and a mogul were compared, and, if possible, were classified as one. This is but a single instance, but comparisons of like and like had to be made on every hand and the whole reduced to the smallest compass.

The car repairs were undertaken first, for they did not present so many pieces to be counted and estimated as the locomotive. The construction of the freight cars, though varying greatly, was still found to be similar enough to be classified together.

When all this had been done, the honest purpose of the master had to appear. Under the old system the men have been paid for the time which they spend about the shops and yards as well as for the work performed. Two inducements must exist before the change can be made. The men should receive more pay for the time they spend; the company should receive more work for the money it expends. If these two results are accomplished, it goes without saying that both parties will be content. But the grasping of the corporation must be restrained, and its management should not look to the wages paid to its men as individuals, and think that it costs them twenty-five per cent. more per man, be he laborer or foreman, than it does a competitor. But it should look at the total cost and see that it is paying from fifteen to twenty per cent. less for certain work than the neighbor, whose men are earning less per capita.

This requires a strong and sterling honesty. The temptation to cut and grind must be resisted with a "get thee behind me, Satan," and the men be made to feel that they have their own fortunes in their hands, and that the disposition to make them slaves does not exist.

Cutting can be done, and must be done to make the system pay, but not so as to prevent the men earning increased wages. This must be governed by circumstances and the knowledge that the master has of the detail work be brought to play.

When the rates are once made they must be enforced. The men can work in gangs, and schedule rates be given to the whole. Sometimes a gang will be composed of laborers, apprentices, and machinists. In the system under consideration, the apprentices get a fixed sum per day, according to the year of his apprenticeship; and when he works in a gang the latter must pay his wages, while all that he earns at schedule rates goes to the gang. The receipts of each member of the gang are proportioned at a certain rate of daily wages. For instance, if a gang is composed of four laborers, a second-year apprentice and two machinists, the laborer will be rated at $1.25 per day, the apprentice at ninety cents, and the machinist at $2.40. The wages for the gang, discarding apprentices, will be $9.80, of which $5 is for the laborers. A division will give 51 per cent. for labor and 49 per cent. for the machinist. Then at the end of the month, if the schedule dues are $340, the apprentice gets ninety cents per day for say twenty-five days, or $22.50, leaving $317.50 for the rest. Then 51 per cent. of this, or $161.92, belongs to the laborers, or $40.48 each, netting them nearly $1.62 per day. The machinists receive $77.79, or over $3.11 per day. These are not exceptional wages.

This calculation is complicated to a slight extent by absence or tardiness in gang work, but very little, as each man is paid *pro rata* for the time that he works.

The method employed is to work in gangs to a certain extent. Piece work is now universal on the road in question. The men write on a common slip of paper every night what they have done; this is O.K'd by the foreman and handed to the timekeeper. The schedule rate for each item is credited to the proper man, and the work is done. In shops employing from four to five hundred men, three timekeepers do all the work required.

I have given some of the details for the machine and carpenter work. Those for labor are fully as complete. It is necessary to have a system for handling material, and to have a place for everything and to keep everything in its place. So, in the case of a railroad shop, we have a storage track for drivers, another for front truck wheels, another for tender wheels and axles, and the same for cars. A gang is given a certain price for taking a pair of drivers from the storage track to the lathe, the erecting floor, or *vice versa.* The price is fixed for handling axles, castings, boiler plates and cinders, for jacking up an engine or a car, for taking out a pair of wheels, and for every step that a man may take about his work.

Inspectors are employed to supervise and pass upon the work.

These men are paid a stated sum per month. The foremen are also paid by the month. Gang foremen are, however, paid by the amount of work that the gang performs, and at stated times. For instance, the foreman of the erecting gang receives a fixed amount for taking an engine into the shop, jacking it up, stripping it, erecting, taking to the paint shop and getting it out upon the road. One quarter is due when the engine is stripped, another when it is set up, the third when it is run into the paint shop, and the last when it is ready for the road. The men, meantime, are paid for the work they perform; this keeps the foreman from neglecting the work that he has in hand. If he were paid only when the work is done he would be apt to rush one engine through to the neglect of all others; but as he gets his pay on the instalment plan, he can afford to carry on several at a time. When the engine is set up he gets one-half the schedule, but to prevent neglect the remainder is held back until he puts the locomotive in the painters' hands. However, if the paint shop is full he gets his pay before, as the company do not hold him responsible for their own lack of space. The car work is done on the same principle and the work scheduled in the same way.

The effect of the system is an interesting social study. Men work for themselves, and their pay is due to skill alone. The injustice of an arbitrary rating is done away, and the poorer class must fall below their fellows. At first the system was met with a tooth-and-nail opposition, and the schedule was denounced as a corporation steal. A strike took place, but one by one the men came back and were surprised to find their pay increased from 25 to 30 per cent. After this they discovered ways and means to hasten work; they keep a double set of tools on hand so that the lathes are always at work; they do not take an aimless walk, and stand and talk; they carry what they want to their lathes, and do not wait for a laborer to do their errands. Besides this, they help each other, and do not waste their time. If there is work to do, they spring to with a will. You see no difference, whether the master is at hand or not, and if he speaks to any man, the latter seems to be in a hurry to get away to work.

I called attention to the industry of the laborers in the remarks that I made on this same subject at the meeting of the association in New York last fall.

The opposition that was met at first has entirely disappeared, and now the opposite obtains. Once in a while a piece of work comes up for which there is no schedule rate. The man to whom it is assigned will immediately ask for piece work. The foreman can then make a bargain for the job.

The flexibility of the schedule rates is shown in other ways. There are, for instance, certain lathes equipped for axle work and the schedule is made to fit them. If, however, a press of work comes in, and other lathes, less suited to the purpose, are called into action, the men who run the inferior tools are allowed a higher rate per piece. Then, too, if a man runs two lathes, his rate per piece is cut away, and he gets only a two-thirds rate, although this gives an increase in the rate per day in every case.

The effect on wages has been to send them up, and it is rare to find a man that does not earn more than when the daily pay was given. This increase varies with the man and the work that he has to do, from 25 to 30 per cent.

On the other hand, the effect on cost is to cut away. In the case in hand it has been shown to be more than 20 per cent. saved in the work done, as compared with the cost under the old system of daily pay.

This is an outline of the plan. The details are innumerable to start with, but when once established and the system put in operation, this detail seems to "vanish like the breath into the wind," and the works to run themselves. There can be no *pros* and *cons* where demonstration such as I have shown lies ready to the hand. The advantages are overwhelming and cover the only vital points at issue. The men want increased pay and get it; the company wants decreased cost and that is given them; and then, to clinch the matter and bind it fast, neither party would in any way consent to give it up. Its practicability is fully demonstrated by experience and practice. Injustice in rating is done away with; the better class of men will be allowed to rise and make the pay that they deserve; the leveling process which seems to be the aim of strikes will disappear; the poorer men will have to give way to those that are better, and, distressing as it may be to the kind-hearted philanthropist or social reformer, the great law of the survival of the fittest will be fully exemplified.

And now but one word more: This system cannot be established in a day, but time and care will furnish every requisite; and then, when once the work is done, one-half of the problems of the labor question will be solved, the work of keeping shop accounts be greatly simplified, and satisfaction take the place of discontent on every hand. But remember, when the scheme is once assumed, be honest with your men and do not try and squeeze the sponge too dry.

Mr. Daniel Ashworth.—I do not believe that the great source of failures on the part of piece work and profit-sharing emanates from the employer. We can find many cases in the history of this question where it has been directly the opposite, and the last words of that communication touched that very point. If we enter into a contract of this kind with our operatives with that sincerity and that good feeling, which has been referred to by Mr. Stirling, I believe that it will be successful. But when this is put in operation and when the result is not favorable, in nine cases out of ten there has been a disposition to rearrange the programme. It is like the story of the distribution of prize money in the Navy—it was sifted through a ladder; that which remained on the rungs was for the men; that which fell through was for the officers.

Again I have known of many establishments that have entered into this field with a view of each having a financial interest in it. That has been eminently successful in many sections of the country. In western Pennsylvania that system is flourishing; but there have been cases where it has failed almost on the same basis by the process of reconstruction. I believe that Mr. Towne will succeed in his plan. I say that profit-sharing is the quintessence of justice and is bound to succeed, depending entirely upon the spirit of those controlling it.

Mr. Chas. E. Emery.—It may be of interest to the members of the society to know the method adopted, after the strike of the New York Steam Company, in March, 1836, to make it to the interest of the men to remain faithful to their trusts and resist the blandishments of the labor organizers and walking delegates. From the language used by many here, I do not think they have been through a well-organized strike. When you find men who have not only been well treated, but are willing to acknowledge it and say that it is not a question of wages, and yet yield to the blandishments of the labor organizers and strike simply and only to force employers to become what they call "union shops," and employ only union labor, and perhaps insist that old, well-tried foremen and employees should be dismissed because they have hindered them in their plans for freely organizing the men, then, indeed, it is necessary to stand out and say to the men that you will have none of it. Such a combination must be completely beaten in order that the parties attacked may ever have immunity from similar difficulties. An attack as unreasonable and wicked as this was made on the New York Steam Company. Nine-tenths of its employees at its boiler station left their work without notice, and 8,000 horse-power of boilers in full blast, under 80 pounds steam pressure, were left to the attention of only two men who remained faithful to their trust. Fortunately there were available young educated engineers, bookkeepers, and loyal men in other departments, who hastily assembled and kept up the steam pressure until other labor was brought in to release them, and, after the usual month's siege, feeding and bunking men in the establishment, and carrying on necessary operations on the street, subject to intimidation and violence, the strikers were dispersed and not one of them has since been re-employed. In order to make it for the interest of new men to remain, each one was required to deposit $50, which was to be forfeited in case a sufficient number of men went out together to show any signs of a conspiracy, though of course, any man could leave at any time and draw out his money by mutual consent. Each man entered into a regular contract, in which it was agreed that the interest of the money should be used as a nucleus of a benevolent fund for sick and injured employees. The amount deposited acted as a check in two ways; first, a man did not wish to lose his money; and second, he could use this as an argument to bluff the labor organizers. In case of sickness in the family of one of the trusted employees, it has been the habit to permit him to withdraw his deposit, and to restore it again by instalments from his weekly wages. In fact, the fund was mostly accrued on the instalment plan in this way. The men look upon their deposit as a sort of savings bank, they are more willing to learn our ways, and try to secure economy of fuel, and the plan works in every respect satisfactorily.

My previous remarks on this subject are apparently in antagonism to the greater part of what has been said here; but as one who has been through a difficulty without a particle of reason in it, I have felt it my duty to say what I have, that all may be prepared for what, under present conditions, is sure to come to many here present. Something without foundation will be brought up, no matter how much good feeling is established, which will cause the men to injure the interests of their best friends to avoid what is called "antagonizing the interests of labor." Notwithstanding checks in various localities, the labor difficulty is like an epidemic spreading through the country. If what Mr. Towne has intimated, or any such remedy, will prevent the spread of this epidemic, we say God-speed. At the same time we say that if you are attacked without reason by misguided men, under the direction of labor organizers or walking delegates, many of them of bad character, and all of them working on bad principles, it will be necessary, for your own existence, o resist, and until the power of such men is broken your business can not go on, and there will be no rational way by which you can deal with your employees.

Mr. Allan Stirling.—Mr. Emery's last remark has stirred the blood in my veins a little, and I rise again. The remarks that I made were the result of much experience and observation on this subject. I have been through one of the worst strikes in the country. I was in charge of the engineering department of one of the large works of the country, when they started, after a six months' strike. They were started by bringing a large number of Pinkerton detectives and posting them around the works. Operatives from distant points were brought by rail directly inside the works. The machine shops were given up for bunks for the operatives, my office was used as a bedroom. If a workman strayed outside of the line of soldiers he was pretty sure to be shot. I went through all of that for a time. Finally the matter was arranged. I have had a good deal to do with companies, and with some that have treated their men fairly and generously and in a kindly spirit, so that the men knew that their employers were feeling kindly to them, and I would simply repeat what I said before, that after a great deal of observation with strikes and without strikes, I am erfectly satisfied that if employers will have this kindly feelin to their men and show it by their actions, and, as has been stated, not squeeze the sponge too dry, there will be very little trouble.

Mr. Thomas J. Borden.—The points in discussion have been applicable chiefly to cases where only single establishments were located, and the relations between employer and employees were not affected by complications with others in similar business near by them. It is a matter of common observation that all large lines of manufacturing and mechanical business tend to concentrate in certain localities.

When a leading center is established in any line of business the wage question for that particular industry for the whole country, or that particular section of the country, is settled chiefly at such center. The isolated establishments are obliged to be governed largely by the customs and rates of wages established at such leading centers.

At these centers the most liberal and humane course pursued by any one employer will not relieve him from the effects of labor controversies arising in other establishments.

The very general organization of labor in all leading industrial centers naturally results in the demands made by employees being frequently inaugurated and generally controlled by the labor organizations of that locality, and their edicts are enforced in all establishments alike.

Hence the labor question is fast becoming one of much breadth and requiring the general concurrence of considerable numbers on either side of the question in well-defined general policies, which will commend themselves to the approval of the mass of the more intelligent classes of both employers and employees.

CCLVII.

GASLIGHTING BY INCANDESCENCE.

BY JAMES DREDGE, LONDON, ENGLAND,
(Honorary Member of the Society).

THERE are many reasons why progress in the general application of the electric light—especially as regards its domestic use—has been far less rapid in England than in the United States, but only three of the various causes need be referred to here—speculative financiering, repressive legislation, and cheap gas. The chilling effect of the prolonged excitement, which commenced about 1878, when the wildest dreams of inventors were converted into golden realities by company promoters and stock operators, to the ultimate disappointment and ruin of thousands of credulous investors, will pass away; legislation, which, with the best possible intentions, was devised, as if designedly, to place impediments in the way of enterprise, and so set up a barrier to the advancement of electric lighting from central stations, is now being modified, and may be regarded as a temporary difficulty. But the low price at which gas is distributed in England, varying from 30 cents per 1,000 cubic feet to 75 cents, constitutes the most formidable antagonist to the more modern and luxurious form of illumination; the more so because, with improved and economical methods of producing currents suitable for lighting purposes, the price of gas can be still further reduced, so that, in England, it may be predicted that, so far as cost is concerned, it will always defy competition from the electric light for general house illumination.

For several years past, however, the public has become familiar with higher standards of illumination than of old, and the pure, steady brilliance of the incandescent lamp has thrown the pulsating gas flame somewhat in the shade. So, while it is recognized that the change to electric lighting is, except for the rich, for some years to come, at all events, an impossibility, there is a universal demand for a system of illumination which shall approximate in steadiness and brilliancy to the incandescent light, and which, at the same time, can be adapted to domestic use, without revolutionizing existing fittings, involving skilled supervision, or entailing heavy first cost and expensive maintenance.

The successful solution of this problem involves so many difficulties that it would, at first sight, appear to be almost hopeless. But though, in its present phase, it has only recently attracted the attention of inventors, there are already two systems before the public which, if they fall short of all requirements, at least demonstrate the practicability of the principle, and render it probable that incandescence illumination by means of gas will before long become a generally-used means of domestic and even of public lighting.

Oxyhydrogen Light.—Since 1826, when Drummond first flashed signals with the oxyhydrogen light, numerous schemes on the same lines have been proposed for obtaining high degrees of illumination, but none of them have been suitable for anything but special and intermittent purposes. Apart from other practical objections, any system which involved the use of oxygen for general lighting purposes was bound to fail, partly on account of the cost of oxygen, but chiefly because a double system of mains and service pipes was required to carry the two gases to the burners.

Since Tessié du Motay's experiments in public lighting in London, Paris, and Brussels, about 1872, when a combined supply of oxygen and common gas was used, the former objection has been recently removed to a certain extent by a cheaper process for producing oxygen, the manufacturers claiming that they can supply it commercially for about $15 per 1000 feet. Experiments are in fact still in progress on the Metropolitan District Railway, with a view to utilizing the oxyhydrogen light for train illumination. A number of small difficulties, however, appear to combine to render the idea impracticable, such as failure of the incandescent medium, irregularity in the light produced, impossibility to maintain a constantly uniform mixture of the gases, and so forth. In this connection it may be mentioned that in May, 1868, Tessié du Motay obtained a patent in England for the preparation and use of zirconia in a compressed form as an incandescing medium, but no practical results followed beyond the temporary ones above referred to.

Before any improved means of illumination adapted for general domestic purposes can be considered successful, a variety of difficult and conflicting conditions must be fulfilled, some of which are as follows:

1. Existing fittings should not be interfered with, excepting so far as burners and their adjuncts are concerned, and no separate system of service pipes must be required.

2. The system must give a better and steadier light than the ordinary gas flame; in other words, the inducement to employ it must be obvious and decided.

3. The cost of new fittings, and of altering existing ones, must be moderate.

4. The management of the system must be as simple as that required for gas, facility of breakage or derangement must be impossible, and the duration of the lighting medium must be so great that the cost of renewals (which must in any case be insignificant) should be inappreciable.

5. The consumption of gas must not be increased, but, on the contrary, a very marked saving either in consumption or its equivalent in increased light must be secured.

6. The combustion of the gas burned must be more perfect than at the ordinary gas burner, in order to reduce to a marked degree the objectionable and destructive effect of gas consumed in dwellings.

