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CONTENTS.

PAPERS.

ILLUSTRATIONS.

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349.

(Vol. XVI., January, 1887.)

THE SIX-HUNDRED TON TESTING MACHINE AT THE WORKS OF THE UNION BRIDGE COMPANY AT ATHENS, PA.

By CHARLES MACDONALD, M. Am. Soc. C. E.
READ JANUARY 19TH, 1887.

WITH DISCUSSION.

A brief description of a testing machine capable of exerting a tensile strain of 1 200 000 pounds, recently constructed at the works of the Union Bridge Company, at Athens, Pa., and successfully applied in pulling to destruction a number of eye-bars of unusually large dimensions, may prove of interest.

In general it may be described as consisting of:—A Hydraulic Cylinder securely fastened between two longitudinal girders, which form the frame of the machine:—A Tail Block attached to the webs of the girders at convenient intervals:—and Two Connecting Blocks to receive the test pieces, attached respectively to the piston of the cylinder and the tail block: they are carried upon finished wheels, running upon an accurately lined and finished track resting upon the lower flanges of the girders. The strain upon the test piece is assumed to be equivalent to the hydraulic pressure upon the piston, which is measured by a Shaw mercury column and a spring gauge, both being referred to the center of the cylinder. The stretch is recorded upon a natural scale.

A reference to the drawings will indicate the details of this simple piece of mechanism, and, it is hoped, furnish evidence of the accuracy of the methods by which important results have been obtained.

Inasmuch as the arrangements for applying compression strains have not yet been perfected, although they are in a forward state of preparation, your attention is directed to the machine as at present adapted to tensile strains only, reserving to the near future a presentation of the completed machine.

Plate I represents a general plan and elevation with sections. The cylinder is of cast-steel, 4 feet 3¾ inches diameter and 6 feet 0½ inch long, giving an effective area of 2 039 square inches, and a working stroke of 4 feet 11 inches. The maximum water pressure for which provision has been made is 600 pounds per square inch, which for a piston area of 2 039 inches produces a total strain upon the test piece of 1 223 400 pounds, under the assumption, which is believed permissible, that the resistance due to friction is sufficiently small to be neglected. For the purpose of facilitating observations, it was intended that the cylinder should have an effective area of exactly 2 000 square inches, so that one pound upon the gauges would indicate a ton of pressure, but a defect in the casting made a slight increase in the bore necessary. It is secured to the girders by steel bolts and angles, and the outer end is left open for inspection. The piston and rods are packed with ordinary packing, to be more fully described hereafter. The main girders are of wrought-iron, 60 feet long by 3 feet 5_8 inches high, built up of plates and angles rolled in one length. Holes are bored through the webs, 6½ inches diameter and 18 inches apart, for convenience of attachment of the tail block; along this portion of the webs the thickness of the metal is 2½ inches. They rest on, and are secured to, 12-inch cross-girders, which are bolted to masonry foundations. The top flanges are held in line by cast-iron brackets, *G*.

The tail block, *A*, is a steel casting, which may be attached to the girders, at intervals of eighteen inches, by two short steel pins on either side, 6½ inches in diameter, and any intermediate adjustment is obtained by four geared steel nuts, *C*, working on the rods, D^2. These nuts are turned by a central pinion on the shaft, *E*, the nuts, pinion and shaft being contained in the plate box, *F*.

The connecting block, B^2, is a slotted steel casting resting on wheels, and attached to the tail block, *A*, by four steel rods, D^2, 5⅝ inches in diameter, having the adjustment at *F* above described. Provision is made for recoil by a steel rod, *H*, fastened to B^2, and passing through a brass friction-clamp, *I*, in the tail block. It will be observed that the rods, D^2, are held fast in the block, B^2, by double nuts, while they are free to push through the tail block, *A*. The effect of recoil at this end is therefore controlled by friction upon the rod *H*, and the amount of the friction required for that purpose is regulated by adjustment in the clamp *I*.

A vertical slot, disposed centrally between the rods D^2, admits the head of the eye-bar, which is secured by a pin passing through a pin-hole 7½ inches diameter, and slotted 1½ inches. When smaller pins are required, collars are added to fill. The object of this elongation of the pin-hole is to admit of recoil in the test piece itself—a no inconsiderable quantity in large bars. This recoil is taken upon a wooden block placed between the back of the slot and the end of the eye-bar.

The connecting block *B*, is similar in all respects to B^2, except in that it is attached to the piston by rods, *D*, of same size as D^2, the recoil in this instance being transmitted without injurious effect upon the piston.

Plate II is an enlarged view of cylinder head and piston, showing the copper-wire packing between head and barrel, also the piston and piston-rod packing, and the connection of cylinder with main girders.

Water is delivered from the pump through the pipe *P*, 3 inches diameter, and is discharged through the pipe P^1, of the same diameter, into a tank outside the building. The vertical distance from center of the cylinder to the surface of the water in this tank is 4 feet 6 inches.

Plate III illustrates on a still larger scale the detail of the piston packing. A sample of the packing itself is also submitted with the paper; it does not differ from that in general use, and is too well known to require description. This packing is "set up" by a brass gland and packing bolts, with thread and nut adjustment, until the leakage, under maximum pressure, is reduced to a thin film of water discharging uniformly about the periphery of the piston. After a test has been completed and the piston remains at a distance from the head of the cylinder equal to the stretch of the piece, it is brought back to a normal position by opening the discharge cock in the pipe P^2, and allowing the water to pass out under the head of 4 feet 6 inches, when it is found that the partial vacuum thus obtained, which is equivalent to 2 pounds per square inch upon the piston, is sufficient for the purpose. This is equal to about four thousand pounds total pressure, and inasmuch as the pressure upon the packing, when properly adjusted by its gland for a maximum water pressure, is believed to be a constant quantity, it is assumed that four thousand pounds represents the maximum reduction which should be made as compensation for frictional resistance. This is scarcely one-third of one per cent. of the highest strain indicated by the gauges, and for all practical purposes it may be disregarded.

Pressure is supplied in the cylinder by a pump having three single-acting plungers, 2¼ inches diameter and 10-inch stroke, working at slow speed, and giving steady and uniform movement to the piston. An engine having one cylinder, 8 inches diameter by 8 inches stroke, is sufficient to work the pumps with such regularity that little or no fluctuation is noticeable in the gauges.

In operating the machine, the tail block, *A*, is attached to the web of the girders at the nearest range of holes corresponding to the length of test piece; the block B^2 is adjusted to exact position by the spindle *E*. The test piece is lowered into the slots by an overhanging traveler, and when the connnecting pins are driven the pressure may be applied. Upon starting the pumps the gauges begin to rise at a uniform rate, and continue until, for a moment, they cease to move, which fact is assumed as indicating permanent set. After this limit is passed the advance is at a gradually decreasing rate until the ultimate or highest pressure is reached; at this point they remain stationary, or with very slight vibrations, often for a considerable time, the stretch in the piece meantime continuing with increasing rapidity. When the stage of actual rupture is initiated the gauges begin to fall, slowly at first, afterwards with rapidly increasing rate, until the piece is broken. In order to prevent injury to the gauges by the sudden reduction of pressure at this instant, small check valves are placed in the supply pipes just under the gauges. They are light, and close by gravity, allowing the pressure to be relieved gradually.

The effective length of the cylinder being, as already stated, 4 feet 11 inches, it is possible to stretch a specimen to that limit before withdrawing the pressure in order to set back the tail block. This represents 12 per cent. stretch for an eye-bar 40 feet long, which is the limit of the machine, and for a great majority of cases this range is in excess of the ultimate stretch.

Under the maximum pressure for which the machine has been designed, the principal members are subjected to initial strains up to the following limits:

Main girders.........	7 100	pounds compression	per square inch.
Steel castings........	15 000	" "	"
" "	13 000	" tension	"
" connecting rods.	15 000	" "	"
" bolts...........	12 000	" shear	"

All strains are referred to the net or effective sections, and this margin of safety appears to be sufficient to provide against injury from the sudden release of strain at the moment of rupture of test piece.

A fragment of a steel bar, 8 by 2¼ inches section, which has been tested to rupture on this machine is exhibited herewith; upon it will be found a full record of the test, in regular form, as follows—on page 6.

The maximum strain applied was 1 187 050 pounds, or 66 539 pounds per square inch on original area of 17.84 square inches. The remaining portion of this bar has been sent to Mr. B. Baker, M. Inst. C. E., in London, as a specimen of the material and workmanship for the Hawkesbury Bridge, for which tests were required.

Numbers of bars, ranging in section from 5 to 18 square inches, have been tested with similar results, and without the slightest injurious effect upon the machine. The specimen above referred to represents the largest bar thus far strained to rupture. The material was open-hearth steel, specified to stand 67 000 to 74 000 pounds per square inch on small specimens, ¾-inch diameter. Two steel bars, 8 by 2⅜ inches in section, have been strained up to 1 223 760 pounds without causing rupture, when it was thought prudent to discontinue the tests.

It has been previously stated that, at the moment of rupture a considerable reduction of strain is indicated by the gauges. A few observations of this reduction have been made; and, as a matter of interest, it may be stated that, in the case of the fragment exhibited, the strain per square inch at the moment of fracture, referred to the original area, was 57 464 pounds per square inch, as against 66 445 pounds maximum indication before final reduction began. If the area at point of fracture be considered, the actual strain upon that area was 118 867 pounds per square inch.

Your attention is called to the flow of metal at the zone of fracture, to the elongation of pin-hole, and in fact to the general appearance of the fragment as a whole, indicating, as it does, far better than any mere verbal description, the capacity of the machine.

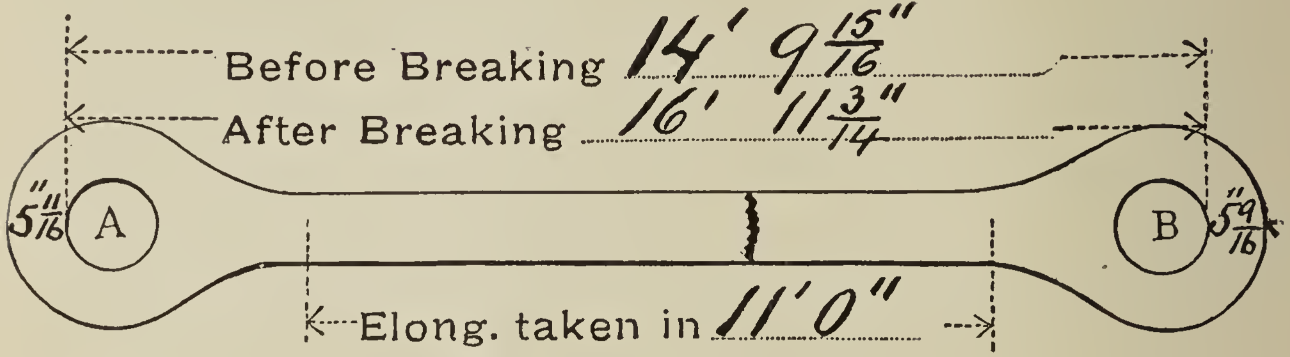
WORKS;
ATHENS, PA.
Late Kellogg & Maurice.
Capacity, 14,000 tons.
BUFFALO, N. Y.
Late Cent'l Bridge W'ks,
Capacity, 12.000 tons.

UNION BRIDGE COMPANY,
CIVIL ENGINEERS AND CONSTRUCTORS OF BRIDGES,
NEW YORK OFFICE, 18 BROADWAY.

Test No. 26.
Contract No.....
Original Mark, U.

TESTING DEPARTMENT,
AT ATHENS, PA.
November 1, 1886.

Full-sized test of Hawkesbury Bridge Steel Eye Bar, rolled by Steel Company of Scotland. Manufactured by Buffalo Shop, Union Bridge Company.



Head: Dimensions, $18\frac{3}{16}$ x 2.25 in.
Excess, 40.8 per cent.

Head: Dimensions, 18 x 2.18 in.
Excess, 34.1 per cent.

Diameter of pin-hole, 7.02 in.
Elongation of pin-hole, 1.25 in.
Nominal section, 8 x $2\frac{1}{4}$ in.

Diameter of pin-hole, 7.02 in.
Elongation of pin-hole, 1.76 in.
Actual section, 8.0 x 2.23 in.

Original area, 17.84 sq. in. Fractured area, 6.52 in. x 1.63 in. = 10.6270 sq. in.

Gauge reading for elastic limit......297 = 605 765 pounds.
" " ultimate strength, 582 = 1 187 050 "

Elongation in 12 in., $3\frac{3}{8}$ in.

Elongation in 11 ft. 0 in., 1 ft. $9\frac{5}{16}$ in.

Elastic limit....................33 955 pounds per square inch.
Ultimate strength...............66 539 " "
Elongation in 12 inches..........................28.12 per cent.
" 11 feet.............................16.14 "
Reduction of area at fracture.................. = 40.42 "

Fracture 75 per cent. fine crystal, balance fibrous.

REMARKS.

Eleven spaces of 12 inches each, elongated as follows:

A 2 ft. $0\frac{15}{16}$ in. + 1 ft. $1\frac{11}{16}$ in. + 1 ft. $1\frac{15}{16}$ in. + 1 ft. $1\frac{11}{16}$ in. + 1 ft. $1\frac{13}{16}$ in. + 1 ft. $2\frac{1}{16}$ in. + 1 ft. $3\frac{3}{8}$ in. fracture + 1 ft. $2\frac{5}{16}$ in. + 1 ft. $1\frac{9}{16}$ in. + 1 ft. $1\frac{1}{2}$ in. + 1 ft. $1\frac{1}{2}$ in. + 1 ft. $1\frac{7}{8}$ in.

UNION BRIDGE COMPANY,
By MILLARD HUNSIKER, *Inspector*.

At the present writing its sphere of usefulness is limited to the testing of tension members, not exceeding 40 feet in length, to a maximum strain of 1 200 000 pounds. The largest pin-hole provided for eye-bars is 7½ inches diameter, but flats, rounds and squares can be tested without pins by a simple attachment to the connecting blocks. When the plans for applying compressive strains are perfected, it will be possible to test specimens up to 32 feet in length and 800 000 pounds pressure.

The machine was designed by Mr. Charles Kellogg, M. Am. Soc. C. E. The late Mr. J. L. Marsh rendered valuable service to Mr. Kellogg in the preparation of plans and in superintending the construction. His death occurred immediately after its completion.

It is not contended that this is an instrument of precision, as for experimental research, or that in sensitiveness or minute accuracy it is the equal of the United States testing machine at Watertown Arsenal. Mr. Kellogg himself would be the last person to invite comparison in that respect with the invention of Mr. A. H. Emery. What he has accomplished has been the construction of a machine, at moderate cost, which will test to destruction full sized sections, as they are required for structural purposes, with rapidity and reasonable accuracy, of which the records submitted are sufficient evidence.

In reply to an inquiry regarding the Watertown machine, the writer has been favored with the following information by F. H. Parker, Major Ordnance Department U. S. A.

"A description and account of the machine is published in the Annual Report of the Chief of Ordnance, U. S. A., for 1883. From that you will see that the capacity of the machine is 800 000 pounds for tension tests and 1 000 000 pounds for compression. In the combination of the qualities of capacity, accuracy, sensitiveness, and convenience of manipulation, it is believed to stand alone, and precautions have been taken to prevent injury by recoil or reaction.

"The machine is continually operated, not only in testing large members of structures, but also small hand specimens where the greatest accuracy is desired; and it is necessary to use it in such a manner as will in no degree impair this latter quality.

"The machine has frequently broken bars to nearly its full capacity; but, in view of the constant demands made for accurate work in testing cannon metal, and in making tests for industrial purposes, it is not thought advisable to run any risks of injury or delay by breaking bars of great length and large cross-section combined. The testing of such bars is carried far enough to give, probably, all useful information required.

"Government work on the machine occupies a great deal of the time; but considerable work for private parties is done."

From all of which it would appear that the magnificent piece of mechanism from which we had hoped to derive such valuable information; which was so admirably described by the late A. L. Holley, M. Am. Soc. C. E., in a paper read before the Institute of Mining Engineers, Vol VII, 1879, and for which not a few of our Members devoted valuable time and "influence" at Washington in quest of an "appropriation," is, in all probability, destined to occupy an honorary position in engineering science, and will be quite beyond the reach of engineers in the active practice of their profession.

Perhaps this is a consummation for which we should be devoutly thankful. It is un-American, to say the least, to approach the General Government for assistance, except in such cases as may be fairly considered beyond the reach of individual enterprise. It was thought at the time of the agitation for a Government testing machine, that the great expense of its construction was a sufficient reason why it could not be undertaken by private means, and this was true so long as the question was complicated by a desire to secure an instrument which was alike suited for laboratory experiments and the testing of large sections. It was a mistake, however, to attempt the construction of such a machine. The two lines of investigation are separate and distinct, requiring mechanical appliances differing as radically as do the amounts of applied strain; hence it would have been far better, and cheaper in the end, to have built two machines, one of which should be adapted to delicate work upon small specimens, and the other of sufficient power to develop the strength of full sized members without attempting to secure minute accuracy in the measurement of ultimate strains.

In this connection, engineers are more particularly interested in the working properties of structural material in its completed form; and a machine which will develop these properties expeditiously, and at moderate cost, commends itself, without inviting invidious comparison with others having different objects in view.

DISCUSSION ON

TESTING MACHINES.

Major F. H. Parker, United States Army (by letter).—This paper, which has courteously been referred to me, with the "hope" that it "may present matters of sufficient interest to draw from you (me) an expression of opinion" seems to call for the following remarks:

It has been read with interest, and it is satisfactory to know that another machine capable of testing heavy bars is in operation. The demands of the day now require of constructors a full measure of tests of their materials and members of structures before use, and there is, in my opinion, room for more heavy machines.

While the paper as a whole, from its matter and manner, will doubtless receive commendation, that part which refers to the Government machine at the Watertown Arsenal seems to be founded upon some misapprehensions as regards its present status, the work it has done, the work it is doing, and may in the future do.

It is thought that the comments upon and deductions drawn from Major Parker's letter are misleading, and are unwarranted either by the letter itself or the facts as exhibited by the annual reports of work done, and the record of numerous private tests made every year for parties from all parts of the country.

The lament that follows the quotations referred to is entirely gratuitous, and it is desired, in correction of the misapprehension which gave occasion to it, to briefly state the use that the machine has been put to since its completion, and its contributions to engineering knowledge.

It was built by funds appropriated by Congress, and it is required that the tests made each year shall be reported to Congress. This has been complied with, and for several years public and private tests have been constantly carried on and reported.

A synoptical review of the amount and kind of work accomplished by the United States testing machine at Watertown Arsenal, shows that in round numbers 14 000 specimens have thus far been tested, comprising a range of tests, by tension and compression, from the finest wire up to full-sized members of engineering structures, requiring stresses between the limits of 1 pound and 800 000 pounds, the sizes of specimens varying between limits quite as remarkable as the range of stresses, the shorter specimens having been less than 1 inch in length, and the longest were compression members 31 feet 6 inches.

The limits of the machine are 32 feet 4 inches for compression and 37 feet 3 inches for tension bars; the limiting length for tension bars could, however, with slight modification of the machine, be considerably extended.

The styles and shapes of specimens which the machine is called upon to test are very numerous; its capabilities in this, as well as in many other respects, excite admiration.

The amount of testing has increased year by year. Already about three hundred and twenty thousand pounds weight of material has been tested to destruction, the gross amount of the stresses required to rupture those specimens having a strength exceeding 100 000 pounds, and within 800 000 pounds reaches the sum of over 125 000 tons.

Even this enormous amount of work does not represent what the machine has actually done, the number of repetitions of stresses in the ordinary course of testing would at least quadruple this amount.

It is hardly necessary at this time to more than enumerate some of the lines of investigation which have been carried on with the aid of the United States testing machine. The annual reports of the Watertown Arsenal tests exhibit in complete detail all this information. Suffice it to say that extensive tests have been made upon wrought-iron columns, flat and pin-ended, of various forms in cross-section, and of different lengths. Large cast-iron columns have recently been tested. Brick piers in cross-section, dimensions from 8 by 8 inches to 16 by 16 inches, and in heights up to 12 feet, solid and hollow cores, laid up with face and common hard-burnt bricks, in different kinds of mortars. It is believed that heretofore no tests have been made upon brick piers of any considerable height. These tests were carried out in the same manner that tests with wrought-iron columns were made, that is to say, micrometer observations are recorded of the compresions of the piers under different loads, and of their recovery upon the release of the loads. Similar observations showed the modulus of elasticity of the bricks and of the mortar employed in the construction of the piers. Numerous tests of large-sized wooden posts have been made in single sticks; also two, three and four in combination.

The above are representative compression tests of full-sized members.

Free compression tests have been made to determine the ultimate resistance of wrought-iron, cast-iron and steels of different percentage of carbon; ascertaining the pressures required to cause continuous flow of ductile metals; also tests of metals after having been subjected to cubic compression in a hydraulic press.

Among the tensile tests may be mentioned a very extended series of riveted joints in thickness of plate from $\frac{1}{4}$ to $\frac{7}{8}$ inch.

In these experiments it has been the aim to devolop fundamental principles governing the strength of riveted structures. In the light of these tests, many apparent anomalies of earlier experiments are explained, and a comprehensive knowledge of the subject gained.

Wrought-iron and steel eye-bars have been tested. In connection with the tests of these bars, it was shown that stresses beyond the elastic limit of the metal caused a temporary reduction in the modulus of elasticity, from which the metal recovered after a period of rest.

Preliminary work has been done in a series of tests with hot bars to be carried out with wrought-iron, cast-iron, and steel bars of several grades of metal. A number of the riveted joints were tested at temperatures from atmospheric up to 700 degrees Fahr.

Incidentally, many important facts have been developed in the above tests and other tests relating to the strength of metals employed in the construction of ordnance. The latter tests comprise a very considerable part of the work of the testing machine.

What has been accomplished in the test of material exposed to long-continued service—the tests of hemp, manilla, and wire-cordage, chain-cable, the adhesion of nails and wood-screws in various woods—might be dwelt upon; but enough has doubtless been referred to, to show the general scope and usefulness of the work which has been carried on, the results of which are made public through the reports made each year to Congress.

In addition to the above, there have been made numerous private tests for engineers, iron and steel works, manufacturing concerns, railroad corporations, boiler-makers, bridge works and others; tests to show the quality of the metal, and also upon full-sized tension and compression members.

The parties who have availed themselves of the opportunities, which are extended to all citizens of this country, of having tests made upon this machine, represent varied and important industries, and no inconsiderable benefits are being derived from these tests.

In view of this record of work done, there is no reason to suppose that the Government testing machine is going to cease to be useful, nor to conclude that it is "quite beyond the reach of engineers in the active practice of their profession."

Mr. G. Bouscaren, M. Am. Soc. C. E. (by letter).—Every one interested in bridge-building will be glad to learn that they now have within reach a testing machine of sufficient power to break full-size bridge members of steel. The rapid accession of this metal to the succession of iron in all its industrial applications, has rendered useless for many purposes the elaborate foundation of experimental facts gathered with great efforts by two generations for the support and improvement of engineering practice in the art of building with iron. The close relationship of the two metals is a dangerous snare to those who would presume of their close acquaintance with the one to take liberties with the other. It is quite clear that steel should be one of our best servants, possessing as it does in an exalted degree all the best qualities of iron in addition to some very precious and valuable of its own. But it is also apparent that, owing to a delicate constitution, readily affected by heat and mechanical work, as also by the presence of foreign bodies in homœopathic quantities, it is apt to surprise its friends by very strange behavior, under circumstances where iron would have been entirely trustworthy. Hence the necessity of a thorough understanding in each case of the metal in its finished state before it is used. This knowledge as regards finished structural members can only be had through testing machines of large caliber.

It was thought at one time that the necessary facilities for investigation of this kind would be afforded by the United States testing machine at Watertown. The writer endeavored to have some steel eyebars tested in this machine last spring, but was informed that the larger bars could not be tested there, the Government officer in charge being adverse to straining the machine anywhere near its limit of capacity, and the smaller bars could not be tested before six months. It is only too true that this machine, upon which so many hopes had been founded, is "quite beyond the reach of engineers in the active practice of their profession."

That its deficiency should have been supplied by the initiation and private enterprise of members of the profession, is a subject of legitimate gratulation for the American Society of Civil Engineers. It can only be hoped that the good example will be followed by other bridge companies.

No shop can be considered as being fully equipped for the construction of large steel bridges without a testing machine of at least one million pounds capacity.

Mr. George S. Morison, M. Am. Soc. C. E.—If no one else is going to say anything, there are two or three points which are practical rather than scientific, which I think it is important to mention in regard to this testing machine.

I have had quite a large number of steel eye-bars broken at the Watertown machine; I have had quite a number broken by the machine which Mr. Macdonald has just described; and I have had iron eye-bars broken by various machines in different parts of the country, and a few steel eye-bars broken by other machines.

When the Watertown machine was first given us (except for the inaccessibility of its location), it was all that I could have asked for. It was undoubtedly a very expensive machine, containing many peculiarities which were unnecessary for the class of work which we usually want done; but it was an excellent machine. The work was done there promptly, and results and reports were given in a way which bore evidence of their accuracy. I had all the test of full-sized bars in the Plattsmouth Bridge made at Watertown. The tests of full-sized bars of the Bismarck Bridge were also made at Watertown. I had the full-sized bars of the Blair Crossing Bridge generally tested at Watertown. But towards the end of the time that tests were made of bars for that bridge, the Watertown machine seemed to become practically useless. The great time expended in getting the tests made, and the facts that the officers in charge seemed to be afraid to use the machine up to anything like its full capacity, rendered the last tests that were made there of little value. The last delivery of eye-bars that were sent to Watertown to be tested were reported on several months after the bridge was open for traffic. Speaking from my own experience, I fully agree with the statement made, that so far as the engineering profession is concerned, the Watertown machine is entirely out of the field. We cannot get reports from it in time to be of any use for the structures that they are made for, and we cannot get bars broken there when the strain required is more than three or four hundred thousand pounds. They will strain them beyond that, but they will not break them.

As regards other testing machines of large capacity, I know of no machine which can be trusted for anything like accuracy excepting this machine at Athens. There are quite a number of hydraulic machines at different works throughout the country which are very useful. They work generally under high pressure, and they will break bars of any ordinary dimensions. I do not think there is a lever machine of high capacity exceeding one hundred or one hundred and fifty tons anywhere in the country which is of the slightest use. There may be. I am not acquainted with the machine at Phœnixville. It is possible that that is sufficiently accurate. This machine at Athens has two or three very decided advantages over any other machine I have seen. It will break any bar, or practically any bar, of a length which can be taken into the machine, with a single stroke. That practically doubles or trebles the capacity of the machine, because a great deal of time is always lost in changing from one hold to another. It applies the strain in a very steady and uniform manner.

The one point which it seems to me we do not know about is, how accurate that machine is in its readings. The evidence would seem to point to its being as accurate as any machine can be. Well, not as any machine can be, but as any machine ever has been which registers simply through hydraulic pressure. For most purposes it is accurate enough. In the specifications which I have been using recently—partly because there was no machine which could be trusted except the Watertown machine, which is inaccessible—I have required no particular elastic limit or ultimate strength on full-sized bars, but have simply required a certain elongation before fracture and a certain character of fracture. So far as I can see, this machine at Athens gives readings, the error of which is very decidedly within any limits which we could get of uniformity of material. It occurred to me though, when I saw the machine, that if it had been built with the cylinder movable and the tail-piece at the other end fixed, it would have been possible to apply at some future day a weighing apparatus either of the Emery style or perhaps of some other, which would have rendered the machine as perfect as anything you need ask for. I will not say that it is not entirely accurate now. The only thing is, we have no means that I can see of measuring the accuracy of its readings. It evidently is much more accurate than any ordinary hydraulic machine.

In the works which I now have on hand, this machine has been a great deal of use. It has broken very recently a number of eye-bars for the new Omaha Bridge, a number for the John Day 400-foot span which is going out to Oregon, and I expect it will be called on to break some bars for the Rulo Bridge.

The Chairman (Gen. George S. Greene, Past President Am. Soc. C. E.).—What is your opinion of the friction of the piston when that film of water is passing through it all the time?

Mr. Morison.—That is something we do not know.

The Chairman.—Do you think there can be much friction there?

Mr. Morison.—I do not think a great deal, but I do not think we know. I think that is what we want evidence of. I wanted to see tests made of this kind—let a bar be tested in that machine to a certain point which exceeds the elastic limit. Then let that same bar be sent to Watertown and tested and a comparison made between the highest strain put on it in the Athens machine, and the strain at which stretching begins again in the Watertown machine. Tests might also be made at less strains with micrometer measurements.

The Chairman.—Would not the rest that takes place between the test in one machine and the test in another have some influence?

Mr. Morison.—The seven or eight months' rest which would take place if it were sent to Watertown might have some effect, but if it were tested at once, I think it would give a very valuable comparison.

Mr. A. H. Emery.—There are two or three points that I would like to call attention to. First, as regards the use of the machine at the Arsenal and the great delay that has occurred, more especially during the last two years, in getting tests made for outside people. I know personally that for three or four years the department officers have been trying very hard to get Congress to appropriate money for a small machine to relieve the large machine of a great quantity of work of this kind. Every band, every trunnion-piece, every jacket, every barrel of every piece of ordnance that is made over by converting smooth-bore cast-iron guns into rifles by lining, each has to have a piece tested at the Arsenal, and sometimes two pieces from each of those parts. And parties who are doing work for the Government tell me it is not unusual for them to take out a test piece from a band or a barrel and send them to the Arsenal, and sometimes those pieces lie there waiting four months before the returns of the test can be had to determine if the piece is to be finished or condemned.

Mr. Macdonald.—What sized pieces, Mr. Emery?

Mr. A. H. Emery.—Very small pieces, that could be tested on a small machine better than on a larger one.

Now the executive officers should not be blamed for that state of affairs, but Congress, that has declined year after year to make this appropriation. I am happy to say, however, that an appropriation is likely to be made within a few months for a small machine.

Now as regards the accuracy of this machine and the one at Athens, I understand the writer of this admirable paper in describing this machine, which certainly seems to be one of great utility, to state that the hydraulic packing friction, that is, the friction of the packings and piston-rod, etc., may be entirely neglected. My own experience differs so far from that, that I should say if we entirely neglected it we will be very greatly in the dark. It is an element of great variability, of great uncertainty, and I am sorry to say of much greater magnitude than is generally supposed. Captain Eads, in constructing his bridge (or Mr. Flad), made a hydraulic machine and a hydraulic gauge for testing it. Experiments on the gauge show that the friction of that gauge-piston was a very considerable percentage; a good deal more than two or three or four or five or six-tenths of one per cent.; a good deal more than two or three or four or five per cent. Mr. Hicks' experiment on pistons of half-inch in the gauge showed those frictions to be very large. In my own experience in that line, I had in the machine at the Arsenal a piston 20 inches in diameter which was packed both ways, and the piston-rod also packed, that is, 10 inches, so that we had three packings; each ran on a very smooth surface. The copper lining was thoroughly rolled and dressed repeatedly, nine different rollings and dressings being taken to increase the bore less than two one-hundredths of an inch. When that cylinder was completed, the surface of the copper was as smooth as glass. The piston-rod itself was finished with emery cloth, so that it was smooth. The packing around the 10-inch piston-rod has to slide on the rod, and the two twenty-inch packings had to slide on the copper. It has a cup-packing, and a brass ring comes in and fastens it down, and the pressure of the water packs the leather against the bore of the cylinder. We usually in tension or compression carry from forty to sixty thousand pounds back pressure; that is to say, if we had 300 000 pounds on the tension side of the piston, we would have 50 000 or 60 000 back pressure on the other side. So if we had 350 000 pounds on a specimen, we shall have a pressure of 350 000 and a back pressure of 50 000. The surface of these three packings together would represent about one-eighth of the entire section of ram; about the same proportion which we have in this new machine from the long packing and the larger diameter of the cylinder and the long packing on four rods.

Mr. Theodore Cooper, M. Am. Soc. C. E.—When the piston moves, one packing has to move backwards ?

Mr. A. H. Emery.—Yes. But it is the packing that has the small pressure on it. When this machine was contracted for, the Board believed that the scale which was to be provided would be ample, thoroughly accurate and thoroughly reliable, and that nothing further was needed; but up to that time no scale was made of the variety which is in that machine, larger than ten tons, and it did not seem to them that it was right or proper to make a machine of that magnitude without providing something else, if it could be done. Mr. Charles E. Emery, M. Am. Soc. C. E., provided a system of hydraulic presses for testing machines; provided a plan for a machine for the Board in which the ram was rotated, the motion of rotation being large in proportion to the motion of translation. If, for instance, this motion of rotation is one hundred times the direct motion, then the hydraulic friction or the other friction of sliding the ram, whichever it may be, will be reduced in that proportion. As a matter of fact, those motions, as I put them in there, were much more than 100 to 1. Now, if we would let the ram stand still and not rotate it, the difference between the reading of the scale of the gauges with a load of three or four hundred thousand would be twenty or thirty or forty thousand pounds. Immediately, however, on setting the ram rotating, the scale and gauges correspond. The moment you commenced to rotate the ram, that thirty or forty thousand would disappear.

Now, as to the packing in this Watertown machine we have this case: Mr. Sellers finds that he can put in a recess a little piece of leather and the liquid will flow behind it and throw it out and pack the ram. He says he does not need any gland to force it out; simply put that ring of leather in, and the water will pack that very well. The greater the pressure, the greater the flow of water to hold it up, the pressure between the packing and the cylinder varying with the pressure of the liquid.

I remember very well the first time I went to Phœnixville, the engineer there, Mr. Griffen, showed me their hydraulic forging machine, which was also used for testing. I said, "What do you do with the packing friction?" He said, "That doesn't amount to anything." "How do you know? I say it is very great and very variable." "Well," he says, "we have a 3 000-pound weight which returns it when the load is off." I said to him, "The friction is very large and very variable. It is much more with the new packing than with the old one." We came along later in the tramp through the works to an accumulator. I said, "Is that sufficient for your forging?" He answered, "Well, yes; except sometimes, when we put in a new packing, it is slow." I said, "Oh, there is no difference in the packing." He saw the point, and the blunder he had made in supposing the friction to be small.

The accumulator at Watertown is connected with hydraulic gauges and with the holders. These gauges show the pressure on two 14-inch rams in the holders. Now the rams run up a short distance and seize the specimen. They become stationary. The gauges show the pressure of the liquid forcing them against the specimen; but as they are running up, the accumulator is running down. The friction of motion is very considerably less than the friction of quiescence or rest; and as the two 14-inch rams move up slowly, they seize the specimen and gradually come to rest. The 10-inch ram which was driving them will be slowly settling down. Now, watching the gauges which connect with the holders, we found this state of affairs, that on this 10½-inch ram the load shown on the two 14-inch rams by the gauges would run up, as the accumulator ram was moving down, rapidly, to about three hundred and ten thousand; but as that motion gradually ceased, the holders came to rest by the specimens refusing to yield any further, and the ram, therefore, standing still, this pressure gradually ran down to two hundred and ninety or two hundred and ninety-two thousand. Those differences do not represent the friction; they represent the difference of friction of that 10½-inch ram with a load of about eighty thousand referred in this case to the two 14-inch rams in the holder; but the proportion of loss is the same. You see it is a difference of friction, which represents a very considerable amount. From two hundred and ninety-two to three hundred and ten thousand is a very considerable percentage, which is not the friction, but the difference of friction between the ram when it was in motion and at rest. So I must consider that the friction on that Athens ram is much more than was attributed to it on high loads, and that it is a very variable one. And, aside from that, my experience in the distortion of all forms of pieces when loaded, is such as to cause me to say that a cylinder supported as this is, receives the load applied on the head, as a beam, with a depth of the web equal the length of the cylinder and a thickness equal that of the walls of the cylinder. Those parts of the cylinder constituting the web of the beam are in the curved lines which constitute the walls of the cylinder. The yielding of those is such as to bring a considerable pressure on the piston. That pressure is unknown and variable, depending on the load, and depending on the position the piston may have at different times in the cylinder. As the piston moves toward the middle, it will be different from what it would be at the end. So that those two sources of packing friction and the friction of the piston itself in the cylinder, are very considerable and of variable and unknown amounts.

Now when we come to the pressure itself, it is measured by two gauges whose exact properties I know not. But my experience with all the gauges that I have tested would tend to cause me to say: You must not rely upon that as being absolutely correct. I went to Mr. Willing to make me a gauge, aud he said if I would let it down to 600 pounds to the square inch he would make a very accurate one. I said, "Make me one." He made what is known as a Bourdon gauge. That gauge showed its full reading, 600 pounds, with a load of 525 pounds to the square inch. Now you are not to infer that these gauges they are using are of any such degree of error as that, but we may infer that there is a possibility of considerable error. Now another point with all of these gauges which I have tried, both the Willing and the others, is that they do not run up and down at the same points with the same load. That is to say if I bring a regularly increasing pressure which should bring me a certain curve, when I come down to the same pressures I will not get the same ordinates. In the case of the Willing gauge mentioned, which was the bent-tube gauge, it represents a very large class of our gauges, and that varied from nothing to 25 pounds in the nominal readings going up to 600. That should be reduced in the proportion the other way—from 600 to 525; but it never would run up twice precisely alike with the same pressure, and never run down in the same curve at which it ran up. I have no doubt they (the Athens people) have a much better gauge than that—I should hope so; I should presume they have—but how much better it is I cannot say. I should doubt very much if that gauge was true within one per cent.; I should be very much surprised if it were found so. I would mention here as to how I tested these gauges to know their exact pressures. We have what I call a plate fulcrum machine—a lever machine which is absolutely frictionless—on the platform of which we carry 60 000 pounds, or any smaller load we choose. It will show on that platform a load of a tenth of a pound distinctly. On that platform we set a hydraulic support with a carefully made area of 80 square inches, 50 square inches, $33\frac{1}{3}$ square inches, 10 square inches or 6 square inches, as the case may be. We connect liquid with that and the gauge. There is a little free span, the size depending on the magnitude of the pressures, however great, varying all the way from as high as three-tenths of an inch down to five one-hundredths of an inch. We connect the liquid with this support with the gauge to be tested. This represents a large column. This is magnified. If this column moves up and down a little during the work, there will be but little force put into this diaphragm to bend it. I have applied a gauge to find how much the column yields with a load. They yield from half a thousand up to two-thousandths of an inch with a full load. Loading this pressure support in this weighing scale, we apply a pump and force in a quantity of liquid just sufficient to keep this column at a constant height, and the gauge by which we regulate that will show very distinctly a ten thousandth of an inch; so there is no difficulty in keeping that column within a ten thousandth of a level all the time. Thus we have the means of absolutely knowing that our pressures here are substantially what we represent them to be, and they are not what the gauges represent them to be. But, as I have said before, they differ very materially,

In regard to testing some large bars up to and past the limit of elasticity in the Athens machine, and subsequently sending them to the Arsenal to see what load will increase their stretch, I would say, all my experience goes to show that a certain load having been applied to such a bar, sufficient to pass the limit of elasticity and subsequently removed, the second load, which will start that again and give no increased stretch, will be found to be very considerably larger than the first. If then we will compare those loads, we must take that bar at a certain temperature, and load, not sufficiently to reach the limit of elasticity, but keep below it; or if we do pass it, let the bar cool down and begin again, and pass carefully in certain times and with certain carefully weighted loads, and note carefully the corresponding lengths of the bar. Then put it in the other machine and see if those lengths remain the same. We shall find the bar in that case will act as a very excellent dynamometer—if we do not pass the limit of elasticity—provided we keep it at the same temperature, and the measuring apparatus at the same temperature.

There is one more point I would like to mention. The desirability of a large and small machine is fully recognized, and I have designed a small machine. The stroke of the piston in this machine is 25 inches, or sufficient to move the entire length of the specimens which can be tested therein, and they are measured in just about this position before the observer [indicating a height about fifty inches from the floor]. The tie-rods or side members are 21 inches apart in a vertical plane, so there is every facility to see exactly and observe what is going on. The compression tests are in another part of the machine, which is not disturbed while you are testing for tension, nor are the tension-holders disturbed while you are testing for compression. You pass from one to the other without disturbance or change of the machine in any way.

In regard to large machines, I would say that during the past year I have designed one which was called for by one of the departments. They have not succeeded yet in getting their appropriation. It was a machine for testing all sorts of bars—rounds, flats, squares, etc.—up to 25 feet in length, with loads up to 400 000 pounds, and chains 90 feet between pin centers with 6 feet stretch. The piston of that machine has a motion of 8½ feet. I have also designed during the past year another machine for loads of 1 200 000 pounds for both tension and compression, which will take in lengths of 50 feet between pin centers and stretch them, if the metal is equal to it, 12½ feet. The columns which would go in that case would also be 62½ feet length at pin centers. The holders for that machine will take round bars up to nearly 6 inches, all sizes down to a little fine wire; square bars, all sizes, from the smallest made up to the maximum size of iron or steel, which 1 200 000 pounds will break; also all sizes of plates or flat bars up to widths of 16 inches, and eye-beams, 15 inches of wrought-iron, and pull them in two. As regards recoil, the machine stands up and takes it. There are no appliances, as there are in the Athens machine for taking them up by friction, but the machine is able to take all the shock of recoil without injury. The recoils in the case of steel links of the maximum length which can be tested, and which break through the eye of the bar at either end with the maximum load of the machine, are very large; would give us a recoil in that case of about seventy thousand foot pounds. We do not expect any injury whatever from a recoil of 70 000 foot pounds. The stroke of the ram is 13 feet. This machine is not built, except on paper. The press moves from one end of the machine to the other in two or three minutes by the mere starting of a little belt. I should say two minutes is sufficient to carry it from one end of the machine to the other, and it is stationed at any desirable point very quickly and without any back-lash whatever. The holders for tension have, as I stated, unusual capacity; they do not have to be removed in passing from tension to compression. The scale is changed from tension to compression by a little turn of a crank. The compression holders, however, must be put on the front of the tension holders, and then removed when you want to pass to the tension business; but the tension holders are not removed for any operations in the use of the machine.

Mr. Theodore Cooper, M. Am. Soc. C. E.—There has been much error upon the subject of hydraulic packing. The careful experiments made at St. Louis upon hydraulic packing, I believe have never been published. As it is desirable to have them more widely known, an abstract of them, with the apparatus for determining them, will be submitted as an appendix to this paper.

Past President Henry Flad devised a very simple apparatus for making these tests and supervised the experiments. From a careful study of these and other experiments I have no belief in the claim made as to the great friction (relative) of hydraulic packing in such sized rams as are usual for hydraulic testing machines. On little plungers of the size of one's fingers, you can get friction enough to resist motion under any practical pressure, but as we pass to plungers of larger diameter the friction rapidly diminishes, and for plungers of 18, 20 or 24 inches diameter I doubt if the friction of a properly made packing under pressures of 600 and more pounds per square inch, amounts to one-half of one per cent. of the total pressure exerted.

Certainly for practical purposes we can call a testing machine accurate if the results can be obtained within this limit. Here is the merit of the claim made by Mr. Macdonald for the machine under discussion.

He does not pretend to weigh the strains to such a degree of accuracy as would be required in the extreme refinements of laboratory investigations. For testing the strength of practical work we do not desire to find the strains within such refinements as a fraction of 1 per cent. Life is too short to attempt, in practical work, this extreme accuracy that my friend Mr. Emery would desire. I admire him for his desire for extreme accuracy, but it is misplaced when applied to every-day practice.

When searching for the laws governing the strength of materials and analyzing the various influences effecting the same, accuracy is imperative. For such purposes the Watertown machine may be admirably fitted.

But for the constant demands of practical construction, the machine here described by Mr. Macdonald is more accurate than a more delicate and refined machine. It gives us directly, by one application of the pressure, the ordinary elements of the test-piece. Where it is necessary to take several hours to make a test and elongate the piece by successive steps we get a different result, and in my opinion one of less accuracy for comparative purposes than when pulled directly at one operation.

Mr. Emery.—May I interrupt you a moment?

Mr. Cooper.—Certainly.

Mr. Emery.—I think there is no length tested on that machine but that one setting of the ram is sufficient to carry it through.

Mr. Cooper.—I may be mistaken then, but I have been informed that much time is wasted in changing for a new attachment.

Mr. Emery has made one point that may be important in reference to the distortion of the cylinders by the attachment of the side rods. This, however, is merely a matter of the proportions of the cylinder.

Mr. Charles E. Emery, M. Am. Soc. C. E.—This machine undoubtedly has questionable features, but on the whole I like it.

Briefly reviewing the whole subject: Mr. A. H. Emery has mentioned my connection with the bids for the Government testing machine at Watertown, but the enormous friction of hydraulic packings can be still more forcibly illustrated by the experience there than he has stated.

I submitted a plan of a hydraulic machine in which the ram was to be revolved by power as it was forced out by the pressure to produce the strain. By making the rotary movement very much greater than the movement of translation, the surface of the ram would evidently have the same motion as if it were a fine screw, and the friction due to such movement would be distributed between the direct and transverse movements, precisely in proportion to the relative distances moved. For instance, if the rotary movement were ninety-nine times that of translation, ninety-nine per cent. of the friction would be overcome by the external power revolving the ram, and the difference between the pressure in the cylinder and that on the specimen could not be greater in any case than one per cent. The principle is well illustrated by workmen moving a heavy loose wheel on a shaft. If two or three men revolve the wheel, a workman at the side can push it along the shaft with his heel, the two forces causing a spiral motion, even though the sliding force be very much too small to produce movement unless combined with the other. The principle was illustrated before the Board by placing a heavy weight on a slide, when it was found that the most delicate diverting force would move the weight laterally, so long as the slide was pulled through beneath it. Some members of the Board expressed a wish that this principle could be embodied in the straining press end of the A. H. Emery machine, so that if any of the delicate work he proposed to provide for the weighing end should require experiment, or cause delay, the Board would still have something which would give closely approximate results in a reasonable time, and one apparatus would act as a check on the other.

Mr. A. H. Emery finally arranged with me to apply my device to his machine, though it could not be done as simply as in the plan I submitted, on account of his double-acting piston and other differences in construction. To accomplish this, he designed a separate piece or apparatus—practically a short duplicate of his straining cylinder—to take the thrust of the piston in the latter, and upon the carriage supporting the same he erected the machinery for revolving the ram and pistons.

Mr. A. H. Emery will please excuse me if, at this point, for the purpose of showing the great friction of hydraulic packings, I call attention to differences of opinion which arose between us originally on the subject. My experience with the variable friction of steam-engine valves and packings led me to suppose it might be possible, at times, that the friction of the packings would be as high as ten per cent. of the total load on the hydraulic plunger. At any rate, to be safe, I designed the rotating apparatus for this strain. A large heavy worm-wheel, with coarse pitch, was to be secured to the ram and operated by two worms, engaging with opposite sides of the wheel. Mr. A. H. Emery insisted that the provisions made were unnecessarily strong, and in making his designs he applied a smaller sized spur-gear with teeth of less pitch, and operated the same with a pinion on one side only.

Mr. A. H. Emery.—On both.

Mr. C. E. Emery.—I had forgotten that; at any rate it was found on trial that the apparatus provided would not revolve the ram up to the full capacity of the machine.

Mr. A. H. Emery.—Not half.

Mr. C. E. Emery.—Not half the capacity of the machine. It seems that my caution or obstinacy was nearer right than his calculations in that matter.

Mr. A. H. Emery.—I must plead ignorance. The friction is fully twice what I expected.

Mr. C. E. Emery.—Everything was fitted micrometrically in all that work, and whether or not this had something to do with the result I do not know. The whole A. H. Emery machine, from one end to the other, is marvelous in its ingenuity, marvelous in its execution, and marvelous in its performance. It is a monument of which any one may well feel proud, and a credit to American engineers and mechanics. I may add that his machine, as originally designed, operated so well that there has never been any occasion to perfect the attachment made to apply my principle. The experience had with that part of the apparatus strengthens the point that the friction of ordinary hydraulic packing is very large. I, however, as a steam engineer know something about the kind of packing used in the testing machine now under consideration; and it is curious that it never occurred to me before that this was the kind of packing to be used under such circumstances. The friction of such packing, under certain practical conditions, really *reduces* as the pressure increases, and I know it.

Very frequently, in taking what is called a "friction diagram" from steam engines, for the purpose of ascertaining the power required to operate the engine when unloaded, we get a result which is called the friction of the engine, but is really no such thing. I have tested the same engines when loaded, with indicators on the cylinders and dynamometers on the shafting, and found that the friction of the whole load, which included the friction of the packings, was less than was shown in taking what were called friction diagrams.

Mr. A. H. Emery.—Under the same speeds?

Mr. C. E. Emery.—Yes, under the same speeds. I will explain, for the information of those not familiar with the technical details, that the friction of the load, as we understand it in testing engines, is the additional work thrown on the bearings when the engine is loaded and doing external work. At such times the pressure on the piston is greater, and this causes increased pressure on all the working parts and in all the bearings, which increased pressures produce the additional friction termed the friction of the load. Now I say that I have observed actually, in some cases, that the friction of the whole load, tested carefully in the way stated, was less than was shown as the friction of the unloaded engine when the latter was determined by the use of the indicator. I explained this on the theory that the stuffing-boxes were necessarily screwed up to resist the pressure of the steam at the maximum load, and that, when there was no load on, the pressure in the cylinders being diminished, the elasticity of the packing caused it to grip the rods like a vise. I therefore, afterwards, in writing out instructions for engine tests, directed that the packing on all the rods be loosened before taking friction diagrams. In the testing machine under consideration, the packing is of the same kind as is customarily employed on the rods of steam engines, and is compressed by a gland in the same way. The pressure on the gland must be sufficient to force the packing against the side of the cylinder, and upon the piston rods with sufficient force to resist the pressure of the water. It seems to me reasonable, therefore, that when the water pressure is reduced, the elasticity of the packing causes a greater pressure against the surface of the cylinder, and a greater friction than when the pressure is at its maximum. The paper states that a tank is provided 4½ feet below the cylinder, and that the partial vacuum produced by the 4½ feet head of water is sufficient to retract the piston. This is done when there is no pressure in the cylinder, and when, on the principle above explained, the friction is at a maximum. It follows, therefore, that the friction can at no time be greater than is represented by the load due to a head of 4½ feet of water on the area of the piston, and from the consideration of the principles above stated, that the friction must be actually less than this when tests are being made and fluid is admitted under pressure to produce the strain.

If the facts and principles here stated apply to this particular case, and I see no reason why they do not, the machine is certainly not only very useful for rough work, but will show practically accurate results for moderate strains. The point raised by Mr. A. H. Emery, that the method of connecting the frame to the cylinder may cause distortion of the latter, and produce friction which cannot be calculated, is an important one which should not be overlooked. That matter needs investigation.

Mr. Cooper.—May I interrupt you to ask a question of Mr. Macdonald? Do you notice in the working of the machine whether the volume of water passing the piston is changed?

Mr. Macdonald.—I was going to say that one object of leaving the cylinder open was that a thorough inspection might be made during the test, and the observation of that thin volume of water would convince either of these gentlemen that there was no distortion there which was appreciable.

Mr. C. E. Emery.—That was a matter, I said, which needed investigation. If such investigation has been given, the question is already answered. I am very much gratified to see this application of a very well-known device. I will only add that, even granting the applicability of the principles I have stated, the accuracy of the machine will depend upon the way in which it is maintained. If finally it needs a head of water of eight or ten or more feet to retract the piston, it will show that the packing is out of order. If the machine is to be used regularly by a man who has pride in the work, and the conditions stated in the paper be maintained, to wit, the packings kept so free that the piston will be retracted by a head of water of four and a half feet, that, of itself, it appears to me, will insure the substantial accuracy of the machine, and for that reason I say I like it.

In general, as has been stated, there will be required machines of different classes, one for the very fine work for which the A. H. Emery machine is so well adapted, and which it does so well, and others of the rough-and-ready type, which will do practical work in a practical way in a reasonable time, and which will give practically correct results. The machine under consideration, it appears to me, will answer the latter requisite, and from such examination as I have been able to give the subject, I think it will do it well.

Mr. Cooper.—During the construction of the St. Louis Bridge, the steel members and samples of the steel were tested at the steel works upon a large hydraulic testing machine of a crude construction. I personally used this machine for months, testing thousands of samples. Occasionally duplicate samples were forwarded to Pittsburgh to be tested upon the St. Louis lever machine, a very accurate machine. A comparison of these duplicate tests made upon this machine by another observer with those made by me on the hydraulic machine at the steel works, satisfied me that there was no great error in this machine. An allowance of one per cent. for friction was made upon the strains obtained.

I do not believe that in a practical machine of this character the friction will be enough to give any reasonable doubt of the results. Of course, with new packing or badly-made packing the friction is considerably higher than with good and well fitted packing.

In regard to the gauge mentioned as used with the machine under discussion, the Shaw mercurial gauge I believe to be a very good one when kept in good order, and occasionally verified by testing. Like all apparatus it needs care and regulation. I have used this gauge in connection with one devised by Past President Henry Flad to check its correctness. The latter gauge was made of a selected steel ribbon about ten feet long, carefully tested for its comparative elasticity under the range of pressures desired, and the pressures recorded by suitable multiplying gear. It is an inexpensive apparatus.

Mr. C. E. Emery.—I did not quite cover all the points I intended in my remarks. Referring to the two kinds of machines desirable—one, the accurate machine which we have already in the A. H. Emery machine, and the other, the rough-and-ready one, to be sufficiently accurate not to mislead us—I have hoped and urged that the parties now controlling the A. H. Emery machine, as well as my own ideas on the subject, would bring out a machine with a rotating ram as I proposed, and put it on the market, as it would furnish a comparatively cheap apparatus sufficiently accurate for general purposes. Referring to the machine under consideration, there will necessarily be the same feeling in the minds of engineers that Mr. Morison has shown here, that there is a chance of error and a doubt as to how much it is. It is therefore desirable to have machines the accuracy of which can be tested. I trust that the provisions for retracting already used in the machine under consideration do furnish such a test on the principles I have already enunciated; but to prove that this is true, I would suggest to Mr. Macdonald that he have a light steel eye-bar tested in the A. H. Emery machine, well within the limit of elasticity, and carefully note the elongations under different strains with a microscopic device or a compound lever apparatus of the kind used by Colonel W. H. Paine, M. Am. Soc. C. E., and then keep the same as a test bar to be strained within the same limits by the machine under consideration in order to compare the gauges, and prove accurately what the friction is. The same can then be shown engineers when and as often as they may desire. Meanwhile I wish that Mr. A. H. Emery had a contract for fifty of his very nice machines. I also wish he would turn his mechanical talents to building some of the rotating-ram machines according to my idea, to be used for the very purpose for which the machine under consideration has been designed.

Mr. A. H. Emery.—I would like to say in that connection, that the design of the six hundred-ton machine I have made is a cheaper machine to build than the other would be, designed equally good.

Mr. A. M. Wellington, M. Am. Soc. C. E.—I would like to ask Mr. Charles E. Emery one question in connection with this ram that he speaks of. If it revolves one hundred times the speed it moves lengthwise, does it not almost completely eliminate the friction?

Mr. Charles E. Emery.—It would. The friction would be as the velocities. If the transverse movement of the surface of the arm were 99 times the movement of the translation, the pressure would represent the strain on the specimens within 1 per cent.

APPENDIX.

By THEODORE COOPER, M. Am. Soc. C. E.

Tests of Friction of Hydraulic Cupped-Leather Packing.

Made at St. Louis.

The accompanying tests were made at St. Louis in the early stages of the construction of the St. Louis Bridge, to determine the friction of the packing in the hydraulic testing machine to be used for testing the materials of construction. The apparatus was devised by and the tests made under the supervision of Henry Flad, Past President Am. Soc. C. E.

Plate IV shows the details of the apparatus for making the tests. The casting representing the cylinder contains two leather packings. These were varied in size, as shown in full upon the drawing, from $1\frac{3}{8}$ inches to $\frac{1}{8}$ inch in width. The friction was measured by actual weights imposed. The pressure (hydraulic) was obtained by a hand pump, and was measured by means of a mercurial column 46 feet high. Two sizes of cylinders were used, one 9 and the other 6 inches in diameter.

Column one of the table gives the pressure in feet of mercury; column two the pressure per square inch in the cylinders (this has been corrected for the errors in gauge due to the falling of the surface of the mercury in the reservoir); column three gives the total pressure upon the area of a cylinder 9 inches in diameter; column twenty-three gives the total pressure upon the area of a cylinder 6 inches in diameter. The columns headed W give the actual weights necessary to move the cylinders against the friction of *the two packings* under the several pressures.

As two or more tests were made on each packing, it will be noticed how the friction reduced for the last tests in comparison to the first test, showing thus the difference between a new packing and the same after a moderate use. The difference due to the size of the packing is also shown by the several columns headed $1\frac{3}{8}$, $1\frac{1}{8}$, etc., and which give the width of the bearing side of the packing.

Columns twenty, twenty-one and twenty-two, headed percentages for one packing, give the relation between the friction reduced to one packing (the apparatus contains two packings) and the total pressure upon a ram of the size experimented upon. The first of these columns gives the maximum friction in percentage; the second, the average of all the tests; and the third, the maximum friction. So that the range can be seen at a glance.

It will be noticed that the relative friction reduces with the increase of the pressure per square inch for both sized rams. Also that the relative friction of the rams is less as the diameter of the rams is increased.

The assertion made by the writer in the discussion upon Mr. Macdonald's paper, that for such rams and pressures as were usually employed in testing machines the friction would not be one per cent. of the full load, and probably less than one-half of one per cent., is supported by these tests. These conclusions are confirmed by a comparison with Hicks' experiments. (See page 32.)

In addition to the tests on the friction of hydraulic packing with the longitudinal motion of the plungers, tests were made at St. Louis upon the friction of rotating the plungers. They were made by inserting a lever in the eye-bolts shown on the drawing of the apparatus, and weighing at a fixed leverage the amount of the resistance to rotation. The following table gives an abstract of these, reduced to the circumference of the plunger:

AVERAGE FOR ONE PACKING.

Size of the Packing.	9-inch Ram.		6-inch Ram.	
	Pressure = 0.	Pressure = 273 pounds.	Pressure = 0.	Pressure = 273 pounds.
Inches	Pounds.	Pounds.	Pounds.	Pounds.
$1\frac{3}{8}$	23	300	8	140
$1\frac{1}{8}$	61	250	12	164
$\frac{7}{8}$	50	250	8	114
$\frac{5}{8}$	6	200	8	118
$\frac{3}{8}$	6	184	6	104
$\frac{1}{8}$	6	147	....	....

ABSTRACT OF HICKS' EXPERIMENTS.

Friction of a Plunger one-half inch diameter, Leather Washer Packing.

Pressure per square inch.	Friction in Percentage of Total Pressure on Plunger.		
	New and stiff leather.	Leather used before.	Second leather.
Pounds.	Per cent.	Per cent.	Per cent.
10	26	18	18
20	12.5	8.5	13
30	12	7.6	10
40	10	6.5	10
50	9.6	5.4	9.6
60	9	4.9	9.0
80	5.6	4.1	7.7
100	5	3.8	7.4
120	4.3	3.3	6.3
160	4.7	3.0	5.6
200	3.3	3.3	4.8
240	4.1	3.4	4.1

Friction of a Plunger four inches in diameter.

	Leather, new and stiff, and sparingly lubricated.	Leather, well worn, and well lubricated.
Pounds.	Per cent.	Per cent.
188	4.6	2.13
446	2	1.25
673 to 5 865	1.55 to 1.07	0.95 to 0.63

Friction of a Plunger eight inches in diameter.

443 to 1 882	0.46	0.42
6 375	0.50	0.33

PACKING.

S						
S	ES.		1⅛ INCHES.			
Gauge, feet.			W =			
0	10	10	22	20	20	20
2	23	21	40	35	46	44
4	32	27	53	55	64	62
6	38	36	70	75	80	77
8	44	41	90	83	88	85
10	51	47	93	91	88	98
12	58	52	101	92	104	103
14	66	58	110	96	116	107
16	65	63	119	102	124	112
18	70	67	133	107	124	116
20	76	74	144	117	124	116
22	83	78	144	122	126	121
24	85	85	148	127	131	132
26	91	89	148	135	137	142
28	99	95	148	135	150	149
30	107	102.	153	153	150	149
32	107	106	158	153	153	149
34	115	108	172	155	159	151
36	123	115	174	163	163	167
38	129	120	176	166	168	167
40	132	123	180	180	175	177
42	137	131	188	180	185	175
44	141	137	198	184	188	182
46	146	142	198	188	193	193
0	10	...	...	...	...	...

rvoir.

e second column.

TESTS ON FRICTION OF HYDRAULIC CUPPED LEATHER PACKING.

Made at St. Louis.

Size of ram.			9-inch ram.																		
Size of packing.			$1\frac{3}{8}$ inches.			$1\frac{1}{8}$ inches.			$\frac{7}{8}$ inch.		$\frac{5}{8}$ inch.		$\frac{3}{8}$ inch.			$\frac{1}{8}$ inch.					
			Weight W to move ram.																Percentage for one packing.		
Gauge, feet.	Pressure,* lbs. per square inch.	Total pressure on area of ram	$W=$			$W=$			$W=$		$W=$		$W=$			$W=$			Max.	Aver.	Min.
0	0	0	24	24	...	100	85	86	120	102	26	24	28	28	24	24	20	24	....	....	....
2	11.87	775	54	50	...	122	102	103	130	116	40	32	58	36	46	50	44	39	8.	$4\frac{1}{2}$	2.
4	23.74	1 510	84	70	62	131	109	113	140	128	55	59	66	66	66	68	58	58	4.3	2.7	1.8
6	35.61	2 265	110	25	70	144	123	126	152	138	63	75	87	80	76	82	76	76	3.35	2.0	1.4
8	47.48	3 020	113	110	88	164	134	134	164	148	86	92	97	92	86	109	88	88	2.7	1.85	1.4
10	59.35	3 775	123	120	103	180	147	142	176	158	94	104	110	102	98	124	100	107	2.4	1.64	1.24
12	71.22	4 530	133	130	119	183	158	148	188	175	104	110	122	112	108	136	116	122	2.0	1.50	1.15
14	83.09	5 285	154	140	131	193	168	156	200	185	116	122	137	122	120	146	130	132	1.9	1.40	1.1
16	94.96	6 040	170	152	139	203	180	166	208	194	127	132	143	134	131	158	142	147	1.7	1.30	1.05
18	106.83	6 795	174	168	150	213	189	178	220	203	139	144	153	144	143	168	162	160	1.6	1.24	1.02
20	118.70	7 550	184	178	164	224	198	186	230	211	159	154	167	153	153	180	174	172	1.52	1.20	1.00
22	130.57	8 305	197	188	178	234	208	194	240	220	170	166	176	162	161	194	186	190	1.44	1.15	0.97
24	142.44	9 060	201	200	192	244	218	202	250	230	176	176	186	172	171	203	196	200	1.38	1.11	0.94
26	154.31	9 815	212	215	206	255	228	214	258	241	186	186	196	180	179	213	208	212	1.30	1.08	0.91
28	166.18	10 570	220	225	220	263	238	226	268	249	196	192	206	191	187	223	218	222	1.27	1.05	0.90
30	178.05	11 325	230	239	234	273	248	236	278	257	210	205	217	201	198	233	228	230	1.22	1.02	0.87
32	189.92	12 080	242	253	246	283	260	250	286	268	220	215	227	210	208	343	242	244	1.18	1.00	0.86
34	201.79	12 835	252	267	260	293	272	254	294	280	228	225	238	218	216	254	258	260	1.14	1.00	0.84
36	213.66	13 590	264	283	260	303	282	264	302	290	238	237	248	228	226	264	268	268	1.15	0.97	0.83
38	225.53	14 345	280	299	274	308	292	271	312	300	246	244	257	238	236	278	278	268	1.08	0.96	0.82
40	237.40	15 160	308	305	292	315	304	279	322	310	256	254	266	248	246	288	288	268	1.07	0.94	0.81
42	249.27	15 855	336	326	306	327	314	289	332	318	266	264	276	258	256	307	299	278	1.06	0.94	0.80
44	261.14	16 610	336	326	317	333	320	299	340	326	276	276	286	264	256	315	311	285	1.02	0.91	0.79
46	273.00	17 368	336	326	329.	338	328	309	348	334	286	284	286	272	266	315	323	289	1.00	0.90	0.70
0	......	......	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	....	....	....

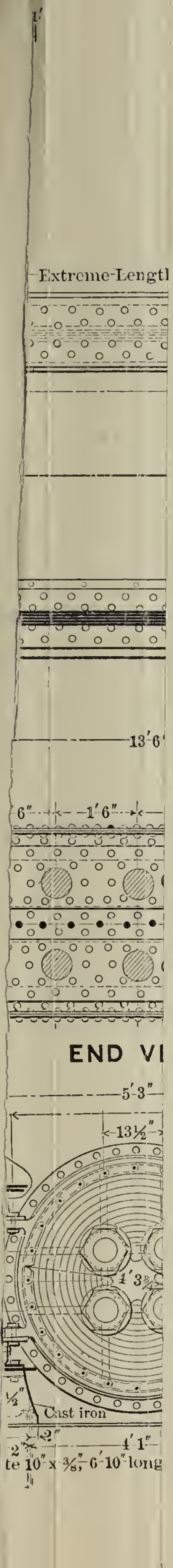
Size of ram.	6-inch ram.																						
Size of packing.		$1\frac{3}{8}$ inches.				$1\frac{1}{8}$ inches.				$\frac{7}{8}$ inch.				$\frac{5}{8}$ inch.				$\frac{3}{8}$ inch.					
		Weight W to move ram.																				Percentage for one packing.		
Gauge, feet.	Total pressure on area of ram.	$W=$				$W=$				$W=$				W =				$W=$				Max.	Aver.	Min.
0	....	25	10	10	10	22	20	20	20	22	15	15	15	17	15	15	...	10	9	9	9	....	....	....
2	335	51	20	23	21	40	35	46	44	42	30	21	23	29	28	29	...	25	19	22	19	7.6	4.45	2.83
4	671	66	30	32	27	53	55	64	62	58	40	27	29	39	34	37	...	36	23	28	22	4.92	2.99	1.64
6	1 007	71	39	38	36	70	75	80	77	70	47	33	33	47	38	45	...	46	33	38	32	3.97	2.48	1.59
8	1 352	81	43	44	41	90	83	88	85	76	57	40	39	58	43	51	...	57	43	44	40	3.33	2.15	1.44
10	1 687	91	49	51	47	93	91	88	98	81	67	47	47	64	49	57	...	64	49	51	46	2.91	1.92	1.36
12	2 014	105	56	58	52	101	92	104	103	95	67	52	53	70	57	63	...	73	57	57	54	2.61	1.79	1.29
14	2 350	65	62	66	58	110	96	116	107	100	75	46	53	70	63	70	...	78	63	63	60	2.47	1.59	0.98
16	2 685	71	71	65	63	119	102	124	112	100	80	51	59	75	71	79	...	82	69	69	68	2.31	1.50	0.96
18	3 022	81	77	70	67	133	107	124	116	104	90	65	68	81	79	86	...	82	74	73	74	2.20	1.44	1.07
20	3 358	91	81	76	74	144	117	124	116	115	94	71	74	87	85	89	83	86	78	80	82	2.14	1.37	1.06
22	3 693	91	85	83	78	144	122	126	121	117	94	78	78	97	91	93	87	92	82	86	88	1.95	1.31	1.06
24	4 029	97	93	85	85	148	127	131	132	121	100	82	92	101	97	99	95	97	86	91	94	1.83	1.27	1.02
26	4 364	108	98	91	89	148	135	137	142	121	110	88	98	111	113	107	101	101	90	87	101	1.69	1.24	1.00
28	4 700	116	100	99	95	148	135	150	149	129	110	94	98	117	119	112	105	101	95	101	106	1.59	1.21	1.00
30	5 035	122	109	107	102.	153	153	150	149	134	131	98	104	120	125	117	110	105	100	107	114	1.52	1.19	0.97
32	5 371	130	117	107	106	158	153	153	149	139	134	102	108	129	129	124	119	105	105	113	116	1.47	1.16	0.95
34	5 706	134	124	115	108	172	155	159	151	148	135	108	112	137	133	128	124	110	110	121	121	1.51	1.14	0.94
36	6 042	141	132	123	115	174	163	163	167	152	143	117	117	143	137	132	128	115	115	125	125	1.44	1.13	0.95
38	6 377	149	137	129	120	176	166	168	167	155	147	125	123	153	141	136	134	119	119	132	131	1.38	1.11	0.93
40	6 713	149	141	132	123	180	180	175	177	159	157	127	127	157	145	140	140	123	123	137	137	1.34	1.09	0.92
42	7 048	156	150	137	131	188	180	185	175	164	161	133	133	161	151	146	146	128	128	141	145	1.33	1.08	0.91
44	7 384	163	156	141	137	198	184	188	182	169	165	137	137	165	155	152	152	133	133	151	151	1.34	1.07	0.90
46	7 719	170	166	146	142	198	188	193	193	175	169	141	141	167	167	158.	158	138	138	164	164	1.28	1.06	0.89
0	....	...	10	10	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	....	....	....