7. The light obtained must remain constant, and not deteriorate with the use of the illuminating medium.

The foregoing are all probably the important requirements attendant upon successful system of gas lighting by incandescence, and if they can be fulfilled, there is no doubt that the introduction of electric light for domestic uses would be indefinitely postponed in countries where gas is produced and sold cheaply.

Auer von Welsbach.—About eighteen months since considerable interest was excited by reports coming from Vienna that an Austrian physicist, Professor Auer von Welsbach, had, after long experiment, succeeded in producing an exceedingly thin hood or mantle of zirconium oxide, capable of receiving a high degree of incandescence, when submitted to the action of heat, without becoming disintegrated, and of sufficient strength to resist the ordinary shocks of service. Naturally enough the first stories that were set afloat as to the remarkable nature of this method of illumination, called subsequently for considerable discount, but when all allowances had been made, it became evident that Professor Auer von Welsbach had devised a beautiful means of utilizing the long-known property of zirconium, and had partially elaborated a system that promises to have before it a certain field of usefulness.

The English patents bearing on this particular invention, so far as they are made public, are dated December 12, 1885, March 13, 1886, and July 29 of the same year.

In the first specification, which is the only one that calls for consideration here, it is set forth by Professor Welsbach that his invention relates to the manufacture of an illuminant appliance in the form of a hood, rendered incandescent by gas or other burners. The proportions of the substances used for impregnating the hood, as already described, are given as follows:

60 per cent. zirconia or oxide of zirconium.
20 " oxide of lanthanum.
20 " " yttrium.

or, as an alternative:

50 per cent. zirconia.
50 " oxide of lanthanum.

The claim in this specification runs as follows: "The manufacture, substantially as herein described, of an illuminant appliance for gas or other burners, consisting of a cap or hood made of fabric impregnated with the substances mentioned and treated as set forth."

It is to be regretted that although nearly a year and a half have elapsed since Professor Welsbach's arrangement excited scientific interest, but little has been done in practice, and the only available results are those obtained from the laboratory. In Vienna—its birthplace—some public offices, shops, and private dwellings have adopted it, and in England, where the patents were acquired at the close of 1885 by a syndicate, now merged in a large company, two or three places of public resort have been lighted by it, but only so recently as to afford no means of judging whether the inventor's claims will be justified by experience. In London, indeed, the whole of last year appears to have been lost in perfecting details of what was supposed, six months before, to be an already perfect invention, and for this reason there is no public experience to draw from as to the practicability and permanence of the light. That the inventors and their friends are profoundly convinced of the immense value of the invention is shown by the fact that they have recently launched a two-million-dollar company for working the process.

The arrangement of the Welsbach lamp may be described in a few words. A hood of fine fabric, such as net, is impregnated with the zirconium solution already referred to, and when dried and shaped symmetrically, is suspended by platinum wires over a Bunsen burner, the whole being preferably inclosed within a glass chimney. When the burner is lighted the combustible portion of the hood is rapidly burned away, and the water of the solution evaporated, leaving a skeleton of zirconium oxide—a perfect replica of the original fabric. Under the action of the heat this delicate surface is brought up to a vivid incandescence, though it does not reach its maximum illuminating power for several hours. If not broken by accident, through flaws in the structure or from other causes, it maintains its maximum brilliancy of six or seven candles per foot of gas burned per hour, for a long but apparently variable period, dependent probably upon a variety of external and structural conditions; this period of maximum usefulness is stated to range from 300 to 1,000 hours, but the writer gives these figures not on his own authority; it is clear, however, that a time comes, when, supposing the delicate oxide skeleton has withstood the ills to which it is peculiarly heir, begins to fail, and through another period of gradually decreasing efficiency, becomes ultimately useless, unless; indeed, as the writer believes in the case in Vienna, the hood is reinforced by the addition of new impregnating fluid, a delicate operation hardly within the scope of the average user.

So far as can be judged from present imperfect knowledge, the Welsbach light complies fully with some, but not by any means with all, of the conditions mentioned above as being necessary for successful domestic lighting. Existing fittings are disturbed only as far as new burners are necessary, and no additional service pipes are required. The light is far better than that given by gas, and is absolutely steady; the color, too, is preferable, though it may be mentioned that, normally, the Welsbach light is intensely, almost ghastly, white; it would appear that a yellower tone is considered preferable, and it is believed that hoods giving this color will be issued to the public. A remarkable feature of the light is the relative small amount of heat given off, which is not without its advantages. Of cost the writer is not in a position to speak, except that the two-million-dollar company already mentioned propose to make a profit of $1.25 for each light sold; not an unreasonable amount, apparently, though evidently a large number must be disposed of to pay interest on so heavy a capital. The chief weakness of the new system would seem to lie in the excessively delicate character of the hood, which, according to the writer's experience (though he would carefully point out that this must not be taken as conclusive) is gradually disintegrated under small and frequent vibrations, such as those caused by passing vehicles in an adjacent street. It is, moreover, scarcely possible to believe that any zirconium hood would have a long life, under ordinary treatment in general use, and the annoyance and expense attendant upon more or less frequent renewals would militate greatly against its successful adoption, to say nothing of the difficulties attendant on the transport of these fragile films, and the delicate operation of fixing them on the burners. The remaining conditions, except that relating to duration of efficiency, are certainly established beyond dispute, but it will be readily admitted that the fragility of the vital organ will retard its general adoption until thorough experience shall have proved the danger to be imaginary, or some means have been devised of strengthening the hood.

Williams.—Attention should be drawn to the specification of a very prolific inventor (English patent, January 16, 1882), Mr. J. S. Williams, of Riverton, New Jersey, in which a "thermo-candle" is described in somewhat distracting variety. Several sentences of considerable significance, however, occur as foreshadowings of the Welsbach methods, *e.g.*, "I impregnate or coat a gauze of any suitable material as a base or form for the deposition of metal or metal alloys, and thereby obtain an extended open surface for the development of the light with a comparatively small amount of material to be heated. . . . I construct or form the light-emitting portion of the thermo-candle by the deposition of highly refractory metal or alloys upon a base or form composed of other materials. . . . I employ metallic salts or oxides, both in solution for coating, or both impregnating and coating the fibrous or other piece, which serves as the form ; for instance, I can first dip the piece of fibrous gauze, asbestos, muslin, silk, metal, or other form or piece in salt, or oxides of magnesium, or other material. I can employ for this purpose any suitable salt or oxide." And a little before he says that metals possessing the greatest refractory quality can be spread over a surface as a mere film in thickness, and can be alloyed with a cover and coat of other materials, such as oxides of magnesium, calcium, zirconium, and the like.

Clamond.—Following another line of experiment, M. Ch. Clamond, of Paris, has succeeded since 1880 in evolving a system in which a brilliantly white light is obtained by incandescence, but which, for several reasons—chief among them the fragile character of the incandescent medium, and its short duration—does not seem to recommend it for adoption except in special cases. M. Clamond's original idea is contained in an English patent granted on May 24, 1880, and is embodied in the following paragraph:

"The object of the apparatus is to produce a flame which, sustained by atmospheric air, is rendered capable of producing the luminous effects of flames sustained by oxygen, this result being obtained by a preliminary heating to a high temperature of the air that sustains the flame, so that little or none of the heat evolved at the point of combustion is absorbed in heating the inert nitrogen. For the combustion of gas in this manner, the air, on its way to the flame, is caused to pass through a tube of refractory material, which is heated to a high temperature by jets of the gas playing against its external surface, and in order that the air may be more thoroughly heated, the interior of the tube is divided by partitions having apertures through which the air has to pass in a zigzag manner, being subdivided into numerous streams directed against the heated sides of the tube. From the tube the heated air issues through small apertures and mingles with the ignited gas, producing a flame of intense heat which, directed on refractory material, such as lime, causes incandescence."

Subsequent patents were obtained by M. Clamond in 1882 and 1883 for improvements and modifications, the special feature of the system being the use of magnesia filaments as an incandescent medium.

Lights of this kind are used in Paris to a limited extent, and are now being introduced in London, with what prospect of success the writer is unable to say. The consumption of gas by this system is stated to be about 1 cubic foot per hour for 5 candles, and its principal drawback appears to be the short life of the magnesia basket, and which, according to a French writer, varies from twelve to fifteen hours.*

Fahnejelm.—As bearing directly on this point of the subject, reference should be made to a very interesting application of the Strong water gas, carried out in 1884, for illuminating purposes, at the works of Messrs. Schultz, Knaudt & Co, Essen, Germany, where the gas has, for a long time, been largely used for numerous purposes, especially for welding. The gas contains about 90 per cent.,

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* "*L'Eclairage dans la Ville et dans la Maison,*" by Ph. Delahaye, Paris.

by volume, of combustible constituents—50 volumes hydrogen, 40 volumes carbonic oxide, 5 volumes carbonic acid, and 5 volumes nitrogen. Prolonged experiments were made to convert the intense non-luminous combustion of the water gas into a luminous flame by the addition of heavy hydro-carbons, but no success attended these efforts, and the proposals of a Swedish engineer, Mr. Otto Fahnejelm, of Stockholm, were favorably entertained. The system of incandescence lighting devised by this inventor involved the use of a number of thin rods of magnesia, rather thinner than the lead in an ordinary drawing pencil. They are made by making magnesia made into paste with a glutinous medium, and forcing it through dies. These rods are then brought to a very intense heat in a crucible, when they become almost like porcelain. A double row of them is clamped in a metal holder and suspended over the flame of water gas issuing from an ordinary burner. The light emitted is very white and beautiful, and the whole of Messrs. Schultz, Knaudt & Co.'s works are lighted by it, the ordinary gas fittings having been employed. The rods of magnesia last from 80 to 100 hours each, and each comb complete costs about five cents. It is stated that the cost of the gas is less than fourpence per 1,000 feet. This method of lighting formed the subject of a patent taken out in England by Mr. Fahnejelm on the 5th of December, 1883.

Quite a number of inventors have obtained patents for the use of wire gauze made from refractory metals, and shaped into caps or cones, to which the gas and air are conducted, for the most part under pressure, involving, of course, the use of special apparatus, and consequently placing the device beyond the limits of general application.

Popp.—Among these are two patents of Victor Popp, of Paris, dated September 16, 1882, and March 1, 1884, containing descriptions of pressure regulators, fittings, and gauze incandescence cones, but which call for no special remark. Other inventors whose schemes are placed on record in the English Patent Office are as follows:

Palmer.—J. D. Palmer, of London, filed a specification on January 22, 1876, for the combination of a finely-woven wire gauze cap of platinum, iridium, or other refractory metal, with a base through which atmospheric air and gas at ordinary pressure were admitted; these, mixing within the cap, produced, on ignition, sufficient heat to incandesce the gauze, the effect being increased by the addition of a central metal rod that was brought to, and maintained at, a very high temperature by the burning gases.

Reckenzaun and Redfield.—Messrs. A. Reckenzaun and J. H. Redfield have a patent dated March 14, 1882, for producing light and heat. This patent refers mainly to apparatus for producing gas from a mixture of air and hydro-carbon ; reference, however, is made to a burner formed of a coil or other form of platinum or iridium gauze to which the gas is led, and when it is ignited, it is stated that this arrangement is suitable for heating purposes.

Cooper.—Thomas Cooper, of Norfolk, filed in January, 1883, a provisional protection for a form of lamp in which air, impregnated with hydro-carbon, was to be led to a cap of platinum or iridium gauze and ignited.

Wicken.—W. B. Wicken, of London, filed a provisional specification March 19, 1883, for a regenerating gas lamp, in which the source of light was to be a spherical mass of loosely compacted platinum wire suspended over the heat of a Bunsen burner.

Lewis.—We now come to the last group of published patents having reference to that system of illumination by incandescence. These were taken by Mr. James Lewis, of London. There are five of these patents in all, taken out between April 14, 1881, and October 21, 1884, a period which, it will be observed, includes the dates of the Clamond, Popp, and other patents already referred to, although, of course, the use of platinum or other refractory metal gauze had been described long before.

The leading idea in Lewis's first patent, developed and carried into practical form in the subsequent ones, is for the combination of certain parts forming burners supplied with gas and air, the latter under pressure, in such a way as to secure an intense heat on combustion, used to incandesce a platinum gauze cap.

Although none of the Lewis methods seem as yet to have proved adapted for ordinary use, yet it is difficult to conceive a more brilliant, steady, or beautiful light than he obtains from the incandescence of the platinum cone, when the combustion of the gas is supported by air under pressure. A small cap of platinum gauze yields a light of at least 200 candles with a consumption of 30 to 35 feet of gas per hour. A curious feature of this system is that the temperature to which the platinum is raised is so great that it may be exposed to a heavy rain, or water may be thrown over it, without any apparent effect, the light continuing as steady as when being under shelter. On the other hand, not only is the system inapplicable for general use on account of the double mains being a necessity, but the noise produced at the burner render it unsuitable except for lighting very large covered spaces, or for out-of-door work. Many of these lights have been worked in and near London, where they have done good service under special conditions.

Sellon.—Attention will now be called to some improvements upon the Lewis system of gas lighting by incandescence, which appears, so far as experience and experiment allow a conclusion to be drawn, to fulfill, more nearly than any other plan yet devised, the varied and difficult conditions required for domestic and general use. Nothing has yet been made public upon the details, which have, however, been under careful and continuous test for the past four months, nor are the patent specifications relating to it published, so that it is only possible to describe the system generally, to record what results have so far been obtained, and to exhibit a specimen of two types of lamp that you may judge how nearly practical success has been obtained. The patentee of these improvements is Mr. J. S. Sellon, whose name is familiar throughout the world in connection with electric lighting. Mr. Sellon is, moreover, one of the partners of Messrs. Johnson, Matthey & Co., who, in Europe, at all events, take the lead as workers of platinum and other rare metals, so that in this respect Mr. Sellon has enjoyed especial facilities in experimenting with alloys of refractory materials such as are requisite to withstand usage and give a high incandescent duty. Like Mr. Lewis, Popp, and many others, Mr. Sellon makes use in one of his types of a cone of metal gauze, the exact nautre of which is not yet made public, but which has proved during the last four months its light-giving and resistant properties. The cone is formed with three projections or ribs running symmetrically from its apex to its base. While these ribs assume the useful purpose of strengthening the cone and making it practically rigid, their presence is explained by the ingenious and simple mode of manufacture.

Strips of metallic gauze of a width equal to the depth of the cone are cut into triangular pieces of such a form that when bent and the edges turned, three of them put together make up the hood. In this way there is no waste of material, and the process of manufacture is made absolutely simple. The pieces are then fused together at the edge by the oxyhydrogen blowpipe, and a very rigid cone is obtained. The cost of such hoods is about half a dollar, and when worn out they can be renewed for a few cents. The Bunsen burner is of a form perfected by Mr. Lewis; it will be noticed, Fig. 170, that an ample length of tube is allowed between the gas and air inlets and the point of combustion, the result being a quiet and intense flame, not materially affected by the ordinary differences in gas pressures, and presenting no liability to light back. As will be seen, the arrangement lends itself admirably to table lamps, and is equally well adapted for chandeliers and other fittings. Sufficient time has not yet elapsed to decide the ultimate life of the cones, but four months' severe test has failed to develop any material change either in the structure of the alloy or in its incandescent properties, and, to all appearances, the useful limits of the cone are sufficient to insure their practical utility. The light is of a very agreeable color, and the steadiness absolute; it will, moreover, be at once appreciated that the stoutness and the rigidity of the cones adapts them to resist the shocks of any ordinary wear or accident; if necessary, they can be taken off, cleaned, and replaced by any unskilled person, and renewals can be made with equal facility. As regards consumption of gas and illuminating efficiency, the tests carried out over a comparatively long period show with a burner consuming 6 feet per hour the efficiency of 5 candles per cubic foot of gas burned, being an economy of 45 per cent. as compared with the Board of Trade standard Argand.

Another form given to this system is the so-called "star lamp," an arrangement combining a large amount of efficiency with a pleasing form. In this lamp, the mixed gas and air flow from the Bunsen burner into the lower half of the glass globe, which is completed by another half globe above, finishing with a chimney; these half globes are coupled by any convenient attachment, but are separated by an asbestos diaphragm, in the center of which is cut a star-shaped or circular opening, in which is placed a flat piece of gauze woven from the refractory alloy. Combustion takes place on the upper surface of the gauze, which is almost immediately brought to a vivid state of incandescence, giving an illuminating efficiency with a burner consuming 7 feet per hour of 6 candles per foot of gas. There is no visible flame upon the surface of the gauze, and the brilliancy of the light attests to the perfection of combustion. A shade surmounting the globe renders this form of lamp specially suitable for domestic use. In a modification of this star lamp, the heated air rises through a metal chimney surrounded by larger tubes that serve to carry downward the gas and air supply into the glass globe beneath, where they mix, and, ascending through the gauze, become ignited. This form is intended particularly for more powerful illumination than is generally required for house lighting, except for entrance halls or similar situations, where the Wenham lamp now finds a large application. Two allegations against this system naturally suggest themselves—danger from explosion of the mixed air and gas in the lower part of the globe, and the obscuring of the surface of the latter by deposition from the gas, which would thus gradually cut off a portion of the light. With regard to the first point, the writer is assured that exhaustive experiments have been made to test the security of the lamp, and that, under the most trying conditions, he has failed to bring about ignition except on the upper surface of the gauze. As for the latter point, the globes appear to want cleaning no oftener than, under ordinary conditions, they require to be attended to on account of the accumulation of dust. This system of lighting does not show the character of the Welsbach light in being comparatively cool, as its radiant heat is about the same as that of gas. This, however, does not seem to be wholly a disadvantage, as, probably, in the majority of households, the heat given off by gas is not unwelcome in the winter months. Upon the whole this latest development in the science of gas lighting by incandescence seems to be by far the most perfect that has yet been proposed, as it fulfills, practically, all the conditions required for domestic lighting. Existing fittings are scarcely interfered with, except as regards the burners; the light is absolutely steady, and the economy over standard gas burners is about 45 per cent.; the first cost is insignificant, the management is simple, and the duration of the incandescing medium has been proved by eight months of service; renewals are inexpensive and easily made by unskilled labor, while the permanence of the light given is fully maintained. It would, however, of course, be premature to form anything like a definite opinion upon the ultimate merits of this ingenious and promising system; its future can only be decided on by public approval after considerable time. But it seems to the writer that the subject forms sufficient interest and importance to be placed before this meeting, especially as this is the first occasion in which the Sellon light has been exhibited in the United States.