* Corrected for error of mercurial gauge due to the depression of the mercury in the reservoir.

Indicates that the pressure was let back and readings taken on the falling pressure in the second column.

. TES

R SQUAR



Extreme-Lengtl
13'6"
6" 1'6"
END VI
5'3"
13½"
4'3½
Cast iron
½"
2"
2"
4'1"
te 10" x ⅜" 6'10" long

GENERAL PLAN OF 1,200,000 LBS. TESTING MACHINE.

MAXIMUM WATER-PRESSURE 600 LBS. PER SQUARE INCH.

PLATE I.
TRANS. AM. SOC. CIV. ENGR'S.
VOL. XVI, NO. 349.
MACDONALD ON
TESTING MACHINE

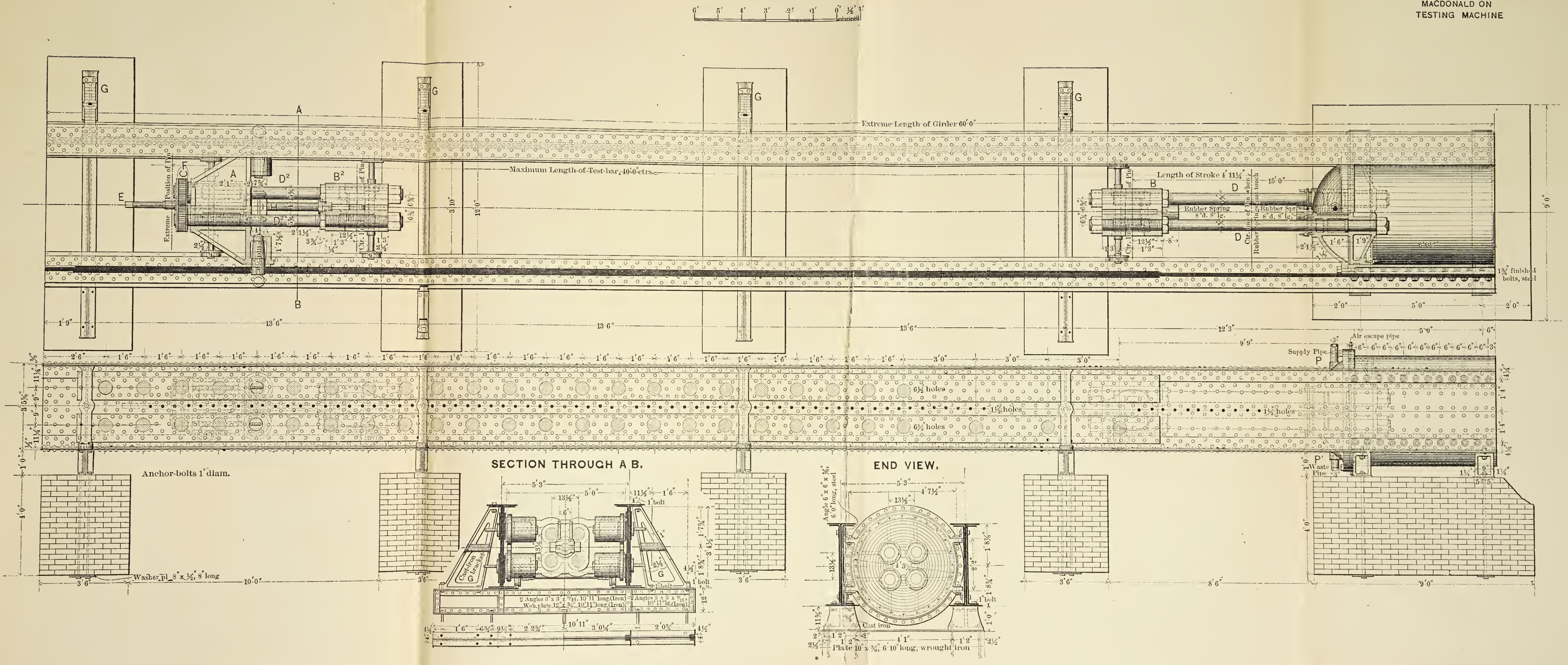
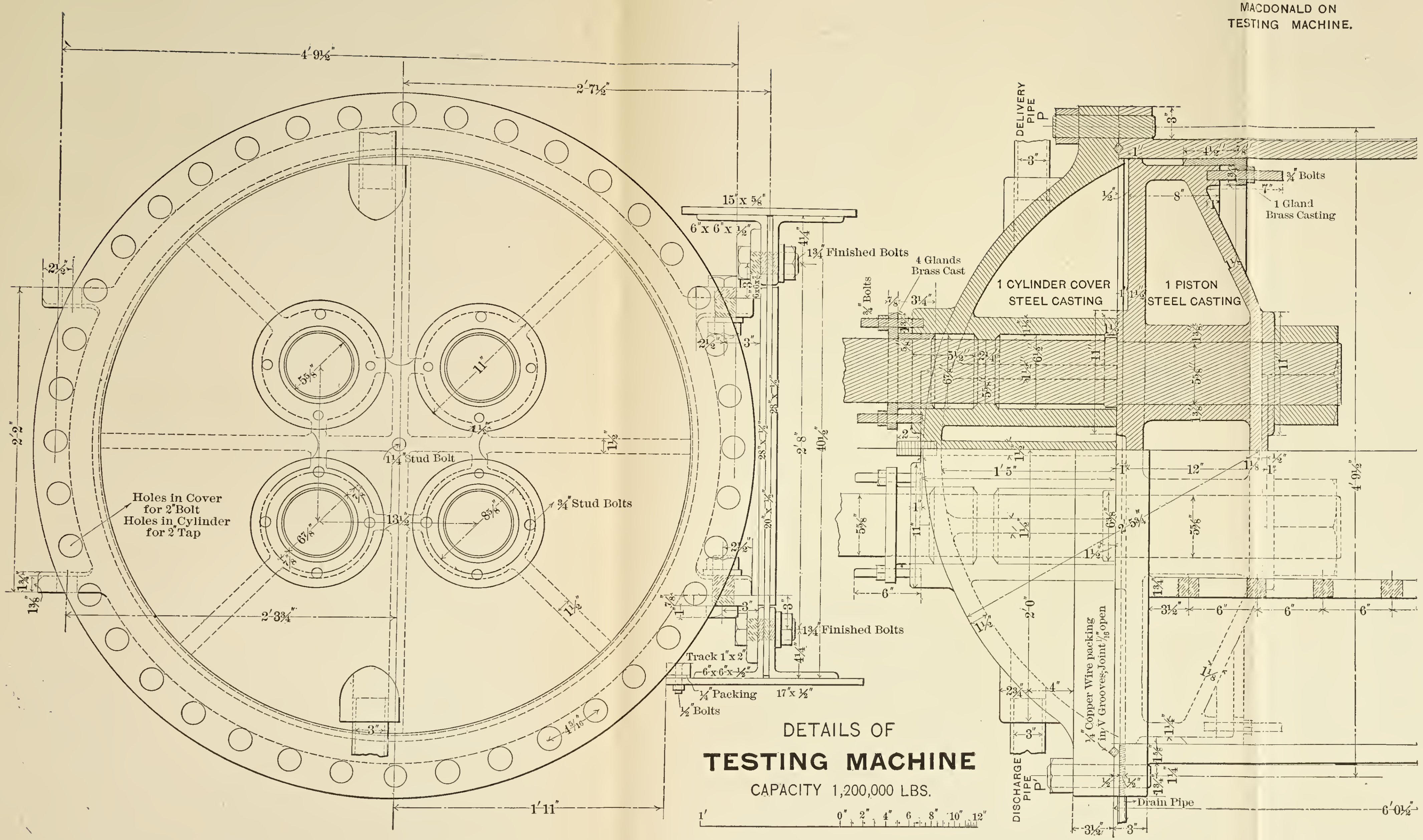


PLATE II.
TRANS. AM. SOC. CIV. ENGR'S.
VOL. XVI, NO. 349.
MACDONALD ON
TESTING MACHINE.

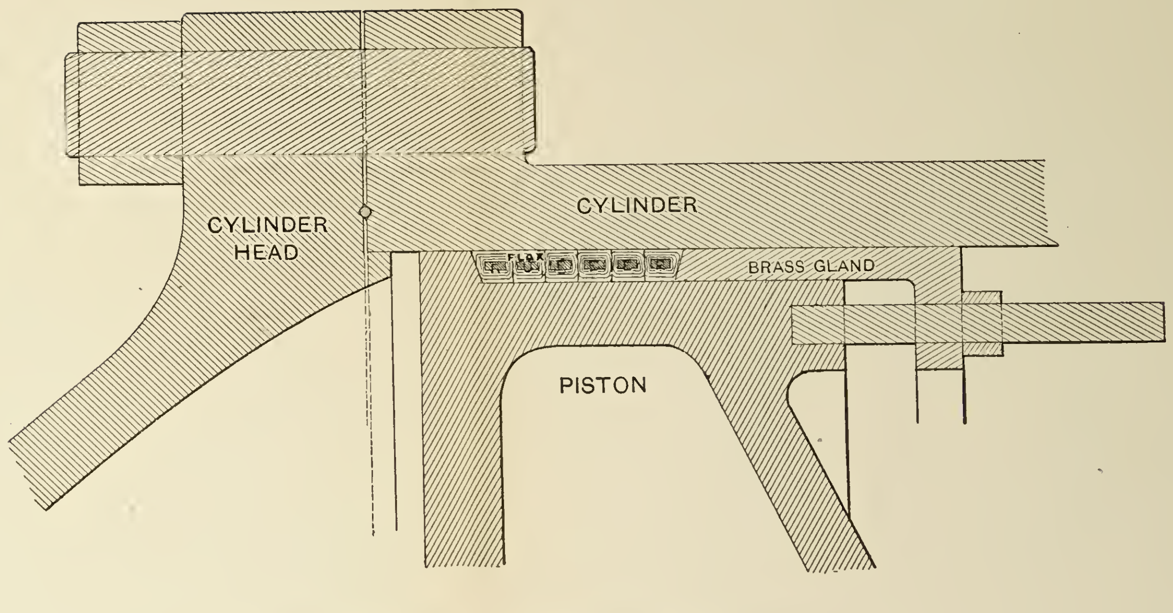


DETAILS OF

TESTING MACHINE

CAPACITY 1,200,000 LBS.

PLATE III.
TRANS. AM. SOC. CIV. ENGR'S.
VOL. XVI, NO. 349.
MACDONALD ON
TESTING MACHINE



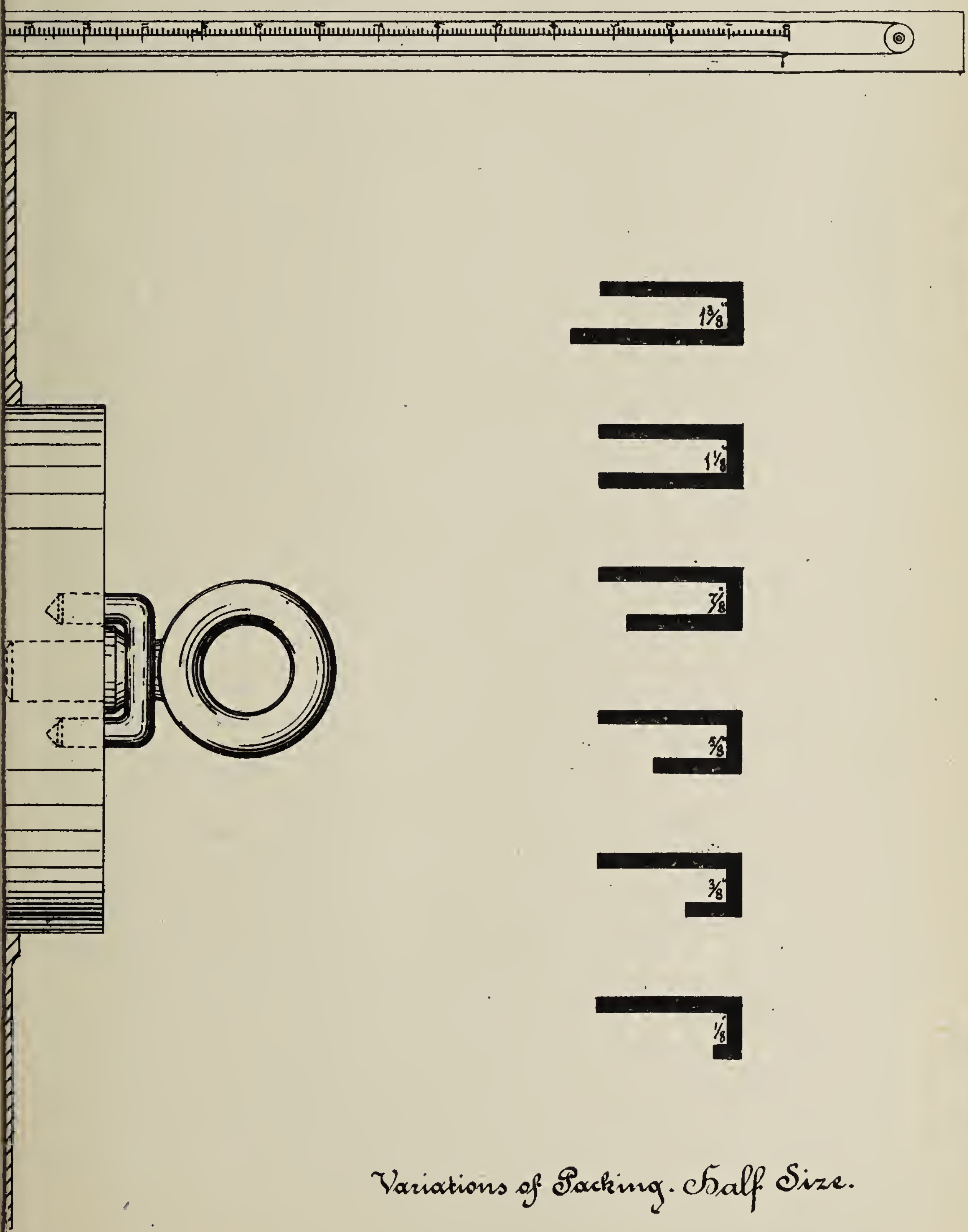
1,200,000 LBS. TESTING MACHINE

SECTION SHOWING PACKING OF PISTON.

s Bridge Company

ction of Hydraulic Packing.

PLATE IV.
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XVI. NO. 349
COOPER ON
FRICTION OF PACKING

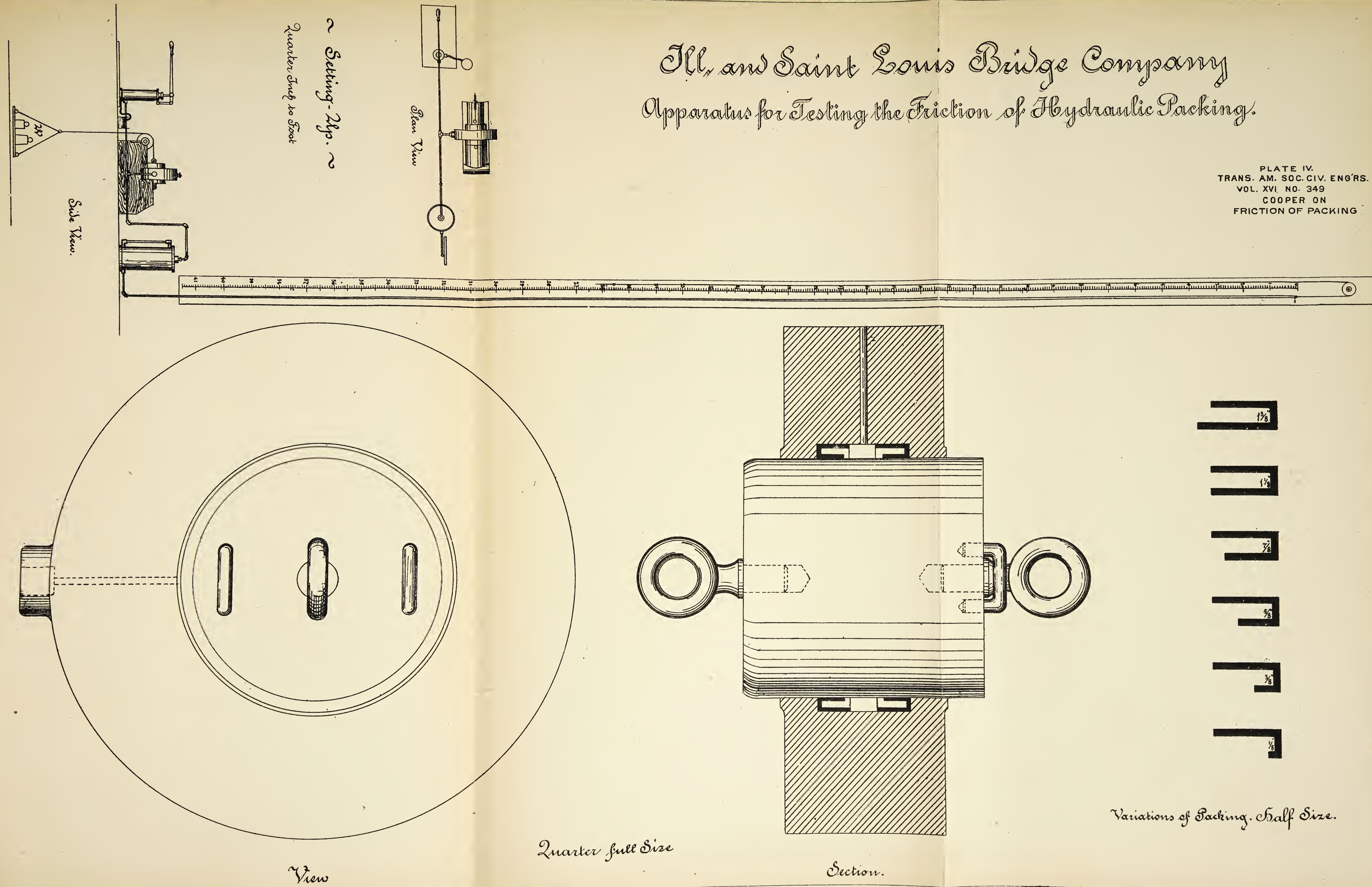


Variations of Packing. Half Size.

Ill. and Saint Louis Bridge Company

Apparatus for Testing the Friction of Hydraulic Packing.

PLATE IV.
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XVI. NO. 349
COOPER ON
FRICTION OF PACKING



AMERICAN SOCIETY OF CIVIL ENGINEERS.

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TRANSACTIONS.

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350.

(Vol. XVI.—February, 1887.)

SPECIFICATIONS FOR THE STRENGTH OF IRON BRIDGES.*

By J. A. L. Waddell, M. Am. Soc. C. E.

Read February 2d, 1887.

WITH DISCUSSION.

One of the most difficult and unsatisfactory tasks which come within the province of the civil engineer, is the preparation of specifications for bridges.

Bridge designing, when done scientifically, is an extremely complicated matter, and there are many circumstances connected therewith which are dependent upon experiment, experience, and even guess-work.

On this account there is, as might be expected, a great variety of opinions concerning many points among those engineers who have made a specialty of bridge-work. This fact is made evident by comparing the general specifications of several of the leading specialists. Not only do they differ essentially on many important matters, but some neglect entirely considerations which in others are considered essential. The difficulty under which all the writers have been laboring is that each has

* See paper on Specifications for Strength of Iron Bridges, by Joseph M. Wilson, M. Am. Soc. C. E., Transactions of the Society, Vol. XV, No. 335, page 389, June, 1886.

been working almost entirely alone, notwithstanding the fact that the finished specifications of the others are at his disposal.

Now, as any one change in a set of specifications will involve a number of others before everything can be brought into harmony, it is clear that, unless there be good opportunities for discussion by the various writers, a wide divergence must result. In submitting his specifications to the American Society of Civil Engineers for discussion, Mr. Wilson has done a good deed for the profession, and it is to be hoped that the Members of the Society will take advantage of the opportunity thus freely offered. If the subject of general specifications for bridges were thoroughly discussed in every particular by every Member of the Society who has made bridges his specialty, it would be possible to prepare a new set of specifications which would embody all the good points of all previous ones, and be as perfect as the present state of our knowledge will permit. If this desideratum were attained, the status of American bridge-building would be so improved as to stand out in even more vivid contrast than it does to-day with the crude and antiquated methods which are still employed by British engineers; and the result would be that for some time to come the majority of the most important structures required thoughout the world would be manufactured in the United States. Bridge-building in this country has become such an immense business that the home demand for structural iron-work is becoming less than the supply, so in order to keep their shops full, the American manufacturers will soon have to turn their attention to competition in foreign countries.

Australia, India, Japan, and even China, are all good fields for this enterprise, and it is not impracticable to compete in England.

The principal objection which can be raised against Mr. Wilson's specifications is their inconvenience, necessitating the use of a rather complicated formula, which involves the determination of extreme stresses in order to find the proper intensity of working stress. Would it not be much better to give, say by diagrams, the intensities of working stresses for each kind of bridge member for all practical cases?

"General specifications" are usually made to cover too extreme cases. Would it not be better to make them cover ordinary cases only, leaving extraordinary cases to be dealt with by special specifications?

For instance, cantilevers, very long spans and braced piers cannot be conveniently designed by specifications which were prepared especially for ordinary spans. In the first two cases it may be advisable to use material of more than ordinarily high ultimate strength and elastic limit in order to reduce the dead-load; and in the last case the conditions affecting the design are or should be very different from those affecting that of the spans. Simplicity, rather than an elaborate system built up on deeply scientific methods, should be one's object in preparing specifications. As an example let us take Mr. Wilson's specifications for beam-hangers, viz.: "Floor-beam hangers must have an additional section of twenty-five per cent. above that given by the before mentioned limiting stresses," which limiting stresses are determined by Launhardt's complicated formula. Would it not be much simpler and more satisfactory to say that "floor-beam hangers shall be proportioned for a working stress of ——— tons per square inch," covering thereby not only the effect of extremes of stress, but also that of impact?

As another example, let us take Mr. Wilson's method of proportioning top flanges of girders. It reads as follows: "In all cases for compressed flanges of beams or girders (subject to transverse stress), the permissible working stress in such flanges shall be computed by Rankine's formula:

$$c = \frac{a}{1 + \dfrac{l^2}{5\,000\, w^2}}$$

Where a = permissible stress previously found.

c = allowable working stress per square inch.

l = unsupported length in inches.

w = width in inches.

In no case shall a stress greater than that for a length equal to twelve times the width be used."

Let us see what are the various steps to be taken in determining this value of c, which most engineers assume to be constant and equal to four tons. First we must find the ratio of dead and total loads for the girder, and determine a by Launhardt's formula, then assuming w, substitute in the equation, and find the value of c. Of course the amount of work involved may be reduced greatly by the use of two diagrams. But is all this refinement really necessary? When we consider that the percentage allowed for impact is the result of mere guesswork; that the web is assumed as not helping to resist bending when it really does so assist; that the effective depth of the beam is inexact; that the rivet-holes may or may not be counted in the effective sectional area according to the opinion of the designer; and that the "secondary strains," as pointed out by Bender in his "Principles of Economy in the Design of Metallic Structures," are decidedly great, we may conclude that the last question may be answered in the negative. There is hardly a single particular in plate-girder designing where theory will apply—the web thickness cannot be analytically determined; the proper spacing of stiffening angles is a mere matter of opinion; the rivet-spacing in the flanges, when the effect of concentrated loads is considered, is best arranged by making it uniform from end to end of span, and the effect of the web in stiffening the upper flange is an unknown quantity.

Is it not much better, then, to proportion beams and girders by a few empirical rules, which are the result of experience and good judgment, rather than by a deeply scientific method which is based upon assumptions that are known to be, if not absolutely incorrect, at least very loose approximations?

Again, the formulas of Launhardt and Weyrauch were established for tension only. Is it then advisable to adopt their complicated method of determining the intensities of working compressive stresses until it be proven that their deductions apply to compression as well as tension? Mr. Benjamin Baker appears to think that they do not so apply.

Again, in pin-proportioning, can any designer spare the time to find the value of a, multiply it by 1.5, and find the resisting bending movement of one or more pins for the intensity thus determined? The amount of material saved by using this instead of the ordinary method of employing tables calculated for constant intensities of working bending stresses, would not be worth as much as the time that the computer would lose in making such elaborate calculations.

When specifying three classes of engine loading for the same bridge, it would be well to state for each kind of member the cases in which each loading will produce the maximum effect—this would effect a great saving of time for computers.

In calculating the stresses in trusses, if one employ the latest and best method, as given in the third edition of Burr's "Stresses in Bridge and Roof Trusses," it would be incorrect to neglect the weight on the first pair of wheels.

Mr. Wilson, in proportioning top chords, evidently figures each panel length as if hinged at the ends. Although this is not in conformity with general practice, it appears to be correct. If one figures them as hinged, it will be well to make them pin-connected, and avoid entirely field-riveting for top chords and batter braces.

The allowance of five tons for initial tension on each adjustable rod does not appear to be either scientific or practically correct. Surely it takes less initial tension to properly tighten a one-inch than it does to properly tighten a two-inch rod. The allowance which I have given several times in books and papers, is contained in the folowing table:

Diameter of Rod.	In Tension.	Diameter of Rod.	In Tension.
Inches.	Tons.	Inches.	Tons.
1	1.00	1¾	2.50
1⅛	1.25	1⅞	2.75
1¼	1.50	2	3.00
1⅜	1.75	2⅛	3.25
1½	2.00	2¼	3.50
1⅝	2.25	2⅜	3.75

These amounts are not certified to as correct, being the result of mere guesswork; but it is submitted that the method is more rational than that which allows five tons for any rod, irrespective of its diameter. Perhaps the allowance should be a certain amount per square inch, in which case the quantities in my table do not increase quite fast enough. A series of experiments by several experienced bridge erectors upon this matter would be very useful. The tensions measured by a dynamometer might be recorded by an assistant, without communicating their amounts to the men who do the adjusting.

It would appear that Mr. Wilson condemns the use of iron track stringers having no plate on the upper flange. Perhaps this is because he places the stringers more than five feet apart, center to center. If the stringers be placed directly under the rails, I see no reason for insisting on the use of a top plate. Concerning the proper position for stringers, I will have more to say in a subsequent communication to the Society.

I should like to ask Mr. Wilson why he allows the use of continuous spans in deck and not in through bridges. The objections to this arrangement which hold in one case, hold equally well in the other.

Is not a space of ten inches between track ties altogether too great to permit the passage of a derailed car?

In my opinion wooden outer-guard rails are not nearly so efficient as inner-guard rails of angle iron.

Concerning the best arrangement of ties and guard rails, I would like to call the attention of the Members of the Society to the floor system proposed in my "System of Iron Railroad Bridges for Japan," and invite their criticism thereon.

Mr. Wilson, in common with many late writers of bridge specifications, does not provide for a higher intensity of wind pressure upon empty than upon loaded bridges. It is to be noticed, though, that he specifies, in proportioning iron piers, a pressure of fifty pounds per square foot on the unloaded structure. If fifty pounds per square foot will buckle the windward bottom chord (as it will in most of the existing single-track bridges) it is useless to proportion the piers for this pressure.

In conclusion, I wish to observe that, in thus criticising Mr. Wilson's specifications, I do not intend to depreciate their value. They are, perhaps, as good as any others that have as yet been writen. The fact is that all existing specifications for bridges are far from perfect in many respects; and, as before stated, the only way to make them approach perfection is to submit them to thorough detailed discussion.

DISCUSSION.

Joseph M. Wilson, M. Am. Soc. C. E.—I think that the objection which Mr. Waddell raises as to the inconvenience of the application of my specifications is more apparent than real. The work is very much simplified by the use of tables, which we employ to a large extent.

One point in reference to the specification is that it is adapted to cover all general cases, cantilevers, long spans and braced piers as well as short spans; also work on buildings where the variations in live and dead loads are usually much greater than in bridges, and if a higher grade of material is desired, the necessary changes in the constants can readily be made. It is always feasible to make modifications to suit special requirements, and it seems to me that this is the only proper way to treat the subject. One should work from the general case to the special, not from special to the general. It is true that some details, such as floor-beam hangers, might be more simply treated as suggested by Mr. Waddell; but after all, when the stress is obtained by formula for a certain case, the results can easily be used thereafter as a constant quantity.

Concerning the question of the labor of proportioning the top flanges of girders, the value of a is easily obtained from tables by merely dividing the minimum by the maximum, and this allows of any relative variations in live and dead loads from that of a girder having all dead load, as in certain cases in buildings, to that of nearly all live load.

For most cases of plate-girder work, the lateral bracing being placed at such distances apart as not to be more than twelve times the width of a plate, c is obtained by taking a fixed percentage of a for that value of $\frac{l}{W}$, as shown by tables. The formula is more particularly intended to apply for cases in which the value of $\frac{l}{W}$ much exceeds twelve, and where the ordinary column formula will not apply on account of the flange obtaining assistance from the web.

If Mr. Waddell's method of reasoning concerning the proportioning of beams and girders were carried out, it would tend very much toward reducing calculations to a "rule of thumb." The question is: Is it better to work by the nearest approximation to correct rules that one can obtain, or to work to no rules at all? Because these principles may not be absolutely exact, it does not follow that they are not more correct than mere guesswork.

That they are particularly intricate of application I cannot admit. Things which appear intricate by observation, very often are found in practice to be very simple, especially with the use of a few general tables. I argue that it is better to work to a system throughout, and if it is found inaccurate to modify it, rather than throw all system away. No one is more willing to modify than I am, when satisfied that there is something better.

Concerning the proportioning of pins, I have tables that give the bending moments for all proportions of pins, and all that it is necessary to do is to select the proper figures and multiply by $1\frac{1}{2}\ a$. This surely does not require a very large amount of time.

As to the various classes of engines, these specifications were framed for a particular road with certain kinds of engines, being originally made for only two classes, until the "M" engine came in as a later type, being found very heavy for cross-girders and similar parts. We do not advocate the use of these engines for all roads, and in fact modifications would be advantageous for this road now; but they were adopted some years ago under the supposition that they were a sufficient advance over the actual service to cover some years of improvement. Our labors with them have been very much simplified by having their results tabulated. I am decidedly in favor of generalizing by the use of an assumed type of engine, or of loading, that will cover all cases in practice, even at the risk of increasing somewhat the weight and cost of the bridges. My fault in preparing this paper perhaps was that I did not start out to present a new specification of exactly what would be best in every respect for general use to-day, but I gave truthfully and exactly what was at that time standard for a particular railroad. It is difficult sometimes to change a standard at short notice, and what I wanted to do was to show the practice of several years, the results of which are visible in bridges which now exist. Were I to rewrite these specifications, I might improve them in a few particulars, not only from the consideration of just criticisms on my paper, but from later experience of my own, although I am satisfied with their main features thus far, and would not make any material changes.

The method of calculation adopted is by the use of panel loads, and the omission of the front wheels applies entirely to that. It is not in any way essential to the specification, but if the panel-load system is used, it is on the side of safety and simplifies the calculations.

The question of hinged ends in top chords has in the specification a saving clause, to which I would direct Mr. Waddell's attention. Cases occur when from the weight of the chord itself in adjacent panels, or something similar, the chord is incapable of bending in opposite directions on opposite sides of the point of support. In such cases I would not consider it as hinged.

As to initial tension, I would like to ask Mr. Waddell whether he proposes to increase the number of men on the lever for screwing up when he increases the size of the rod. The matter is more a function of the man than of the size of rod. The desire is not to make the small rods too small for practical use, and the rule gives an allowance for screwing up on the small rods for safety. It is more needed on the small rods than on the large ones. We know that all rods are screwed up beyond simple tightness, and that if they be small they are more easily over-strained than if large. We have no idea of putting on an actual initial strain per square inch over all rods in proportion to their size.

Concerning the question of girders with no upper flange plate, I would refer to my previous reply (see Transactions, page 487, Vol. XV).

Mr. Waddell misunderstands my limitation in reference to continuous girders. It is not a question of deck and through bridges. Continuous girders are allowed in drawbridges and also in the upper chords of deck bridges as a girder carrying a floor between panel points. The permission does not refer to the whole truss as a truss.

Ten inches between track-ties is not too great to permit the passage of a derailed car, as I can testify by numerous instances; in fact I have seen cases where a train has crossed over the whole length of a bridge on the old-fashioned 8 by 14-inch white pine floor beams laid 2½ feet apart, center to center, and having longitudinal stringers under the rails, notched on. Very decided marks were left, it is true, by the wheels, but the cars got across all the same. A great point is to hold the floor beams or ties in place and prevent them from piling up, and the exterior wooden notched guard-rails do this, as well as acting to keep the train on the bridge. Inside iron guards can be used in special cases, such as on elevated roads, for exclusively passenger traffic; but if a brake block should fall between such a guard and the rail, a chance not by any means rare, particularly where freight trains are run, it may produce very serious results. An inner iron guard rail as ordinarily arranged will not prevent the ties from piling up, thus weakening the floor system and perhaps letting the train through.

The question of stability of iron piers under wind is dependent a great deal on the load on the bridge. When the bridge is unloaded the tendency to overturn is greater and the rule of the specification is intended to give greater stability to a lightly loaded pier.

I am glad to see discussions on the paper, and desire to profit by any experience that can be brought to bear on the subject, such experience, however, being always open to criticism as well as the original question.

AMERICAN SOCIETY OF CIVIL ENGINEERS.

INSTITUTED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

351.

(Vol. XVI.—February, 1887.)

VIBRATION OF BRIDGES.

By S. W. ROBINSON, M. Am. Soc. C. E.

PRESENTED JUNE 26TH, 1885.

The object of this communication is to present to the Members of this Society a mass of facts and figures obtained in connection with the application of a so-called bridge indicator to railway bridges, and to mention some of the conclusions toward which those figures point.

EXTENT AND RESULTS OF THIS INDICATOR WORK.

The indicator was applied to thirteen different bridges of four different railways, resulting in one hundred and ninety-three indicator diagrams. Most of the diagrams were obtained from bridges of the New York, Pennsylvania and Ohio Railroad, though the first were obtained from a bridge on the Pan Handle Railway, and others later from several branches of the Pennsylvania system and the Baltimore and Ohio. The

NOTE.—This paper is prepared from advance matter from the report of 1884 by the author to the Commissioner of Railroads of Ohio on an investigation of the cumulative vibration of bridges made under sanction of Hon. H. Sabine, Commissioner.

aim in procuring an abundance of figures on the vibratory movements of bridges, was, if possible, to detect any extraordinary movements which might occur but rarely in the lifetime of a bridge, particularly cumulative vibration. In this the indicator itself surprised us with a discovery of vibratory movements due to a cause as startling as it was unanticipated, the cause being a combination of circumstances, including speed of train, car length, panel length, time of vibration of loaded bridge, rigidity of bridge flooring, etc. Besides this cause of cumulative vibration, the anticipated one due to unbalanced locomotive drivers found confirmation.

In this work no claim is made of discovery of new laws; on the other hand everything, as far as yet observed and studied, is traceable to previously well known and comparatively simple laws.

Need of an Indicator.

That a bridge is agitated as a train passes over it at speed, no one questions; but the precise character of the bridge movements during such agitation is a matter which cannot be determined by mere casual observation, because too rapid and complex. Some instrument which shall analyze the movements, separating them into horizontal and vertical components, and record them for subsequent examination, must be conceded to be one good means for determining those movements. This the bridge indicator above mentioned has done, copies of some diagrams from which are given in Plates V, VI, VII.

Previous Instruments and Use.

Movements of bridges under moving load have previously been recorded. The earliest instance that has come to my knowledge was that of J. T. Fanning, M. Am. Soc. C. E., who, in 1875, obtained diagrams by aid of a station near midspan, to which, and to the bridge, a pencil and a card were so attached that the motions of the bridge were marked down on the card, giving a diagram of the actual motions of the bridge. I understand that the taking of such diagrams has been the frequent practice of this engineer. In my tours of inspection of Ohio railways, I have found several instances of the determination of the deflection of bridges by means of a pencil attached to the bridge and a rod set on the ground or bed below, upon which the pencil could mark the deflection. In September, 1881, I procured diagrams similarly, as previously done by Mr. Fanning, but without knowledge of his experiments, copies of which diagrams were published in the Ohio Railway Report for 1881 by H. Sabine, Commissioner of Railways for Ohio, being the first published diagrams of bridge motion that I am aware of. In these instances the card was attached to the station erected at midspan, and the pencil to the bridge, or *vice versa*, it being immaterial which. The diagram resulting from this device is a confused and knotted mass of lines, furnishing comparatively little information.

In the same railway report for 1881, the general character of a more complete and perfect bridge indicator was fully set forth, the same contemplating clock-work for uniformly moving a strip of paper before the two pencils, one of which marks the horizontal and the other the vertical movements of the bridge as the paper moves along. Some two years subsequent to this outlining of the complete instrument, a similar one was used by a Mr. Biadego on a three-span continuous girder bridge, notice of which was given in a foreign paper.

The numerous results found in tables given in this paper were obtained from diagrams taken between August 1st, 1884, and the end of that year.

Present Indicator.

The indicator used in these experiments might be briefly described as consisting of a heavy eight-day brass clock-movement, from which the escapement was removed, and in its place was put a small centrifugal governor, with a spring to counteract centrifugal force, and arranged so that, for a given position of the governor weights, pads were pressed against a disk, causing friction to absorb excess of driving power, and upon a shaft of which clock-work was attached a drum for moving the paper strip with two pencils, so arranged as to move lengthwise the drum, all being mounted on a base board and portable. In use the whole is clamped in position by a bolt passing through the base. The paper is first wound upon a separate drum held by slight friction on a pin. From this it is unwound as it is wound up again on the driving drum attached to the clock-work under the pencils. A fine slit along the length of the drums served well to secure the end of the paper as the latter was wound up; no great length was allowed to accumulate to enlarge the drum. The paper was speeded at about fifteen inches per minute. One pencil recorded the vertical movements and the other the horizontal. To secure the greatest freedom of pencil movement, the latter were secured on the ends of light bars about a foot long, the opposite ends of which were pivoted. The pencils thus moved in circle arcs, though for the small movement as compared with the radius, the lines of pencil movement were nearly straight and crosswise to the strips of paper.

The pencils were moved by cords, one going direct and the other going over a pulley to change its direction to a right angle, in order to bring the pencil movements upon the same strip of paper. The cords were attached to the bars at 0.64 of their length from the pivot, so that the diagrams as taken were correspondingly widened; hence any ordinate or amplitude on a diagram must be multiplied by 0.64 to obtain the correct measure of bridge movement in inches.

A third and stationary pencil held by a spring was used to mark a reference line upon the same strip of paper.

Application of Indicator.

Several ways of using the instrument were tried, but reliable diagrams could only be obtained by placing it on the staging or support brought up from the bed below, so that it could be quiet, while the cords moving the pencils were tied to the bridge. When the indicator was attached to the bridge the jarring of the bridge was found to jostle the pencils too much, breaking the lines into dots, and the particular governor in the instrument used was sensitive to agitation. But there is evidently no reason why the instrument may not be attached to the support, and the cords to the bridge, instead of the reverse.

In one case the instrument was placed on the ground under the bridge, water not being under that span. But the best plan consisted of setting up a tripod, resting on the bed below, and reaching up through the bridge flooring without touching it, and extending to a convenient height and position for receiving the indicator. In one instance, on the double track Pennsylvania Railroad, there were two bridges, one for each track. Here the instrument was placed on one bridge, and the cords attached to the other, but the diagrams taken for trains moving over the bridge to which the indicator was attached were imperfect. Experience with this indicator, as mounted upon the bridge itself, would indicate quite conclusively that the harsh and fine cut tremor of the parts of a bridge during the passage of a train, is too severe for the good of any instrument that might be brought in contact with them.

Greater refinements of registry might be attempted than were carried out in these experiments. Pencils were used to do the marking, but they are faint. Diagrams made with an inking point would be much preferable for reproduction in printing. Also no special devices were employed for recording speed of train, as might have been done by electricity. It is to be regretted that the passage of every wheel of the train was not electrically recorded to aid in determining the relation of car lengths and time of vibration of bridge; also the down position of crank pin relative to panel.

The reference line pencil was used to record by hand and eye the revolution of drivers, and the time for a train to move from one signal to another at the ends of a given measured base; but automatic registry is much to be preferred, as far as can be, so as to leave the operator free for making any notes he may desire about the train, such as number of engine, kind of cars and location, flat wheels, etc. Present experience shows that the indicator should be made to do all the recording possible. If another campaign were to be undertaken by myself, greater demands would be made of the indicator for registry with more or less of electrical attachments.

The Diagrams.

Accurate copies on Plates V, VI and VII give a good idea of the diagrams taken in this indicator work. Of the three lines traced as above mentioned, the upper one, $A\ B$, is that showing the vertical movements, and $M\ N$ the lateral movements, of the bridge during the passage of the train, the lower line, $X\ Y$, being the line of reference.

For all the diagrams the indicator was placed at the panel point nearest mid-span, the cords connecting with pencils being always attached to parts of bridge near this point, and always to one truss only. In this way $A\ B$ is the record for the vertical motion, and $M\ N$ for the lateral, of the panel point of the one truss mentioned. Now, when a train approaches the bridge the indicator is started, making the straight lines at the left of A, M and X. But when the train strikes the bridge the bridge is disturbed, and the pencils respond accordingly, continuing so to do as long as any disturbance lasts, and that cannot be less than the time the train is on the bridge.

When the engine and train move upon the bridge, the latter is deflected on account of the load, and hence the pencil recording vertical movements responds to this deflection as well as to vibration, causing the very strong rise of the whole line $A\ C\ D \ldots B$ above a straight line from A to B, a deflection of the bridge being here noted as a rise in the pencil. A smooth line drawn through the middles of the sinuosities of $A\ C\ D \ldots B$ shows by the height of any point above a straight line, A to B, the statical deflection of the bridge at the corresponding time, and as the paper moves uniformly from A, where the train first touches the bridge, to B, where it leaves it, the deflection corresponding to any part of the train can be accurately located. While a smooth line through the sinuosities of $A\ C\ D \ldots B$ answers to statical deflection, the sinuosities themselves answer to vibratory disturbances, the same appearing more or less regular, according to approach of bridge toward the condition of actual vibration. The above remarks relative to the record of the vertical movements of the bridge apply equally to $M\ N$ for the lateral movements, except that here we do not look for statical deflection unless the bridge has a curved track, or that wind is blowing while the train is passing.

The track was straight in case of all the diagrams shown on the plates.

To the left of A the lines are always smooth; but beyond B there may be sinuosities due to residual vibration of bridge. These are less apparent in A B than in M N, though sufficiently numerous in each to give the time of vibration, lateral and vertical, of the unloaded bridge.

The diagrams 173, 182 and 192 differ very much from the others in appearance, owing to the fact that they are for passenger trains while the others are for freight. In the former the sinuosities at C are wide in amplitude, while the part D E is comparatively smooth. On the other hand the freight diagrams are comparatively smooth at C and at places wide in amplitude of sinuosity beyond D.

The diagrams selected for the plates are among the more interesting ones, although others present like characteristics, as can be seen by consulting Table No. 2.

The speed of the paper when the diagrams were taken was from 15 to 15.5 inches per minute, and carefully noted at each setting of instrument; consequently the length of time any train occupied in going over a bridge can be measured from the diagrams. The length of 137 from A to B is 6.5 inches, so that the train was agitating the bridge about twenty-five seconds. The lateral movements of the pencils for A, B and M, N is in excess of the actual movements of the bridge by the ratio of 1 to 0.64. The points a and c give simultaneous positions of the two indicating pencils, and likewise for the points b and d.

The Tables.

Table No. 1 is a table of bridges, 1 to 13, noted in the order in which the indicator was applied to them. No results of indicator work are noted in this table, it being intended as an embodiment, in condensed form, of the leading particulars of the bridges.

In the column of stringers the term "I beam" means a solid rolled I beam; while "I section" means a beam built of a plate and four angle bars, and perhaps a cover plate, riveted together.

Table No. 2 is intended to include all results of value obtained from the diagrams, such as statical deflection ordinates; amplitude of vibration, vertical and lateral; number of vibrations per inch of diagram; inches of length of diagram for each train; number of revolutions of drivers per inch of diagram; and remarks.

The number of cars in column 3 was obtained from railway officials as far as possible, and checked by count taken at time of indicating bridges, though in some cases the number is stated simply from count. The number in column 4 is made out by aid of columns 12 and 15 for the cases of cumulative vibrations, on the supposition that the half-car lengths coincide with the time of vibration of bridge for such vibration. An agreement is corroborative of the fact of cumulative vibration for freight trains where

$$\text{Number of cars} = \frac{\text{number of vibrations per inch}}{2} \times \text{number of inches per train diminished by inches for engine.}$$

For instance, in diagram 61, averaging the vibrations 13.5 and 17 gives 15.25, and the length is 1.4 inches. These in the above give

$$\text{Number of cars} = 10.6 - 1.7 = 8.9 \text{ cars;}$$

a figure which agrees with the count within less than one car. In this a car is reckoned at 30 feet and an engine at 50 feet, making the engine about 1.7 cars.

The data in column 5 are made out from field notes and statements of railroad officials. Column 6 is made out entirely from information furnished by the railroads.

The speeds in column 7 were determined in three ways. See footnote to table.

Quantities in columns 8, 9, 10, 12, 15, 17, 18, and part of 16, were taken directly from the diagrams by measurement and count, a portion of 16 being found from columns 3, 5, 6, 15, and car length.

The percentages in column 11 were obtained by dividing half of the amplitude of vibration by the statical deflection of column 9, the latter being the height from the line $A\ B$ to the mid-height of the sinuosities of the upper part of the diagram.

Column 13 was calculated by the aid of a theoretical formula published in the Ohio Railroad Report for 1881, with constants determined for each bridge.

Column 14 is calculated from twice the number of whole car lengths as divided by inches of diagram per train.

Columns 19 and 20 contain remarks as noted from the appearance of the diagram. Any remark applies to the line or bracket at which it is found. To determine whether the remark "cumulative" applies to the engine or to the following part of train, reference may be had to the eighth column, where any remark applying to the engine will be found at abscissas less than about 0.6 inch.

Table No. 3 exhibits, collectively, the results from those diagrams which record the more marked cumulative effects as due to the engine itself, and mainly due to unbalanced drivers.

In this table, columns numbered below 20 are to the same purpose as those of like number in Table No. 2. Column 22 is added to show what influence the floor beam distribution may have upon the vibration.

Table No. 4 is similar to Table No. 3, except it is intended for the train itself instead of the engine. A column for floor beams might have been added, but it would be so nearly identical with 14 that the latter may practically be taken for it, except in case of Bridge 6. See Table No. 1, where the floor beams are two feet apart or less.

Table No. 5 is intended to present points of interest as determined

PLATE V
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XVI Nº 351.
ROBINSON ON
VIBRATION OF BRIDGES.

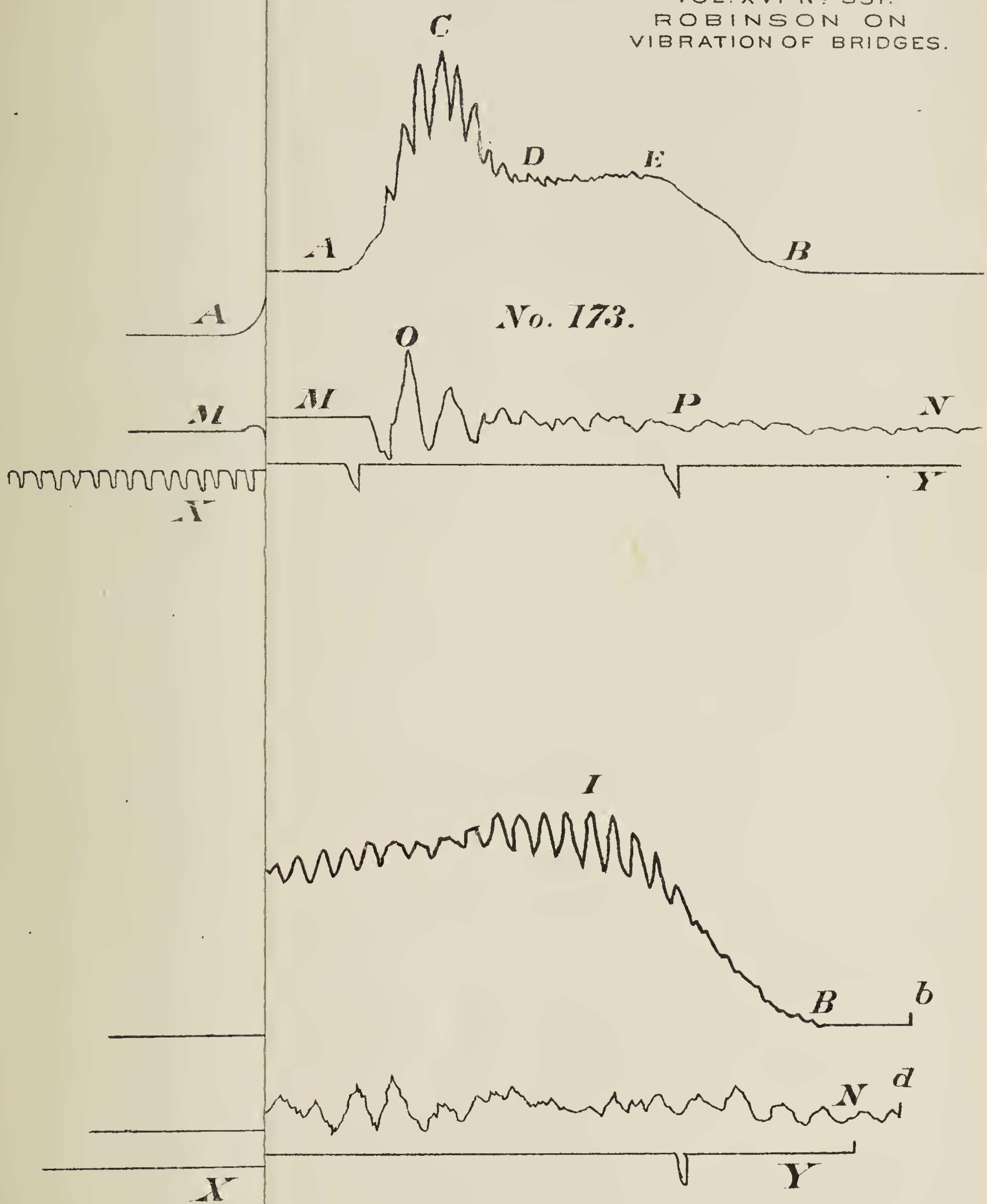


PLATE V
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XVI Nº 351.
ROBINSON ON
VIBRATION OF BRIDGES.

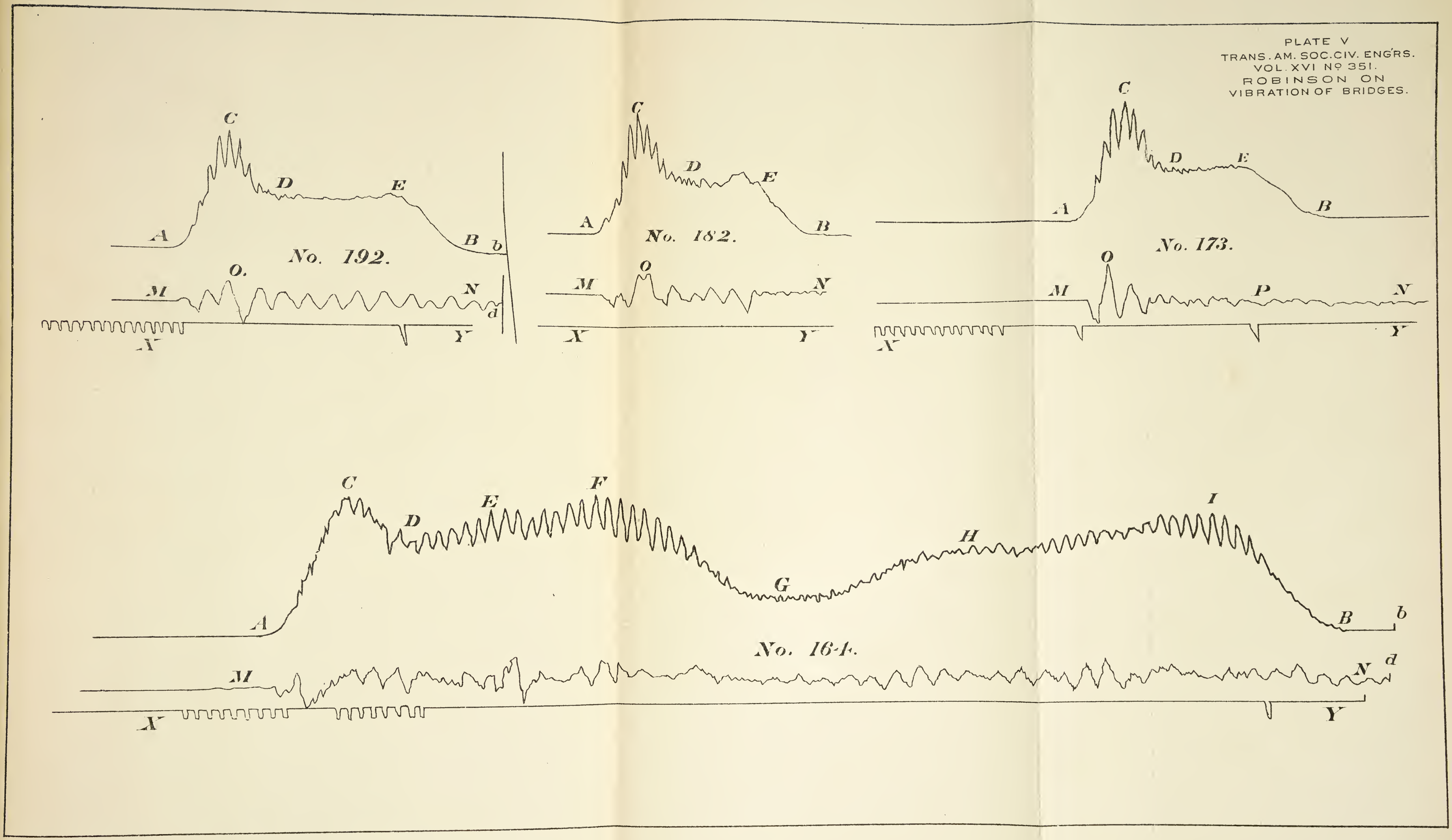


PLATE VI
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XVI Nº 351.
ROBINSON ON
VIBRATION OF BRIDGES.

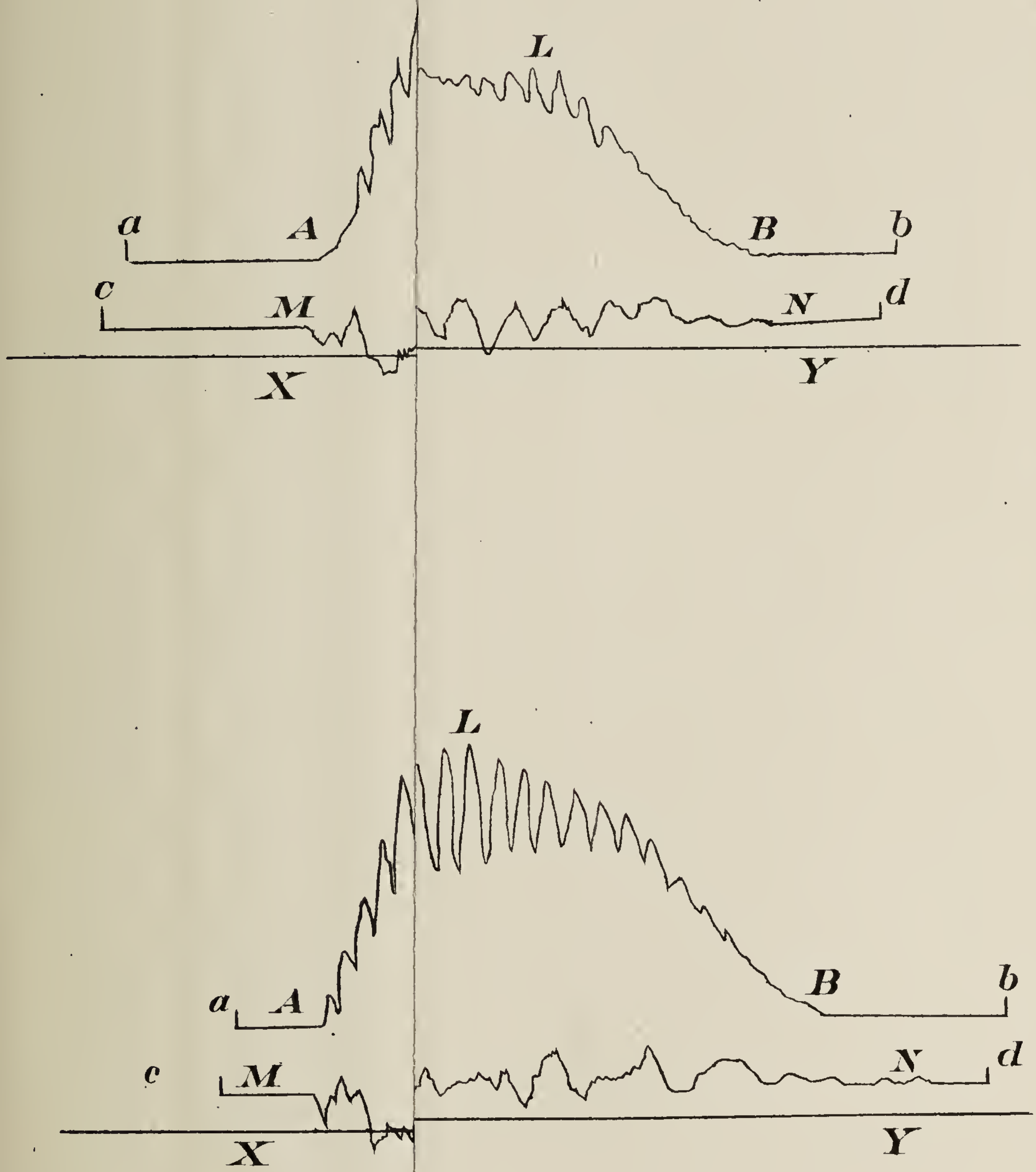
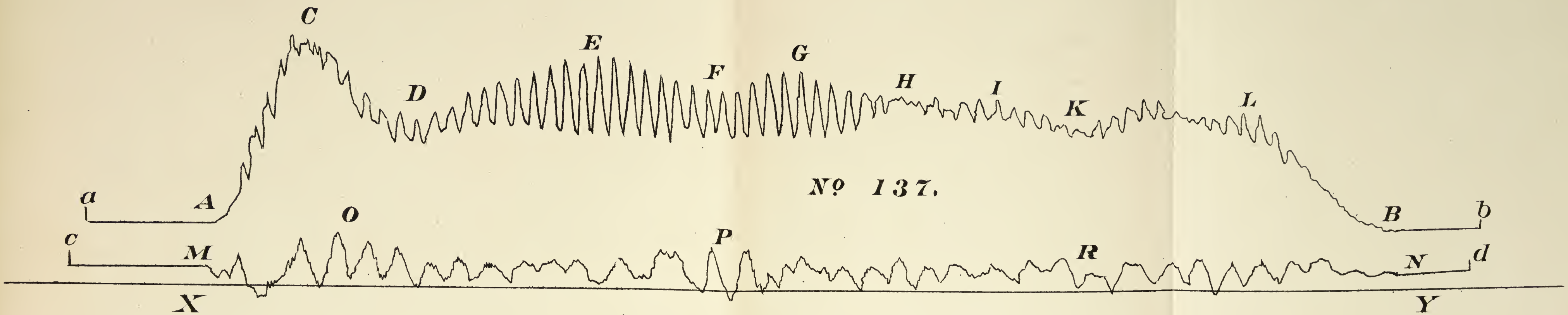


PLATE VI
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XVI Nº 351.
ROBINSON ON
VIBRATION OF BRIDGES.



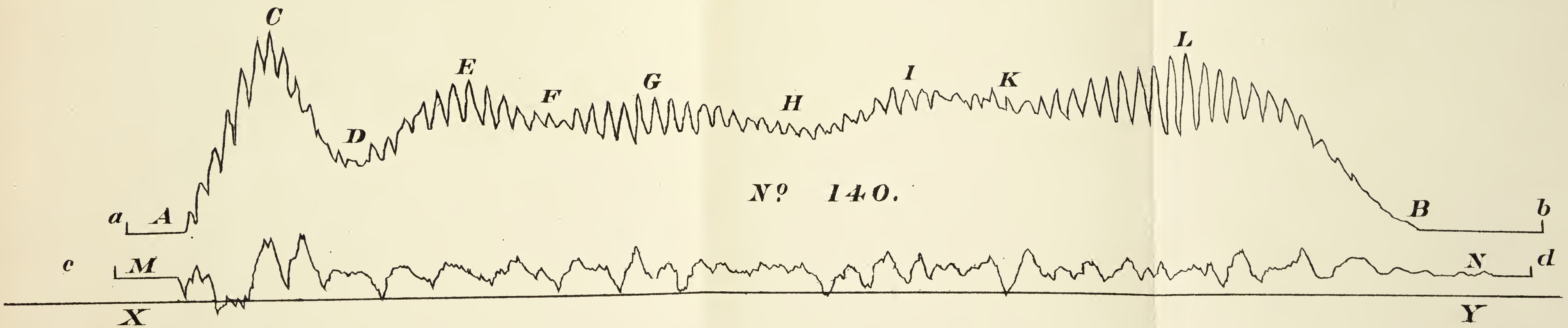
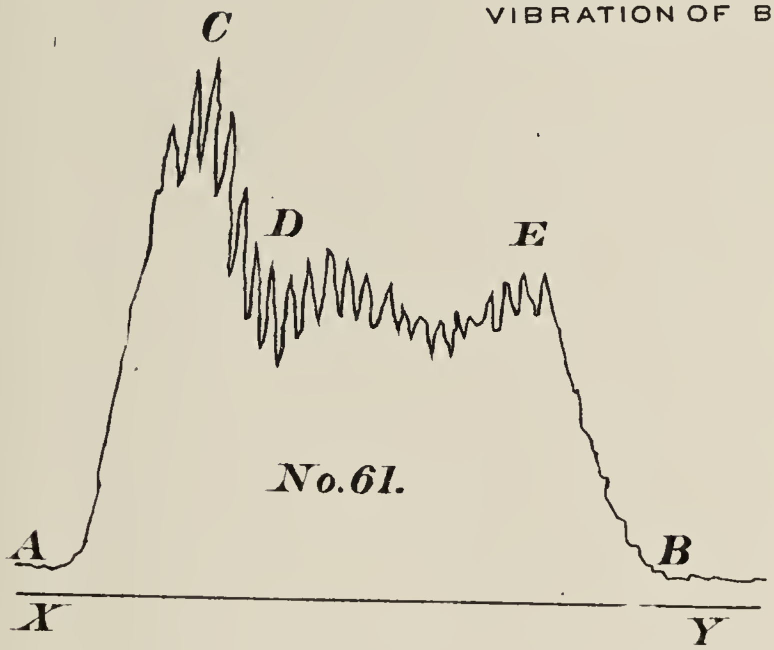
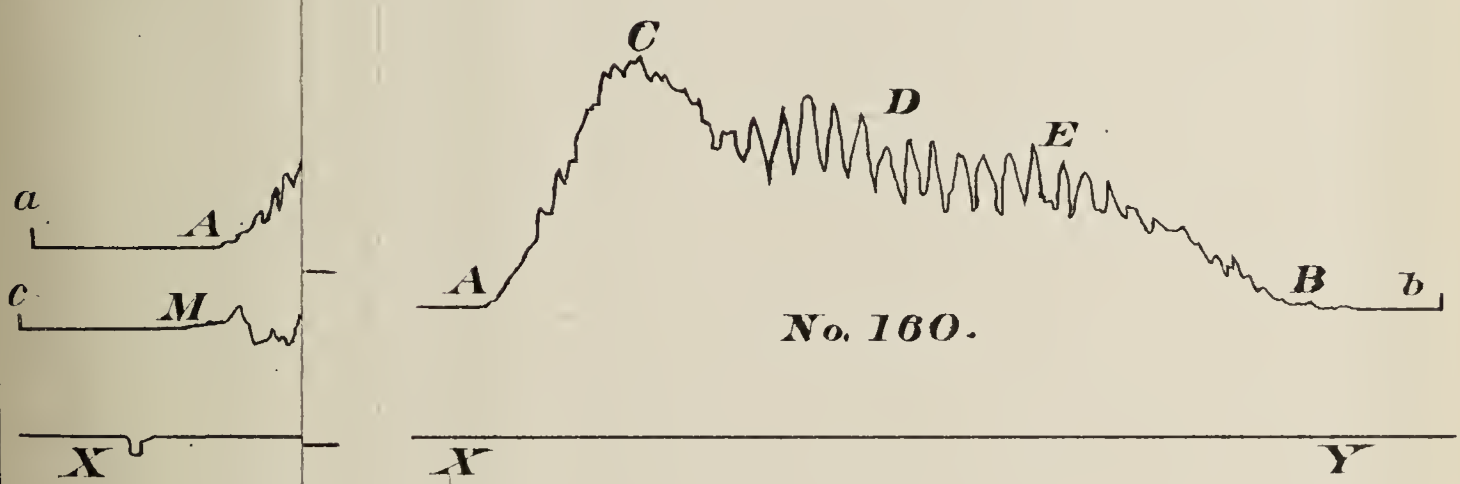


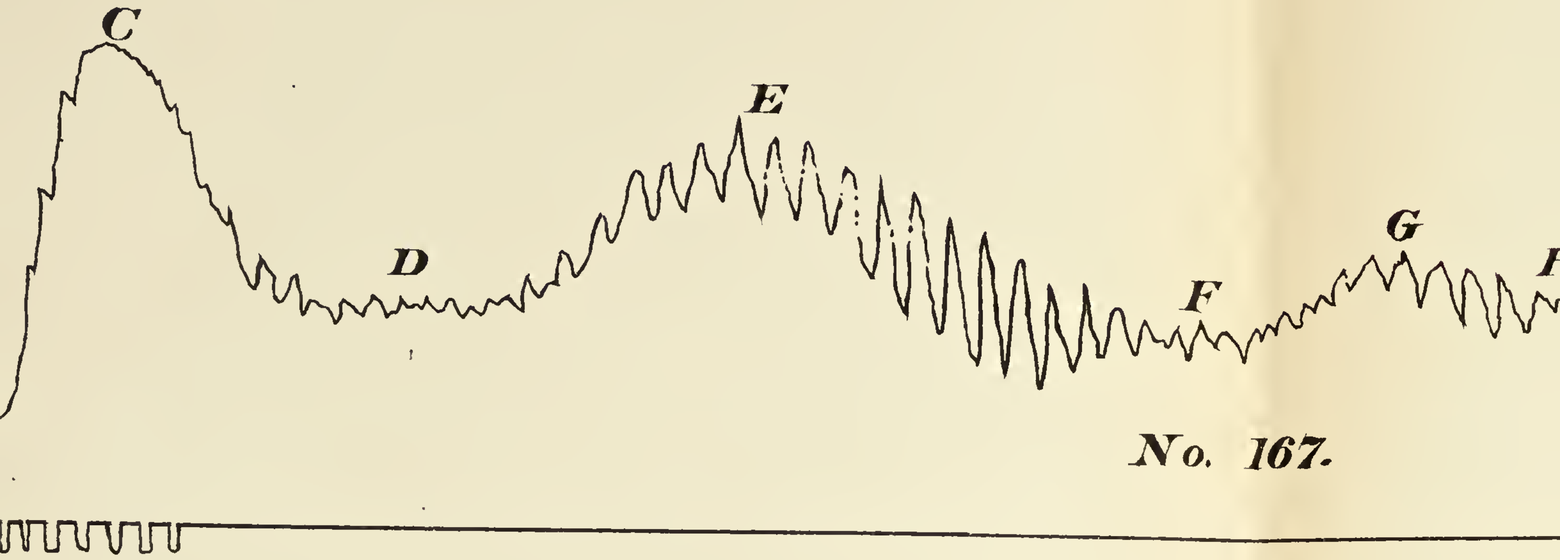
PLATE VII
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XVI Nº 351.
ROBINSON ON
VIBRATION OF BRIDGES.