(Added by the Secretary.)

The star lamp referred to in the latter paragraphs of the paper and sent on for exhibition at the meeting is illustrated in Fig. 170. A is the upper glass chimney, and D is the lower one, similar but shorter. B is a brass ring secured to the lower globe or chimney, D, by four brass set screws and bearing four others, *c*, by which A may be secured to it as the ordinary globe is fastened on a gas bracket or chandelier. The lower screws secure an asbestos or talc diaphragm at the joint of A and D. This asbestos diaphragm, *h*, has a star-shaped opening cut in its center, which is covered by the special gauze, *m*. At the bottom of D is the "gallery," E, as the author calls it, which is fitted with spring clips in the usual way to hold a cylindrical chimney. For the star lamp this gallery has a flat top with side openings. For the smaller and simpler lamp, using a straight cylindrical chimney, the cone of gauze fits right over the central turret, within the chimney. In either case, the gallery screws upon the top of an improved Bunsen burner tube, F, the latter having an enlargement at G, where the air inlets are. Below that the stand is like any table standard base. The tube screws off at G from the gas inlet, and various nipples can be substituted to suit various pressures in the street mains to secure best efficiency. For the star lamp an ordinary shade holder screws on the top of F just below the gallery, so that a porcelain shade, fitting just over the convexity at the bottom of A, may throw the light downward. The gas supply is through the usual ⅛-inch tube used for table standards.

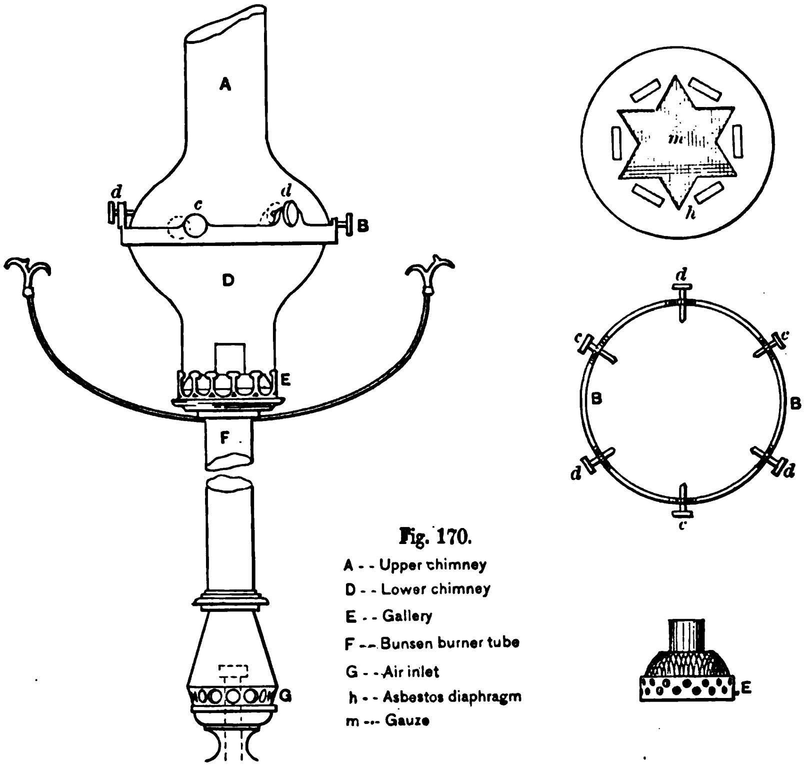

Fig. 170.

A - - Upper chimney
D - - Lower chimney
E - - Gallery
F - - Bunsen burner tube
G - - Air inlet
h - - Asbestos diaphragm
m - - Gauze

CCLVIII.

PRESIDENT'S ADDRESS, 1886. *

BY COLEMAN SELLERS, PHILADELPHIA, PA.
(Member of the Society, and President, 1885-86.)

A LONG and tedious illness, covering the whole of my term of office as President of the American Society of Mechanical Engineers has prevented me from performing duties which would have been a pleasure to me. When the time came for me to speak to you, I was still too ill to prepare an address, and I cannot review the work of the year, for much of the time has been a blank to me. Nor do I feel inclined to confine myself to any one theme, as I desire to invite your consideration of a variety of topics which appear to me germane to our organization.

The clouds of financial depression which have so long hung over all countries seem now to be lifting, and we have reason to hope for a revival of trade and better opportunities to apply our talents to the good of the mechanical industries of the United States. Last summer the distinguished President of the Institution of Mechanical Engineers of Great Britain, took the depression of trade as the text for his admirable annual address, pointing out causes or supposed causes and making a fair comparison to illustrate his argument between the conditions that held in this country and England. He pointed out the advantages which accrued to America from well-considered and well-organized mechanical enterprises, referring particularly to the American system of manufacturing

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* [NOTE BY THE SECRETARY.—Mr. Sellers was prevented by illness from presiding at any of the sessions of the Society during his term of office, and from preparing and presenting his retiring address at the usual opening session of the Annual Meeting in New York, November 29th, 1886. It was arranged, however, that its preparation might be deferred until the author's recovery should be far enough advanced to admit of its completion, which has fortunately occurred before the volume was entirely ready, so that the Address appears in this place instead of in its usual place at the close of the proceedings of the Annual Meeting at page 45 of this volume.—F. R. H.]

bridges and watches. In regard to the latter industry he pointed to the decline of the watch-making trade of Liverpool, where the work, employing formerly a large number of hands, was carried on in private workshops, with few tools, or at the homes of the workmen, without improved machinery to help them, and without any systematic division of labor. In contrast to this, the great watch factories in the United States have aimed to select a good model, to perfect that model, and then to cheapen the output by systematic organization, with the result so well known now, of gradually destroying the trade in England and rapidly introducing American watches into all countries. The selection of examples cited was good, but these examples were but a small part of the many similar cases which illustrate the value of the methods adopted in our country, and which work to its advantage.

Those American industries which are having so marked an effect on trade are the work of specialists. The tendency of modern times is to encourage specialties. In Medicine and in Law the great men are specialists, and so it is in mechanics. An old time engineer, the head of one of the largest machine shops in Philadelphia, forty years ago pointed out the need of specialists in mechanics, and the great advantage of manufacturing establishments over jobbing machine shops. He used to say that the jobber, selling his thoughts to one purchaser at a time, wastes his brain capital. He spends his energies on each emergency with little or no prospect of repeating the construction upon which he has expended much thought or of applying again ingenious devices worked out for unique cases. The manufacturer on the other hand, invests his mental capital, in what, if salable, yields him a constant income. He can improve his product and in most cases cheapen his output by improved methods of manufacture, or by the introduction of special tools and appliances prepared for him by other specialists in that line. It is here that the true value of the unique patent laws of this country is felt. The trials and tribulations of passing the patent office save subsequent litigation, and the manufacturer, protected, can perfect and install a plant adapted to his purposes, while the unprotected maker cannot venture on such a risk. The product of the manufacturer is cheapened, too, through the perfection of the *personnel* of his establishment. Besides what he may do, his subordinates in their several capacities, with the hope of large earnings, improve their work in quantity and quality. They become more skillful and they take a greater interest in their work if enabled to share the profits by any well devised system of piece work. They suggest improvements or what may lead to improvements.

Forty years ago when this far-seeing man urged the advantage of manufacturing machine shops, there was not much competition in trade as compared to what there is now. Even then separation in competition came with distance apart of the shops in miles, while now distance does not separate. Workshops are not now secure in the trade of their own immediate neighborhood. Steam and electricity have eliminated distance, and the market is for the one who can do the best work for the least money. This old-time engineer was a man almost without education—I mean book-learning. He was a learned man so far as long practice and a good memory had made him so. He was a walking encyclopedia of his own and others' experience. He could remember all sorts of devices contrived by this or that man, and was full of anecdote to illustrate his points. His drawing board was any plank at his command and his instruments a bit of chalk. He knew where every pattern was placed in the loft and could combine them to meet his emergencies. He was very quick in detecting talent in those under him, and employed more good draughtsmen than any other similar establishment of his time in his city. My old friend used to point to certain lathes which he had built from patterns loaned to him by a lathe builder, and upon which he had thought he would save money by making them himself. Those, however, after some years of use, cost more in repairs and proved to be more costly than the finished lathes which he had purchased from the same manufacturer. This was one of his examples of the great advantage arising from specialties in machine making. When in England, in 1884, I was much struck with the mercantile ability displayed in the conduct of one large establishment. This was essentially a manufacturing shop, and the heads of the concern were merchants or had been trained as merchants. Gifted with marked administrative ability, they had secured the co-operation of good mechanical ability, and knew just what to make, how to make it, and how to sell it. The engineer who counts cost as nothing, as compared to the result, who holds himself above the consideration of dollars and cents, has missed his vocation. The time has come when we must bring into the conduct of our shops the best clerical ability for collecting and tabulating the statistics of business. This, however, can only be carried out to advantage when the engineering head of the establishment is able to do the clerical work himself, so as to direct and to teach those who are employed to do this work. Presently I shall come to speak of the education of engineers, and I will at the same time take exception to the system of teaching in vogue in the public schools, where the mercantile education is carried farther than need be; and I feel satisfied that the mercantile part of our profession should be thoroughly taught in the higher technical schools, just in fact where it is not taught. I am safe in saying that no profession requires a broader education than that of the mechanical engineer. He must be a physicist, a merchant, a lawyer, a chemist, and he should know how to express himself in his mother tongue and be master of the modern languages far enough to have access to the scientific publications of other countries.

My own experience from the time when as a boy I worked during holidays in my father's pattern shop, covers more than half a century, and it has been my good fortune to have had to do with many mechanical trades, from the farm to the rolling mill, in the lowest grade of workman, before I was fortunate enough to connect myself with a manufacturing establishment, where I could learn the necessity of counting cost in everything and where I fully learned the value of the mercantile part of the profession.

An engineer must learn early to view all things equally, to be patient in his scheming, never to be satisfied with good enough. Nothing is good enough if there is a chance to make it better. It is the least part of an engineer's work to accomplish a given result; it is the greatest part to accomplish the result in such a manner as almost to defy improvement. The world is full of examples of engineering enterprises which are crude and ill considered. I remember standing in a forge yard in England where there was a vast collection of broken engine shafts and other pieces of machinery. I was there to look at the crank shaft of one of the largest of the ocean steamers. Standing on that broken crank shaft I could see around me hundreds of mistakes—good workmanship cast aside on account of errors in design. I thought what a fearful record it would make if we could see the aggregate cost of all these mistakes. In former times such mistakes as still occur were more excusable than they are now. Through these many mistakes, however, we have come to be possessed of a vast amount of useful knowledge. The steam engine has grown by the survival of the fittest, and from the perfected steam engine has been worked out the theory of thermodynamics. The steam engine in its highest form preceded the theoretical knowledge which has been written up under the name of thermodynamics, just as the music of the great masters was written and played long before the science called acoustics had been worked out: long, too, before the laws which govern harmony had been formulated and placed in the student's hands. These laws are now the common property of all students. In view of what we know in regard to steam and its conditions, we are not guiltless if we repeat the errors of the past for the want of seeking after truth which is accessible to us. Even forty years ago we were more excusable for our want of knowledge, but now we have schools to teach mechanical engineering, schools to teach us how to observe facts. Books are multiplying. Our own Society is doing good work in the direction of original research through the well considered experiments of its members, and we widen our conception of the laws which govern matter and of the material with which we have to work. The day for empiricism in mechanics has gone by. As I have already pointed out in lectures before the Franklin Institute of Philadelphia, the wonderful progress in modern times is due to our method of observing facts and of grouping them in proper order; working out the laws which govern matter and proving that these laws are correct by our inability to find any exceptions to them. Some of these laws are so well established that it is needless for us to experiment farther, and we can follow them with confidence. As the knowledge of the world becomes formulated through individual experiments or through the work of this and kindred societies, we will have before us the accumulated experience and knowledge of those who have gone before us. It is true that we have much to learn, but with the light already at our command we have no excuse to repeat the errors of others or to err grossly on our own account.

The engineer must of necessity be a hard student; his school-days never end. I hope the engineer of the future will begin his studies earlier in life than he now does. At the present time that period of a boy's life called his school-days may end, and, assuming he is to be an engineer, he may enter the shop ignorant of all the objects about him, with hands untrained to work, the butt of those who have come before him. I hold that some portion of his engineering training might well begin with his spelling-book. A child who can hold a pen to write is old enough to learn to draw. Even before that time he might begin to train his hands in useful manual labor. It is admitted, that man having hands is by reason of those hands better than the beast of the fields, and because he has hands he should be early trained to use them. What will fit him to enter the workshop in better condition than now, will fit him better also for any other walk in life.

Must a mechanical engineer of necessity be a master workman? I know of some very worthy engineers who are making their mark in the world, who are themselves very clumsy in the use of their hands, and I doubt much if they have ever worked at the bench. I know they would have been the better for more hand training. Personally, I do not know how to separate the engineer from the skillful workman. In my own case I had a wise mother, who placed the tool-box ahead of the grammar, and though bed-ridden herself, she sent out and bought tools for me when I was a very small boy, and taught me how to use them with the skill which she had learned from her talented father. She was a striking example of what a woman can do to educate her boys in a direction not commonly followed by the modern mother. The man who is ready in the use of tools, can not only direct workmen more intelligently and systematically, he can also appreciate the value of the work done for him and be nearer to the workmen and be better fitted to deal justly with them. We may say that a knowledge of thermodynamics will be of no use to the man who is, day in and day out, engaged in the repetition of a single operation, such as boring cylinders or turning axles. Indeed, I am quite sure that some of the very best workmen in these specialties are men with no education at all, many not being able to read or to write. But from the lowest workmen to the highest there must be a gradually increasing amount of knowledge. Let me illustrate this with an example. It is known the world over that we can utilize labor-saving machines to better advantage in this country and with less trouble from the uneducated workmen than is possible in any other country—that is in the other old countries. American labor-saving machines are noted the world over, and very many of them are the outcome of the brains of workingmen, who have devised them and made them. Not many years ago I had a call from an English engineer who desired to visit some of our large establishments. Having helped him to see what he wanted, he afterwards called on me before going home, and then explained what had brought him to this country. It seems that he was at home engaged in making large cutlery and had sent out to America for certain labor-saving machines said to be used in this country to advantage. The machines in the hands of his workmen in England had been of no use, and in fact had been discarded, while his partners had taken objection to his having urged the purchase from America. His visit was to see these machines in use and note their efficiency here. He said he was astonished to find that they were doing better work and very much more expeditiously than anything he had been able to get out of them. He told me he had satisfied himself that the cause of their failure to use them to advantage came from the workmen employed on the machines. These men had not objected to working the machines, but the best men they could employ were inefficient as compared to the American workmen. He then continued: "I am satisfied that your workmen are at least two generations ahead of ours, and that only from the fact that they are better educated and more self-respecting." The education which makes a man more of a man and less of an animal, will tell in the work he will do, even if he cannot apply high mathematics to turning axles or running a lathe or planer. Walk as I have through the streets of Glasgow, for instance, and note the workmen going home from the shops in the evening. The men are grimed with dirt and look as if they had not washed for a week. A friend said to me as he drove through the streets of Glasgow, one afternoon at six o'clock, as the men were quitting work, "We cannot get our men to wash up as they do with you; they seem to care nothing about their looks, and we cannot make them change their habits. They must quit work for breakfast and have their nooning, too, when we, in our dark winter days, are losing by these customs the best part of the short daylight." He told me that very many of them could neither read nor write. To me it was a sad sight. I thought of the marked difference between these men and the same class of workmen in America. I overheard a horse dealer on an ocean steamer talking about American workmen. He said he was brought up in England on a farm, and he was familiar with the English workingmen, but that any one of the western farm hands who had been to school could out-work any two of the same class in England. They were not only better workmen and did more in the same time, but they were more ready in resources, more "handy" with their hands.

To those who say that education is of no use to the laboring man, who can make no application of his knowledge in his particular trade, I say that he is the better for his schooling if he is made a cleaner man, a more self-respecting man. Education which spoils a man for his work by placing him above manual labor through false pride will continue to do harm. Although a few are injured in this way, far more are made better.

To return to the advantages to come from manufacturing establishments confining their enterprise to any one or more lines of trade. The successful manufacturing establishment, in any particular direction in mechanical engineering, must, in the long run, represent a vast amount of practical experience, and its latest output is likely to be the highest type of its kind. It represents the accumulated thought of many years, unfettered by the whims of would-be scientific engineers who have not yet learned their trade. Some one has said that this is the age of blue prints. Copies of drawings are easily made, and the young engineer, say of a rolling mill, thinking he knows more of rolling-mill practice than the manufacturer, who has had the building of hundreds of mills, schemes out a new plant, based on what he has seen, but differing in minor points, differing, however, in what will make the erection more costly and which will necessitate the making of new tools to build it. He sends these blue prints over the whole country, to engineering establishments, good, bad, and indifferent. Some, who know what they are about, bid for the work on the basis of the increased cost of construction, and do not get the job. One knowing less, thinks the changes in design of less importance, ignores them in his estimate of cost, gets the job and loses money on it. This goes on to the detriment of trade, and will continue to go on, until people are fully alive to the value of special manufacturers' ability. In times of depression machinists are more apt to bid on these crude designs than they are in times of prosperity when work is plenty for all.

American bridge-building is attracting the attention of the world, and bids are asked from us for even the British colonies. The President of the British Association, in the address to which I alluded, took bridges as one of his examples of American systematic manufacturing. I will do so also, but perhaps to use the example in a different manner. Every well organized machine shop has a system of sizes, has methods of work. In the designing rooms of these establishments drawings are made to conform to the means of execution in the shop. The more perfect the system of shop sizes in an establishment, fewer minor tools will suffice to carry on the work, and these tools may be more expensive and more perfect. The most trifling deviation from the system involves cost and detracts from perfection—the perfection of long practice with certain methods and sizes, with the minor tools and the gauges which aid in the inspection of the work. When a type of bridge has been selected by a maunfacturer and he has perfected his plant for its production, his output is cheapened. His bridges will cost less than those requiring new tools, new gauges, and new modes of construction. They will be better, by reason of their being the aggregate of long practice, and the survival of the fittest in the art of construction. It is next to impossible for any bridge engineer, separated from the shops, to acquaint himself with the methods of all the bridge makers. His drawings to be worked to either differ from the practice of all, or, if conformable to one shop, place other builders at a disadvantage. As the cheapness and efficiency of the bridges built by any one concern comes to be appreciated, bridge buyers will learn to instruct their engineers to confine their specification to the requirement of each case, without cramping the manufacturer by special features. They should make their specification cover the strength which is needful, and throw on the manufacturer responsibility for the perfection of the detail of the plan.

I have by me, as I write, the specification for bridges and other matters pertaining to a projected railroad enterprise, sent to me for examination, not for my use as a bidder. My comment on this specification was that it was too full in a direction which would involve the engineer who wrote it in the hazard of experiment, and would exclude the use of good models of things which were to be had in the open market. I see an inclination to include in specifications chemical qualities in the materials to be furnished, while the user of the material is only interested in its physical perfections. This is, I think, a very grave mistake. We are entering on the age of steel. We are going out of the iron age. The progress of the art of steel making is wonderful. We should do nothing to check that progress. I say most emphatically that we, as engineers, have to do only with the physical quality of the steel used, not with the chemical qualities. We want material which will stand certain strains, and our specifications should clearly set forth what we do require. After that we should devise physical tests which will enable us to assure ourselves that the material furnished conforms to our specification as to strength, durability, etc. The Pennsylvania Railroad will not accept steel for boiler plate, which has a greater tensile strength than 65,000 pounds to the square inch coupled with 25 per cent. elongation. This is because they have found that all steel of higher tensile strength, even if coupled with great ductility, is capable of hardening if heated and plunged in water. I think, in this case, I would be inclined to insist that, if the steel did "proof" higher and had proper ductility, it should then be made to stand the test of hardening, and be liable to rejection on that account. I would do this in hopes of obtaining a better low steel. We do not know what can be done in the way of good steel, and we should put no blocks in the way of progress.

I am writing after the death of a great mechanical engineer, Sir Joseph Whitworth, of Manchester, England, who has been taken away at a ripe old age, full of honors. He was the great champion of shop perfection. He earnestly urged the simplification of grades of sizes, both in machines and in the tools of construction. When all England was in a snarl about screw threads, and each machinist had his own collection of taps and dies, each differing from that of the other, he introduced his system, which tried to come the nearest to the general average of the threads in use, and represented a graduated scale, far better than the confusion of the times, lacking mainly in that he did try to accord with practice, and so involved the need of special tools for the production of the threads. He might have cut loose from all precedent and given the world a system which would have enabled ordinary workmen, without special tools, to produce standard taps to a given formula. The introduction of such a system in this country was, perhaps, rendered more easy by what Mr. Whitworth had done. His production of surface plates, by scraping one to the other in sets of three, was an example of how he thought out an idea and then made that idea an accomplished fact by his faculty for infinite *taking of pains.* When he advocated the introduction of what we call "trade sizes" in all things manufactured, and instanced the gain to be had from the adoption by architects of certain graded sizes of windows and doors which would permit of such things being made in quantities at the factories, he said, speaking of our country, "They manage such things better in America than we do." He had visited this country at the time of the first great International Exhibition in New York and had been impressed with the work already done here in the very direction which he was advocating, and more so in the fact that the introduction of labor-saving machines met with no opposition from the workmen, who seemed to feel an interest in what would lessen labor and perfect the output.

Sir Joseph Whitworth's system of screw threads is in common use in all Germany and in other metric-using countries. It is not very long since the German engineers, in formal conference on the subject, adopted the Whitworth system, then in common use, as the system advocated by the Society. This they did, in spite of its necessitating the adoption of the English inch in the case of all screw threads, though they call the sizes by metrical names, saying 25 millimeters equal one inch, and so on, although they know that the size of a standard inch tap of the Whitworth system, which they have adopted as their own, is more than 25 millimeters in diameter. The German or French maker of taps and dies, though compelled by law to use the French meter as his standard of measurement, is obliged to make use of the English standard plugs and rings for his sizes, and must use English standard hobs to cut the chasers which are to shape the threads of the taps, to give the right form to the thread and to insure the angle of the sides being 55 degrees, and to have their bolts and nuts interchangeable with those made in England.

I had the pleasure of meeting Sir Joseph Whitworth in 1884, after I had visited his very wonderful works in Manchester. He had much to talk about in the direction I have indicated, and he dwelt for some time on the existing confusion of sizes. He said the best plan to make the metric system of measurements conform to the English and American would be for the French (since it is now known that the basis of their system bears no exact relation to the circumference of the earth) to increase their metre a very little in length. "Make," said he, "the French meter forty inches long, say, roughly, five-eighths of an inch longer than it now is, and then the two will be commensurate." He urged me to work in this direction, as I was younger than he. He said feelingly, "I am too old to make the fight." He was right; he was then standing with one foot in his grave, so feeble did he seem to me to be.

He was making an effort to construct, at his house in England, an apartment to be a winter garden, in which he could simulate the climate of the south of France and in which he hoped to pass his winters in comfort. I fear his effort was of little use, for he seems later to have sought the genial sunshine of Monte Carlo; there the thread of his long and useful life was broken. A great master mechanic was lost to the world, but his work will live after him. Most earnestly do I hope, yet fear I am hoping against hope, that his ardent wish, expressed to me in Manchester, may be carried out.

Germany and France will not give up their so-called scientific metric system; America and England cannot afford to give up their more useful and practical system of weights and measures, for the reason that it is secured and made permanent by the perfection of exactness of implements in common use, in immensity of cost and material, in tools identified with the English inch, and in libraries of books which express our knowledge, formulated in the system we use. If France and Germany will but add to their meter the small fraction asked for by Sir Joseph Whitworth, then the great nations of the earth will harmonize and make commensurate their metrical systems, and 25 millimeters will equal one inch, and forty inches will be one meter.

This being done, if in time it comes to be proved that the French system has any advantages in practice over the system of measurement we have in use over the English-speaking part of the globe and over the great area of Russia, the transition from one to the other will be easy. In this view of the case I cannot but think, that those who are now trying to induce the Government of the United States to force the use of the French system by law would do well to aid in making the two systems commensurate by urging Sir Joseph Whitworth's plan. It might in the end help them in their work. Let me also express the hope that the members of this Society will take in consideration the proposition of Sir Joseph Whitworth and exert themselves to bring about this change. Messrs. Pratt & Whitney have shown with their admirable comparator that the instruments of measurement in common use in France and Germany are far from being accurate—a new set of scales put into the German market and made 25 millimeters to the English inch would most likely agree with many scales now in use in that country. In fact it might be well for our Society to memorialize the German Societies of Engineers on this subject.

The American Society of Mechanical Engineers has already given expression to the views of some of its members on the subject of the education of engineers. As your President I have been favored by the Institution of Engineers and Ship Builders of Scotland (Incorporated), with a paper by M. Henry Deyer, C.E., M.A., on the same subject, and I have been asked by the Council of that Society to join in the discussion of the paper more particularly as to my opinion on the economical questions involved in such education. We may well ask what is the money value of proper education. Illness prevented my answering this request in time, but I cannot refrain from introducing the subject in this address. At the annual dinner of the Institution of Civil Engineers in London, in 1884, I had the pleasure of listening to the address of Sir Lyon Playfair in his reply to one of the toasts. In this address he commented on the want of technical schools in England, saying that it would be to the advantage of the people of England to consider well what was being done in America, in regard to the founding of technical schools rather than model on the schools of Germany or France. I think we may well be pleased with what has been already done in this country in the high schools and colleges, but I am very far from being satisfied with what is being done in the primary schools of the land.

Our attention should be turned to the primary schools, so that those who enter the advanced schools may do so with a ground better fitted to receive what is to be planted and cultivated in the direction of the natural sciences.

I think the engineers to whom I am addressing myself will bear me out in the assertion that few boys who enter the shops from the public schools, or from private schools either, and who have not been through the technical schools, seem to have any notion of even the names of the tools and appliances which are to be used by them in their daily work. They cannot read the simplest mechanical drawing, nor does their knowledge of arithmetic seem to be in the direction of its application to questions of relative speed or questions of calculation of strength and so forth.

In one of the weekly mechanical journals the other day I noted among the queries a very well worded request for information as to the size of a pulley required on the spindle of a dynamo to insure a speed of 1,500 revolutions per minute which was to be driven from an existing countershaft having a speed of 250 revolutions per minute, upon which was a driving pulley which the writer desired to use of 24 inches in diameter. Here is a very simple example in the rule of three; I feel almost sure that if the writer was asked a question as to relative cost of articles involving the same application of the use of the rule of three, he would answer it without hesitation. Thus if he had bought 250 water-melons at 24 cents each and he proposed to trade them off even for 1,500 cantelopes, how many cents would each cantelope be worth? The latter question would recall to his mind the familiar examples of his school days, all in the direction of barter and trade. This, however, is not the worst feature in this matter. His instruction has been through the attempted memorizing of many rules. He has not been taught by object lessons and practical examples the way to think out these questions on a basis of good, sound common sense. In looking back at my own school days I can remember my slowness in learning the rules of arithmetic and the quickness with which I could master the solution of all problems in geometry. The fixed rules gave me no practical information ; the questions involved in geometry were things which I could see and appreciate; just as a picture will tell with a few well placed strokes what pages of writing would not make clear, without the reader making great mental effort to master the subject. When I go into a school room and see the walls covered with maps and diagrams illustrating geography, and listen to the recitation, I find it just as it used to be in my own childhood ; there is the endless memorizing of names of rivers and the boundaries of countries, all of which will soon be forgotten, and I cannot help thinking how much could be given and retained, if the teacher was to carry the child in imagination over the great railroads and through the water channels by sea and by land and the young student be directed to work out the route by means of the railroad guide books in every day use. How much quicker would he learn what would be of use and how much more thorough would be the instruction. Then let some of the examples in arithmetic be in the same direction. The railroad and the steamboat route will suggest endless examples : how far apart are telegraph poles placed? If in five or ten seconds we pass a certain number of telegraph poles, how fast are we traveling? The bicycle or the tricycle might be used to show the relation of the diameter of the wheel to its circumference. A child will take with avidity to questions as to the relative advantage of gearing up or down from the treadle to the driving wheel of the tricycle, and can readily master the advantage or the disadvantage of one or another size of wheel on his bicycle. Questions of mechanics, if presented in connection with objects of every-day use and of personal interest, will be easily mastered and held fast in memory. Conservation of energy sounds meaningless to many, but the child who is old enough to learn to read, is ready to be taught that power cannot be created, but that what force there is lying dormant in the world can be used to more or less advantage. He can be taught the correlation of all forces and the advantage to be derived from economy in the expenditure of his own and of all other forces, as from economy in matters of dollars and cents. A wise and practical education which will teach how to observe and think, will guard the mind of the student later in life from believing in impossible motors which are to consume nothing but which when perfected the week after next, are to supersede steam and utilize the " vibratory forces " of nature. This improved system of education is, however, only to be obtained by the radical change involved in the substitution of the education of the hands and the eye for some of the useless cramming from books.

We are almost daily astonished by some great discovery in the arts and sciences. These discoveries render the work of the tricksters who are exploiting their alleged still more wonderful discoveries easier. People well educated, so far as books can teach them, but who are ignorant of the first principles of mechanics, fall an easy prey to the adventurers. They fail to see that all the startling discoveries which really are made in each and every case fall under some well established law of nature, and are wonderful in their very simplicity. No one of all the discoveries which have been made or that ever will be made points to the possibility of our obtaining a result from nothing.

In our schools we are cramming brains with what taxes the memory to the utmost, but which sends into our workshops boys who are themselves startled to find how little they know as compared to those who, almost ignorant of book learning, are wise in the knowledge of the things about them and skillful in the use of their hands.

Education of a high order seems in such cases to fail in its result, and so it comes that there are many thinking men who are inclined to consider a college education as a detriment rather than an advantage in the case of a young man wanting to be a mechanic. A very noted mechanical engineer, himself a millionaire from the gains in his profession, in a speech at a University celebration, he himself being a Trustee of that University, made in substance the following assertion : "The University may give to the student who is to follow the learned professions all that he requires, but the workman who is to earn his bread by the skill of his hands is not bettered by what the University and its school of science can give him. For our skilled workmen we must depend wholly on the teaching in he workshops, not in the schools, and that a very little schooling would suffice for them." I have noted many examples of young men injured by long schooling; far better for them had they been driven to hard work with their hands, rather than to have been forced in the Blimber hot-houses of learning. Strong minds in weak bodies are injured when the mental food they long for is given to them without any training of the hand or other parts of the body. Teach the head less and give at the same time more for the hands to do, and a system will be found which will be much more generally useful than any other plan yet devised for teaching.

It seems to me as clear as day that even the modern system of athletic training at college, which, under the guise of sport, trains the eye and the hand and strengthens the muscles, brings out the manhood in the scholar and makes him better fitted to fight the battles of life. This training is well enough in its way, but is as nothing compared to the perfect training of the hand under competent masters in the direction of working with tools. Athletic sports should come in the time devoted to play, not in school hours. The manual training must come in the regular school hours; must be in time taken from other studies. Now that this subject of the education of engineers is attracting the attention of engineers in Scotland, as I have already mentioned, Mr. W. Renney Watson, of Glasgow, tells the Institution of Engineers and Ship Builders that he, impressed with what he had seen in the American schools, had influenced his partner to send a son to the Boston school, and he took pleasure in exhibiting the work of this pupil, who had been taught by masters the art of using his hands just as a music teacher teaches the use of any particular musical instrument. He insisted on the fact that this hand training had helped, not hindered, the head work, and that the boy had learned more in school in a given time than he would have learned in the shop under the haphazard way of picking up what he can among the other workingmen.

It is this hand training which we, as engineers, must insist upon. We need educated workmen and must encourage the home supply by insisting on an education which will bring to our shops young men whose fingers are not all thumbs, and who can be utilized to advantage quickly and with less loss in spoiled and wasted material. Drawing goes a great way toward training the hands, and in illustration permit me to recall an event in my own early life in school which will show how it can aid in other studies.

My father began to teach me the use of a pencil when I was about two years old, and by the time he was taken from me, when I was seven, I was able to draw rudely such objects as I was familiar with, from having been taught how to observe form as it is presented to the eye and not to the imagination. A favorite lesson of my father's was to make me draw for him a cubical box on his desk, he placing this box in different angles of view. So I did know how to draw a picture of a tea box being thrown out of a ship by a wild Indian, in fancy dress of feathers, and could figure tea boxes floating on the waves, which was one of many illustrated stories he told me. Start any boy on the road to draw, and, my word for it, he will sketch on his slate at any time in preference to doing sums, if he has the chance. That was so in my time when drawing during school hours was a crime punishable by sundry stripes on the offending member, or, if persisted in, on other parts of the human form, which had taken no part in the offense. When I had advanced, I presume, so far in my arithmetic as doing sums in long division, I remember over-hearing a class reciting before the master, near whom I had my seat. The recitation was, as I afterward learned, in mensuration, from the writings of one Bonnycastle. The example given was to the effect that a certain farmer had loaned a neighbor a cubical bale of hay, measuring ten feet on each of its sides, and the borrower did afterward return two bundles of hay, at different times, each measuring five feet on the side, and the question was asked: Did the farmer receive back his due? and if not, how much short of it? I, hearing this, made a sketch of a cubical block on one corner of my slate, and on this perspective picture drew two smaller blocks, each half the length per side of the big one, and I saw that but one-fourth had been returned. Presently I was startled by the master calling to me, "Sellers, what do you say about it?" I asked: "About what?" "The hay," he replied; "you have heard the question." I ventured to explain that the borrower should have sent back eight bundles each five feet on the side; he has only sent one-quarter of what he owed. "How do you know?" I was asked. My answer was that I saw it. I am quite sure that at the time I drew that cubical figure and *saw* the answer to the question I knew nothing about cubing or squaring or extracting roots. I never got over my fondness for the graphic method of calculating, and hold that it should be well taught in every school. Supplement figures with the graphical method of computing and the tasks which are painful will become easy and interesting to the pupil.