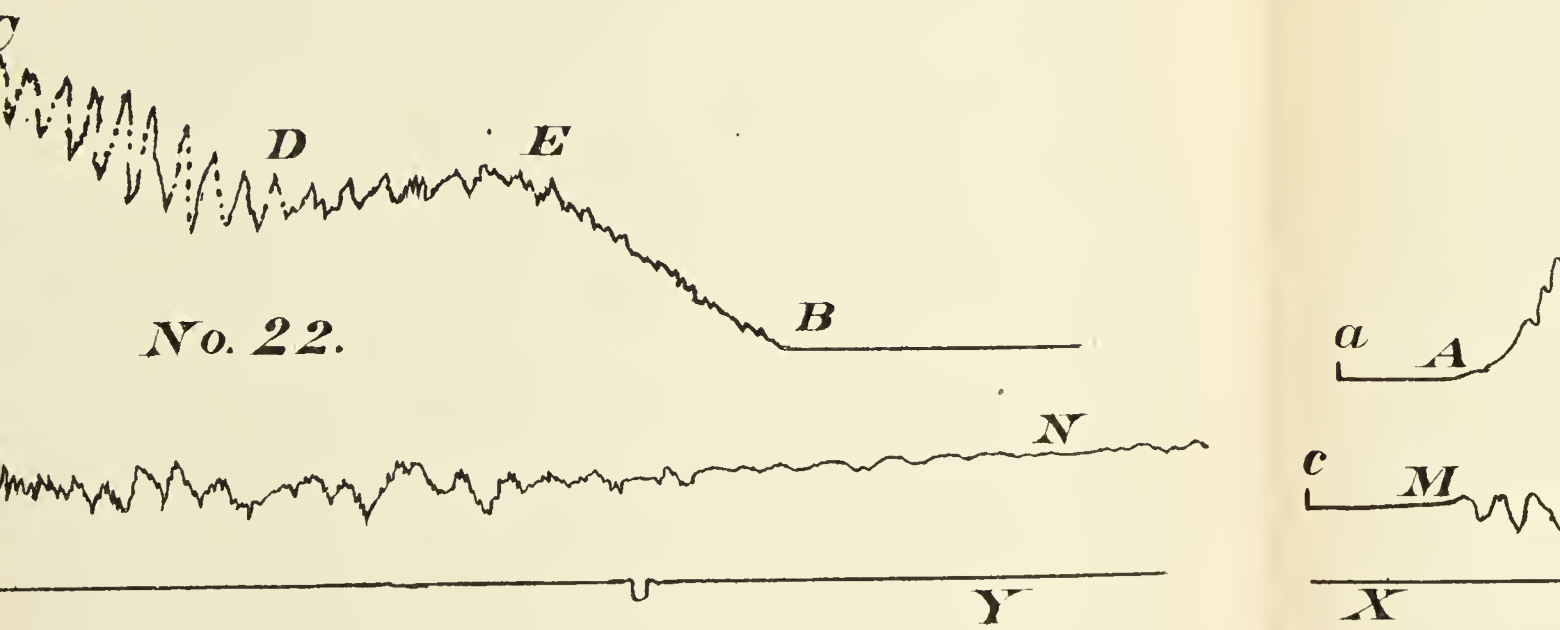
No. 61.



No. 160.



No. 167.



No. 22.

by indicator respecting the statical deflection of bridges as due to a locomotive and first portion of following train; as due to uniform and ordinary train load; and also the time of vibration of bridges, vertical and lateral, loaded and unloaded.

Bridge Vibration and Oscillation.

I have applied the term vibration to the movements of bridges now being considered, instead of the term oscillation, for the reason that these movements exist, as to and fro motion, by reason of the elastic forces of the bridge itself. The movements of a long suspension bridge from the action of wind and gravity might be termed oscillation, because due to the action of forces existing outside of the structure. Vibration is usually much more rapid than oscillation. A glance at the diagrams is sufficient to show that many instances of real vibration have been recorded. The duration of one movement is too short for oscillation. These remarks, however, apply with full force only to such bridges as have all parts bound together so as to form a unit for elastic reaction, as in Pratt, Howe, Post and similar trusses. In a Bollman truss, for instance, where each floor beam is supported nearly independently of the others, and by tie-rods of different lengths and inclination, we find conditions very unfavorable for vibration. This view is supported by the few indicator records obtained from a Bollman bridge. This fact may serve to offset some of the unpopular features of Bollman bridges. But in the Pratt truss, to which the experiments were mostly confined, the upper and lower chords are joined firmly by bars into a whole, so that if one point is depressed by weight, the whole truss is sprung into an elastic curve. Such a structure may therefore be regarded as an elastic body, and susceptible of elastic vibration.

Vibration of Elastic Bodies.

Vibrations may be set up in an elastic body, 1st, by delivering upon it a severe single blow; 2d, by suddenly releasing it from a straining force; or 3d, by the successive application of a series of comparatively very small impulses, provided the impulses are applied at proper intervals of time. The most favorable condition is realized when the time intervals between successive impulses are equal one to another, and also equal to the periods of simple vibration, the impulses being applied to and fro with the movement of mass. A less favorable condition exists when the impulses are half as frequent as above, but applied all in the same direction. Other, though less favorable, conditions exist; but the present investigation has not shown that they take effect in bridges. Vibration from the third cause, slight at first, but eventually reaching a high intensity, may properly be called cumulative vibration.

A common hand-saw may serve to illustrate. Fasten the tip end in a clamp, or into a kerf sawed in a post, so that the blade will stand out horizontally and flatwise, with the handle at the free end. If now the handle be struck, or if it be pulled down some distance and released, the handle will move up and down repeatedly, illustrating the first and second cause of vibration.

To illustrate the third cause, or to obtain a cumulative effect, place the saw at rest, tie a thread to a tack and suspend it about eight inches from the hand. Now, dropping the tack upon the saw near the handle, a slight deflection may be observed due to the weight of the tack; as soon as the deflection takes place, lift the tack from the saw. When the saw handle has made its return and is ready to descend again, drop the tack on it once more; this adds a new impulse, and the vibratory movement will be greater than before. By continuing this the saw handle will soon gain an intensity of vibration that would hardly be expected by one who never tried it. But a more rapid increase of amplitude will follow the application of impulses in both directions, up when the saw goes up as well as down when the saw goes down, thus realizing the most favorable condition mentioned.

This illustration of cumulative vibration shows that the applied impulses must succeed each other at intervals which are in keeping with the vibration period, or that they must be "in time." To harmonize observed facts in bridge vibration with this principle, the time of vibration of the bodies concerned should be calculated, as of the bridge itself, the car on its springs, the engine on its springs, etc.

Calculation of Bridge and Car Vibration.

For a car, let the weight of the body and load be W, including all carried on the main car-springs. Let P be a force that will compress all springs under a car a distance d, and let y be the compression for a force F. Now, as the distortion of perfectly elastic bodies follows Hooke's law of elasticity, viz.: Distortions are proportional to the distorting forces; and as bars, beams, springs, or even structures within the elastic limits follow this law in deflection, compression, extension, etc., so closely that it may be taken for the true law, then we may make the following simple statement for a car on its springs, viz.:

$$P : d :: F : y,$$

or

$$F = \frac{P}{d} y.$$

Applying a simple expression of dynamics for the action of a force on a mass, we have

$$\frac{d^2 y}{d t^2} = f = \frac{\text{acting force}}{\text{mass}} = -\frac{g}{W}\frac{P}{d} y,$$

the sign depending on contrary direction of y and F.

Integrating once, we obtain

$$v^2 = \frac{d\,y^2}{d\,t^2} = -\frac{P\,g}{W\,d}\,y^2 + C = \frac{P\,g}{W\,d}\,(h^2 - y^2), \tag{1}$$

the constant being determined on the supposition that $v = o$ at the beginning of a movement where $y = h$, or where an applied compressive force, F, might be suddenly released, allowing the car body and load to bound upward. When the car has risen to the position of rest $y = o$, giving a high value of v, at $y = -h$, $v = o$ again, and this is the opposite point in the amplitude of vibration where a return movement begins, and so on in repetition. The whole amplitude will then be $2\,h$.

Solving for t, we obtain, using the negative sign

$$t = \sqrt{\frac{W\,d}{P\,g}} \int \frac{\pm\,d\,y}{\sqrt{h^2\;y^2}} = \sqrt{\frac{W\,d}{P\,g}}\;\cos.^{-1}\,\frac{y}{h}$$

which expresses the time for the car to move from a compression h of the springs up to a compression y. If we make $y = h$, $t = o$ for a starting point of reckoning time, and this corresponds with the position where $v = o$. For $y = -h$, the opposite limit of amplitude of vibration, we have

$$t = \pi\sqrt{\frac{W\,d}{P\,g}} \tag{2}$$

and this is the time of a simple vibration. Twice this, or $2\,t$, will of course be the time of a "complete" or "double" vibration, which is the time occupied in a complete movement forward and back to the same point again.

If $y = +\,h,\; -\,h,\; +\,h,\; -\,h,\; +\,h,$ etc.

$$t = \sqrt{\frac{W\,d}{P\,g}}(o,\; \pi,\; 2\,\pi,\; 3\,\pi,\; 4\,\pi, \quad \text{etc.}$$

which indicates isochronous periodic motion independent of the amplitude, and continued indefinitely, and is a case of repeated "harmonic motion."

As an example of calculated time of vibration of a freight car and load on its springs, the Pennsylvania Railroad make $P = 66\,000$ pounds for $d = \frac{7}{8}$ inch, while the car body and its load weigh 55 000 pounds $= W$. Introducing these, and we find for the time of a complete vibration of such a car on its springs,

$$t = 0.14 \text{ seconds, and } 2t = 0.28 \text{ seconds,}$$

which is about the same as some of the periods of bridge vibration given in Table No. 5 for loaded bridges.

The time of vibration of a locomotive on its springs may be similarly calculated.

If $W = P$,

$$t = \pi\sqrt{\frac{d}{g}}, = 0.16\,\sqrt{d} \text{ nearly,}$$

if d is expressed in inches. That is, the time of a simple vibration of a weight attached to a spring is equal to 0.16 times the square root of the deflection in inches of that spring due to placing that weight upon it.

If $d = 1$ inch, $2t = 0.16$ second, as for a car body and load which would settle the springs 1 inch.

For a bridge with parallel chords, the exact calculation is more difficult. The bridge will vibrate similarly as would a rod supported at its ends, the differential equation of the amplitude curve* for which is one of the fourth order. But it is unnecessary to go to this refinement, because the bridge is not precisely like a rod in vibrating. Probably the amplitude curve for a bridge is very nearly circular, differing from it by greater convexity near the abutments where the panel diagonals are most strained, the chord strains being uniform. Regarding it as a circle, an almost exact formula will be obtained. But, for convenience, first compare the vibration of the bridge, where the amplitude curve is regarded as a parabola having a middle ordinate d, with a hypothetical equivalent vibration of the whole bridge from end to end to a uniform amplitude of $\frac{2}{3}d$. In this comparison the relation $\frac{2}{3}$ depends on the well known relative heights of a parabolic section and of a rectangle of equal area.

To show that this comparison is legitimate, reference is had to a principle of harmonic motion, viz.: When a body vibrates through an amplitude $2h$ from the action of a force varying as the distance from the middle point of amplitude, then the acting force at the limit of amplitude is equal to the centrifugal force for the same body whirling in equal time in a circle of diameter $2h$.

The truth of this principle is made evident from the fact that when a stretched string vibrates, as on a violin, the tone resulting is the same whether the string vibrates in a plane, or swings around, describing a conoid of revolution. In either case the force operating to return a given weight of an elementary piece of string in plane vibration, when it reaches the limit of amplitude, is the same as the centrifugal force of like weight similarly situated, at any moment for the conoidal vibration. Hence we may consider the question from the standpoint of centrifugal force.

Thus for the case of the car, the centrifugal force is

$$P = M\frac{v^2}{d} = \frac{W}{g}\frac{4\pi^2}{t^2}d \qquad (3)$$

whence

$$t_{,} = 2\pi\sqrt{\frac{W d}{P g}} = 2t \qquad (2)$$

the same expression as previously found for the corresponding complete vibration. Here P varies as d, so that for any point in the amplitude curve, the value of P for an element of mass is proportional to the ordinate for that element. Hence, as the total value of P is to be made the same in the comparison, we have only to find the height of a rectangle whose area and length equals the parabola, and use that height in a formula like the above. Calling d the middle ordinate of the assumed parabolic curve of deflection or amplitude curve of bridge, we may adapt the above formula (2) for car vibration to the case of the bridge with parabolic amplitude curve by putting $\frac{2}{3}d$ for d, viz.:

$$t = \pi\sqrt{\frac{2}{3}\frac{Wd}{Pg}} \qquad (4)$$

where W represents the combined weight of the bridge and uniform

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* The expression amplitude curve here means the curve assumed by a vibrating rod when supported at its ends, and freely vibrating between, particularly for the limits of amplitude. This curve resembles that seen by observing a vibrating violin string.

load, while P is the weight of the uniform load which causes the middle deflection, d.

If we regard the amplitude curve as of the form of the segment of an ellipse where $d =$ a quarter of the minor axis, then we should adopt $.7\,d$ in place of $\frac{2}{3}d$, and the actual ordinate probably lies between these.

As an example of calculated time of bridge vibration, take the case of bridge 10 in Table No. 5, where the deflection $= 0.4$ inch, the weight of bridge $=$ 223 575 pounds, and assume the train load at about 1 400 pounds per foot, then

$$2\,t = 0.24 \text{ second,}$$

while the values in the table for like conditions range about at 0.29 second.

For the elliptic value $.7\,d$

$$2\,t = 0.25 \text{ second.}$$

differing inconsiderably from the result 0.24 obtained for the ordinate $\frac{2}{3}\,d$. The value $.68\,d$ is probably nearer the actual one.

For the case of a concentrated load at mid-span, and weight of bridge neglected, the formula (2) applies without modification, because here the bridge simply serves as the spring.

When a locomotive is at mid-span, followed by a train much lighter in load intensity, we have nearly the case of a concentrated load, a uniform load for a half span, and a uniform load of bridge for whole span. Calling the weights W, W_1 and W_2 respectively, and applying the principle of centrifugal force, we have for the total centrifugal force:

$$P = \frac{W\,4\,\pi^2}{g\,(2\,t)^2}\,d + \frac{W_1\,4\,\pi^2}{g\,(2\,t)^2}\,\frac{2}{3}\,d + \frac{W_2\,4\,\pi^2}{g\,(2\,t)^2}\,\frac{2}{3}\,d.$$

$$= \frac{\pi^2\,d}{t^2\,g}\left(W + \frac{2}{3}\,W_1 + \frac{2}{3}\,W_2\right)$$

whence

$$t = \pi\sqrt{\frac{d}{P\,g}\left(W + \frac{2}{3}\,W_1 + \frac{2}{3}\,W_2\right)} \qquad (5)$$

which is adapted to the case of an engine alone by making $W_1 = 0$. P is here the total weight concerned in causing the deflection d.

As an example, take the seventh case of bridge 10 of Table No. 5, counting the tender in as part of the train, at 1 400 pounds per foot. Then $W = 72\,150$ pounds; $W_1 = 84\,000$ pounds for 60 feet; and $W_2 = 223\,575$ pounds. But $W + W_1 = P$ only are concerned in the observed value of $d = 0.49$ inch.

Introducing these and

$$2\,t = 0.30 \text{ second.}$$

while the observed value given in Table No. 5 is 0.29.

For a bridge and uniform load of cars, the car springs being considered, let $W_2 =$ the weight of the bridge, w the weight of the cars above the springs, w^1 the weight of the trucks, etc., below the springs, and h the compression of the springs due to the car load; then, applying the principle of centrifugal force,

$$P = w + w^1 = \frac{\pi^2}{t^2\,g}\left(w\,\frac{2}{3}\,(d + h) + w^1\,\frac{2}{3}\,d + W_2\,\frac{2}{3}\,d\right)$$

or

$$t = \pi \sqrt{\frac{2\,d}{3\,g}\left(1 + \frac{w\frac{h}{d} + W_2}{w + w^1}\right)} \qquad (6)$$

In this formula the greater the value of h the greater is t, so that the time of vibration of bridge and load is increased by the presence of the car springs.

As an example, take the same as employed in testing formula (4), where

$W_2 = 223\,575$ pounds.

$w = 1\,100 \times 148 = 162\,800$ pounds.

$w^1 = 300 \times 148 = 44\,400$.

$h =$ compression for w, and $= .5$ inch.

$d = .4$ inch, the mid-span bridge statical deflection due to $w^1 + w$.

Then

$$t = 0.29 \text{ second.}$$

a value which agrees better with the observed time of vibration noted in Table No. 5 than the result obtained from (4), and it also agrees well with the time of vibration of a car on its springs, as noted in the example for formula (2). These facts are in support of the supposition that when a bridge vibrates the cars also vibrate on their springs.

The close agreement with the observed times of vibration of these results, calculated by the principles of dynamics, is believed sufficient to show that the theory of bridge vibration is not a myth.

These formulas were used in calculating the times of vibration, and vibrations per inch, given in the tables as "calculated;" except that the work was shortened by determining the constants from the observed results; one set of constants being used for the uniform load throughout, and another set for the engine at mid-span.

It may seem that the span and depth of truss should enter the formulas as well as the weight of bridge. Their absence is to be accounted for in the fact that the deflection d will vary with the span and depth. To determine the law of relation of d to the span and depth in similar trusses proportioned for like maximum strains, the elongation of the iron will be the same in all cases, viz., $\frac{5}{1000}$ to $\frac{6}{1000}$ of an inch per foot for a 10 000-pound working strain. By aid of this fact, and an outline diagram of truss, it is easy to see that d varies directly as the square of the span, and inversely as the depth, so that d varies directly as the span in diagramatically similar trusses equally strained.

Causes of Bridge Vibration.

Notwithstanding the fact that a bridge, a car, etc., may vibrate, and that the vibration period may be calculated theoretically, yet we would not expect such vibration to originate without due cause. In studying this we must distinguish between mere lurches of bridge without law, and genuine vibration. The diagrams indicate a considerable tendency to non-systematic lateral movements, much more so than to vertical. This is believed to be due largely to the wandering of the trucks from side to side on the clearance between gauge of rails and wheels, and also to crooks in the rails. The latter may give cause for irregularities in the diagrams of vertical movements, also low or worn joints, flat wheels, etc.

Hence irregularities may appear in the diagrams. But genuine vibration must be regular, at least in vibration periods, and we would never expect an absolutely instantaneous ending of vibratory movement, nor even beginning, unless caused by a blow.

Relative to the operation of the three causes named under "Vibration of Elastic Bodies:"

First.—Almost a blow is struck upon the bridge when a locomotive at 40 miles per hour drops its heaviest part upon it within a space of less than half a second. Undoubtedly the effect of this would be modified by bridge camber, but in any case it would seem that the bridge would be depressed somewhat beyond the position of equilibrium, then return, or spring back, etc., or vibrate. The diagrams give evidence of such action to some extent, notably Nos. 137 and 140, Plate VI. But this half-second blow would occupy only an eighth of an inch length of diagram, and as a vibration would occupy less than a tenth of an inch, it would seem that the effect of this blow should be fully developed within a space of a quarter of an inch at the initial point of diagram. Though 137 and 140 show it, the others given on the plate do not so much; 61 and 164 almost none at all. In none do we find an intense vibration established within the first quarter inch; on the other hand, at *C*, in 61, 173, 189 and 192, we find a high intensity of vibration established at about a half inch from the initial point of diagram, which, from the above considerations, must be due to some other cause than the plunge or "pitch" of the engine upon the bridge; particularly so inasmuch as in some diagrams we find the highly developed vibrations, and in others not, as witness the points *C* in 148, 160 and 167.

Second.—Releasing the bridge from strain, either partially or wholly, in no instance has been found sufficient to cause vibration.

Third.—Repeated impulses, mentioned under "Vibration of Elastic Bodies," is found to be a most potent cause of bridge vibration, as amply witnessed by the diagrams. What other cause can be assigned for the intense vibration recorded at *L* on No. 140, also *E* and *G;* and at *E* and *G* of 137, or *D* on 148 and 160; or again, at *E*, *F* and *I* on 164, and at *C* on 173, 182 and 192. A large number of other diagrams taken present like characteristics.

Evidence that these instances of vibration originate in repeated impulses, is found in the fact that the vibration never starts abruptly into full intensity, but, on the other hand, that it usually increases nearly uniformly from comparative quietude to a wide amplitude; a marked instance of this being presented in the band *K L* of 140.

Such repeated impulses are found to arise in connection with the engine, and unavoidably as now built; and also, under certain conditions, in connection with the train, from the following causes, viz.:

First.—Impulses due to the engine find cause in the non-balance of the drivers, there being an impulse downward when the excess of balance is downward, and upward when the excess is upward. These are sure to occur every time an engine passes over a bridge, but vibration will not occur unless the times of revolution of drivers coincide with the periods of double vibration, in which case the effect is cumulative, and intense vibration is the result.

All the points C in diagrams 61, 173, 182, 192 and others, are examples of cumulative vibration due to the non-balance of engine drivers, while points C in diagrams 148, 160, 167, etc., are free from vibration owing to want of harmony between revolution and vibration periods.

The above considerations suppose a rigid floor of bridge; but when the floor beams are stiff and the stringers flexible, there will be greater variation in the vibration intensity produced, because when the excess of balance strikes down at mid-panel and up at panel points, the engine will fall and rise through an increased amplitude with corresponding effect on the bridge, while for contrary conditions there will be the opposite effect. To secure the greater impulses at every panel of bridge, the panel length must agree with the drive-wheel circumferences. With all conditions favoring, viz.: flexible stringers, excess of balance down at mid-panel, equality of panel length and driver circumference, and also of revolution and vibration periods, the greatest amplitude at C will be developed. This coincidence of conditions, though rarely occurring, is likely to result in serious cumulative vibration.

Second.—Impulses due to the train will occur with some bridges, and with others not, and depend on the circumstances of flexible stringers and coincidence of panel length with half-car length. When these are both satisfied, the speed of the train must be a half car for each complete vibration; then, as each car has two trucks, and as in a freight train the trucks are not far from uniformly distributed, it follows that when one floor beam is under a truck, each and every floor beam will be under a truck or nearly so; and then, with flexible stringers, it follows that as the trucks strike the mid-panel they will drop a little by reason of the yielding stringers, and when they reach the floor beams they will be correspondingly lifted, thus either causing all the cars in the train to fall and rise, or the bridge to rise and fall, or both. Under these circumstances a downward impulse will be imparted to the bridge on each arrival of the trucks at mid-panel; this indeed at each and every panel throughout the length of the bridge. A ten-panel bridge will thus receive ten simultaneous impulses at each complete vibration of the bridge, or twenty impulses for each car length of advance of train, or about eight hundred to one thousand impulses per train with cumulative effect.

But all this presents nothing new in principle. The breaking of step of marching soldiers when crossing a bridge is for the purpose of avoiding cumulative vibration. The famed fiddler might break the bridge if the jerks of his arm "keep time" with the vibrating bridge.

In the car and its load we may find conditions favorable or unfavorable for vibration of bridge. If the load is solid, like coal, pig iron, etc., it will offer less resistance internally to vibratory influence than if it be yielding, like bales of hay, live stock, etc.

Inasmuch as the car and load are found to vibrate on the car springs in about the same time as an ordinary Pratt truss of 150 feet span, there would be harmony between them. Then the impulses, acting through a given amplitude, such as the versed sine of spring of stringers, would occasion a given vibratory amplitude of bridge; and for the cars a greater one when on springs than when not. Then this increased car amplitude would excite an increased reaction upon bridge at limits of amplitude, and hence in turn an increased amplitude of bridge.

In the bridge itself we find conditions which act to modify vibration; for instance, as the chords lengthen and shorten in response to the vibratory strains, a resistance to this like sliding of chord terminals or pedestal blocks will hinder vibration.

The position of pedestals and form of truss also have a bearing on vibration. A fish-shaped truss, with both chords joined into common terminal blocks situated at the neutral axis of truss, would vibrate without disturbing those blocks. Wood stringers extending from the bridge out into the bank, and at such height in the bridge as to necessarily slip when the bridge vibrates, will hinder the vibration.

From these considerations of the three causes of bridge vibration, it appears that the first occasions but mild results, scarcely worth considering; the second, none; while the third is capable of producing results of unknown severity, and which may be styled cumulative vibration in bridges.

Cumulative Vibration in Bridges.

The conditions favoring cumulative vibration in railroad bridges may be classified as follows, viz.:

Primary.

Under suitable train speed:

First.—Non-balance of drive-wheels in locomotives as now constructed.

Second.—Yielding stringers in bridge floors, with equality of panel and half-car length.

Secondary:

a. Vertical vibration of car on its springs.

b. Equality of drive-wheel circumference and panel length of bridge.

c. Excess of non-balance of drivers down at mid-panel.

d. Free pedestal blocks, as on expansion rollers.

e. Fish-shaped trusses, pedestals at neutral line.
f. Absence of parts overreaching banks causing friction.
g. Firm instead of yielding load.

Evidence of the Operation of the Cause and Favoring Conditions Producing Cumulative Vibration, as Found in the Present Indicator Work.

First.—As due to an engine heading a train.

In the following table are given the results obtained from nine diagrams of passenger trains, the first three being illustrated in Plate V. These three exhibit unusual sinuosity at *C*, while other portions of diagram are nearly smooth. These sinuosities *C* were recorded as the locomotive was going over the bridge.

TABLE No. 6.

Synchronism of conditions favoring cumulative vibration for case of an engine with a train following over a bridge.

Diagram.	Per Ct.	Obs. Vib.	Cal. Vib.	Rev.	Floor Beams.
173	22.6	13.3	13.6	13.5	15.8
182	21.7	15.0	13.8	16.2	18.9
192	22.1	12.8	14.0	14.0	16.3
129	18.2	13.0	13.3	11.0	12.1
134	16.2	13.3	13.0	11.5	12.6
141	17.8	13.5	13.0	....	13.2
147	14.3	13.3	13.0	12.8	14.4
162	14.1	12.6	13.4	14.0	....
171	16.4	13.5	13.0	14.0	....

This table is made up mostly from Table No. 2, selected with reference to the high percentages of superadded deflection due to vibration, as given in column 2; the third column is the number of complete vibrations per inch of diagram as observed; the fourth column is the same calculated; the fifth column is the number of revolutions of drivers per inch of diagram; and the sixth column is the number of floor beams passed per inch of diagram.

According to the first primary condition above, the fourth and fifth columns should agree, as in fact they do very nearly. A comparison of the fifth and sixth columns shows that the secondary condition *b* is very nearly realized, so that we find reason to expect the high percentages actually recorded.

Where there is a want of harmony in the quantities represented in the last three columns, the percentage of added deflection due to vibration will generally be much less, as shown in the following table, the first five diagrams of which are illustrated on the plates.

TABLE No. 7

Discord of conditions for cumulative vibration for case of an engine with a train following over a bridge.

Diagram.	Per Ct.	Obs. Vib.	Cal. Vib.	Rev.	Floor Beams.
137	6.4	....	12.7	11.9	9.5
140	15.4	12.3	12.7	12.4	9.8
160	5.0	....	13.8	12.4	11.3
164	9.0	....	12.7	11.1	9.7
167	2.5	....	12.5	9 to 11	8.8
131	2.6	....	12.6	7.8	6.5
157	2.2	....	12.4	10.6	8.4
158	6.1	....	13.4	10.0	8.4

The last three columns are seen to be much more discordant than in the like columns of the preceding table, with a corresponding lower percentage of superadded deflection, except in 140, where the vibration was sufficiently well defined to be read off, and which is seen to harmonize very well with two of the last three columns. 164 gives a percentage neither low nor high, and for this we find fair agreement in the last three columns. The last four comparisons of the table show greatest discord, and, with the exception of the last, the lowest percentages.

There are some exceptions to the rule, with unaccountable causes. An explanation may yet be found for the exceptions when more elaborate indicators and methods shall have been put to the task. The problem involves a large amount of complexity, the load on the tender being one variable element.

The first of the two tables, Nos. 6 and 7, happens to represent passenger trains only, and the second, freight; but it is difficult to see why the difference in the train should make a difference in the vibration as due to the engine itself, except for the fact of the usually higher speed and larger drivers of passenger trains. The one exception in Table No. 7 to the general low percentage shows that we may have high percentages for an engine followed by a freight train; and on the other hand diagrams 30, 39, 65, and about a dozen others not given in Table No. 2, from passenger trains show small percentages, so that both high and low percentages have been observed for the engine followed by both passenger and freight trains. Hence the dependence of the vibration upon favoring conditions, and their independence of the kind of train.

Tables Nos. 6 and 7 show a better agreement of driver revolutions than of floor-beam frequency with the calculated time of vibration; and in Table No. 3 we find higher percentages for rigid than for yielding flooring; facts which favor non-balance of engine-drivers as a disturbing cause rather than floor-beam frequence, and rigid rather than yielding stringers for the case of an engine at the head of a train.

But we observe that there is not exact agreement of drive-wheel circumference with panel length, and that this throws the excess of balance "out of time" as the wheels repeat their circumferences along the bridge, thus rapidly cutting down the combined influence of the stated conditions "2 *d*" and "*c*." But when there is agreement, it is undoubtedly the fact that in at least that half of the cases when the excess of non-balance arrives upon the center of panel in the down position, the flexible stringers will conspire with the drivers towards a percentage largely in excess of that obtainable on rigid flooring.

Second.—Vibration due to train.

Relative to cumulative vibration as due to the train and not the engine, we see by referring to the plates that the spells of vibration are of much greater duration. The engine may pass over the bridge in three seconds, whereas the recorded belt *D H*, in 137, 2.5 inches long, was about ten seconds in making, and a portion of train two and a half times the length of the bridge passed over during that time. 140 and 164 present belts of about two inches in length; hence strong vibratory action is transferable from one set of cars to another along a train. From this fact the possibilities of cumulative effect from long trains are seen to be very great, not only as regards duration, but acquired intensity.

But the observations show that the conditions of loading vary greatly along the train, so that a vibratory condition may be destroyed by the passage of one or two cars differently loaded.

A change of load intensity is sure to break up the continuity of vibration, but there are other influences less apparent which come to act. On 137 the loading appears to be nearly constant to *L*, and almost precisely so to *K*. Judging from the diagram the vibration should continue at least to *K*, and, in the absence of information, the cessation of vibration at *H* would necessarily be unaccountable. As previously stated, numerous unexplainable facts have appeared which might be made clear when more elaborate indicators and methods shall be put in use. The truth of this cannot be more forcibly impressed than by here pointing out the real cause of the lost intensity at *H*, which, fortunately, in this case we know. At *D* there followed three cars of 33 feet length. Then came fourteen box cars of 28 feet length, followed by stock cars of 33 feet length. The whole number of cars in the train is thirty, of which the fourteen box cars of 28 feet length is nearly half (allowing, of course, for the engine). Hence for the particular speed of this train the 28-foot cars were of favorable length and load, while the 33-foot ones were not. Another fact worthy of note here is with regard to the loading itself, the cars following H being stock cars, while those between D and H were not. Thus the condition g, of rigid loading, finds support, while all doubtless can appreciate the antipathy of a cow's back for elastic vibration.

Hence the change in car length and condition of car load at H constitute two reasons why the vibration should cease at H. Similarly the neck in the 2-inch belt of 140 might be explained provided the necessary information were at hand. But reliable specific notes concerning the trains were hard to get, for the reason that the observer's opportunity for it lasted but a few seconds—for 137 about twenty-four seconds.

The bands of vibration record on the plates are seen to begin by a rising scale, showing that the origin of the vibration is not a sudden shock, but a succession of impulses, as already explained. Such impulses were not anticipated as occurring in connection with the train itself; and the indicator greatly surprised us when it brought out the records, the eyes of my field observer, Mr. E. O. Ackerman, being the first to behold them. The record once an existing fact, an explanation was soon found in the yielding elastic stringers, and equality of half-car length and bridge panel.

The various results of this cumulative action are collected in Table No. 4, the greater part of which were obtained from the one bridge, 11, for which the panel length is 15 feet 8 inches, or very nearly the half-car length, and the stringers were wood. Bridge 10, on the same road, has panels of about the same length, but with stiff iron stringers.

An anomaly is found in diagram 61, in the fact of apparent cumulative vibration for train, when no explanation can be found for it in the field notes. Of the twenty-three diagrams taken from the same bridge, however, this is the only one exhibiting such record. The bridge panels are 8 feet, and the rails are laid directly on the floor beams, placed 2 feet or less between centers.

In the seven diagrams of bridge 7, no cumulative results were observed, as indeed might be expected from the discordant panel length of 19¼ feet.

For all entries in Table No. 4, except the one diagram, 61, we have a near coincidence of half-car and panel length. Hence, column 14 might be regarded as nearly representing another like it, headed "floor beams per inch of diagram."

Now, columns 12, 13, and 14, or virtually four columns, are seen to agree almost perfectly in conditions favoring cumulative vibration, whereas Table No. 2, so far as it presents the figures for other diagrams for the train, does not exhibit like agreement in the columns referred to. See 126, 132, 144, 145, 157 and 189.

Other bridges upon which the indicator was placed, with panels differing much from 15 feet, gave no cumulative records for trains.

For conditions "*d*" and "*f*," the experiments as a whole showed that bridge 11 was much more sensitive to vibration than the other bridges "indicated." This bridge had square abutments, with seats for the pedestal blocks up free and clear, with expansion rollers at one end, and all apparently in condition favoring vibration as regards "*d*" and "*f*."

The Lateral Vibration.

The lateral vibration for engine and train appears to be much more accidental in its character than the vertical vibration. When the engine strikes the bridge, the latter makes a few lurches apparently without law, but, afterwards, while the train is passing, it seems to settle down by spells to approximate regularity. Thus, at *O*, in 137, the sinuosities are quite regular, but fade away, until approaching *P* fresh lurches occur from some cause. No specific explanation can as yet be given for such freaks of the indicator pencil. It would seem that the cause is connected with that forming the indentation at *F*, as though a car here passed the bridge with loading one-sided. But this is only conjectural. See also first page under "Causes of Bridge Vibration."

The times of lateral vibration, and number per inch, given in the tables, have been made out from such portions of *M N* as are most systematic. These seem to run approximately at about twice the period, or half as many per inch as for the vertical vibration. But this, in the bridges examined, is evidently accidental, as the period of vibration depends largely on the stiffness of the trussing, it being less where the lateral truss-rods begin at end of bridge with 2-inch bars instead of 1.25 inch. These rods for bridge 11 were 1¾ inches in diameter at the abutment panels.

The amplitude of the lateral vibrations as given by the diagrams is to be found in Table No. 2. As soon as the deflection and strain due to the wind pressure is known, the superadded strain due to the lateral vibration can be approximately calculated. But the diagrams give nothing from which to make out a percentage between vibration and wind strain.

Frequency and Dynamic Effect of Observed Cumulative Vibration.

Of the 193 train transits indicated, the 25 of Table No. 3 give an average percentage of 18.4 for the engine heading a train, which is about one in eight.

For the train itself, Table No 4 gives an average maximum percentage of 26.4 for the eleven transits noted, which is 1 in 18. For both it is about one in five. That is, according to observation, cumulative vibration occurs as often as once in every fifth time a train goes over certain bridges.

The maximum observed percentage of superadded deflection occasioned by vibration is given at 28.6 for the engine heading a train, and 50 for the train. This may be regarded as expressing the superadded strain in percentage of the statical strain due to live load, and properly termed cumulative dynamic effect. In providing for these strains in designing bridges, the greater should be taken, unless it is found that the train load, plus 50 per cent., is less than the engine load, plus 28 per cent.

Assuming that the vibration is likely to occur with equal percentages to trains of all loads, then if the train load ever equals the engine load the 50 per cent. is to be taken. Referring to the diagrams, 82 makes the train load over 7 per cent. greater than the engine load, and in 158 it is slightly greater. In a number of other diagrams it is fully up to the engine load; from which it appears that the higher of the two percentages must be adopted.

But, relative to the specific percentage, it is evident that the highest ever likely to occur in the lifetime of a bridge should be provided for in that bridge, and it is not likely that in watching a bridge a week or month under an indicator the highest possible percentage will be caught, because it has already been shown to be of unknown limit, unless it be that which would jump the bridge from its seat, and this, for a span of 150 feet, would be at about 150 per cent., or only about three times as great as the highest already observed.

But, if we allow the 50 per cent. for cumulative dynamic effect, then the factor of safety might be correspondingly changed, the percentage of which should be determined with due regard to the dead load as well as live load. This done, the working strength of iron might be raised from 10 000 pounds per square inch to about 13 000 pounds; or, allowing for 100 per cent., to about 16 000 pounds per square inch for spans of 150 feet, since for such spans the strains due to the dead load, the live load, and the superadded percentage are about as 4 to 6 to 3 for the 50 per cent., and as 4 to 6 to 6 for the 100 per cent.

Taking 15 000 pounds per square inch as an admissible working strength, where all strains are accounted for, then, by so designing our bridges as to destroy the cumulative dynamic effect, a reduction in weight of bridge can be realized nearly in the ratio of 15 to 10, and all standing on a more rational basis.

For passenger trains no cumulative dynamic effect has been observed for the train itself, a fact probably due to the discordant relation of panel and car, and to the variation, 45 to 60 feet, in passenger-car lengths. The most favorable panel length for vibration that is likely to be found in existing bridges would seem to be about 18 feet, or a third of the average car length, for which the favorable speed would be about 40 miles per hour. But only two of the bridges indicated had panels near this length, and only three diagrams were obtained on which a cumulative result might have appeared. As none were obtained, it is to be supposed that the conditions were not favorable.

To Reduce Cumulative Dynamic Effect.

First.—For the locomotive it is necessary that the machinery be so counterbalanced as to occasion no vertical components of non-balance. This can readily be done, even as engines are now built; but it is not at all likely that it will be, because of the resulting excessive jerks then appearing as longitudinal components. This is due to the utter impossibility of perfectly counterbalancing the pistons of ordinary locomotives by counterweights in the wheels. In the Shaw locomotive we have an example of one free from the evil of non-balance.

Second.—If the bridge stringers could be perfectly rigid there would be nothing to fear from the train; but as this is impossible, let them be made very deep; not less than the depth of the floor beams, and then make the panel length so that no multiple of it will equal the length of any car, freight or passenger.

Third.—If expansion rollers at pedestal blocks be discontinued, and some provision be made for sliding, as upon "end wood" saturated with tallow, the "working strains" in the chords will probably be less than in use of perfectly free expansion rollers, and the greater the friction the less the chord strains, because, for either a rising or falling temperature, when a maximum train passes the bridge the lower chord will be elongated about a third of an inch per 100 feet, with free pedestals; and considerably less with severe friction, with corresponding strains. The endurance of abutments and piers is no part of the present problem. When the vibration is great enough to slip the pedestals back and forth against friction, the frictional resistance will largely counteract vibration. Thus for a span of 140 feet, maximum live and dead load 500 000 pounds, coefficient of friction 0.2, and vibration 50 per cent., the slip of pedestal is about a quarter of an inch for each vibration, and the foot pounds of resistance at each double vibration is about 2 084, and it will neutralize an equal applied impulse. At a vibration of about 30 per cent. the slipping begins. Hence substituting sliding for rolling serves a twofold purpose: first, to reduce direct chord strains; and second, to diminish vibration and the superadded strains.

Impact, Dynamic Effect, etc., Previously Provided For.

It has been customary to allow a diminishing percentage for impact, it being from 20 to 50 per cent. for short spans, and vanishing at spans of about 100 to 150 feet. But the cumulative dynamic effect will vary the opposite way with the span, and be 50 per cent. or more at 150 feet spans, where impact is neglected, from which it would appear that a

TABLE No. 1.

Giving Particulars of Railway Bridges to which the Indicator was Applied in 1884.

Vol. XVI, p. 62.

Number in order occupied.	Date.	Road.	Designation by Railway Company.	Location.	Kind of Bridge.	Span. Ft. In.	Number of Panels.	Length of Panel. Ft. In.	Depth of Truss. Ft. In.	Floor Beams.	Stringers.	Height of Track above Chord Pins.	Number of Diagrams taken.	Remarks.
1	Aug. ..	Little Miami.	....	Columbus, O. Over the Scioto Riv.	Pratt Truss.	136 0	9	15 $1\frac{1}{3}$	About 24′	28″ deep and strong.	Iron I sec. 18″ deep, and flanges $10\frac{1}{4}$″ across.	Abt 24″	14	
2	Aug. 19 and 20.	N. Y. P. & O. Main Line.	70	About $2\frac{1}{2}$ miles N. E. of Dayton, O. Over Mad River.	2 Spans through Pratt Truss.	140 6	9	15 $7\frac{1}{3}$	24 0	Skewed $\frac{1}{3}$ panel 31″ deep, with flanges $10\frac{1}{4}$″ across.	Iron I sec., 24″ deep, flanges $10\frac{1}{4}$″ across.	About level with top of lower chord.	9	Bridges continuous over middle pier. Lower chords of panel next to mid-pier are compression members.
3	Aug. 21 to 23.	do	69	About 6 miles N. E. of Dayton, O.	do	156 0	10	15 $7\frac{1}{5}$	24 0				16	
4	Oct. 16	Penn. R. R. Del. Exten. of Phila. Div.	Arsenal Bridge.	Over Schuylkill River.	Through Pratt Double Intersec.	189 7	20	9 $5\frac{3}{4}$	19 0	Floor beams directly on lower chord 4 per panel 8″ by 12″	Stringers under rails; no ties.	Abt 24″	4	Has b'n condemned for two years.
5	Oct. 17.	Penn. R. R. Phil. & West. Chr. Div.	1, or Mayland B'g	About $\frac{1}{2}$ mile W. of So. St. Sta., Phila., Pa. Over Mayl'd Creek	Two separate Deck Pratt Trusses for double track. North truss.	128 3	9	14 3	16 6	Floor beams are from 18″ to 2′ from center to center; they are 8 by 14 in., and rest directly on lower chord	No stringers. Rails are laid directly on the floor beams.	21″	12	Guard rails are 8 by 8 inches.
6	do	do	do	do	South truss.	128 0	16	8 0	15 0			21″	11	
7	Oct. 18.	Phil. Wlm & Bal. R. R.	Chester Bridge.	600 or 800 ft. W of Chester. Over Chester Creek.	Through Pratt Double Track.	154 6	8	19 $3\frac{3}{4}$	26 0	3′ 8″ dp,; suspended.	Iron I sec., 22″ deep, 6′ apart c. to c.	On level with pins.	7	Very strong and stiff bridge.
8	Oct. 23.	B. & O. R. R. Main Line.	Harper's Ferry Bridge.	Over the Potomac River and Canal.	7 spans through Bollman 4th span occp'd.	135 0	10	2 ends $=17\frac{1}{2}$′ Others $=12\frac{1}{2}$′	17 6	16 by 12 in. boxed.	2–15″ I beams, 5″ across the flanges.		4	For rail and wagon road, and has 3 trusses.
9	Nov. 21 and 22.	N. Y. P. & O. Main Line.	55	Just S. W. of Greencamp, O. Over the Big Scioto.	Through Pratt Truss.	150 0	10	15 0	24 0	Riveted to columns 36″ dp.; flanges $10\frac{1}{4}$″ across.	I sec., 26″ dp.: flanges $9\frac{1}{4}$″ across.	4′	7	
10	Nov. 24 to Dec. 24.	do	57	Between Claibourne and Broadw'y, O. Over Beans Creek.	do	148 0	10	14 $9\frac{3}{5}$	24 0	do	do	4′	32	
11	Nov. 29 to Dec. 15.	do	32	Abt. $2\frac{1}{2}$ miles W. of Leavittsburg, O. Over Mahoning River.	do	141 0	9	15 8	About 24′	Suspended iron I sec., about 30″ deep.	3 — 8″ by 18″ wood; resting on floor beams.	1′	60	
12	Dec. 1.	N. Y. P. & O. Mahoning Div.	9	do	do	143 0	8	17 $10\frac{1}{2}$	24 0	Suspended iron I sec.	Iron I sec., 16″ deep; riveted to floor beams.	On level with pins.	4	
13	Dec. 2 and 3.	N. Y. P. & O. Main Line.	29	About 1 mile E. of Orangeville, Pa.	Through Pratt Double Intersec.	156 6	11	14 $2\frac{3}{4}$	About 24′	Suspended iron I sec.	Wood 16″ dp.: resting on floor beams.	16″	13	

Total number of bridges, 13. Total number of diagrams, 193.

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Number in order occupied.	Date.	Road.	Designation by Railway Company.	Location.	Kind of Bridge.	Span.	Number of Panels.	Length of Panel.	Depth of Truss.	Floor Beams.	Stringers.	Height of Track above Chord Pins.	Number of Diagrams taken.	Remarks.
						Ft. In.		Ft. In.	Ft. In.					
1	Aug. ..	Little Miami.	....	Columbus, O. Over the Scioto Riv.	Pratt Truss.	136 0	9	15 $1\frac{1}{3}$	About 24'	28" deep and strong.	Iron I sec. 18" deep, and flanges $10\frac{1}{4}$" across.	Abt 24"	14	
2	Aug. 19 and 20.	N. Y. P. & O. Main Line.	70	About $2\frac{1}{2}$ miles N. E. of Dayton, O. Over Mad River.	2 Spans through Pratt Truss.	140 6	9	15 $7\frac{1}{3}$	24 0	Skewed $\frac{1}{3}$ panel 31" deep, with flanges $10\frac{1}{4}$" across.	Iron I sec., 24" deep, flanges $10\frac{1}{4}$" across.	About level with top of lower chord.	9	Bridges continuous over middle pier. Lower chords of panel next to mid-pier are compression members.
3	Aug. 21 to 23.	do	69	About 6 miles N. E. of Dayton, O.	do	156 0	10	15 $7\frac{1}{5}$	24 0				16	
4	Oct. 16	Penn. R. R. Del. Exten. of Phila. Div.	Arsenal Bridge.	Over Schuylkill River.	Through Pratt Double Intersec.	189 7	20	9 $5\frac{3}{4}$	19 0	Floor beams directly on lower chord 4 per panel 8" by 12"	Stringers under rails; no ties.	Abt 24"	4	Has b'n condemned for two years.
5	Oct. 17.	Penn. R. R. Phil. & West. Chr. Div.	1, or Mayland B'g	About $\frac{1}{2}$ mile W. of So. St. Sta., Phila., Pa. Over Mayl'd Creek	Two separate Deck Pratt Trusses for double track. North truss.	128 3	9	14 3	16 6	Floor beams are from 18" to 2' from center to center; they are 8 by 14 in., and rest directly on lower chord	No stringers. Rails are laid directly on the floor beams.	21"	12	Guard rails are 8 by 8 inches.
6	do	do	do	do	South truss.	128 0	16	8 0	15 0			21"	11	
7	Oct. 18.	Phil. Wlm & Bal. R. R.	Chester Bridge.	600 or 800 ft. W of Chester. Over Chester Creek.	Through Pratt Double Track.	154 6	8	19 $3\frac{3}{4}$	26 0	3' 8" dp,; suspended.	Iron I sec., 22" deep, 6' apart c. to c.	On level with pins.	7	Very strong and stiff bridge.
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9	Nov. 21 and 22.	N. Y. P. & O. Main Line.	55	Just S. W. of Greencamp, O. Over the Big Scioto.	Through Pratt Truss.	150 0	10	15 0	24 0	Riveted to columns 36" dp.; flanges $10\frac{1}{4}$" across.	I sec., 26" dp.: flanges $9\frac{1}{4}$" across.	4'	7	
10	Nov. 24 to Dec. 24.	do	57	Between Claibourne and Broadw'y, O. Over Beans Creek.	do	148 0	10	14 $9\frac{3}{5}$	24 0	do	do	4'	32	
11	Nov. 29 to Dec. 15.	do	32	Abt. $2\frac{1}{2}$ miles W. of Leavittsburg, O. Over Mahoning River.	do	141 0	9	15 8	About 24'	Suspended iron I sec., about 30" deep.	3—8" by 18" wood; resting on floor beams.	1'	60	
12	Dec. 1.	N. Y. P. & O. Mahoning Div.	9	do	do	143 0	8	17 $10\frac{1}{2}$	24 0	Suspended iron I sec.	Iron I sec., 16" deep; riveted to floor beams.	On level with pins.	4	
13	Dec. 2 and 3.	N. Y. P. & O. Main Line.	29	About 1 mile E. of Orangeville, Pa.	Through Pratt Double Intersec.	156 6	11	14 $2\frac{3}{4}$	About 24'	Suspended iron I sec.	Wood 16" dp.: resting on floor beams.	16"	13	

Total number of bridges, 13.

Total number of diagrams, 193.

TABLE No. 2.

General Table, including Results from all Diagrams of Special Interest.

Serial number.	Bridge (see Table No. 1.)	Number of Diagram.	Train. Passenger or freight.	Number of cars.	Number of cars from Columns 12 and 15.	Engine. Class and length, pilot to buffer.	Engine. Diameter of drivers.	Speed. Miles per hour.*	Abscissa, or distance from initial point of diagram.	Ordinate, or statical deflection of bridge.‡	Amplitude of vibration.‡	Percentage of statical deflection superadded by vibration.	Number of vibrations per inch of diagram.† Observed.	Number of vibrations per inch of diagram.† Calculated.	Number of half-car lengths per inch of diagram.	Inches of diagram per train.	Number of revolutions of engine drivers per inch of diagram.	Lateral movements. Number of vibrations per inch of diagram.†	Lateral movements. Amplitude.‡	Remarks on movements of bridge. Vertical.	Remarks on movements of bridge. Lateral.
0		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
							Inches.		Inches.	Inches.	Inches.	Per cent									
18	2	4	E. B. Pass	5	....	Pass. Eng..	66	....	0.32	0.70	0.40	28.6	14.5	....	....	1.4	....	....	0.25	Cumulative.	
20	2	6	W. B. "	7 or 8	....	" 48'	66	46.5	0.30	0.60	0.28	23.3	15.5	....	....	....	16.2	....	0.15	"	
21	2	7	E. B. "	6	....	" 48'	66	37.9	0.40	0.72	0.35	17.3	10.	....	....	....	13.2	6.	0.3	"	
22	2	8	W. B. L. Ft.....	10 box and 1 coach.	10 box and 1 coach.	Frt. Eng.........	..	28.8	0.60	0.72	0.14	9.7	....	....	12.5	....	....	....			
..	..	..	......			......	..	....	1.00	0.54	0.30	29.8	13.	....	....	....	....	....	0.10	"	Irregular.
..	..	..	......			......	..	....	1.50	0.40	0.10	12.5	....	....	....	....	....	....			
23	2	9	W. B. Pass......	....	....	Pass. Eng........	66	57.0	0.30	0.68	0.30	22.0	19.	....	....	....	19.8	....	....	"	
27	3	4	W. B. "	4	....	"	66	46.6	0.30	0.73	0.26	17.8	16.	....	....	....	16.3	....	0.80	"	
29	3	6	W. B. "	6	....	" 48'	66	47.6	0.27	0.66	0.28	21.2	17.	....	....	....	16.6	....	0.50	"	
30	3	7	E. B. "	4	....	" 48'	66	42.7	0.35	0.76	0.30	19.8	14.	....	....	....	14.9	....	....	"	
31	3	8	W. B. L. Ft.....	..	....	Eng. No. 92	..	Low.	1.40	0.40	0.15	18.7	16.	....	....	....	....	....	....	Slightly cumulative.	
..	..	..	......	..	....	......	..	....	....	0.00	....	....	20.	....	....	....	....	....	....		
32	3	9	W. B. Pass.....	4	....	Pass. Eng........	66	46.2	0.30	0.81	0.30	18.5	16.	....	....	....	15.4	....	. . .	Cumulative.	
39	3	16	W. B. "	4	....	"	66	48.7	0.26	0.71	0.14	9.6	17.4	....	....	....	17.0	....	....		
44	5	1	W. B. "	2	....	Eng. No. 202.....	..	....	....	0.94	0.30	15.9	....	....	....	0.1	....	....	0.44	"	Not smooth.
48	5	5	W. B. "	4	....	" 202.....	..	....	0.35	0.88	0.28	15.9	....	....	....	0.7	....	....	0.30	"	
49	5	6	W. B. "	1	....	" 216.....	..	....	0.30	0.94	0.26	13.8	....	....	....	0.2	....	8.	0.45	"	
61	6	6	E. B. Ft........	8	8.9	" 216.....	..	Abt. 40.	0.40	1.40	0.32	11.4	13.5	....	13.5	1.4	....	No latl.	....	"	
..	..	..	......	..	....	......	..	....	0.65	0.80	0.30	18.7	17.	....	for box	....	....	....	....	"	
..	..	..	......	..	....	......	..	....	1.20	0.74	0.06	...	....	....	cars.	....	....	....	....	"	
65	6	10	E B. Pass......	3	....	Eng. No. 213.....	..	....	0.40	1.35	0.26	9.6	13.	....	....	0.9	....	....	....		
85	10	1	W. B "	5	....	Pass. Eng........	..	....	0.37	0.78	0.26	16.7	16.	....	....	1.0	....	....	....	"	
89	10	5	W. B	5	....	"	..	....	0.38	0.78	0.30	19.2	15.2	....	....	1.1	....	....	....	"	
96	11	2	W. B. Frt......	..	....	Condn. 50'	48	....	....	·1.00	0.38	19.0	. ..	....	....	....	....	....	....	"	
103	11	9	W. B. "	..	....	......	..	....	. .	0.72	0.20	13.9	....	....	....	....	....	....	....	slightly.	
124	13	12	W. B. Pass.....	..	....	Std. H. P. 48'	66	....	0.55	0.83	0.38	23.4	....	....	....	....	....	....	....	"	
126	11	1	E. B. Ft........	33	....	Ft. Eng..........	..	21.1	0.52	0.96	0.12	6.5	....	....	....	7.8	....	....	0.30	Sinuosities irregular.	
..	11	..	......	..	....	......	..	....	2.52	0.67	0.10	7.4	11.0	11.0	8.8	....	....	....	0.20	" "	
129	11	4	E. B. Pass......	10	....	Std. H. P. 48'	66	33.1	0.37	0.82	0.30	18.2	Abt. 13.	13.3	....	2.9	11.0	....	0.40	Cumulative.	
..	11	..	......	..	....	......	..	....	1.80	0.47	0.05	5.3	....	....	....	....	....	....	....		
..	11	..	......	..	....	......	..	....	....	....	...	....	....	....	....	....	....	6.2	....		
130	11	5	E. B. Ft........	32	....	Ft. Eng..........	..	23.4	0.55	0.95	0.10	5.3	....	....	....	7.0	....	....	0.20	......	Sin. irregular.
..	11	..	......	..	....	......	..	....	3.67	0.70	0.13	9.3	11.9	10.9	9.5	....	....	....	0.15	......	"
..	11	..	......	..	....	......	..	....	7.82	....	....	....	....	....	....	...	....	....	....		
131	11	6	E. B. Ft........	30	....	Condn. 50'	48	17.8	0.80	0.97	0.05	2.6	....	12.6	....	8.7	7.8	....	0.25	Sinuosities irregular.	"
..	11	..	......	..	....	......	..	....	3.25	0.75	0.07	5.0	....	....	....	....	....	....	0.06	Slightly cumulative.	"
..	11	..	......	..	....	......	..	....	3.60	0.67	0.16	12.0	12.6	11.0	7.3	....	....	....	0.18	" "	"
..	11	..	......	..	....	......	..	....	3.95	0.70	0.05	3.6	....	....	....	....	....	....	0.15	" "	"
..	11	..	......	..	....	......	..	....	9.75	....	Small.	...	22.0	22.0	....	....	....	....	....	......	"
132	11	7	W. B. Ft.......	30	....	Ft. Eng..........	..	21.6	0.60	1.05	0.22	10.7	....	....	8.7	7.2	....	....	0.30	Cumulative.	"
..	11	..	......	..	....	......	..	....	2.80	0.63	0.10	7.8	11.8	11.2	....	....	....	....	0.15	Sin. broken into on both sides.	"
..	11	..	......	..	....	......	..	....	5.88	0.90	0.15	8.3	9.5	9.8	....	....	....	....	0.25		
133	11	8	W. B. Ft.......	11	....	Ft. Eng..........	..	23.2	0.59	1.17	0.07	3.1	....	....	....	2.8	....	....	0.25		
..	11	..	......	..	....	......	..	....	2.25	0.70	0.21	15.0	10.0	10.9	8.8	....	....	....	....	Sinuosities regular.	
134	11	9	W. B. Pass.....	4	....	Std. H. P. 49' 7"..	68	34.5	0.35	0.89	0.29	16.2	13.3	13.0	....	1.2	11.5	Abt. 4.	0.45	Cumulative.	Sin. nearly reg.
136	11	11	W. B. Ft.......	32	....	Ft. Eng..........	..	23.3	0.62	1.06	0.17	8.2	Abt. 9.7	....	....	7.1	....	....	0.25		
..	11	..	......	..	....	......	..	....	4.52	0.75	0.16	10.8	10.0	10.6	9.5	....	....	....	0.10		
137	11	12	E. B. Ft........	30	32.	Condn. 50'	48	26.1	0.50	0.97	0.12	6.4	....	12.7	....	6.1	11.9	5.7	0.33	......	Sin. irregular.
..	11	..	......	..	....	......	..	....	1.12	0.52	0.13	13.0	....	12.1	....	....	....	5.5	0.20	Highly cumulative, begins at end of 3 33' cars. Where 14 28' box cars begin and breaks up at 2 33' stock cars.	
..	11	..	......	..	....	......	..	....	2.30	0.72	0.46	33.2	11.0	10.7	10.7	....	....	....	0.15		
..	11	..	......	..	....	......	..	....	2.90	0.65	0.25	19.2	11.0	11.2	for box.	....	....	4.5	0.30		"
..	11	..	......	..	....	......	..	....	3.35	0.70	0.42	30.4	11.0	10.9	....	....	....	....	0.10		
..	11	..	......	..	....	......	..	....	3.88	0.70	0.02	1.8	....	10.9	9.1	....	....	....	0.18		
..	11	..	......	..	....	......	..	. ..	6.77	....	Small.	....	22.0	22.0	for	....	....	....	....		
139	11	14	E B. Ft........	28	....	Ft. Eng..........	..	24.0	0.60	1.02	0.10	4.9	....	....	stock.	6.8	....	....		Sin. regularly broken.	"
..	11	..	......	..	....	......	..	....	1.45	0.75	0.15	10.0	9.5	10.6	8.7	....	....	....	0.20	......	"
..	11	..	......	..	....	......	..	....	4.90	0.67	0.02	1.8	....	....	....	....	....	4.5	0.20	......	"
140	11	15	E. B. Ft........	31	31.	Condn. 50'	48	26.6	0.52	0.97	0.30	15.4	12.3	12.7	....	5.9	12.4	....	0.35	......	"
..	11	..	......	..	....	......	..	....	1.00	0.37	0.04	4.9	...	....	....	....	....	....	0.10	......	"
..	11	..	......	..	....	......	..	....	1.62	0.71	0.26	18.4	11.0	10.8	10.9	....	....	....	0.12	Cumulative.	"
..	11	..	......	..	....	......	..	....	2.10	0.57	0.06	5.3	....	....	....	....	....	....	0.18	......	"
..	11	..	......	..	....	......	..	....	2.55	0.62	0.27	22.0	11.0	11.3	10.9	....	....	....	0.22	Cumulative.	"
..	11	..	......	..	....	......	..	....	3.65	0.53	0.05	4.6	....	....	....	....	....	....	0.10	......	"
..	11	..	......	..	....	......	..	....	4.78	0.66	0.07	5.7	....	....	....	....	....	....	0.25	"	"
..	11	..	......	..	....	......	..	....	5.68	0.75	0.46	30.8	10.7	10.6	10.9	....	....	....	0.10	......	"
..	11	..	......	..	....	......	..	....	6.25	0.65	0.12	9.6	....	....	....	....	....	....	....	Cumulative.	"
141	11	16	E. B. Pass......	4	....	Pass. Eng........	..	35.9	0.72	0.87	0.31	17.8	13.5	13.0	...	1.2	....	....	0.30	"	"
..	11	..	......	..	....	......	..	....	0.90	0.45	0.02	....	....	....	....	....	....	....	....	Sinuosities irregular.	"
144	11	19	W. B Ft........	6	....	8-wheel Ft.......	57	....	0.49	0.84	0.07	....	....	....	....	1.4	....	....	0.15	" "	"
..	11	..	......	..	....	49' 4" "	..	....	1.38	0.32	0.07	....	Abt. 15.	14.1	10.6	....	....	....	0.15	......	"
145	11	20	W. B. Ft........	32	....	Ft. Eng..........	..	....	0.45	1.07	0.20	9.3	....	....	....	6.0	....	....	0.25	Sin. irregularly broken.	"
..	11	..	......	..	....	......	..	....	1.80	0.40	0.07	....	Abt. 11.6	13.2	10.2	....	....	5.3	0.12	" "	"
..	11	..	......	..	....	......	..	....	4.80	0.55	0.15	....	" 11.5	11.9	10.2	....	....	4.7	0.18	" "	"
147	11	22	E. B. Pass......	4	....	Std. H. P. 49' 7"..	68	39.5	0.30	0.87	0.25	14.3	" 13.3	13.0	....	1.1	12.8	....	0.30	" "	"
148	11	23	E. B. Ft........	10	10½	" 49' 4"..	57	....	0.50	0.75	0.02	....	....	....	....	2.2	....	....	0.20	......	"
..	11	..	......	..		......	..	....	0.87	0.50	0.06	....	....	....	....	....	....	5.7	0.20	Cumulative.	Sin. regular.
..	11	..	......	..		......	..	....	1.75	0.52	0.25	23.8	11.1	12.1	10.6	....	....	....	0.10	"	"
..	11	..	......	..		......	..	....	2.35	0.30	0.02	....	....	....	....	....	....	....	0.10	"	"
149	11	24	W. B. Pass.....	3	{See columns 6, 15, and 16.	Pass. Eng........	..	....	0.42	0.75	0.16	10.8	12.3	13.5	....	1.2	....	....	0.15	Sin. irregularly broken.	"
..	11	..	......	..		......	..	....	1.84	0.00	Small.	....	22.0	22.0	....	....	....	....	....		
150	11	25	W. B. Ft...	Abt. 35		Ft. Eng..........	..	....	0.60	1.15	0.17	7.6	11.0	12.5	....	6.7	11.8	....	0.30	" "	
..	11	..	......	..		......	..	....	4.48	0.52	0.15	14.3	10.2	12.1	Abt.10.8	....	....	....	0.10		
151	11	26	E. B. Pass......	9		Std. H. P. 48'	66	29.8	0.40	0.75	0.25	16.7	12.0	13.6	....	2.9	10.0	4.0	0.42	Cumulative.	"
155	11	30	W. B. "	4	4.	" 49' 7"..	68	....	0.37	0.87	0.25	14.2	13.5	13.0	....	1.3	11.0	....	0.25	"	"
157	11	32	W. B. Ft.......	34	36.	Condn. 50'	48	23.0	0.67	1.12	0.05	2.2	....	12.4	9.4	7.6	10.6	....	0.25		
..	11	..	......	..	....	......	..	....	1.15	0.82	0.20	12.1	10.0	10.4	9.4	....	....	6.0	0.16	Slightly cumulative.	"
..	11	..	......	..	....	......	..	....	1.57	0.52	0.05	4.8	....	12.1	9.4	....	....	....	0.20		
..	11	..	......	..	....	......	..	....	2.50	0.75	0.15	10.0	....	....	....	....	....	....	0.15	"	
..	11	..	......	..	....	......	..	....	2.90	0.55	0.15	13.6	....	....	....	....	....	....	0.20		
..	11	..	......	..	....	......	..	....	3.45	0.72	0.07	5.2	Abt. 8.7	10.7	9.4	....	....	5.5	0.12	......	"
..	11	..	......	..	....	......	..	....	4.00	0.67	0.20	14.7	....	11.0	9.4	....	....	....	0.15		
..	11	..	......	..	....	......	..	....	4.30	0.47	0.05	5.3	....	....	....	....	....	....	0.10	Bridge very quiet..	
158	11	33	E. B. Ft	20	21¼	Ft. Eng..........	..	23.0	0.62	0.80	0.10	6.2	....	13.4	....	4.6	10.0	....	0.20		
..	11	..	......	..	....	......	..	....	2.35	0.75	0.22	14.9	10.0	10.6	9.4	....	....	....	0.18	Slightly cumulative.	
..	11	..	......	..	....	......	..	....	3.40	0.67	0.10	....	....	10.0	....	....	....	6.2	0.20	"	"
159	11	34	W. B. Ft........	..	....	Ft. Eng..........	..	....	0.44	0.80	0.25	15.6	Abt. 11.5	....	....	2.0	12.5	....	0.35		
160	11	35	E. B. "	11	11.	Std. 48' 7".......	55	31.0	0.42	0.72	0.05	3.4	....	13.8	....	2.0	12.4	....	....		
..	11	..	......	..	....	......	..	....	0.70	0.50	0.05	5.0	....	12.3	....	....	....	....	....	Cumulative.	
..	11	..	......	..	....	......	..	....	0.90	0.50	0.25	25.0								"	
..	11	..	......	..	....	......	..	....	1.20	0.33	0.20	26.3	Average 12.5	Abt. 13.5	12.7	....	....	....	....	"	
..	11	..	......	..	....	......	..	....	1.70	0.40	0.25	31.2								"	
..	11	..	......	..	....	......	..	....	2.00	0.27	0.05	9.1	....	14.8	....	....	....	....	....	Load rapidly decreasing.	
..	11	..	......	..	....	......	..	....	2.45	0.00	Small.	....	21.3	22.0	....	....	....	....	....		
162	11	37	E. B. Pass......	10	....	Pass Eng........	..	....	0.38	0.80	0.22	14.1	Abt. 12.6	13.4	....	2.7	14.0	....	0.40	Cumulative.	Irregular.
164	11	39	E. B. Ft........	33	34.	Condn. 50'	48	23.9	0.57	0.95	0.17	9.2	....	12.7	....	7.2	11.1	....	0.16	Sinuosities broken.	
..	11	..	......	..	....	......	..	....	0.95	0.65	0.07	5.8	10.3	11.2	9.7	....	....	5.9	0.16	Cumulative. Load slowly increasing.	Regular.
..	11	..	......	..	....	......	..	....	2.70	0.85	0.35	20.6	10.0	10.1	9.7	....	....	....	0.10		
..	11	..	......	..	....	......	..	....	3.27	0.45	0.02	2.8	....	12.7	9.7	....	....	....	0.10	do	
..	11	..	......	..	....	......	..	....	3.70	0.25	....	....	20.0	....	....	....	....	....	....		
..	11	..	......	..	....	......	..	....	6.50	0.72	0.05	3.4	....	10.7	9.7	....	....	....	0.20	Cumulative. Load rapidly falls off and vibration ceases.	
..	11	..	......	..	....	......	..	....	7.05	0.74	0.27	18.6	10.7	10.7	9.7	....	....	....	0.12		
..	11	..	......	..	....	......	..	....	7.50	0.40	0.05	6.2	....	13.3	....	....	....	....	0.10		
165	11	40	W. B. Pass.....	4	....	Std. H. P. 49' 7"..	68	....	0.38	0.87	0.25	14.3	12.0	13.0	....	1.3	11.0	5.0	0.38	Cumulative.	Regular.
167	11	42	W. B. Ft.......	29	32.	Ft. Eng..........	..	24.1	0.25	1.00	0.05	2.5	....	12.5	....	6.2	9. to 11.	....	....		
..	11	..	......	..	....	......	..	....	1.55	0.55	0.10	9.1	....	11.9	9.9	....	....	....	....		
..	11	.	......	..	....	......	..	....	2.37	0.70	....	....	11.0	....	9.9	....	....	....	....		
..	11	..	......	..	....	......	..	....	2.67	0.37	0.37	50.0	11.8	13.5	9.9	....	....	....	....	Cumulative, with load rapidly falling off,	
..	11	..	......	..	....	......	..	....	3.02	0.30	0.07	12.5	....	14.4	9.9	....	....	....	....		
..	11	..	......	..	....	......	..	....	3.91	0.40	0.17	21.9	....	13.2	9.9	....	....	....	....		
..	11	..	......	..	....	......	..	....	6.60	0.00	....	....	....	....	....	....	....	....	....		
171	11	46	E, B. Pass......	4	....	Pass. Eng........	..	....	0.35	0.87	0.29	16.4	13.5 to 15.	13.0	....	1.1	14.0	....	....		
172	10	1	E B. "	5	....	"	..	39.8	0.33	0.72	0.27	19.0	15.0	14.0	....	1.3	15.5	....	0.25	Cumulative.	Sin. irregular.
173	10	2	W. B. "	5	....	" 48'	66	40.8	0.37	0.77	0.35	22.6	13.3	13.6	....	1.3	13.5	5.5	0.45	"	Sin. regular.
176	10	5	E. B. Ft........	2	....	Std. Ft. 46'.	63	17.9	0.95	0.82	0.10	6.1	12.0	13.4	....	....	6.2	....	0.10	......	Sin. irregular.
..	10	..	......	..	....	......	..	....	2.48	....	Small.	....	22.5	24.5	....	....	....	....	....		
179	10	8	E. B. Pass......	5	....	Pass. Eng	66 ?	45.1	0.29	0.75	0.20	13.3	17.0	13.8	....	1.1	15.0	....	0.30	Cumulative.	"
182	10	11	W. B. Pass.....	5	....	"	66 ?	48.2	0.34	0.75	0.32	21.7	15.0	13.8	....	1.1	16.2	....	0.25	"	
183	10	12	W. B. Ft.......	Abt. 21, and 1 pay car.	....	Std. Ft...........	63	37.3	0.46	0.75	0.15	10.0	12.5	13.8	....	3.0	13.0	....	0.30	......	"
..	10	..	......		....	46'	..	....	1.50	0.42	0.15	17.6	13.5	14.2	...	....	....	6.5	0.15	Cumulative.	"
..	10	..	......		....	......	..	....	3.65	....	Small.	....	24.6	24.5	....	....	....	....	....		
185	10	14	W. B. L. Ft.....	10 ft. and 1 coach.	....	Std. 47' 8".......	63	43.0	0.38	0.67	0.15	11.1	13.3	14.3	....	1.7	15.0	....	0.25	......	"
..	10	..	......		....	......	..	....	1.58	0.30	0.12	20.8	29.0	15.8	15.4	....	....	6.0	0.12	Cumulative	
186	10	15	W. B. Pass.....	5	....	Pass. 48'	66	51.1	0.30	0.75	0.30	20.0	16.0	13.8	....	1.1	17.0	....	0.25	"	
187	10	16	E. B. "	5	....	" Eng.......	66 ?	55.3	0.22	0.74	0.20	13.6	17.7	13.9	....	1.0	18.4	....	0.32	"	
189	10	18	W. B. Ft.......	24 ft. and 1 coach.	....	Std Ft...........	61	26.7	0.55	0.70	0.10	7.1	....	14.1	....	5.7	9.6	....	0.22	......	"
..	10	..	......		....	47'	..	....	2.60	0.25	0.10	20.0	18.0	16.7	9.6	....	....	6.2	0.04	Cumulative.	
..	10	..	......		....	......	..	....	3.65	0.61	0.05	....	....	....	....	....	....	.. .	0.15	......	"
192	10	21	W. B. Pass.....	..	....	Pass. 48'	66	42.1	0.38	0.72	0.32	22.1	12.8	14.0	....	1.6	14.0	....	0.40	Cumulative.	"

* Speed for Nos. 18 to 39 inclusive, obtained by signals and measured base.
Speed for Nos. 129 to 172 inclusive, obtained from columns 3 and 15.
Speed for Nos. 173 and following, obtained from diameter and revolution of engine drivers.