An attempt is being made now to introduce manual labor into the public schools, and in some instances where teachers have been found who take the right kind of interest in the subject, considerable success has attended the effort. The trouble for a long time will be in finding teachers. Schools for the instruction of the teachers in this direction have been established and will aid in this matter. I hope engineers will give attention to a subject of so much public importance, and aid by their advice those who are inclined to prosecute the introduction of hand training. The introduction into schools should involve little expense. It is in my opinion a great mistake to fit up workshops in the primary schools with lathes and planing machines or other costly tools. To train the hands, labor saving machines should be discarded. The Russian system, as it has been introduced into the Boston Institute of Technology, is the proper method. Teach a boy to whittle, plane, or file material into shape, to size and line, and that with hand tools only, and the hand training will be accomplished with ease and certainty. Young men from such schools will be ready to enter any of the workshops and take up any trade quickly, because they will be less clumsy and will be in some degree informed as to the nature of material. I am well assured, too, that it will be found that the time spent in this direction during school hours will increase, not diminish, the amount of head work done, and with less brain fatigue or less injury to minds not over strong.

A system of education which will teach the student to think and to reason without depending on fixed rules will in the end be of the greatest practical benefit. The next best thing to having knowledge one's self is to know where to find what we want, or to know to whom to apply for the knowledge we need and who can impart it to us. When we cannot have access to minds capable of telling us, by word of mouth, what we want, we can go to books. Books, however, are not all repositories of truth. We must be taught to exercise our reasoning faculties and to separate the true from the false. Gradually the written knowledge of the world is being tabulated. No one, who has access to books, ever thinks of going through the operation of squaring or cubing or extracting roots. We all refer to the printed tables prepared for that purpose, and our *Engineers' Note-book* is the safe in which is locked up the valuable items of information which we have daily to use and to which we refer, never thinking of burdening the mind with what we can obtain so readily and with such certainty as to truth. I would like to see these books of reference introduced into schools and the child taught to refer to them and to depend on them rather than to spend weary hours in repeating operations which will never be used in after life. It is charged that the confusion of the metrical systems of different countries involves the student in long hours of mental work in memorizing this knowledge. It is also claimed that if one universal system—let us say the French system for instance—could be introduced everywhere, then this memory work would be dispensed with and much time be saved which could be applied to other purposes.

Now, my idea is that it is of no use whatever to teach these tables of the weights and measures of all countries or even of the country in which the student lives, unless the learning can be applied to some practical purpose at the moment of its acquisition, but, instead of teaching the tables, place the tables themselves in the student's hands as we have them in our engineering pocket-books, and show them how to use them, how to find the information when they want to use it. It has been said that the great advantage of the French system of weights and measures is that it can be so readily learned. This I will admit, but it will not stay learned any longer than any other set of tables unless used in every-day practice. A chemist attached to the Assay Department of the United States Mint, a man of great mental ability, who had become familiar with the French system of weights in his daily chemical operations, and who afterward went into another line of industry, and who no longer had occasion to think of grammes, told me, not long ago, that he was surprised at the ease with which this knowledge had passed from his mind. In fact, as I tried to draw him out, I found that he could not explain the metrical system without considerable mental effort, and even then he made sad mistakes. We must index our knowledge and then teach our children how to find what they want, and not kill them with trying to cram into their little heads what we cannot retain ourselves, and would be none the better for if we could.

The Engineers' Club, of Philadelphia, is doing good work in publishing the contributions of its members to a gradually growing book of reference. I think every society should do the like, and even try to index the information of books on subjects germane to its interests, and publish these references.

We have now brought about the introduction of drawing lessons into the public schools, and even made some progress in the introduction of manual training. We, as engineers, should exert ourselves to push these two things, and never lose a chance to prove the practical utility of the new system of education. We should compel the schools to follow the lead of the shops in teaching. I will illustrate what I mean by an example. We take young men into our drawing-rooms or into our shops as they come from the public schools, and we find that we have yet to teach them much that should have been taught them at school. Time and again mistakes have been made in our own designing departments, by views being wrongly placed, all because the young man taught to draw in college has been told to place the top view below the elevation, and the end elevation of the right-hand end to the left of the front elevation. All this because the examples in teaching descriptive geometry are so placed, that is in the first angle, and not in the third, as they are termed. The common-sense system of drawing, as Prof. MacCord, of the Stevens Institute, calls it, the system in use in all workshops, was not, until recently, taught in some of the greatest schools in the land, and even the text-books published for the use of the schools were all wrong in this respect.

The readiness with which some of these great schools changed their system of teaching, when it was shown to them that they were not giving the kind of information needed in practical life, shows what can be accomplished if we will lend our aid to teaching the teachers; showing them what we want them to know and what we want the boys who come to us from them to know. The highest schooling is needed to make good engineers, but such schooling can be made easy if we take the right methods and begin early enough. We must teach the money value of knowledge and how to determine it. We must measure all things by the test, *Will it pay?* There is no use in spending money in teaching school, if we cannot show that the teaching pays. To make it pay, it must be of practical utility. The true measure of the value of an education lies in what the scholar can *do*, not in what he knows. The system which will train the hand will pay better than any other when taught in connection with the right sort of mind training, not mind cramming. Some of the most helpless and useless men the world has known have been great students—book-worms—who battened on facts or figures and could make no use of the stuffing with which they were crammed, to the advantage either of themselves or their neighbors. These helpless, useless, brain-crammed book-worms will, for all time, be the legitimate butts of the caricaturist and the wit. Very few of the boys who come into the workshops as apprentices can have been through the high schools or the technical colleges. They must be not more than sixteen years old to have enough years of minority left to make profitable their services as apprentices. What is wanted, therefore, is that some part of their early school life shall have been used in acquisition of just such information as will be of use in each and every one of the mechanical industries, as well as in mercantile operations. Such men as Mr. McAllister, Superintendent of the Public Schools of Philadelphia, President Henry Morton, of the Stevens Institute, General Walker, of the Massachusetts Institute of Technology, and many other advanced thinkers, are quite alive to just what I am urging, but they have to combat the prejudices of the teachers who have themselves been wrongly taught and the parents who, under like tuition, have had no insight into the advantages to be derived from the education of the hand and the eye.

It is the fashion, nowadays, for the young man who has spent the whole of his minority in the higher schools, to be modest and self-depreciative, and, most likely, honestly so. His examinations and his crammings have shown him how much he does not know, after all. On the whole, this may be in some respects better than the conceit formerly shown by the boy fresh from college, but at the same time it has its disadvantages, for the self-confident youngster, after a few hard knocks, in his effort to hold his own, comes out a stronger man for the battle.

I hope the American Society of Mechanical Engineers will keep the matter of education in a foremost place and throw its influence in favor of a sound practical education of the hand as well as of the head, in the direction which I have so feebly pointed out. I am sorry I had not the pleasure of meeting you and speaking to you, but I earnestly hope my words will reach many and be food for thought. If other people look toward American mechanics as being in the lead, and say that American engineering schools should be taken as models, let us make the greater effort to keep our place in the front rank. The American Society of Mechanical Engineers can do no better work than in devising a good system of education and compelling its introduction into our public schools. Then will those who come after us take command and lead in the industries of the world, and the product of their skill and knowledge find ready sale in all the markets of the world.

CCLIX.

TOPICAL DISCUSSIONS AND INTERCHANGE OF DATA.

WASHINGTON (XVTH) MEETING.

No. 259—39.

What have you found the best methods for removing gas and smoke from blacksmith shops?

Mr. Allan Stirling.—I suppose the trouble with us is that we have had experience enough in this matter and our experience has not been very pleasant. I have tried to get some good way of taking care of the smoke from a blacksmith's shop. I fitted up quite a large shop some years ago, connecting the hoods over the fires by large pipes into an underground flue, and I ran this flue off to the chimney—a large chimney 125 feet high. I had the utmost confidence that that would cure the evil, but it did not, and I do not know of any better way to treat this matter than to follow the course suggested by an alderman. They were discussing the question of a sewer in some outlying part of the city, and this alderman made a motion that the dirty water in that part of the city "be allowed to take its natural course." I suppose we shall have to do that with the smoke from blacksmiths' shops.

Mr. Thomas R. Almond.—It would seem to me that coking the coal before it is put to use would be the best thing to do. I remember in the Howard Agricultural Works in England they coked all of their soft coal, and I remember no inconvenience from smoke in the blacksmith shop. They had perhaps three hundred blacksmiths there, and in coking the coal they did it under their boilers so as to get considerable heat out of the coal first. That is as well as I remember, but being a boy only eighteen years of age, I will not pretend to be certain as to whether their method was exactly what I say. The point I would make is that they coked the coal first. In my practice in a small way I do it to-day. I use very little, though.

Mr. Charles W. Barnaby.—I think that the suggestion of the last speaker in regard to coking the coal is a very good one. Of course it depends upon the kind of coal used. Through our section (Eastern Ohio) we use soft coal.

At the works with which I am connected the forges are provided with sheet-iron funnels with shields on three sides and pipes leading to ordinary chimneys built up in the walls of the building. No inconvenience is experienced from smoke after the coal is coked. Formerly when the blacksmiths coked all of their own coal the smoke was very troublesome for an hour or so in the morning when the coal was being coked for the day's work, especially in the pattern shop, which was connected at one end with the blacksmith shop in such a manner as to allow a great part of the smoke to pass into it, almost smoking the pattern-makers out. For some time past the coal has been purchased already coked,* which obviates the difficulty.

Mr. James A. Tilden.—At a well-known steam-engine works they use a method by which they are enabled to get rid of the gas and smoke in the blacksmith shop. The shop is large and has in it seven or eight forges, and they simply deflect a portion of the blast used for the forge into the smoke flue of the hood directly over the forge, creating a draft in that way after the manner of an inspirator. I have seen two fires started, one without the exhausting blast and the other with, and while one smoked a good deal at starting and gave out a good deal of gas, the other was entirely free from both. The parties claim the device works to their entire satisfaction.

Mr. Olin Scott.—I will mention one case when I saw six fires taken care of by running the pipes from each fire into a main pipe, overhead, through which a moderate blast was maintained. The arrangements produced a good draft, but it was found necessary to have large hoods over the fires, and the hoods placed quite low, to catch and carry off the products of combustion. Without the large hoods so placed, the smoke and gas from a new fire of bituminous coal would fall and settle and stay in the lower part of the shop and nearly suffocate the men. It was found that the amount of air taken out of the shop by the smoke ventilators caused such an influx of cold air in winter as to cause complaint of cold feet by the workman.

Mr. H. R. Towne.—During the war I was for two years engaged

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* The coke referred to is a special quality used for forge purposes. The blacksmiths find it very satisfactory for tool-dressing, small forging, and welding, but prefer to coke their own coal for heavy work.

on marine engine work in the Charlestown, Mass., Navy Yard, and in this connection may mention that the blacksmith shop as it then existed there (and I presume it is so still) was perhaps the cleanest and best ventilated one in the country, although more expense was incurred in accomplishing this than most private concerns would care to go to. There was a high brick chimney located at one side of the building, apart from it, with underground flues laid through the shop to connect with the forges and a sheet-iron hood over each forge with an inverted pipe leading down into the underground flues, and thus connecting with the stack at its base. Artificial ventilation was maintained, if I recollect correctly, by the waste products of combustion from the boilers being discharged into this same chimney. It was a most exceptionally clean and comfortable shop, and one of very large size.

Mr. Allan Stirling.—In the case to which I referred, when we built the fire in the chimney the draft was so strong that it would take away a good deal of the gas, and also in this particular case the coal used was not coking coal. I do not think it would be practical to coke it so that it could be used.

Mr. J. T. Hawkins.—I have no idea that the remedy for the smoke nuisance in a smithy will be found in the use of coke, except for special kinds of work. Blacksmiths have a very peculiar method of operating fires in ordinary smith work, and it would be very difficult, I think, to convince them that anything but coking bituminous coal can be successfully used in such cases. For some operations they produce a kind of artificial muffle, by allowing a new fire to bank and cake over upon the outside, forming a hemispherical oven-like structure, within which their operations are conducted; and which would be quite impossible to produce by the use of coke. I am not at all sure that he is not correct in his traditional adherence to these peculiarities, and I have no doubt that there are many operations performed by the aid of the smith's fire which could not be successfully done by means of any non-smoke-producing fuel. They would no more think of using coke than soap, in some of their operations.

No. 259—40.

What data can you give from your experience as to the working pressure of gear teeth?

Mr. Alfred B. Couch.—One would hardly suppose, looking through the various engineering works published, that such a question need be asked. The rules furnished are in number bountiful and in variety nearly infinite.

A few years ago Mr. John H. Cooper, one of our members, collected some thirty or forty of these rules, and the collection was a curiosity. Certainly no two of them agreed. The deduction from some of them was that the safe stress on the wheel-tooth would vary as the pitch; from others, as the square of the pitch; from one, if not two, as the cube of the pitch. I am sorry not to have a copy of this collection, because it certainly is a curiosity. Some of these rules assume a fixed proportion of breadth of face to pitch; in others the value is made as the product of the pitch and breadth.

I prefer to speak in a way which does not require the use of so many terms, and assume that the breadth of face is 2½ to 2¾ times the pitch. It is of course sometimes necessary to use finer pitches and broader faces, and sometimes coarser pitches and narrower faces, to get the required ratios and strength within the required limits.

But the rules of which I speak give us pressures which, reduced to the basis of one inch pitch and 2½ to 2¾ inches face, vary from 300 to 700 or 800 pounds for cast-iron. Most of them are based upon the transmission of a given amount of horse-power at a given velocity. That is a deceptive and misleading manner of stating the case.

For instance: Suppose 100 horse-power to be transmitted by gearing from a steam engine whose boiler-pressure is 80 lbs. and whose point of cut-off is at one-fifth or one-quarter of the stroke. That is all very well. But the engine is very likely to start at half-stroke, and to put a strain upon the teeth due to the entire boiler pressure and piston surface at the greatest leverage of the crank. When at its normal velocity and transmitting its usual power, the gearing will be under a strain probably less than half that under which it commenced moving.

And in rolling, punching, shearing, and pumping machinery, and many other kinds in which gearing is used to transmit power, the use of the horse-power at a given velocity as a unit in computing its strength is very misleading.

In my own experience I know of a good many pairs of gears, in which the smaller wheel has not more than 20 teeth and the larger not less than 80, in which the actual pressure at the pitch circle is from 800 to 1,500 pounds per one inch pitch by 2½ to 2¾ inches face. Of course, if a uniform proportion between breadth of face and pitch is preserved, the teeth become similar beams, and their strength is as the squares of their similar dimensions. That is to say: with the same proportionate breadth, a pitch of two inches should be sufficient for transmitting four times as great a stress as a pitch of one inch.

In my own practice, I am well satisfied when I am sure that a pair of gears will never be subjected to any greater strain than that represented by 100 pounds per one inch pitch.

But, along with the question "What stress can the teeth safely bear?" always comes another quite as important: "What is the greatest stress to which they will be subjected in their legitimate use?" And the latter is usually the more difficult question of the two.

In all cases to which I have referred, and in all cases with which I have had anything to do, I consider it an essential condition that the pinion teeth, by the use of different material, or by suitable shrouding, shall be made, of the two, stronger than those of the wheel in which they run. Unless thus strengthened, they are, by reason of their necessary thinness at the root, much weaker. This is a matter requiring careful attention if good results are expected.

Mr. Allan Stirling.—Within a few days I have had occasion to come in contact with a pair of engines which are 24 by 30, running 100 revolutions a minute, developing 500 or 600 horse-power, and they are driving through a pair of wooden-toothed wheels about 8 feet in diameter. The teeth are about twelve inches long and there are two wheels making the length of the teeth twenty-four inches. The pressure per inch of length of these wooden teeth would be about 300 pounds. I feel certain from what I know of the work of these engines that these teeth are doing about all that they ought to be asked to do—about 300 pounds per inch in length.

Mr. F. H. Richards.—One observation which I would like to make on this subject is this: Frequently draughtsmen make their calculations for gearing as if two or three successive teeth were to be in use at the same time. It seems to me that in laying out gearing for machinery, it should be assumed that one tooth is to do all the work, and that the work is all to be applied at the point of the tooth; this plan being designed to guard against accidents. Where calculations are made, as they sometimes are, on the theory that two or three teeth divide the strains between them, and that the work is applied at the pitch line, anything dropping between the teeth is liable to throw too much strain on one point, with a breakdown as the result. I speak of this because I have known several draughtsmen to use, in designing machinery, that mode of calculation—dividing the work up among successive teeth, so that if a chip should drop into the gear, one tooth would have to do all the work and would probably give way.

Mr. W. W. Dingee.—The cog gearing of lever powers used in threshing, owing to the irregular draft of horses, is subjected to heavier strains in proportion to face and pitch of teeth than is usual in stationary machinery, and as an illustration of the possibilities of toothed gearing, working under many disadvantages, may be of value.

In this kind of power (Fig. 169) 12 horses walk in a circle 25 ft. in diameter at the rate of 196 ft. per minute, and to exert the ordinary horse-power would have to draw $\frac{33000}{196}$ = 169 lbs. each.

In the accompanying vertical section of a power, master wheel *a* is 6 ft. in diameter, and is a double bevel wheel with 150 teeth on each face. To this wheel 6 levers are attached, the outer ends of which describe the 25 ft. circle, in which the horses walk. Taking the center of the wheel as the fulcrum, and the circumference as the point of resistance gives a lever 12½ ft. long, fulcrum at one end, resistance 3 ft. from fulcrum, power applied 9½ ft. from resistance. The gain of power in this lever would be $\frac{9\frac{1}{2}}{3}$ = 3.16—Total draft of horses 169 lbs. × 12 × 3.16=6408 pressure in lbs. on master wheel *a*. The power applied to this master wheel is transmitted to shaft *b* by the bevel pinions *c c c c*. These pinions are 5 inches diameter on pitch line and have 13 teeth each; the two that are keyed to short shafts *d* give their power to shaft *b* through angle pinions, and as there are two teeth of each of the four pinions *c c c c* in action at once, the strain per tooth on these pinions would be $\frac{6408}{8}$=801 lbs. The face of these teeth is 3 in. × ¾ inches or 2¼ square inches each, the movement is 47 ft. per minute.

In the center of shaft *b* is keyed the large spur wheel which is 38 inches diameter and has 140 teeth. This wheel gears with a pinion 4½ inches diameter, having 17 teeth keyed to shaft *f*. Shaft *b* turns 28.8 times per minute, which gives the teeth of the spur wheel a movement of 280 per minute.

In the turning of the large spur wheel the power is applied to the short arm of lever and must be divided by 7.1 to represent strain on its cogs. There are two of these spur wheel cogs in action at once; making no allowance for power required to turn shaft *b*, we

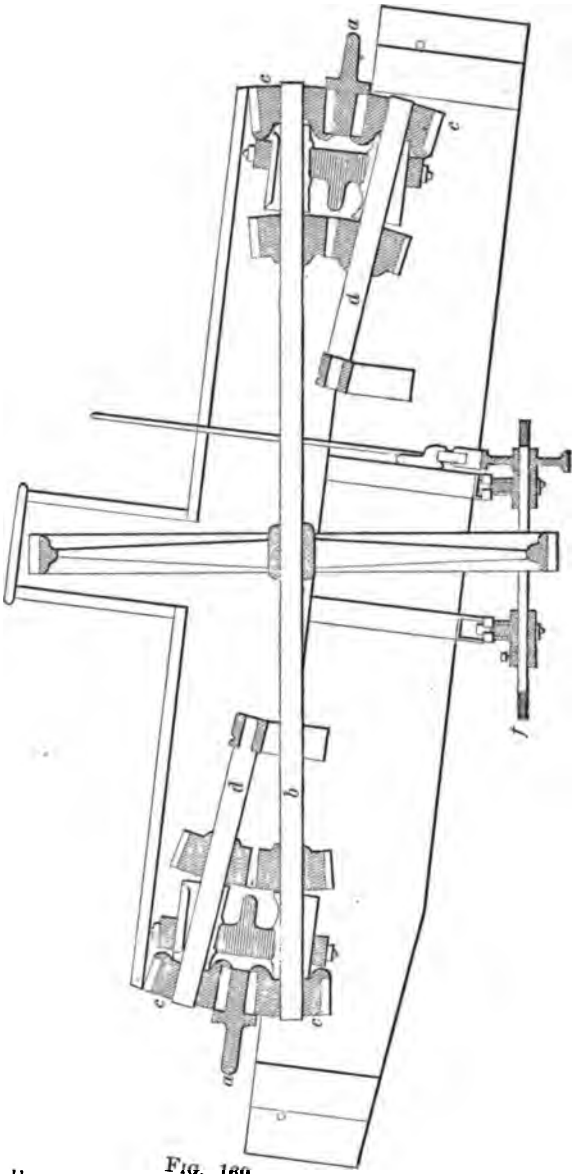

FIG. 169.

$\frac{6408}{7 \cdot 1 \times 2} = 451$ lbs; $2\frac{1}{2}$ in. × 5·8 in. = 155 square inches, face of

erience shows that in this power the spur pinion turning will much outlast the bevel pinions turning shaft *b*. By the data given it will be seen that the pressure per square inch on face of bevel pinion cogs *c c c c* is 356 lbs. and motion 47 ft. per minute, while the spur pinion cogs work under a pressure of about 290 lbs. per square inch and have a motion of 280 ft. per minute.

As the power was originally made, angle pinions were not used and the entire strain passed through the two pinions on shaft *b*; when this was the case the breakage was very great. Over 10,000 of the double-geared powers have been made and used in the past 10 years with little or no breakage.

Mr. Geo. H. Babcock.—It seems to me that on this subject we can lay down no rule, no formula, which will apply to all proportions of teeth, certainly not a formula which will apply to all materials. The object of the question, as I understand it, is to gather data from practical experience, and that is always a very good thing to do. You will see why it is not possible to lay down a formula which will apply to all sizes of teeth, because if we make the pressure proportionate to the strength of the teeth considered as a beam there will come a point, as the tooth is enlarged, where this pressure will be too great for the surface in contact and cause too great wear. If we base it upon a frictional limit, or an ascertained pressure which is proper for the amount of wearing surface, that will be too great for the strength of small teeth. If we make it right for wooden teeth it will be too light for iron teeth, and if we make it right for iron teeth it will cause too much wear on wooden teeth. Any attempt at a formula of that kind will have to be very complicated.

No. 259—41.

What is the best method and form of tool for drilling small deep holes in steel, say a hole ¼ inch in diameter drilled 4 feet into end of a steel shaft.

Mr. Henry R. Towne.—There was occasion in my own experience to drill a hole of about the dimensions indicated, and on making inquiry of others engaged in lines of work which would require such an operation, I was surprised to find that there is very little information obtainable upon the subject, and, in fact, with the exception of what I got from Pratt & Whitney, I did not succeed in getting any real light upon it. I infer from this that it is not a usual requirement to have to drill small, deep holes into metal; but if there are any here who have had occasion to do so, it will be a useful contribution to our general fund of knowledge if they will give the results of their experience as indicating the best means of doing it. The trouble is quite a complex one. As you get deep into the metal, of course there comes the question of how to get oil or soda water down to feed the point of your drill, and how you will get the chips back again. If you have a very small drill, the friction of the tool against the sides of the hole becomes very large, and induces danger of finally twisting off the drill near its outer end, in which case it breaks in the hole. I hope some one here may have experience in this matter.

Mr. Emery.—Have not gun-makers been through all that?

Mr. Towne.—I presume they have with larger holes.

Mr. Geo. M. Bond.—I would say that at the works of The Pratt & Whitney Company the operation of drilling small holes, 30 or 40 inches long, from ¼ of an inch in diameter to 1¼ inches, has been performed with an arrangement of drill and oil pump designed by one of our tool makers, Mr. J. W. Heyer, and has worked very successfully. Half-inch holes are drilled at the rate of 20 inches per hour in lengths up to 26 inches. The drill is fed forward continuously until the hole is completely through. The form of drill is one having a single lip with a groove cut spirally nearly to the center, around one side of it, while the opposite side is left so as to make the oil channel; then a thin piece of steel is soldered in this latter groove to form the channel oil-tight. Oil is forced under pressure through the center of the drill by means of a pump carried by an independent belt. The lathes we have arranged for this work have each a pump with an air chamber giving a continuous flow of oil, which is thus forced in under a pressure of one hundred to two hundred pounds to the square inch. The oil is thus carried to the point of the drill, and this current under such high pressure carries the chips out to the mouth of the hole. In our experience it works well up to lengths of 20 to 30 inches. The drill very rarely runs out of the center more than half a hundredth to a hundredth of an inch. The record, as I have it here, shows that with holes three-quarter inch in diameter, 26 inches long, one man running two lathes, a three-quarter inch drill in one lathe and a one-inch drill in the other, 4½ spindles 26 inches long and 3 spindles 30 inches long were drilled in ten hours. The record for inch and a quarter holes is: 5 spindles, 25 inches long, drilled in ten hours, one man running one lathe. I might say that this drill in running was so completely filled with oil, and thus kept cool and well lubricated, that the dulling of the drill was so slight that the spindles, which were 26 inches long, were all finished with grinding the drill only in the morning at the commencement of the operation. We have attempted to drill gun barrels as small as twenty-five one-hundredths of an inch in diameter, but this small size drill has a decided tendency to twist, though no doubt we shall finally conquer this difficulty also. All our spindles for drills, lathes, and screw machines are now being drilled in one department of our works. I may say that in the *American Machinist* of March 27th, 1886, there is an article by Mr. A. B. Landis describing his system of drilling deep holes, but the oil was introduced in a somewhat different way. The oil was pumped in around the outside of the shell which carried the working part of the drill, instead of within it, as in our practice. The method which we have adopted seems to present fewer difficulties in the way of keeping up the drills, and insures the oil being delivered at the proper place, and also the complete discharge of the chips without clogging during the operation.

Mr. Towne.—I am informed that we have with us here Mr. Supplee, of Philadelphia, who has had experience in this matter. I believe he is not a member of this society,'but I should be very glad if the Chair would invite him to tell us what he knows of this matter.

The President.—We should be pleased to hear from Mr. Supplee.

Mr. H. H. Supplee.—The only experience I have had in this matter is that I witnessed the operation of drilling holes about three feet deep and three-eighths of an inch in diameter. This was two or three years ago. I do not recollect entirely the details, but it was performed by welding an ordinary twist drill to the end of a wrought-iron shank. The shank was sufficiently smaller than the drill so as to allow the chips and dust to escape around the shank, and the result was that the drill was only in contact with the bore for four or five inches. This was done in an ordinary lathe. The drill, I believe, was supported near the end and held on the carriage so that it could be frequently withdrawn for lubrication. I think the chips were withdrawn in that way. In this case it was not essential that the hole should be perfectly true or perfectly smooth, but simply that it should be gotten through successfully without deviating very greatly from the center of the rod, and without any possibility of breaking the drill off in the hole. It was quite successful, and two or three rods were drilled in this manner.

Mr. Emery.—I suppose that in all cases the work turns and the drill is stationary?

Mr. Supplee.—It was so in this case.

Mr. Emery.—Was it so in Hartford?

Mr. Bond.—Yes.

No. 259—42.

Have you made any use of calculating machines or tables, either in professional or commercial work; if so, with what result?

Mr. F. A. Halsey.—For the past five years I have made habitual daily use of the slide rule, and have come to regard it as an indispensable desk adjunct. As a saver of time and mental wear and tear in tedious calculations, its value is great and unquestionable. Its use once acquired, it will not be laid aside. Its only drawback is the amount of practice necessary to acquire facility in its use, for while its method of operation can be learned and understood in an hour, facility in its use only comes with a good deal of practice. This grows out of the fact that, unlike ordinary scales for measuring purposes, its divisions have different values in different parts of its length, and this arrangement continues to perplex the eye after the head has learned to understand it. Its use once learned, work becomes practicable that without it would not be attempted, such as the preparation of tables relating to the user's specialty. In such work the value of the instrument is simply enormous. A single setting will enable one person to read off a column or line of the proposed table as rapidly as another can write down the results. Fractional factors of large denomination can be handled as rapidly as the smallest whole numbers, powers are raised, roots extracted, and areas and circumferences determined by a glance; in fact, a bare statement of its qualities and uses seems extravagant and excites incredulity.

Mr. H. R. Towne.—The slide rule which, after having been well appreciated fifty or seventy-five years ago, and then going out of use, is coming back again, and evidently has great convenience in many kinds of work. There are some kinds of work, however, where absolute precision is requisite, as in calculations involving values where two parties are concerned, and where those values must be precise to one cent. In such cases a slide rule would not do. For troublesome additions, subtractions, multiplications, and divisions my attention was called at one of our meetings and by one of our members to a book called "Crelle's Tables," published in Germany, which I have found is obtainable in New York. These tables are equally valuable for professional and for commercial work, and perhaps have their greatest value in the counting-house. I have found them sufficiently useful to be worth noting and worth trying by almost any one having to make calculations, either commercial or otherwise.

Mr. F. H. Richards.—Without attempting to give very much information on this subject, I would remark that the majority of slide rules are manufactured in a crude way, and the graduating, especially, is done in a manner which, to say the least is very economical. Although I never made much use of them, at one time I was very well acquainted with their manufacture. My idea is—and I think most of you will agree with me—that the slide rule is, in some classes of work, very useful as a means for estimating, somewhat roughly, general results, but that it can not be relied upon where accurate calculations are required. Some years ago I heard Professor H. A. Newton, of New Haven, say some one had made a spiral slide rule ten feet in length, which gave very good results.

Mr. Geo. M. Bond.—In our drawing-room the use of the slide scale is found very convenient by at least one of our draftsmen. This slide rule is one in which the subdivisions and values are all in Danish, and the owner seems to handle them with great facility in extracting square and cube roots and estimating, logarithms, etc. He seems to be able to do it in a quarter of the time it could be done in the ordinary way. It seems to me, however, that it would be necessary to have it in constant use, in order to keep in practice sufficiently to be able to handle it and compete with the ordinary methods, even to the limited degree of accuracy obtained by its use.

Mr. Olin Scott.—There is a point in connection with this which seems to be overlooked by a great many writers and, as a result of my own experience, covering a good many years as a builder of machinery for making measuring instruments, I have come to the conclusion that it is very hard to induce any man, having any mathematical ability whatever, to substitute a machine for mental operations. Now, the common carpenter's steel square, which is used almost universally in this country, was invented by a man near where I was born, and for a great many years those squares were made there solely. He was the inventor of what is known as the board measure, a measure for laying off octagons, and another known as the brace scale, all of which were but little used for many years. After I became the owner of a machine shop and made machinery for constructing those squares, I endeavored to carry that process further by putting on scales by which you could determine the circumference of circles from their diameter. I also got up a scale whereby the contents of a pile of wood could be measured in cords, making the unit of measure the side of the cubical cord. I never could sell it. I also attempted about twelve years ago to make measuring instruments by introducing the decimal system, and they had only a limited use among engineers. I never sold a rod in this country made on the French system of meters. I found a great many men advocating the introduction of the decimal principle or French system, and I made machinery at a cost of eight thousand dollars for making measuring rods, chiefly three meters in length, and I never had an order for them. The point of all this is, that it is very hard to induce any person to substitute a tool or instrument for computation which he can perform mentally.

Mr. C. E. Emery.—The most efficient form of slide rule is probably that formerly made by Mr. John W. Nystrom. It is simply a metal plate of segmental form, with various curves engraved upon it on a groundwork of radial and circular lines. Each curve refers to a particular calculation, and the readings are made by noting the intersections of such curve with the groundwork of regular lines. Many who have used this instrument have found it of great service, and it was by its use that Mr. Nystrom was able to perform so much mathematical work. It would be interesting if parties from Philadelphia could inform us whether arrangements have been made to continue the manufacture of these instruments since Mr. Nystrom's death. Some of his work has proved very valuable. His tables and rules for parabolic construction of ships' lines will be found particularly useful.

In connection with the calculating machine above referred to, which I have described as, practically, a slide rule, it is well to say that very many of the proportions in regular use in a manufactory can be very conveniently plotted in the same way. If sheets are prepared with either rectangular or polar co-ordinates and any progressive system of data laid down upon the same, such as size of piston rods, dimensions of frames or any other parts of a steam engine, a curve may be quickly plotted to connect the points which will aid the eye and judgment very greatly in designing structures of the same kind, though widely different in size.

No. 259—43.

"What limit of stress should be adjusted for chains as used on cranes?"