† To agree with revolution of engine drivers, half-car lengths, panel lengths, etc., the vibrations are here given as "complete" or "double" vibrations, one of which includes two "simple" vibrations, or includes a forward and backward movement.

‡ Values given in this table were measured from the diagram. Multiply by 0.64 to obtain the actual movement of bridge in inches.

NOTE.—In column 2, for "W. B.," read "west bound," and for "E. B.," read "east bound." For "L. Ft.," read "local freight."
In column 5, for "Std. H. P.," read "standard heavy passenger engine," and for "Condn.," read "consolidation engine."
The percentages in column 11 were obtained by dividing half the amplitude of vibration by the statical deflection, thus giving the percentage of superadded deflection, or giving strain on bridge, as superadded by vibration, in percentage of statical strain due to live load.

TABLE No. 3.

Showing Cumulative Vibration of Railway Bridges from a Passing Engine, as Due to Counterbalance in Drivers and Distribution of Floor Beams

Vol. XVI, p. 62.

Serial number.	Bridge (See Table No. 1.)	No. of Diagram.	Train. Passenger or freight.	Engine. Class and length, pilot to buffer.	Engine. Diam. of drivers.	Speed. Miles per hour.	Abscissa or distance from initial point of diagram.	Ordinate or statical deflection of bridge.*	Amplitude of vibration.*	Percentage of statical deflection superadded by vibration.	Observed number of complete vibrations per inch of diagram.	Number of floor beams passed per inch of diagram.	Number of revolutions of engine drivers per inch of diagram.	Remarks.
0	1		2	5	6	7	8	9	10	11	12	22	16	
					Inches		Inches.	Inches.	Inches.	Per cent.				
18	2	4	E. B. Pass..	Pass. Eng............	66	....	0.32	0.70	0.40	28.6	14.5	....	....	
20	2	6	W. B. “ ..	“ 48'.........	66	46.5	0.30	0.60	0.28	23.3	15.5	17.9	16.2	
23	2	9	W. B. “ ..	“	66	57.5	0.30	0.68	0.30	22.0	19.0	22.0	19.8	
27	3	4	W. B. “ ..	“	66	46.6	0.30	0.73	0.26	17.8	16.0	18.0	16.3	Flooring very rigid.
29	3	6	W. B. “ ..	“ 48'.........	66	47.6	0.27	0.66	0.28	21.2	17.0	18.4	16.6	
32	3	9	W. B. “ ..	“	66	46.2	0.30	0.81	0.30	18.5	16.0	17.2	15.4	
124	13	12	W. B. “ ..	Std. H. Pass., 48'.....	66	....	0.55	0.83	0.38	23.4	......	....	....	
129	11	4	E. B. “ ..	“ 48'.....	66	23.1	0.37	0.82	0.30	18.2	abt. 13.0	12.2	11.0	
134	11	9	W. B. “ ..	“ 49' 7"..	68	34.5	0.35	0.89	0.29	16.2	13.3	12.7	11.2	
140	11	15	E. B. Fgt...	Consolidn., 50'.......	48	26.6	0.52	0.97	0.30	15.4	12.3	9.9	12.4	
141	11	16	E. B. Pass..	Pass. Eng............	....	35.9	0.27	0.87	0.31	17.8	13.5	....	....	
147	11	22	E. B. “ ..	Std. H. Pass., 49' 7"...	68	39.5	0.30	0.87	0.25	14.3	abt. 13.3	14.5	12.8	Flooring not rigid.
151	11	26	E. B. “ ..	“ 48'	66	29.8	0.40	0.75	0.25	16.7	“ 12.0	10.9	9.9	
155	11	30	W. B. “ ..	“ 49' 7" ..	68	....	0.37	0.87	0.25	14.2	13.5	12.5	11.0	
159	11	34	W. B. Fgt..	Fgt. Eng............	....	....	0.44	0.80	0.25	15.6	abt. 11.5	....	12.5	
162	11	37	E. B. Pass..	Pass. Eng............	....	....	0.38	0.80	0.22	14.1	“ 12.6	....	14.0	
165	11	40	W. B. “ ..	Std. H. Pas., 49' 7"...	68	....	0.38	0.87	0.25	14.3	12.0	18.2	16.0	
171	11	46	E. B. “ ..	Pass. Eng............	....	....	0.35	0.87	0.29	16.4	13.5 to 15	....	14.0	
172	10	1	E. B. “ ..	“	66 ?	39.8	0.33	0.72	0.27	19.0	15.0	....	15.5	
173	10	2	W. B. “ ..	“ 48'.........	66	40.8	0.37	0.77	0.35	22.6	13.0	15.8	13.5	
179	10	8	E. B. “ ..	“	66 ?	45.1?	0.29	0.75	0.20	13.3	17.0	17.5	15.0	
182	10	11	W. B. “ ..	“	66 ?	48.2?	0.34	0.75	0.32	21.7	14.0	18.9	16.2	Flooring rigid.
186	10	15	W. B. “ ..	“ 48'.........	66	51.1	0.30	0.75	0.30	20.0	16.0	19.8	17.0	
187	10	16	E. B. “ ..	“	66 ?	55.3	0.22	0.74	0.20	13.6	17.7	21.5	18.4	
192	10	21	W. B. “ ..	“ 48'.........	66	42.1	0.38	0.72	0.32	22.1	12.8	16.3	14.0	

* Values given in table were measured from diagram. Multiply by 0.64 to obtain the actual movement of bridge in inches.

Sho

Serial number.
0
22
61
9
10
10
13
14
1
1

TABLE No. 4.

Showing Cumulative Vibrations of Bridges for Passing Trains, as Due to the Speed, the Car, and Panel Length.

Vol. XVI, p. 62.

Serial number.	Bridge (See Table No. 1.)	No. of Diagram.	Train, Passenger or Freight.	Number of Cars.	Number of Cars from Columns 12 and 15.	Length of Engine, Pilot to Buffer.	Speed, Miles per hour.	Abscissa or distance from initial point of diagram.	Ordinate or statical deflection of bridge.*	Amplitude of vibration.*	Percentage of statical deflection superadded by vibration.	Number of complete vibrations per inch of diagram. Observed.	Calculated.	No. of half-car lengths per inch of diagram.	Inches of diagram per train.	Remarks.
0	1		2	3	4	5	7	8	9	10	11	12	13	14	15	19
			Freight.			Ft. In.		In.	In.	In.	Per Ct.					
22	2	8	W. B. Lo.	10 box	10 box	....	28.8	0.60	0.72	0.14	9.7					Cumulative shown on diagram as
..	..	..	......	and 1	and 1	....	....	1.00	0.54	0.30	29.8	12½ or 13.	....	12.5	..	immediately following the en-
..	..	..	......	coach.	coach.	....	....	1.50	0.40	0.10	12.5					gine.
61	6	6	E. B.	8	....	....	....	0.40	1.40	0.32	11.4	13.5	....	13.5	1.4	do.
..	..	..	......	....	....	....	....	0.65	0.80	0.30	18.7	17.0	....	for box	..	do.
..	..	..	......	....	....	....	....	1.20	0.74	0.06	....	....	....	cars.	..	do.
98	11	4	W. B.	15	....	....	....	....	0.62	0.28	22.5	....	....	....	..	Cumulative.
103	11	9	W. B.	....	....	....	....	....	0.72	0.20	13.9	....	....	....	..	do. slightly.
107	11	13	E. B.	15	....	50 0	....	....	0.79	0.34	21.5	....	...	....	..	do.
137	11	12	E. B.	30	32	50 0	26.1	1.12	0.52	0.14	13.1	....	12.1	....	6.1	Cumulative, beginning at end of
..	..	..	......	....	....	....	....	2.30	0.72	0.46	33.2	11.0	10.7	10.7	..	3 33′ cars, where 14 28′ box-cars
..	..	..	......	...	....	....	....	2.90	0.65	0.25	19.2	11.0	11.2	for box.	..	begin, and break up at 2 33′
..	..	..	......	....	....	....	....	3.35	0.70	0.42	30.4	11.0	10.9	....	..	stock cars. See 137, on Plate VI.
..	..	..	......	....	....	....	....	3.88	0.70	0.03	1.8	....	10.9	9.1	..	do.
														for stk.		do.
..	..	..	......	....	....	....	....	6.77	0.00	Small	....	22.0	22.0		..	
140	11	15	E. B.	31	31	50 0	26.6	1.00	0.37	0.04	4.9	....	....		5.9	Cumulative. See 140, on Plate VI.
..	..	..	......	....	....	....	....	1.62	0.71	0.26	18.4	11.0	10.8	10.9	..	do.
..	..	..	......	....	....	....	....	2.10	0.57	0.06	5.3	....	....	for box	..	
..	..	..	......	....	....	....	....	2.55	0.62	0.27	22.0	11.0	11.3	do.	..	do.
..	..	..	......	....	....	....	....	3.65	0.53	0.05	4.6	....	....	....	..	
..	..	..	......	....	....	....	....	4.78	0.66	0.07	5.7	....	....	....	..	
..	..	..	......	....	....	....	....	5.68	0.75	0.46	30.8	10.7	10.6	do.	..	do.
..	..	..	......	....	....	....	....	6.25	0.65	0.12	9.6	....	....		..	do.
148	11	23	E. B.	10	10½	49 4	....	0.87	0.50	0.07	....	....	....		2.2	do.
..	..	..	......	....	....	....	....	1.75	0.52	0.25	23.8	11.1	12.1	10.6	..	do.
..	..	..	......	....	....	....	....	2.35	0.30	0.03	....	....	....	....	..	do.
160	11	35	E. B.	11	11	48 7	31.03	0.70	0.50	0.05	5.0	....	12.3	....	2.0	do.
..	..	..	......	....	....	....	....	0.90	0.50	0.25	25.0	Average				
..	..	..	......	....	....	....	....	1.20	0.38	0.20	26.3	12.5	to	12.7	..	Load slightly diminishing.
..	..	..	......	....	....	....	....	1.70	0.40	0.25	31.2					
..	..	..	......	....	....	..	....	2.00	0.27	0.05	9.1	....	14.8	....	..	Load diminishing rapidly.
..	..	..	......	....	....	....	...	2.45	0.00	Small.	....	....	22.0			
164	11	39	E. B.	33	34	50 0	23.88	0.98	0.65	0.08	5.8	10.3	11.2	....	7.2	Cumulative. See 164, on Plate V.
..	..	..	......	....	....	....	....	2.70	0.85	0.35	20.6	10.0	10.1	9.7	..	do.
..	..	..	......	....	....	....	....	3.27	0.45	0.02	2.8	....	12.7	....	..	do.
..	..	..	......	...	....	....	....	6.50	0.72	0.05	3.4	....	10.7	....	..	Cumulative. Load and vibration
..	..	..	......	....	....	....	....	7.05	0.74	0.27	18.6	10.7	10.7	9.7	..	soon diminish.
..	..	..	......	....	....	....	....	7.50	0.40	0.05	6.2	....	13.2	....	..	
167	11	42	W. B.	29	32	....	21.14	1.55	0.55	0.10	9.1	....	11.9	...	6.2	Cumulative. See 167, on Plate VII.
..	..	..	......	....	....	....	....	2.37	....	....	....	11.0	....	9.9	..	do.
..	..	..	......	....	....	....	....	2.60	0.37	0.37	50.0	11.8	13.5	9.9	..	do.
..	..	..	......	....	....	....	....	3.02	0.30	0.07	12.5	....	14.4	....	..	do.
..	..	..	......	....	....	....	....	3.90	0.40	0.18	21.9	....	13.2	....	..	Momentarily cumulative.
..	..	..	......	....	....	....	....	6.60	0.00							

* Values given in table were measured from diagram. Multiply by 0.64 to obtain the actual movement of bridge in inches.

TABLE No. 5.

Vol. XVI, p. 62.

Showing Actual Deflection and Time of Vibration of Railway Bridges.

						Engine center about ten feet past mid-span, followed by train.						Bridge uniformly loaded with ordinary train.				Bridge unloaded.		
Bridge. (See Table No. 1.)	Span.		Depth of Truss.		Weight of Bridge.	Weight of Engine.	Weight of Tender.	Length of Engine and Tender.		Maximum statical deflection for train. Actual.	Observed time of one complete vertical vibration.	Statical deflection. Actual.	Observed time of one complete vertical vibration.	Calculated time of one complete vertical vibration.	Observed time of one complete lateral vibration.	Observed time of one complete vertical vibration.	Observed time of one complete lateral vibration.	Remarks on vertical vibration for uniform load, and for an engine followed by its train.
	Ft.	In.	Ft.	In.	Pounds.	Pounds.	Pounds.	Ft.	In.	Inches.	Sec.	Inches.	Sec.	Sec.	Sec.	Sec.	Sec.	
2	140	6	24	0	227 500	72 150	43 500	48	0	0.38	0.26	0.23	....	...	0.87	Abt.0.18	0.55	Cumulative vibration observed for both engine and train.
										0.43	0.22	0.34	0.32	....	....	....	0.55	
										0.44	0.28	....	....	....	....	....	....	
						72 150	43 500	48	0	0.46	0.41	....	....	....	....	....	....	
3	156	0	24	0	269 259	72 150	43 500	48	0	0.42	0.24	0.24	0.26	....	0.91	0.18	0.63	Cumulative only for engines of passenger trains. Observed freight trains are slow here.
										0.46	0.26	0.36	0.39	....	0.69	0.16	0.69	
						72 150	43 500	48	0	0.48	0.29	0.42	0.37	....	0.73	0.20	....	
5	128	3	16	6	....	....	....	......		0.37	....	0.53	....	....	0.77	....	0.50	Cumulative for engine.
										0.40	....	....	....	....	....	....	0.47	
										0.40	....	....	....	....	....	....	0.45	
6	128	0	15	0	....	....	....	......		0.86	0.52	0.34	0.30	....	....	0.23	....	Cumulative for engine and for train.
										0.89	0.50	0.53	0.40	....	....	....	....	
7	154	6	26	0	....	....	....	......		0.04	Very small.	....	Very small.	....	Very small.	Very small.	Very small.	No vibrations exceeding one-tenth of an inch. Bridge exceedingly stiff.
										0.06		....		....				
										0.06		....		....				
8	135	0	17	6	....	....	....	......		1.66to2.0	....	....	....	....	....	....	....	Extraordinary deflection, but no considerable vibration.
9	150	0	24	0	....	....	....	......		0.45	....	0.25	....	....	....	....	....	One in the seven observations shows slight cumulative vibration for engine.
										0.45	....	0.25	....	....	....	....	....	
										0.55	....	0.44	....	.....	....	....	....	
										0.62	....	0.48	....	....	....	....	....	
10	148	0	24	0	223 575	70 600	39 200	47	8	0.43	....	0.16	0.22	0.23	0.63	0.16	0.58	Vibrations cumulative for engine, though only slightly so for freight trains.
						72 150	43 500	48	0	0.46	0.31	0.18	0.25	0.25	....	0.17	0.60	
										0.47	0.22	0.19	0.13	0.25	0.65	....	....	
						72 150	43 500	48	0	0.48	0.24	0.25	....	....	0.71	....	....	
						74 700	45 600	48	10	0.48	0.28	0.27	0.29	0.28	0.60	....	....	
										0.48	0.23	0.38	0.28	0.31	0.58	....	....	
						72 150	43 500	48	0	0.49	0.29	0.44	0.33	0.33	Abt. 0.6	....	....	
												0.51	....	....	0.98	....	....	
11	141	0	Abt. 24′		174 426	....	....	48	7	0.46	....	0.16	0.22	0.26	....	0.18	0.63	Remarkable cumulative vibration for freight trains, and none for passenger trains. Also cumulative for passenger engines, and rarely so for freight engines.
								48	0	0.48	0.33	0.24	0.33	0.29	....	0.18	0.63	
						73 000	45 000	49	4	0.48	....	0.24	0.31	0 29	....	0.18	0.58	
								48	0	0.52	0.30	0.26	....	0.30	0.75	0.18	0.58	
						73 000	45 000	49	4	0.53	....	0.33	0.35	0.32	....	....	....	
						81 600	52 400	49	7	0.56	0.29	0.38	0.29	0.34	....	....	....	
						81 600	52 400	49	7	9.56	0.29	0.40	0.36	0.35	....	....	....	
						81 600	52 400	49	7	0.56	0.33	0.41	0.36	0.35	0.75	....	....	
						81 600	52 400	49	7	0.56	0.29	0.43	....	0.35	0.78	....	....	
						100 000	46 500	50	0	0.60	....	0.43	0.31	0.36	0.62	....	....	
						100 000	46 500	50	0	0.62	....	0.43	0.33	0.36	0.65	....	....	
						100 000	46 500	50	0	0.62	....	....	....	....	....	....	....	
						100 000	46 500	50	0	0.62	0.32	0.44	....	0.36	0.65	....	....	
								50	0	0.63	....	....	....	....	....	....	....	
						100 000	46 500	50	0	0.71	....	0.45	0.35	0.36	0.68	....	....	
												0.46	....	0.36	0.65	....	....	
												0.47	0.37	0.37	....	....	....	
												0.48	0.37	0.37	....	....	....	
												0.50	....	0.37	0.83	....	....	
												0.54	0.39	0.39	....	....	....	

Note.—The bridge movements are given in this Table in actual values in inches.

constant allowance of 50 per cent. at least should be made for all spans, unless the trusses are so designed as to avoid vibration.

Influence of Style of Bridge on Vibration.

See remarks on "*Bridge Vibration and Oscillation*" on this point.

The Bollman, so far as observed, remains remarkably quiet under moving trains, while all Pratt trusses observed vibrated, though some but slightly. It is probable that all trusses with an upper and lower chord will vibrate, while others, like the Bollman, Fink, etc., will not.

General Conclusions.*

To avoid cumulative vibration in railroad bridges, it is essential:

First.—That the vertical component of non-balance of drivers be zero.

Second.—That the excess of non-balance of drivers be not down at mid-panel.

Third.—That the panel length and driver circumferences differ.

Fourth.—That the vibration of the engine on its springs, and of the bridge be discordant.

Fifth—That the revolutions of drivers and complete vibrations per second be unequal.

Sixth.—That the bridge panel and half-car lengths be unlike.

Seventh.—That the number of panels and vibrations per second disagree for a passing train.

Eighth.—That the stringers be rigid.

Ninth.—That sliding be substituted for expansion rollers.

Tenth.—That pedestals be not at neutral axis of bridge; that the stringers may be laid out into banks for friction; that the car springs should be so proportioned that the times of car and bridge vibration differ, and that the car loading be not uniform.

Eleventh.—That the possible vibrations by the engine are limited by length of bridge, and those by the train by length of train, unless the bridge jumps out of its seats.

[Acknowledgments are due to Mr. C. F. Marvin, mechanical engineer, for assistance in the construction of an efficient indicator, and for the first application of it to bridges. Also to Mr. H. L. Wilgus, B.S., for continued use of instrument, and to bridges in the East. And also to Mr. E. O. Ackerman, civil engineer, for the remarkable diagrams obtained under his hand, and his valuable assistance in working out final results.]

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* For a more extended discussion of some of these conclusions, and the bearings of the same upon the adoption of unit working stresses for railway bridges, see Transactions of this Society, Vol. XV, June, 1886, page 432.

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THE WATER SUPPLY, DRAINAGE AND SEWERAGE OF THE LAWRENCEVILLE SCHOOL.

By FREDERICK S. ODELL, M. Am. Soc. C. E.

READ JUNE 16TH, 1886.*

The Lawrenceville School, on the John C. Green foundation, situated at Lawrenceville, New Jersey, about midway between Princeton and Trenton, is a high school for boys.

A school was established at this point in 1809 by the Rev. Dr. Brown, which soon attained considerable reputation, which was not diminished after its transfer in 1845 to the Rev. S. M. Hamill, who conducted it until 1882, when the property was purchased by the Trustees of the large fund devised for educational purposes by the Hon. John C. Green, of Trenton.

A considerable portion of this fund has been devoted to the enlargement of the facilities afforded by the College of New Jersey, and the purchase of the Lawrenceville Institution was intended mainly to furnish a thoroughly equipped preparatory school for the course at Princeton.

* Revised to February, 1887.

The ground occupied by the school is nearly a parallelogram, with a frontage of 1 000 feet on the old post road from Trenton to Princeton, and sloping southeastwardly for about 2 100 feet to a small brook, which flows through the property a few feet from its boundary, and about forty feet lower than the north corner of the tract.

The surface at the time of its purchase was in its natural state, slightly undulating, chiefly occupied as farming land, entirely without artificial drainage. The school buildings were huddled together near the road about the center of the frontage, and consisted of a variety of stone and frame buildings, added from time to time within the sixty years of the life of the school, as occasion demanded, with more regard to economy of construction and convenience of access than to architectural effect. The water supply was drawn from wells on the premises, and the privies were of the ordinary rural type.

The trustees decided to bring the institution more into conformity with modern ideas, and secured the services of Mr. Frederick Law Olmsted to lay out the grounds; of Messrs. Peabody and Stearns to design the buildings; and of Mr. J. J. R. Croes, M. Am. Soc. C. E., to design the plans for the water supply and sewerage of the establishment and to construct the works for that purpose and also the roads. The writer was in charge of this construction.

The design of Mr. Olmsted comprises an elliptical lawn, of about 600 by 400 feet, with the main school building at the southern extremity of its longer axis, and with six masters' houses surrounding the ellipse, all facing the south and also looking on the lawn and towards the school building, which, being a memorial building of the founder, is the most prominent feature in the design, and is a very handsome and striking example of the Elizabethan Gothic style of architecture in brown sandstone. The houses are of pressed brick, and they are each to be occupied by the family of a master and a limited number of students. The house of the head master stands apart and nearer the road and the entrance to the grounds. Plate XIV shows the general plan.

Two of these houses are not to be built at present. The other six have been erected, and are now occupied.

There have also been erected on the grounds a gymnasium, a bath house, a boiler-house and laundry, and one of the old school buildings is retained as a dormitory and for business offices.

The arrangement, dimensions and character of occupancy of the buildings being determined by the landscape architect and the architects, the engineer was required to provide for the furnishing of a water supply and the removal of household wastes and the rain water, all of which it was desirable should be accomplished within the limits of the property owned by the school.

The water of the brook which traverses the property is not suitable for domestic use. The course of the stream is through cultivated lands, generally highly manured in the spring, and the flow is very small in the summer and exceedingly irregular at any time. It is a "quick" watershed, and after even moderate rainfalls the water is very turbid. As a surface supply under these circumstances was not practicable, for want of storage room and settling facilities, examinations were made looking towards a supply from the ground water.

In a swale near the south corner of the property there were indications of a supply from springs. Borings were made in that vicinity, and while they showed the presence of considerable water, analyses gave evidence of pollution from sewage, for which no good reason seemed to exist. This was very marked in the case of the boring which yielded the most copious supply.

This boring was in a slight depression towards the southeasterly portion of the grounds, and it was found on examination that near the beginning of this swale, and more than five hundred feet from the well, was the outlet of an old drain conveying the waste water from the kitchen and laundry of the old dormitory building; this explained the mystery of the contamination of the water in this well.

Further experiment with this well established the fact that when large draughts were made upon it, the impurities greatly lessened, and finally, after three days continuous pumping, a sample of water was taken that gave a very good analysis.

As it was thought to be proven that a well at this point would yield a sufficient supply of good potable water when the evident source of contamination should be removed, that location was selected for a well for a permanent water supply. A thorough system of subsoil drainage was put in, diverting into the brook all water from the surface and the upper strata of the soil, and eighteen months later samples of water taken from the same boring gave an excellent analysis, and it has ever since continued to be of very good quality.

Well.

A well 16 feet in diameter and 23 feet deep was sunk, the normal level of the water being 3 feet below the top. See Plate VIII.

The material encountered in the excavation was clay for 6 feet below the surface, and then loose brown sandstone which came out without blasting, and lay so loosely next the excavation that it became necessary to line the well with brick throughout; this was done, leaving weep holes at frequent intervals through the lower portion of the 8-inch brick lining, which was brought up to a dome at the top and keyed by the cast-iron frame of a man-hole cover 2 feet in diameter, the cover being perforated.

The water is drawn from the well by a Worthington steam pump in the boiler-house, 800 feet distant, through an 8-inch cast-iron suction pipe; the vertical pipe in the well being provided with a foot valve and air chamber. The suction pipe is laid level, and when the well is full the water flows to the pump by gravity.

The pumping-main is of cast-iron pipe of 6 inches diameter for 700 feet to the campus, where it branches, one line, of 6-inch pipe, following the line of the easterly buildings for 800 feet; the other branch, of 4-inch pipe, following the line of the westerly buildings and the north front of the property for 2 300 feet to a connection with the 6-inch line, where a 6-inch branch, 200 feet long, leads to the water tower. There is thus a complete circulation, whether the supply be taken from the tower or directly from the pump. There is a check valve near the pump and nine stop-cocks on the mains, with six fire hydrants so placed that with 400 feet of hose two streams of water can be thrown on any building.

The 6-inch pipes are 0.455 inch thick and weigh 29 pounds to the foot, and the 4-inch pipes are 0.393 thick and weigh 17 pounds to the foot.

It was part of the architects' design for the buildings that a library should be erected with a fine tower, and the engineer was desirous of utilizing this tower by placing in it a tank for water supply when the pump was not in operation. The Trustees, however, did not feel disposed to build a library at present, nor would they consent to putting a tank in the memorial building, nor were they willing to go to the expense of an ornamental structure for the water supply. The water tower erected, under instructions from them to make it as inexpensive as possible, is a plain cylindrical shaft of plate iron 10 feet in diameter and 85 feet high.

The plates are lapped at the joints, and riveted and caulked.

The vertical joints are lapped $3\frac{1}{2}$ inches and double riveted for 28 feet above the bottom, and other joints are lapped 2 inches and single riveted.

The plates are of $\frac{1}{4}$, $\frac{3}{16}$ and $\frac{1}{8}$-inch thickness, and the bottom is of $\frac{1}{4}$-inch plate.

The tower stands on a foundation of rubble masonry laid in cement-mortar, which is 17 feet in diameter and 8 feet deep.

Four lugs riveted to the tower are connected with wrought-iron bolts passing down through the masonry.

This tower is acknowledged to be, architecturally, a serious blemish in an otherwise harmonious and elegant design, but for this neither the architects nor the engineer are responsible.

The capacity of the well to furnish a full supply at all times was not considered by the designer of the works to be demonstrated with sufficient certainty to warrant the rejection of any auxiliary supply which might be found available. Moreover, the water from the sandstone, while not hard enough to be absolutely objectionable for steam-boilers and for the laundry, was not a very soft water. It was considered desirable therefore to utilize the rain-water which should fall on the 55 000 square feet of slate-roof surface of the buildings on the plateau. The collection of this water in a covered reservoir on the hill would, it was thought, serve three good purposes.

First, it would furnish a stored supply of good water for use in case of fire, or deficiency or temporary unfitness of the well-water. *Secondly,* it would furnish to the laundry and boiler-house, on the lower ground, a gravity supply of soft water; and *Thirdly,* it would afford a relieving reservoir to the drain pipes which should be laid to carry to the brook the rain water from the campus, which has an area of about 550 000 square feet.

The roof area being 10 per cent. of the total, while the area of the finished road surfaces is only about 10 per cent., the rest being in lawn, it was thought that in a heavy rainfall the subtraction from the drains of so large a proportion of the water which would reach them most quickly and certainly, without any loss from absorption, would warrant a reduction in the size of the outfall drain, which had to be carried about 1 700 feet to the brook.

The rain-water from the roofs was therefore led by an independent line of pipes to a reservoir built underground at a point near that building in the group which was nearest to the boiler-house.

The leaders from the roof are carried down to the depth of 4 feet underground, and the carriers to the reservoir are of 6, 8 and 10-inch vitrified salt-glazed stoneware pipe, laid true to line and grade, and the joints made with Portland cement.

The reservoir, plans of which are shown on Plate IX, is built in a pit excavated for the purpose. It is rectangular in shape, and the bottom is 16 feet below the surface of the ground. The interior dimensions of the reservoir are 36½ by 67 feet. The side walls are 18 inches thick, of rubble stone masonry laid in cement, and well backed against the sides of the excavation, which was in hard clay and soft rock.

Inside of the stone masonry is a 4-inch lining of brick with a 2-inch coating of cement-mortar between the brick lining and the stone wall, the whole being covered with brick arches in two spans of 17 feet 6 inches each. The bottom is of concrete with a surface coating of Portland cement. The interior surface of the brick lining was washed with a preparation of Castile soap and alum in solution to render it more impervious to water.

This preparation is the same which proved efficient on the walls of the Croton Reservoir Gate-house in 1862, and is fully described in the paper by Mr. W. L. Dearborn, Transactions American Society of Civil Engineers, Vol. I, p. 203.

There were used 10 pounds alum and 50 pounds Castile soap. Two coats of the preparation were applied. The surface coated was 2 700 square feet. The work took 18 days of labor of mason and his helpers, and cost $65, or about 2½ cents per square foot.

The capacity of this reservoir is 162 000 gallons, equivalent to nearly 5 inches of rainfall on the roofs which now feed it. It is provided with an overflow pipe to the pipes which carry the drainage of the roads and lawns.

In the reservoir is built a filter well, which consists of a circular 4-inch brick wall 9 feet in diameter, and inside of this is placed a 4-inch cast-iron suction pipe provided with a foot valve and an automatic air valve, and leading to the boiler-house 720 feet distant. The water from this reservoir was intended to be used in the laundry and for feeding the boilers, but can also be pumped directly into the mains and used in any emergency, such as a large fire, should other sources become exhausted.