Mr. C. Seymour Dutton.—I think the answer to this question depends largely on the crane, and principally on the chain, and perhaps somewhat on the character of the work; but so far as that is concerned, I suppose we may assume that there are no sudden shocks to come on the chain, and we have simply to provide for a chain strong enough under ordinary conditions to raise the load with safety. Usually the chain rests on a cylindrical surface sometimes not very large, and if the links are of any considerable length, there is a tendency to a bending action of the links, which is, of course, injurious to it to a certain extent; but, as I said, the difficulty, I think, principally, is the quality of the chain. Now in ordinary iron construction we are accustomed to specify to the manufacturers a certain quality of iron, and to insist on that quality. We make tests of a portion of the bars; specimens are cut out and tested for ultimate strength, elastic limit, elongation, and various other characteristics, and we see that the material comes up to that requirement. So far as I know, in getting chains we do not have any such guaranty as to the quality of the chains. The chains are very difficult to test, because they are made of such a large number of parts that it is difficult to test anything more than the ultimate strength of the chain. Probably some of the very strongest links in the chain for tensile strain are the first to break. I had occasion in the last two or three weeks in our own work to see two chains broken. In one case a crane chain broke and ruined a large brick-mold that was just ready to be dried. In another case it was a sling chain around some machinery. These, by the way, are much more liable to break, because they are much more liable to be twisted or bent. Our foundryman said the links were crystallized from hard use. If there is any one thing in the world that makes me more tired than another, it is to hear the usual talk about the crystallization of iron. It has been gone over by the society at previous meetings, and it is hardly necessary to say anything on the subject; only, if a piece of iron is broken and it appears to be crystallized, you can make up your mind that it was crystallized when the iron was made. I have seen cases in which iron had deteriorated, as I believed, from continuous action that extended probably beyond the elastic limit of the iron. I remember one instance that occurred—or a number of instances that occurred—when I was a boy on a farm. I had occasion to run a mowing-machine a good deal, and after a time the knife bars of the mowing-machine would break. After they got so that they would break, it was no use to do anything further to them; welding or patching them was no good. The appearance of the iron was not crystalline; it was a rounded bright surface, or, as I expressed it, of a granular character entirely different from a cold-short fracture or a crystalline fracture of cast-iron. That has been my experience with a chain that I have seen broken; there was simply a link in it, or a portion of the link, that was full of phosphorus all the way through, and so brittle that it did not stand the action. The practice in bridge-work has been to allow for the tension members a stress of about 10,000 pounds to the square inch, in some cases adding to the material to allow for the effect of impact a percentage of perhaps 25 per cent. on members which received their shock directly, and I cannot see any reason why the limit should be placed any lower than that. I think that would be an entirely safe limit for a chain if we can get our chains of good fibrous iron; iron of a tensile strength say of 50,000 pounds to the square inch, which is about as good as we can get, and have the other qualities we want connected with it.

Mr. Wm. Kent.—A great deal might be learned on this subject of the strength of chain from Commander Beardslee's report on wrought iron and chain cables,* and I think in Mr. Holley's analysis of the effect of phosphorus on iron (included, also, in Commander Beardslee's report) he showed that moderately high phosphorus was not inconsistent with a very good chain iron; that some of the best chain cables have higher phosphorus than some of the poor ones. In regard to the factor of safety, there is a very great difference in the strength of the chain link compared with the strength of the bar from which it is made. The link has from 135 to 170 per cent. of the strength of the bar, so that in getting factors of safety for chains the link may be taken as having 150 per cent. of the strength of the bar, and then allow a pretty high factor of safety after that.

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* John Wiley & Sons.

Mr. John Walker.—It is a well-known fact that if you take a bar of iron and cut a piece from the end, it may look very good and apparently be very strong; at the same time a piece from the other end of the bar might break off with the vibration in cutting same. It is almost impossible to get a piece of bar iron that is consistent its entire length. A short time ago, in Cleveland, we took some bars of iron and investigated this question of different qualities at different parts of the bar. All manner and kinds of strength were represented in the various sections. The iron was 2"x1", and in some of the fractures there was not a quarter of a square inch of good iron; in other parts there would be 33 per cent., and in some places there would be 50 per cent. of good fibrous iron; the balance would be crystallized. When we have iron of such a quality as this put on the market, it is very necessary, in chain work especially, that we have a very high factor of safety. At the Cleveland meeting, it will be remembered, the question of chains was discussed, in reference to their passing over grooved wheels. I would like to inquire of Mr. Dutton if the chain he speaks of that broke some time ago had the links touching on the bottom of the groove, or if the horizontal link touched on the barrel? If a link lies flat on a barrel it is bent, and if it should be twisted in going back it will be reversed and bent the opposite way; this will soon break a chain. If the vertical links are made to bear on the bottom of the groove, we have a different state of things; the vertical link being able to carry the chain without bending; hence we get the proper tension on the horizontal link without bending the same, whereas if the horizontal link lies flat on the barrel, we will bend it.

Mr. Dutton.—The pulleys, the same as the drums in this case, were all grooved and the chain-links lay flat, the two sides of them resting on the barrel.

I would like to say just another word in regard to the quality of the iron. If Mr. Walker had occasion to buy 100 bars of iron, and he specified to the manufacturer that if any one of those bars that he saw fit to test showed cold-short or any other imperfection, that he did not want that the whole hundred should be rejected and taken back, I do not think he would have any of that trouble. I have handled a good deal of iron under those conditions, and have very seldom had to send back any iron.*

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* Mr. Kent refers to a probable improvement in chain due to the presence of a certain amount of phosphorus in the iron.

Mr. O. C. Woolson.—I think this subject of chains is somewhat analogous to the subject of stays in boilers. We have got to handle our material several times in different ways so that the mere testing of a piece of a bar or a whole bar amounts to very little, except as a conclusion to its fitness for working; and I think that this society is just the body to recommend that every chain be tested. There should be some method arrived at whereby these chains can be put to a test before we are asked to use them. I do not say what kind of a testing machine; I merely assume that there is some way of doing it. As regards the lay of the chain on the drum, I think this gentleman on my right is correct. I think some of the best drums that I have seen for the purpose of equalizing the strain through the different parts of the link allow the links of the chain to lie at forty-five degrees, letting every alternate link rest on one side. The result is, we get as free motion between the two links with a somewhat larger surface of wear, at the same time bringing a more uniform strain on the two bars than we would if we put the link flat down. And, what is very desirable, it enables the use of a much shorter drum with equal length of chain.*

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* This is doubtless true, and, while the construction engineer says that so long as he gets certain physical qualities in his material, he cares not for its composition or method of manufacture, it may still be some satisfaction to know what ingredients and processes produce the physical qualities we may or may not desire. The presence of phosphorus in any amount renders the iron more fluid and easily worked while hot, but when cold, hard, brittle, somewhat stronger both in tension and compression under a steady strain, and having a bright crystalline fracture when broken.

Pure iron is very soft, having a rather low strength, and is very easily oxidized; consequently a small amount of phosphorus, carbon, and perhaps other elements which we usually regard as impurities, actually render our iron stronger, harder, and better adapted to our use.

But as phosphorus makes the iron work easy and is hard to get rid of, it is almost always found to excess in bar iron, unless the iron is made under a specification. Old rails are one of the most fruitful sources of cold-short bar iron.

Now it happened in rail manufacture the rail was better if the head was very hard or cold-short, and this was also favorable to cheapness in making; and while the old worn-out rails are cheap, and pile up and weld together very nicely between the rolls, the product is uneven, mostly hard, brittle, and altogether unsatisfactory, and unfortunately this is just the iron we can expect and usually get when we order anything under the head of merchant bar iron.

No. 259--44.

"What is the average efficiency of a man turning a crank?"

Prof. R. H. Thurston.—The efficiency of the man-driven crank depends very largely upon the time expended in continuously operating it, as well as upon the "species and race, the health, strength, activity, and disposition of the animal," and other conditions mentioned by Rankine and other authorities. I have known very nearly a horse-power reached by a heavy and powerful man working for a few moments at a time. A half horsepower is not difficult of attainment by such a man working with frequent intervals of rest. Under the usual conditions of the working day, from ten to fifty per cent. of the horse-power is obtained according to the character of the individual and the adaptation of his apparatus to his purpose.

Ordinarily, I should say that ten per cent. would be about as much as should be reckoned upon. This means probably not far from fifteen per cent. of the power of a horse; for there is at least that difference between the horse-power and the power of the average horse. Rankine (St. Eng. pp. 84–86) makes the power of a man turning a crank about ten per cent. of that of a horse drawing a cart, and 12½ per cent. that of the animal working in a "gin." Mr. George E. O'Neill, a New York mechanic, who has been working with his eyes open, gives me the following: During the period in which a boiler was thrown out of use for repairs, it became necessary to do the work by man-power, and a 15-inch crank was attached to each end of the shaft of the small engine then disconnected. With one man on each crank, turning at the rate of 100 revolutions per minute, three horse-power was obtained. Reliefs were arranged at intervals of three minutes, and four men thus employed worked twelve hours a day for twelve days, at the end of which time, however, they were all, as Mr. O'Neill put it, "played out."

This would give the work of a man by the day at about three-quarters of a horse-power. Had they worked ten hours a day instead of twelve, it is probable that the work might have been continued indefinitely.

Mr. John Walker.—I have had something to do with building cranes in Europe and in this country. I have used the common old rule of 15 to 18 pounds on the crank, and find that about the right amount we may expect an average man to continue to operate a crank at without too much fatigue. For lifting a given load I have to say that I never found it fail when the gearing was according to the load to be lifted.

Mr. Kent.– I would like to ask what speed of revolution, and what radius of crank arm he has used?

Mr. Walker.—That, of course, is regulated by the gearing. The proportion of power will be between the speed of a crank at the handle and the speed of the load lifted; so that if you have a small crank, you must have gearing of larger proportions or more power. If you make a long crank, then you can have gearing of less proportions or less power.

Mr. O. C. Woolson.—I have made some experiments during the past two years which showed that a good man will apply anywhere from 30 to 40 pounds.

Mr. G. M. Bond.—I made a little rough experiment the day before I started for Washington with a hand crane—one of several that are used and were made by the company with which I am connected. We had a lathe bed which was just ready to be taken off the planer, the weight of which was very nearly 2,000 pounds. I timed the lifting of the lathe bed, and found that two men working one on each side of the crane, with a crank radius of 14 inches, raised 2,000 pounds a foot high in twenty seconds; so that the work done was 6,000 pounds raised a foot high in a minute, developing about one-fifth of a horse-power for each man. Then I took a spring balance and attempted to get a pressure at the handle, which I estimated to be about 30 pounds. Afterward I tried the experiment with one man only, and found it took 40 minutes to raise the same weight. I think on this basis that the single man must have been working harder than each of the two men. The communication of the power was through a drum about 11½ inches in diameter, using a wire rope, a worm wheel, and a steel worm, the latter about five inches in diameter and cut with a double pitch thread, so that the loss by friction must have been considerable, even with the use of nicely cut worm gear teeth and smooth surfaces of the worms.

Mr. Wm. Kent.—In regard to the figures given by Mr. Walker I have made a calculation, showing that if a man presses 15 pounds on the end of a fifteen-inch crank and works 30 turns a minute he will develop about one-tenth of a horse-power. That corresponds very closely with Prof. Thurston's calculation.

Mr. John Walker.—The rule which I gave is an arbitrary one, and the man is supposed to work at a comfortable speed without excessive fatigue.

No. 259—45.

"What is the average loss of efficiency from friction in ordinary hoisting machines operated by hand?"

Mr. Geo. M. Bond.—It would depend on whether spur gear or worm gear were used. Of course, we all know from the experiments that were made by Mr. Lewis for William Sellers & Co., that there would be a difference in the two methods of driving.

Mr. John Walker.—I should think that this question is entirely dependent on the construction of the machine and the quality of its work. I should say that if the hoisting machine has an ordinary winding power it would be much less than it would be if chain sheaves were used and a stripper used to force the chain off the sheaves as they rotate.

The President.—I suppose that the idea of the one who formulated the question was that each one in giving his experience would state what kind of hoisting gear he referred to.

Mr. Walker.—Then, of course, the plain drum would be much simpler and more satisfactory, the stripper taking considerable power. In reference to worm and spur gearing we have considerable more friction in the worm and wheel as ordinarily made and compared with spur gearing. This depends largely on how it is made; for instance, we have a case in Philadelphia at the Clem & Morse Elevator Works, where the wheel drives the worm, which is possible only with to superior workmanship; so that these things are dependent, I think, on the construction and workmanship of the machine.

No. 259—46.

"Is there a form of safety valve on the weight and lever principle suitable for boilers on small vessels in rough water?"

Mr. E. P. Stratton.—When lower steam pressures were used at sea, there were many safety valves placed on vessels on the European coasts, which were loaded directly over the valve, yet from the immense amount of weight which it was necessary to concentrate on the valve itself they have failed to give very satisfactory results. To overcome that difficulty, some years since a valve was invented in New York, and quite generally adopted throughout the steamboat service of the United States, which was operated by a system of semi-circular levers on which the weights were placed. This valve gave a tolerably satisfactory result, but owing to the circumscribed space in which it was required to work, the weights ultimately became more or less jammed, and the valve was finally discarded on account of the same result being more satisfactorily obtained through coiled strings placed over the valve. Valves of this type are now, I think, generally adopted in preference to those of the lever system, and their action seems to be more positive and satisfactory on all classes of steam vessels, especially when in a sea way.

No. 259—47.

"Is 6,000 lbs. per square inch, as provided in the U. S. Inspection Laws for steam vessels, a necessary limitation for stays in marine boilers?"

Mr. Allan Stirling.—There is a connection between this topic and the one that we have just finished. The Steamboat Inspection Law until very recently prohibited the use of spring safety valves on steamers. That was also the case in Canada. No spring safety valve could be used on any steamers in the United States and Canada until within a very short time. Recently the laws have been changed in both countries, and now we can use spring safety valves. Those who had used them before on shore knew very well their merits, and knew their superiority to the ordinary lever valve; and, after careful consideration of the subject by the inspectors, the law was changed, and now we use spring valves. Steamboat inspectors have an unusual responsibility. We all know what a disaster it is to have a boiler explosion on shore, but it is a great deal worse on shipboard. Some ships have gone out from port and have never been heard of. I have sometimes thought that a boiler explosion might be the cause of their total loss, and we should encourage the boiler inspectors to be very careful in the laws that they make and sanction for the government of steam power on vessels. Of course the world moves, and things that are right to-day may be wrong ten years hence, and this law, which is now part of the law governing the use of boilers on steamships, that six thousand pounds per square inch is the limit of pressure—no doubt at the time that law was made it was a good law; it may be so still; but it is an important matter and is worthy of investigation. Last evening we discussed the question of our navy, and this question of stays in boilers enters to some extent into the efficiency of any navy. It is well known that steam power is used to a larger extent than ever before in naval warfare. In addition to the use of very fast unarmored cruisers, it is known by those who have given any attention to the subject that even the armored vessels that have the greatest amount of steam power have an advantage for that reason. It is an advantage to have large guns and thick armor, but it is also an advantage to be able to manœuver the ship in a fight with an enemy so as to take it at a disadvantage, and so as to prevent it from taking your vessel at a disadvantage. This depends entirely on the relative steam power of the two ships. Now, there are very few boilers built that do not require stays; it is pretty safe to say that a very large proportion of the boilers that are used have stays, and I do not know of any boilers we put on shipboard that do not require staying. Whether we shall strain those stays six thousand pounds to the square inch or ten thousand pounds to the square inch will make a difference in the weight of the boilers, and a difference in the general efficiency of the whole ship. As I said before, the inspectors cannot be too careful about the laws they make, and there is one reason why this law limiting the strain to six thousand pounds has been made, and that is, it is difficult so to arrange the pressure on each side of a flat surface that each stay will have its proportion of the work to do. You can readily see that for that reason the strain on each stay should be less than we would allow for steel or iron when we know that it was having its share of strain and no more. Locomotive boilers have large flat surfaces that are stayed. For many years they have carried the highest pressures that are carried by any boilers, and with perhaps as great immunity from explosion as most boilers. There does not appear to me, however, to be any good reason, assuming that we can strain the stays equally, why we should limit the strain to six thousand pounds in a stay and allow twelve thousand pounds in a shell. I can imagine that the giving way of a shell in many of our large marine boilers would be more disastrous than the giving way of a stay, and it does not seem that we should conclude without investigation that the strains should be less on the stays than on the shells. The English Board of Trade rules are very similar to ours. They limit the strain on the stays to a much lower point than the strain on the shells. No doubt that matter has received very careful consideration from the British Board of Trade. There are no very recent experiments that I can find that are at all valuable in connection with the very high pressures we are now using both on shipboard and on shore. I think there have been some experiments made. I am told that the Navy Department has experimented in this direction; but these experiments have not been published that I know of, and are not available to the profession generally. The tests made by Colonel Stevens at Hoboken, and about the time the Sandy Hook tests were made, with boilers of comparatively low pressures and with large distances between the stays, are practically valueless with the pressures that we are now so familiar with.

Mr. President, in view of the great importance of this whole subject, I move that a committee of five be appointed to investigate and experiment as far as possible and present to this society such conclusions or recommendations as they may see fit in regard to this law.

Mr. E. P. Stratton.—I have had my attention called to this rule some time since, and it has always appeared to me to be an exceedingly arbitrary one, especially in view of the fact that in taking sheets of metal for boiler construction we are allowed as a working strain one-sixth of the actual tensile strength of the iron, which in many instances reaches sixty thousand pounds. We are allowed to take one-sixth of this as a working strain, and this iron passes through rolls in its formation and becomes the envelope or shell which contains the entire volume of steam and water, many parts of which, for instance on the sides of the furnace and underneath the flues, are inaccessible to either the hand or the eye after the boiler is once constructed. It is somewhat the same case with boiler stays; but they are generally more accessible, and can be taken out and examined or repaired, and I see no just reason why the limit should be placed at six thousand pounds when the sheet forming the envelope of the boiler, which is often more inaccessible in many parts for examination or inspection, is allowed ten thousand pounds as a working strain.

Mr. Wm. Kent.—There may be one reason why we should give a larger factor of safety to stays than to shells; that is the effect of internal corrosion. Suppose you have a shell one inch thick, which gets reduced in thickness by internal corrosion one-tenth of its original thickness; suppose you have a stay one inch diameter which is surrounded by water on all sides and exposed to the same corrosive influences—its whole circumference would be corroded and the extent of metal removed would not be ten per cent. but nineteen per cent. I think that is one reason why we should be very careful in modifying the law concerning the safety of stays.

Mr. Jno. Walker.—Mr. Kent has just expressed my views. Any person here, I think, who has examined boilers, has seen the stays almost gone when the shell was practically perfect.

Mr. Stratton.—The question then resolves itself largely into that of inspection. The law requires that all boilers shall be inspected at least as often as once in each year, and one of the difficulties in inspecting boilers is that the inspector too frequently does not go inside of them to examine the stays, and get at the actual condition of the stays and bracing; if they were half as particular in examining boiler stays as they are in examining the external portions of the shells after putting the pressure on them, many of the difficulties experienced would be eradicated. The shell of the boiler contains the entire volume of steam and water, whilst the condition of each stay only affects a very small portion of the entire area braced; yet the shell is allowed a working strain of about ten thousand pounds, while the bracing is only allowed six thousand, and still the bracing is far more accessible for examination and repair than many parts of the shell which is forced to endure many times the strain and does not have half the attention that the bracing and staying receive.

Mr. George M. Bond.—The importance which the Pennsylvania Railroad Company attach to this matter is shown by the fact that they drill small holes in the ends of the stay bolts of locomotive boilers to determine when corrosion or failure from any cause has occurred. Steam will thus leak through this small hole and the attention of the engineer would be at once called to that particular stay bolt, which may be corroded or broken off next to the crown sheet or outer shell of the boiler.

Mr. L. S. Randolph.—I found in some experiments made a year or two ago a number of stay bolts broken in a locomotive boiler. I think in one boiler we found as many as one hundred and thirty. These bolts were all broken square across close to the shell; I could find no reason for it at first until the idea came to me that perhaps the difference of temperature of the fire-box and the shell had something to do with it. I think the fire-box was ten feet long inside. Assuming a difference of two hundred degrees in temperature between the fire-box and shell, we get a difference of about one-eighth of an inch in the total expansion of the two sheets. In order to test this we fitted up a shaping machine with a single stay bolt four inches between the surface of the plates. The top plate, or the one bolted to the head of the machine, represented the fire-box sheet, and was $\frac{5}{16}$ in. thick of Otis steel. The lower plate, which was bolted to the platen, represented the outside or shell sheet, and was also of Otis steel $\frac{3}{8}$ in. thick. It was found that common iron stood about four hundred vibrations, stay bolt steel about four thousand vibrations. The best results gotten were from Sligo iron, that stood, I think, five thousand vibrations. I found that invariably the stay bolt broke off close to the lower sheet. This seems to be another question with regard to the strength of the boiler, and one which is most difficult to solve. I tried several arrangements—among other things, the threads were turned off, but this only made things worse, as the vibration was localized in some flaw or imperfection in the metal.

About five or six months afterward I presented this subject in a paper read before Section D of the American Association for the Advancement of Science, at the Ann Arbor meeting. It would seem that the greater the diameter the greater the effect this motion would have on the strength of the stay bolts.

Mr. O. C. Woolson.—It is to be remembered that in process of manufacture, the manipulation of material for the braces, by drawing, welding, upsetting, punching, springing, etc., etc., is somewhat different from that of the shell. The braces are of different kinds and characters, a great deal coming from the whims and notions of the builder; whereas the shell is a cylindrical shell, and no builder pretends to make any peculiar shape due to his peculiar notions. It is all left, or largely left, to a mechanic, presumably a good one, and yet he, I think, is more apt to have mistakes occur in that class of work than he is in the shell; at all events it is not so readily detected, and I think on the whole more mistakes and more bad work will be found in the boiler in its stays than in its shell.

No. 259—48.

An improved method of Blue Printing from large originals.

Prof. R. H. Thurston.—It gives me great pleasure to exhibit to the Society a copy of a very large blue print made by Professor E. C. Cleaves of the Sibley College of Cornell University, by a new method devised by him, by means of which almost any desired size may be made.

By the common method the larger sizes are difficult to make satisfactorily; the plate glass needed for the apparatus is very costly, and is subject to serious risk of breakage, and the whole arrangement becomes clumsy and difficult of management. By the process adopted by Professor Cleaves, no plate glass is required; the apparatus is simple and easily and conveniently handled; and the size and cost of apparatus bear very little relation the one to the other. Any size likely ever to be required in any work of the engineer can be as easily made as the smaller sizes, and the cost and difficulty of construction of the apparatus are never likely to be such as to constitute a bar to the use of this system of printing. There is no practical difficulty in getting up an apparatus to print a drawing ten feet wide and thirty feet long, if it should be found desirable. That here exhibited in illustration of what can be done is three and one-half feet wide and eight feet long, and is probably the largest blue print yet made by any process.

Professor Cleaves' apparatus consists merely of a cylinder of a length exceeding that of the widest drawing to be reproduced, and of a diameter such that the longest tracing to be used can be wrapped around it with sufficient space to spare to give room for the clamps by which it is drawn into place and held. The cylinder is smoothly covered with felt and the sensitive paper carefully wrapped about it, the tracing to be copied being drawn over the whole and held smoothly in place by spring clamps which seize its ends. It is found to be easy to lay the tracing smoothly over the surface and to draw it into contact so perfectly that the work done by this method is even better and more certain than that produced by the ordinary plate-glass apparatus, even with the air cushion now so successfully used with it. The print shown has a defect at one corner; but it is the only defective one yet made, and was selected to send simply because it was feared that that there might be some danger in sending it by express, and it was preferred to risk this rather than another. It is easy, with a little care and with some practice, to make these prints absolutely perfect, much easier than with glass.

The apparatus being ready for use, it is mounted on a cradle, supported by its gudgeons, and is revolved in the sun by means of a cord leading from some convenient line of shafting; or it may be turned by hand until the exposure is satisfactorily completed. It requires a little more time to print a sheet by this method than by the old, as the tracing and the underlying sensitive paper are but one-half the time exposed to the rays of the sun. With these exceptionally large prints, however, for which only this process would be employed, this is not an important matter. They are not likely to be made every day.

No. 259—49.

What system of regulating the wages of labor in our manufacturing establishments will tend to make that labor most efficient and produce the largest returns both to employer and employee? Give especially data from actual experience with effects of piece-work, premiums, participation in profits, graduation of wages, with terms of service, etc., etc.

This topic was so closely related with the trend of the discussion of Mr. Kent's paper entitled "A Problem in Profit Sharing" that its discussion is printed in connection with the latter, and will be found on page 649 of the present volume.

CCLX.

APPENDIX.

MEMORIAL NOTICES OF MEMBERS DECEASED DURING THE YEAR.

JOHN CHIPMAN HOADLEY.

Born December 10, 1818, at Turin, in Lewis County, N. Y. Two years in machine shop at wood and iron work. Preliminary survey for railroad from Utica to Binghamton under J. D. Allen, for few months in 1835. After a winter in Utica Academy, began as chainman, May 26, 1836, on enlargement of Erie Canal under Holmes Hutchinson, Squire Whipple and O. W. Storey. Successively rodman, leveler, surveyor, and draughtsman, until in 1840 was in charge of the party locating the enlargement between Utica and Rome; afterward on the Black River Canal, the Chenango Canal, and the enlargement of the Erie Canal between Little Falls and Syracuse. In the summer of 1842, when work on the canal was nearly suspended, and the assistants were generally discharged, it was found that he had performed his work with so much foresight, and had represented, in his notes and upon his plans, the old work, as well as the new, with such thoroughness and completeness, that in the settlement of claims he was indispensable, and he was retained and transferred to other sections of the canal, to apply as far as possible the methods which he had instituted.

While upon the canal, he received an offer of seven hundred dollars a year from the Messrs. Horatio N. and Erastus B. Bigelow, to come to Lancaster, Mass., as civil engineer in charge of the construction of the mills and steam and water-power plants at that place. This offer he gladly accepted, and by diligent study and indefatigable research fitted himself to perform the new duties which in great variety grew upon him with his growing ability. He began when he was twenty-six years old, and remained four years at this work. From this time he turned his energies principally to mechanical engineering; at first, in 1848, at Pittsfield, Mass., with Gordon McKay, in the firm of McKay & Hoadley, for three years, designing and constructing steam engines, water wheels, and other machinery; then five years as superintendent, and part of the time agent of the Lawrence machine shop, designing and constructing woolen, cotton, and paper machinery, water wheels, stationary steam engines, and locomotives. Then in 1857, he began the manufacture on his own account of portable steam engines, which he improved greatly in efficiency, and continued in the business for twenty years. His experience with locomotives had led him into an analysis of the dynamical relations which speed bore to the operation of engines, and the result of his investigations, partly mathematical and partly experimental, was to bring out an engine embodying many features which have since been largely used by others. His engine was the first of the single-valve automatics, with governor at the side of the driving pulley. During this time, also, he was engaged personally for four years in charge of the works of the New Bedford Copper Company; one year in charge of the McKay Sewing Machine Association, in 1866; one session as Representative in the Legislature of Massachusetts (1858), and four months on a mission to England in 1862, to inspect and report upon ordnance for harbor defense for the State of Massachusetts. He was a presidential elector in 1872. He also served from 1873 till 1882 on the Boards of Health, Lunacy and Charity of his State. He was moreover interested in the organization of the Clinton Wire Cloth Company, and was president of the Archibald Wheel Company.

During the later years of his life, and particularly since the commercial crisis of 1873, Mr. Hoadley separated himself from the manufacturing and the commercial side of the profession, and devoted himself more to consulting and expert practice. He was particularly in repute for patent causes, because of his remarkable memory and power of keen analysis, coupled with an unlimited capacity for taking pains and for elaborating a subject to the minutest detail. He acted as expert also in many of the best known tests of water-works pumping engines in New England,

and as judge at many of the mechanical expositions. He was one of the original trustees of the Massachusetts Institute of Technology, and presented much valuable apparatus to its cabinets.

Mr. Hoadley was one of the charter members of the American Society of Mechanical Engineers, and contributed many valuable papers of characteristic thoroughness. He also published many results of his work in pamphlet form, among the most notable of the recent ones being his "American Steam Engine Practice in 1884," which was presented as a paper at the Montreal meeting of the British Association for the Advancement of Science.

His death took place October 21, 1886, at his home in Boston, Mass. Much of his collection of expert apparatus was purchased from his executors by Mr. Stephen W. Baldwin, of New York, and presented entire to the American Society of Mechanical Engineers. He received the honorary degree of A. M. from Williams College, in 1852.

HOMER HAMILTON.

Born in Youngstown, Ohio, in 1836. Apprenticed as machinist in plow works of Predmore and Fellows. Later, the firm of Homer Hamilton & Co. was organized, which again was changed to William Tod & Co., operating the Hamilton Works, one of the largest foundry and machine shops in that part of Ohio, making a specialty of furnace and rolling-mill machinery. Mr. Hamilton was one of the charter members of the society, and held many positions of trust in his native town; he was appointed in 1883 commissioner from his State to the New Orleans Exposition. He had been failing in health for some time before his death, which took place on November 29, 1886, while the seventh annual meeting of the Society was in session.

JOHN B. ROOT.

Was born at Jamestown, New York, January 4, 1830, and died at Port Chester, December 11, 1886. At the age of fourteen years he was fitted for college at Lewiston and Seneca Falls Academies, but instead of entering on a collegiate course he preferred the work of aiding his father and uncle, John B. Ives, in canal construction, on the James River Commission in Virginia.

In California also he built water-works for hydraulic mining on the Sacramento River. In 1846 he began to build steam engines in Brooklyn, and alone and with others he was engaged from 1863 to 1869 in building several of his own special designs, on which he had been granted several patents; but most of these were more distinguished for their novelty than for any definite economic results. He is best known for his many improvements in the field of building sectional steam boilers, on which he was engaged from 1866 to 1885, and took out many patents. In 1876 he made important improvements in machinery for manufacturing riveted spiral pipe, which have been successfully and extensively employed in that industry in connection with the manufacture of the Root boiler. During his early experiments in making spiral riveted pipe he conceived the idea of making spiral *welded* pipe of sheet iron, and the last five years of his life were mainly devoted to inventing and developing machinery for its automatic production. The difficulties which he had to encounter in this last line of experiments were many and hard to overcome, but he finally succeeded in reaching a practical success only a few weeks before his death. Mr. Root joined the Society in April, 1882, and his contributions to the Society's Transactions were made in that year.

BISHOP ARNOLD.

Born in Fairfield, N. Y., June 16, 1853. Died February 16, 1887. Entered Sibley College, Cornell University, in 1869, and studied for two years. He was for five years acting as designer for the Birdsall Company of Auburn, N. Y., making a specialty of traction machinery, on which he had obtained several patents, and at the time of his death was a partner in a firm manufacturing steam-heating appliances.

He joined the Society at its Chicago meeting, in May, 1886, and attended that convention only.

B. F. EMERSON.

Born in Middleton, Essex County, Mass., December 22, 1837. His early life was spent as a clerk in Boston, and he went out at first in that capacity with the Copper Falls Mining Company to Keweenaw County, in Michigan. His executive and mechanical capacity brought promotion, and he was made superintendent in 1873, and was agent for the company at the time of his death.

In August, 1886, during the forest fires of Michigan, when the mills were endangered, Mr. Emerson was superintending the work of protecting the structures from an elevated trestle, but stepping backward he fell and injured his spine. Paralysis set in, and he was brought back to Boston, where he died April 5, 1887. Mr. Emerson left $10,000 in trust to the public library of his native town, and his collection of mineral specimens to the Michigan Mining School. He was elected an associate member of the Society in May, 1885.

WILLIAM LEONARD NICOLL.

Was born at New Windsor, Orange County, N. Y. His early life was spent at home, but when the rebellion began he entered the navy as third assistant engineer. He served on board the gunboat *Marblehead* and the iron-clad *Onondaga*. During the latter part of the war he was engaged in the torpedo service on the James River. After the war he was ordered to the New York Navy Yard and later to the United States steamer *Powhatan*. In 1868 he joined the European squadron, serving on board the flag ship *Franklin* for three years. Since then he was attached to the Bureau at Washington for three years, and for three years he was stationed at the Naval Academy. In 1880 he was ordered to the *Monocacy* in China, on board which ship he remained for four years, being ordered home for examination for promotion in the spring of 1884. From June, 1884, until March 30, 1887, he was on duty in the New York Navy Yard, being attached to the receiving ship *Vermont*. He served upon various experimental boards until the date of the commencement of his last illness. During the war he was made second assistant engineer. In 1868 he was promoted to the grade of passed assistant engineer, and in 1885 he was made chief engineer. He was detached from the *Vermont* and ordered on sick leave on March 30, 1887. His death took place at Southampton, on July 2. He joined the Society as member at the November meeting of 1882.

JACKSON BAILEY.

Mr. Bailey was born May 12, 1847, at Schenectady, N. Y. At fifteen years of age he enlisted as a private in the One Hundred and Thirty-fourth Regiment of New York Infantry, and served three years, to the close of the Civil War. He was at Missionary Ridge and in several other battles, and served in Sherman's army during the march to the sea.

Mustered out of service at eighteen years of age, he entered the State Normal School at Albany, N. Y., from which he graduated in due course and afterward was engaged in teaching. Later on he connected himself with a New York publishing firm, which position he relinquished to become New York representative of the *American Manufacturer and Iron World*, of Pittsburgh.

In November, 1877, the *American Machinist* was established as a monthly journal devoted to the machinery trades, Mr. Bailey retaining the post of its editor from the beginning up to the time of his death. In 1880, when the idea of forming the American Society of Mechanical Engineers took shape in the minds of its founders, Mr. Bailey was active in furthering the plans of organization, and the earliest meeting for definite conference in the matter was called in his office. He came in as associate, under the rules which were later established as to grades of membership, but his interest in the Society and its growth was always keen and enthusiastic. He was First Vice-President of the New York Press Club, and was also a member of the American Institute Mining Engineers, and of the Electric Club of New York. He was also a Mason. His health had not been robust from malarial troubles for over a year before his death, and his associates had compelled him to take a vacation, from which he returned refreshed, but the difficulty developed into consumption of the bowels, from which he died on July 7th.

JAMES SHERIFFS.

Mr. Sheriffs was born in Banff, Scotland, September 22, 1822. Landing in New York in the year 1848, he came to Milwaukee in 1849. As foreman of the Menominee Foundry, which was doing railroad work at that time, he had the honor of making the first locomotive castings, including driving wheels, made in the Northwest. Went to Chicago, taking charge of the Gates & McKnight Foundry there. Came back to Milwaukee and started a foundry in June, 1854, making architectural and general castings, In 1866, commenced building marine work, and has built the largest compound engines built in the Lake Michigan district. Commenced manufacture of propeller wheels in 1873, and has made wheels for nearly all parts of the United States, especially for towing. He died July 18, 1867, after an illness of four months. His membership in the Society dated from the thirteenth meeting, in Chicago.

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[NOTE.—Authors' and Debaters' Names in SMALL CAPS; Titles of Papers in *italics*.]