When drawing from the filter well at the rate of 1 600 gallons an hour, the difference of level between the water in the reservoir and in the filter well is 3 feet. The advantage of having this reserve of water was demonstrated on January 20, 1887, when a fire occurred in the dwelling of Dr. Hamill on land adjoining the school grounds, and a fire stream was kept up from the nearest hydrant to the burning building for eight hours. A hole was broken in the filter well, and the water of both the well and reservoir used. Since then an 8-inch stop valve has been put in the filter well, to enable a free supply to be drawn from the reservoir in such an emergency.

Drainage.

That portion of the grounds in which the buildings are located is generally dry and needed little subsoil drainage, but it was deemed advisable to lay subsoil drains near the buildings, and in three cases entirely around the foundation walls below the level of the cellar floors, so as to insure their being dry at all times. Subsoil drains were also laid along the drives and walks, and the entire play-ground was underdrained by parallel lines of subsoil drains laid 30 feet apart.

These subsoil drains are of round agricultural tile, from one and one-quarter to two inches in diameter, and are laid on uniform grades about three feet below the surface, and have their outlet in the nearest road basin.

To provide for the surface drainage of the drives and grounds, a complete system of drains was laid, following the general direction of the drives, with catch basins opening from the gutters at intervals of about three hundred feet. These drains are of salt-glazed stoneware pipe from 6 to 8 inches diameter, with joints of Portland cement-mortar. They are laid about three feet six inches below the surface, to true lines and grades, and have their outlet in the brook at the lower portion of the grounds.

At the time the outfall system was designed it was thought that the large extent of lawn surface on very flat slopes, and the deduction of the roof area from the water-shed, would so materially diminish the discharge from the rainfall, that a capacity of carrying off 100 cubic feet per minute, or about an inch and a half of water on the road surfaces per hour, would be sufficient.

The experience of the first six months of 1886 showed that this was not sufficient, as the road drains were overtaxed three times during that period, causing pools of water to be formed for over an hour in some depressions of grade, and also causing the water to flow out through a man-hole on the lower level near the engine-house, and flood the boiler-room floor.

This was undoubtedly partly caused by two departures from the plans for constructing and operating the works.

First.—The side drainage of the road in front of the property was not completed according to the plans, and thus a large quantity of water flowed across the road and on the school grounds from an extended slope on the opposite side of the road.

Secondly.—The supply from the well having been plentiful, the steam engineer in charge of the boiler-house found it easier to draw all the water from that source than to open and shut the cocks which change the pump suction from the well to the rain-water reservoir, so that the latter was never used and all the roof water was discharged into the road drains at their connection with the outfall pipe.

But, even if due allowance is made for these irregularities, it is not unlikely that in the case of a heavy rainfall, when the ground on the campus is frozen, the capacity of the outfall would have been found to be too small. A direct connection has therefore been made between the junction of drains at the reservoir overflow-pipe and the pond, by a 12-inch pipe, making the total capacity of discharge 450 cubic feet a minute. The highway drains opposite to the school property have also been attended to, and the road water thus diverted from the grounds. So far (March, 1887) this has proved satisfactory in the heaviest rainfalls which have occurred since the pipe was laid, the rain-water reservoir having been used for the purpose for which it was intended, and the roof water consequently retained in it.

Sewerage.

The necessity of disposing of the sewage within a limited area of the grounds made it imperative that its volume be limited to a minimum, and therefore all surface or subsoil drainage was excluded from the sewers, and disposed of as previously related; then, to insure positive immunity from leaky joints, it was decided to use six-inch cast-iron pipe, with leaded joints, for the sewers.

The pipes were 0.395-inch thick, and weighed 25 pounds to the foot. They were coated with coal-tar varnish, as were all the cast-iron pipe used on the grounds. Details of castings are shown on Plate X.

There are two branch lines of sewers, with a flushing man-hole at the head of each. The lines of the sewers are selected to serve every building with as short house connections as possible, and all deflections are made by special curved pipe. A man-hole is placed at every change in line or grade, and access is had to the sewer through a tee at the bottom of the man-hole, and also at the junction of house connections with the main line where the Y branch has cast in connection with it a vertical tee, from which a pipe is carried up to the surface of the ground.

Any man-hole may be used for flushing purposes. The flushing and cleaning is done very effectually by using a "pill," or spherical hard-wood ball, 5¼-inches in diameter. This has proved more effective than one of smaller size.

The two branch sewers unite near the rain-water reservoir, and continue to the boiler-house and laundry, near which is placed the sewage tank, in which the solid matter in the sewage is allowed time to deposit itself on the bottom, and the partially clarified liquid is retained until it is desirable to discharge it into the sub-surface tiles.

Sewage Disposal System.

The sewage tank is built of brick-work underground, and is in two sections. The first or retaining section is in duplicate, and contains six compartments, three in each set. Each compartment is sixty feet long, about three feet wide and four feet deep. See Plate XI.

The sewage flows into one end of the first compartment, passes along its whole length, and at the other end passes into the second compartment through a quarter-bend pipe, with the mouth turned down below the level of the outlet, to prevent scum on the surface of the liquid from passing over into the second compartment, through which the liquid passes to its further end, and in like manner into the third, at the further end of which it passes over a weir into the receiving chamber, which is circular in form, twenty-five feet in diameter and eight feet deep. From this it is pumped by a pulsometer pump as often as necessary. This chamber is ventilated by a pipe leading into the flue of the boiler-house chimney. It is intended that whenever solids collect in such quantities that the settling compartments require cleaning, the sewage shall be turned in the duplicate set, and the sludge removed from the first.

It is found that nearly all the solids are deposited very near the entrance in the first compartment, and to cause the deposit to be distributed more evenly over the bottom, the water in the first compartment has been siphoned into the receiving chamber two or three times within the past six months. The rapid subsidence of the water, and the flow of incoming sewage during this operation, distribute the solids over the bottom, and enable the compartment to be used longer without cleaning out than would be the case if this distribution were not made.

The pulsometer has been so arranged that by attaching a suction hose, the water in the settling tanks can be pumped out and carried 300 feet through a hose to farm land ploughed to receive it. In January, 1887, the tanks were thus emptied, and the sludge then removed by a farmer to whom it had been sold. There were about 300 cubic feet of sludge removed from the first section of each of the settling tanks.

The irrigation ground comprises about one and three quarters acres, in the lower part of the school grounds, between the boiler-house and the brook. It is still further limited in location by the dam and pond on the westerly side, and an adjoining owner on the easterly side. It is the lowest portion of the school property, is naturally wet, and that portion near the brook (before drainage) was swampy. Its selection was a matter of necessity, it being all the land available for this purpose.

The natural surface of the ground was on a quite uniform slope from the higher portion to the brook, so that very little surface grading was necessary, but its thorough subsoil drainage became of the greatest importance.

To accomplish this, parallel lines of 2-inch round agricultural tile were laid, 40 feet apart, discharging into the brook.

These drains were laid 4 feet below the surface wherever the elevation of the brook permitted this depth; but, by reason of the elevation of the brook, the lower part of the drains were not deeper than from 2 to $2\frac{1}{2}$ feet, and probably the average depth is not greater than 3 feet.

These drains were effective in drying the ground and preparing it to receive the sewage.

The distributing or sub-surface tiles were laid about eight inches below the surface, in nearly parallel lines 5 feet apart, on uniform grades of 9 to 12 inches in 100 feet. See Plate XII.

They are 2 inches in diameter and in 12-inch lengths.

They are laid on bed pieces of the same material and length, which cover the bottom joints. Smaller pieces cover the top joint, leaving an opening on each side of $\frac{3}{4}$ by $\frac{1}{8}$ inch, out of which the water escapes into the soil.

The water enters these lines of sub-surface drains from a 4-inch carrier leading from a chamber into which the pulsometer discharges, and in which are the two 4-inch carrier pipes leading to different parts of the ground, into either of which the sewage can be turned at pleasure and the two sections of the field used alternately.

A special branch joins the 2-inch distributing tile with the 4-inch carrier, the 2-inch tile being so attached that its bottom is at the same level as that of the carrier from which it branches, so that if but little sewage is flowing in the carrier each line of drain will get its share, those in the upper portion of the field being prevented from surcharge by either flattening the grade or throttling the first section of drain.

There are about six hundred feet of 4-inch carrier pipe, and about twenty thousand feet of 2-inch drains on the $1\frac{3}{4}$ acre of ground.

The amount of sewage water averages 6 000 gallons a day.

This is discharged into the irrigation tile eight times in a month, or from 20 000 to 25 000 gallons at a time. The discharge from the outfall drains begins very soon after the tile are charged, showing the ground to be very porous.

No complaint has been made of any offensive odor or fouling of the stream.

The irrigation ground is not worked to nearly its capacity, as it has been found that the sewage does not flush the tiles fully to the lower extremity of the lines, and while the growth of the grass on the upper end of the lines is luxurious and rapid, the ground over the further end has remained bare or with very scanty vegetation.

Dam and Pond.

A small pond for bathing in summer, and skating and supplying ice in winter, had been connected with the school for some years, and was enlarged by building a dam further down the stream, taking material for it from the excavation of the pond. See Plate XIII.

The dam was made of earth laid in 6-inch layers, each sprinkled and rolled. The slopes are $2\frac{1}{2}$ to 1 on the water side, and 2 to 1 on the lower, though the lower slope was afterwards made much less steep by the addition of surplus filling, which was trimmed to an ogee curve and prepared by a coating of top soil for seeding.

A masonry overflow with wing walls is provided near the center of the dam, and a rubble masonry heart wall laid in cement rises within the embankment to the flow line.

The whole work, except the water-tower, was done by the contractors who erected the buildings, they receiving for profit a certain percentage of the cost.

Great care was exercised in laying all pipes to true lines and grades, and in making good substantial joints in both iron and stone-ware pipes.

The cost of laying the pipes was as follows.

The prices given include lead and gaskets for cast-iron pipes, and Portland cement and oakum gaskets for stone-ware pipes, and also a profit of ten per cent.

8-inch	cast-iron pipe	....	$0.22	per foot.
6 "	"	....	0.13½	"
4 "	"	....	0.10	"
10 "	stone-ware	....	0.04½	"
8 "	"	....	0.03½	"
6 "	"	....	0.03	"

The cost of the following structures is made up from accounts kept during construction:

Water-tower (including foundation)..................	$2 100
Well...	1 400
Rain-water reservoir............................	4 450
Sewage tank....................................	2 900
Irrigation grounds..............................	2 000

With the exception of the occasional deficiency in the capacity of the rain-water drains above mentioned, the operation of the works during the year has been very satisfactory.

The regular number of persons now using the water and contributing to the sewage is 180. The works are designed to accommodate 400 people.

The water supplied for all purposes averaged 8 000 gallons a day in 1886, varying from 6 000 gallons a day in April to 25 000 gallons a day during one week in October, 1886, when the lawns were very dry and a new sprinkling cart was put in use on the roads and lawns.

The amount of water and sewage pumped in each month since the works went into operation has been as follows :

Month.	Gallons Pumped. Water.	Sewage.
1885.—October	230 957	178 364
November	348 824	199 227
December	277 475	151 050
1886.—January	199 600	166 500
February	259 100	178 000
March	250 000	185 500
April	181 500	165 000
May	255 000	191 000
June	204 000	189 000
July	184 000	43 000
*August	50 000	23 000
September	172 550	117 000
October	411 000	200 000
November	206 000	191 000
December	157 700	144 000
1887.—January	228 500	180 000
February	175 000	168 000

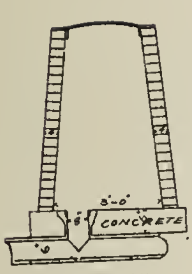
* Vacation.

PLATE VIII
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XVI No. 352.
ODELL ON
LAWRENCEVILLE SCHOOL.

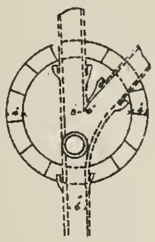
LAWRENCEVILLE SCHOOL

PLAN of WELL
FOR WATER-SUPPLY
1885

SEWER MANHOLE

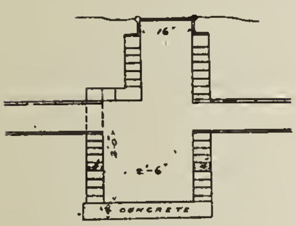


SECTION

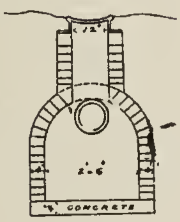


PLAN

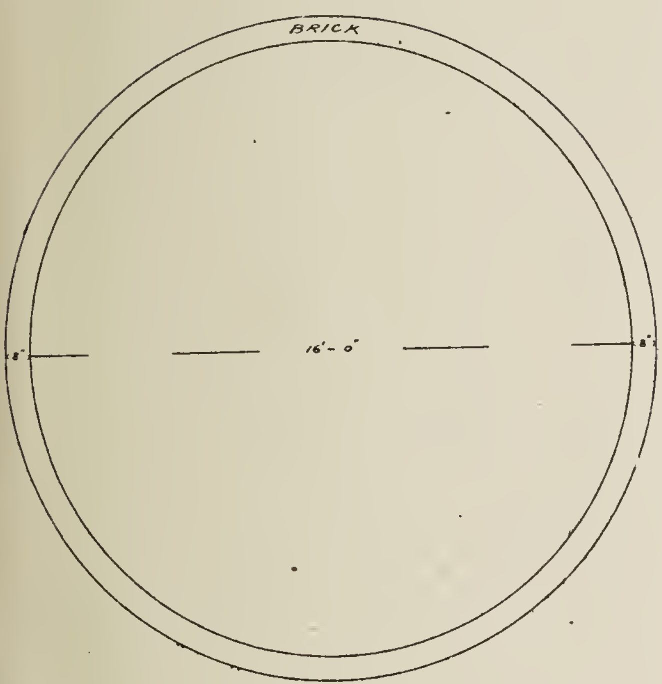
SILT BASIN



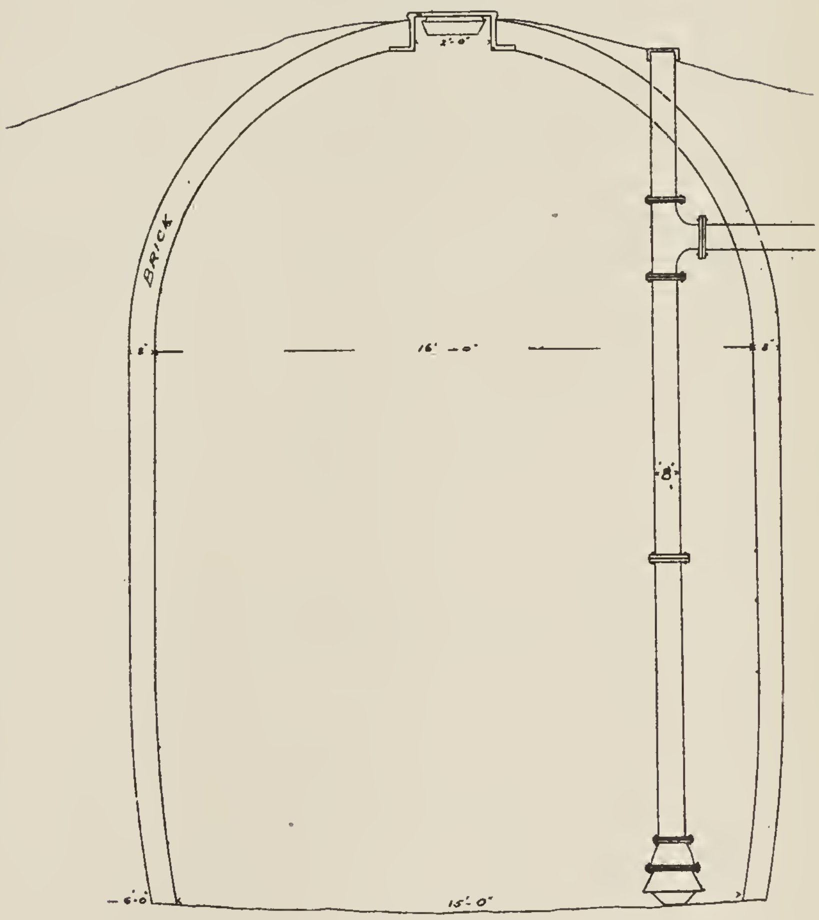
LONGITUDINAL SECTION



CROSS SECTION



PLAN AT SPRING LINE

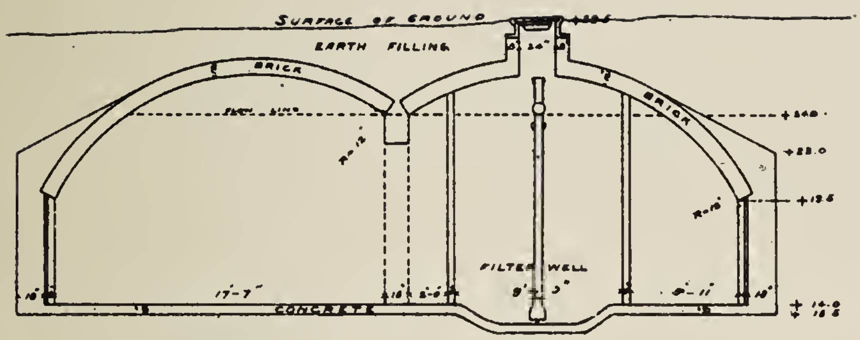


SECTION

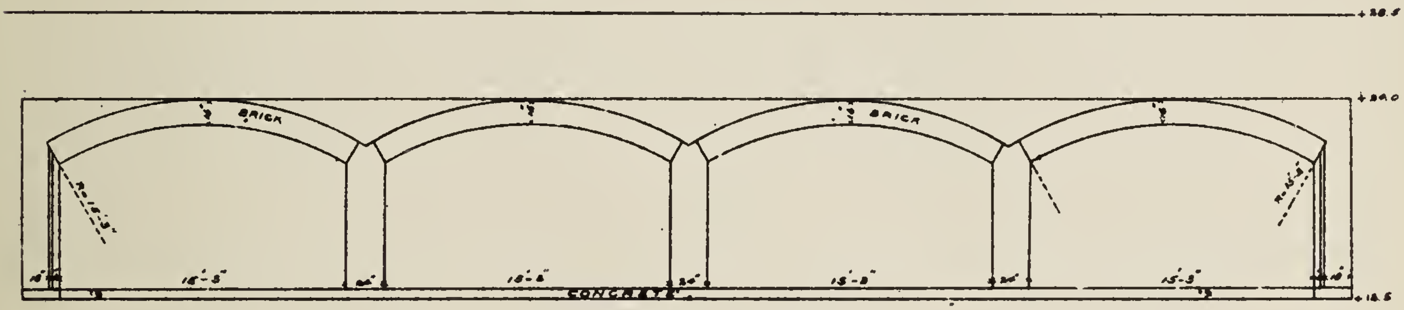
PLATE IX
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XVI No. 352.
ODELL ON
LAWRENCEVILLE SCHOOL.

LAWRENCEVILLE SCHOOL
PLAN OF COVERED RESERVOIR
FOR RAINWATER STORAGE
LAWRENCEVILLE, N.J.
1883

Scale of Feet



SECTION THROUGH FILTER WELL



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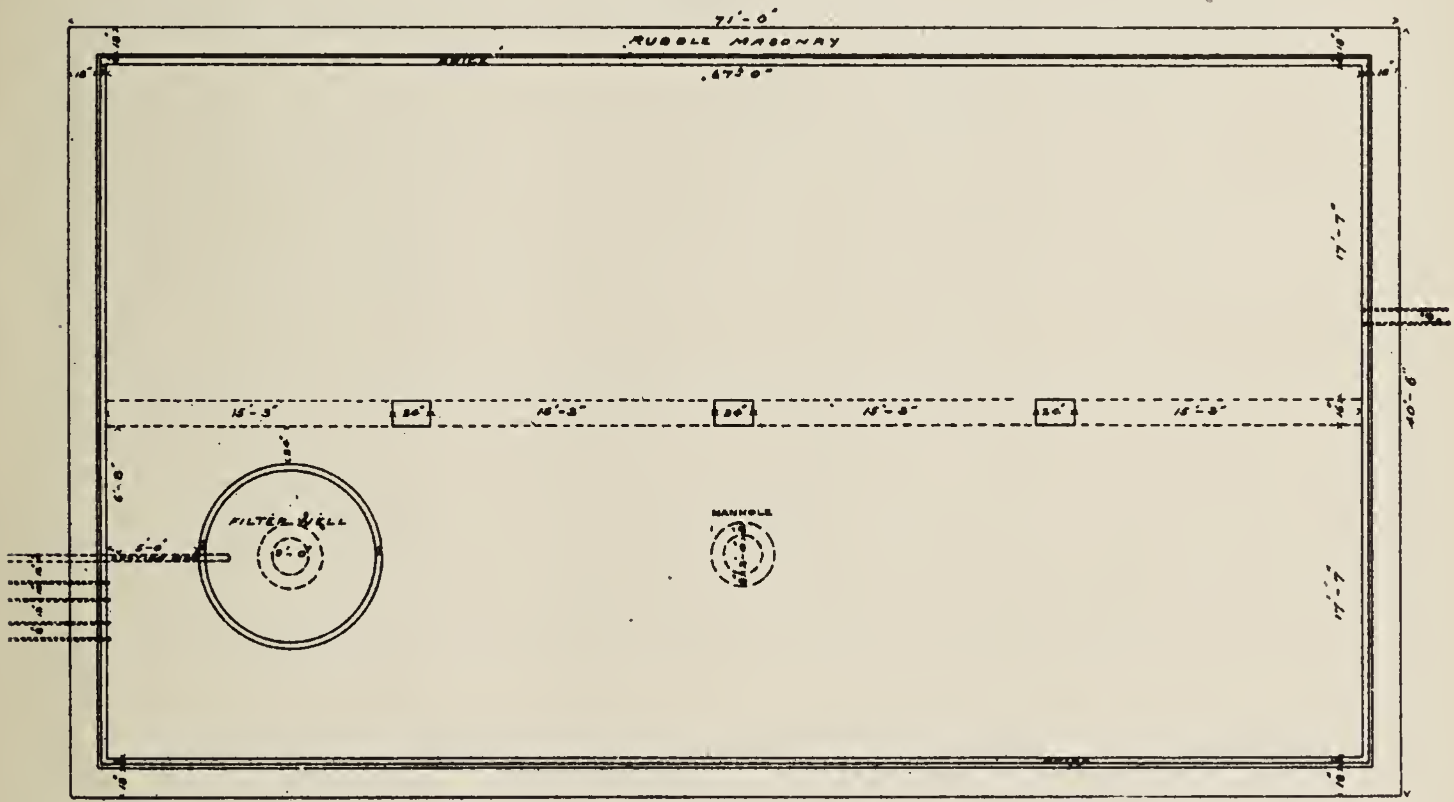

PLAN

PLATE X
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VOL. XVI Nº 352.
ODELL ON
LAWRENCEVILLE SCHOOL.

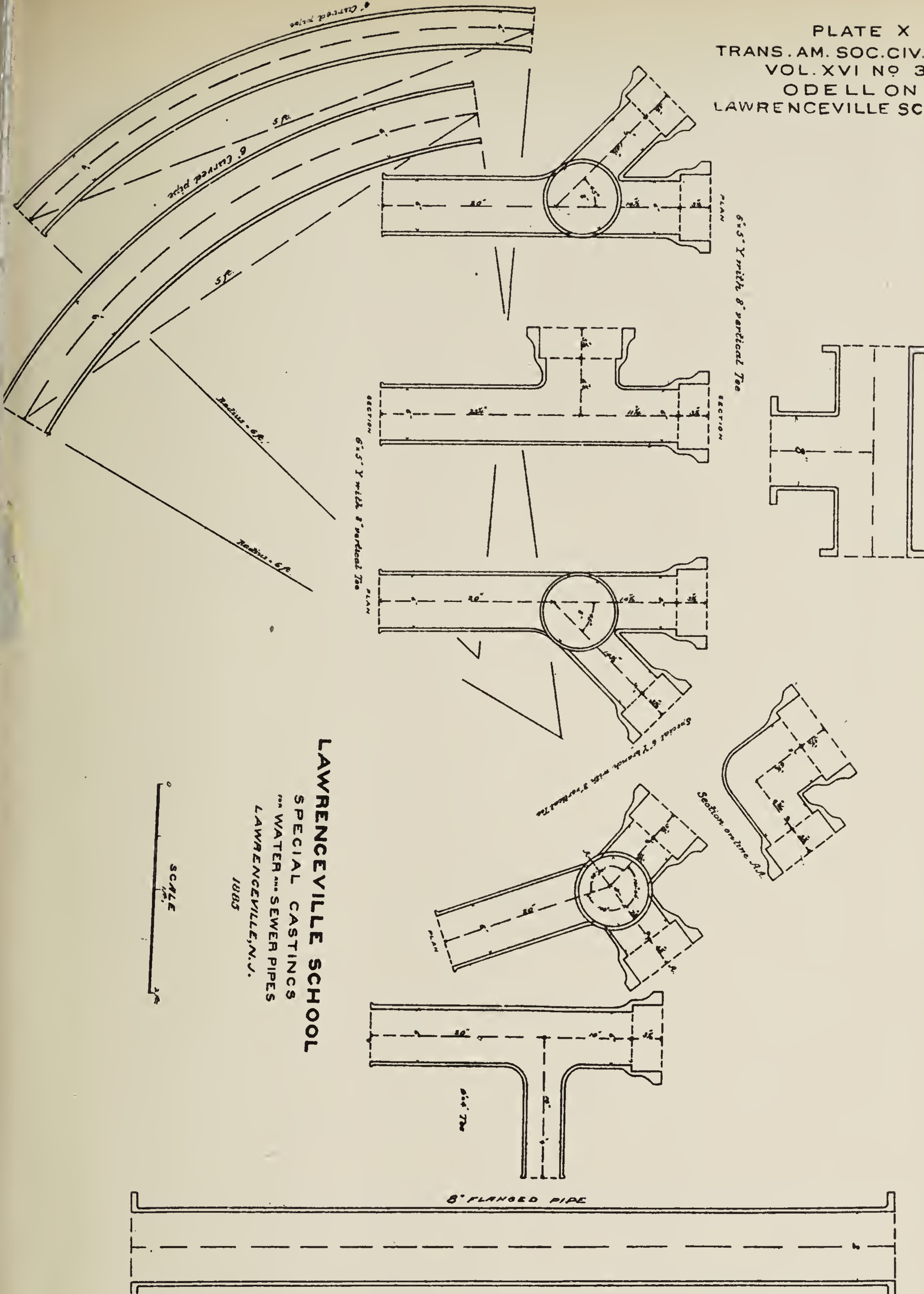

PLATE XI
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VOL. XVI Nº 352.
ODELL ON
LAWRENCEVILLE SCHOOL.

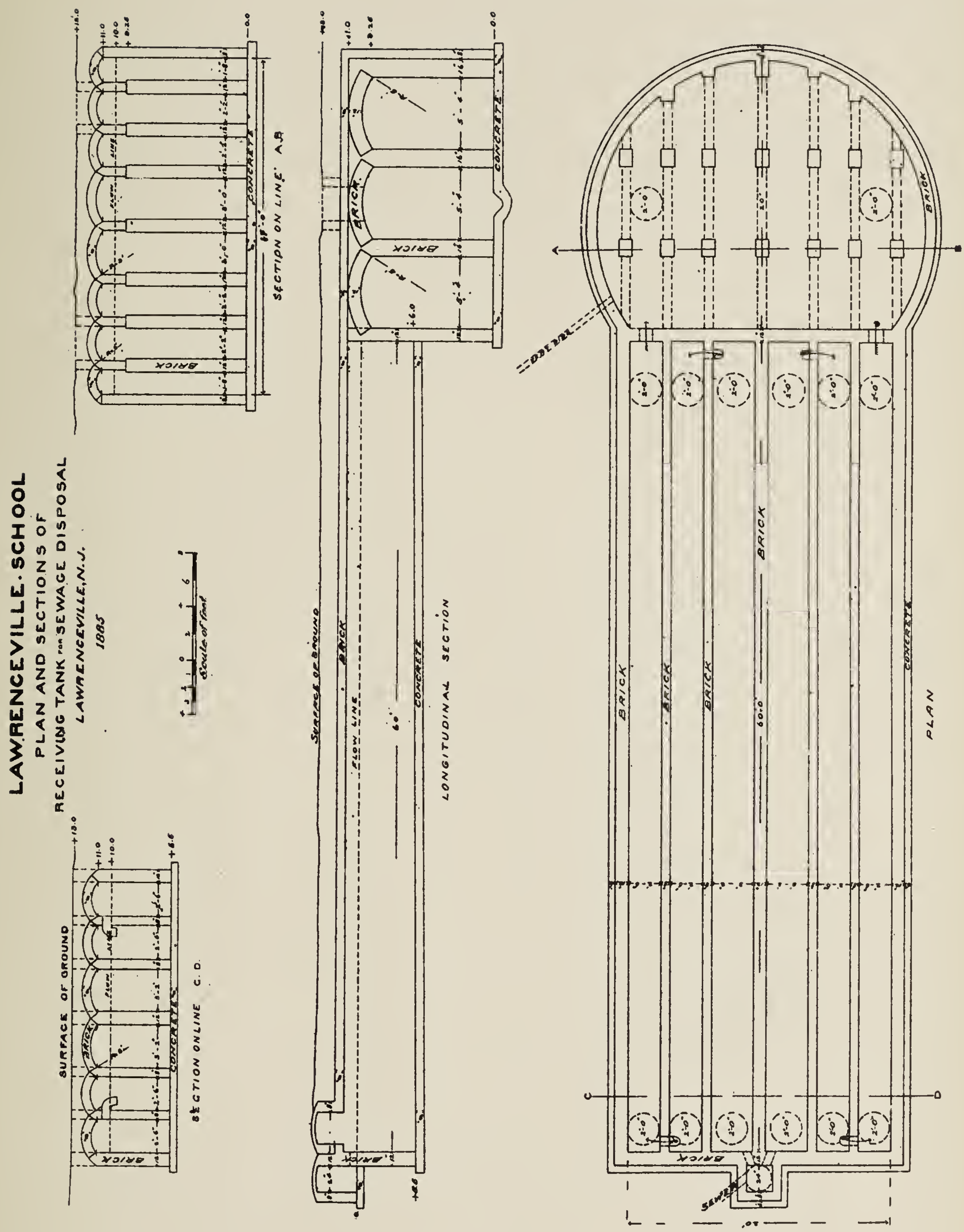

PLATE XII
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VOL. XVI Nº 352.
ODELL ON
LAWRENCEVILLE SCHOOL.

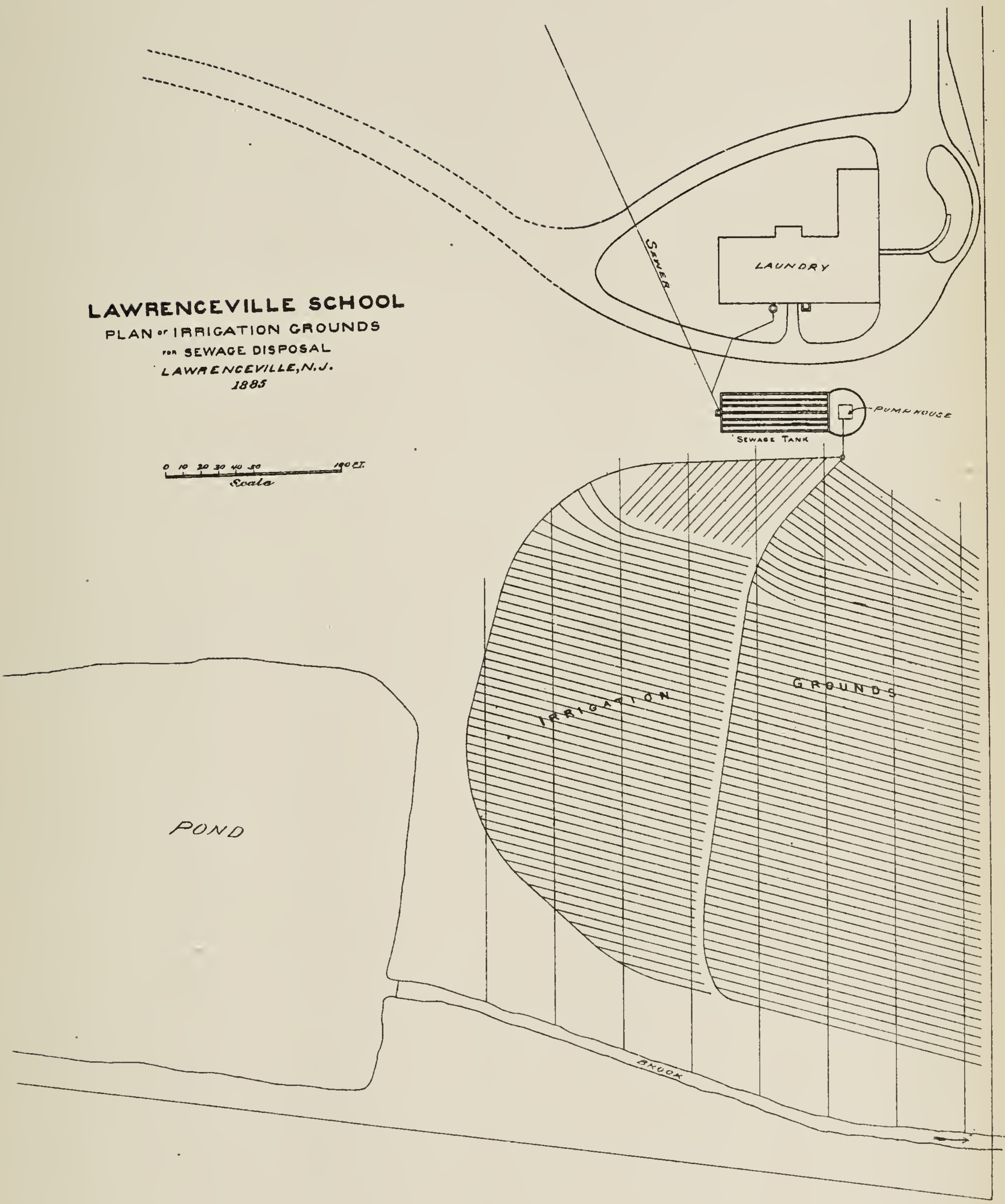


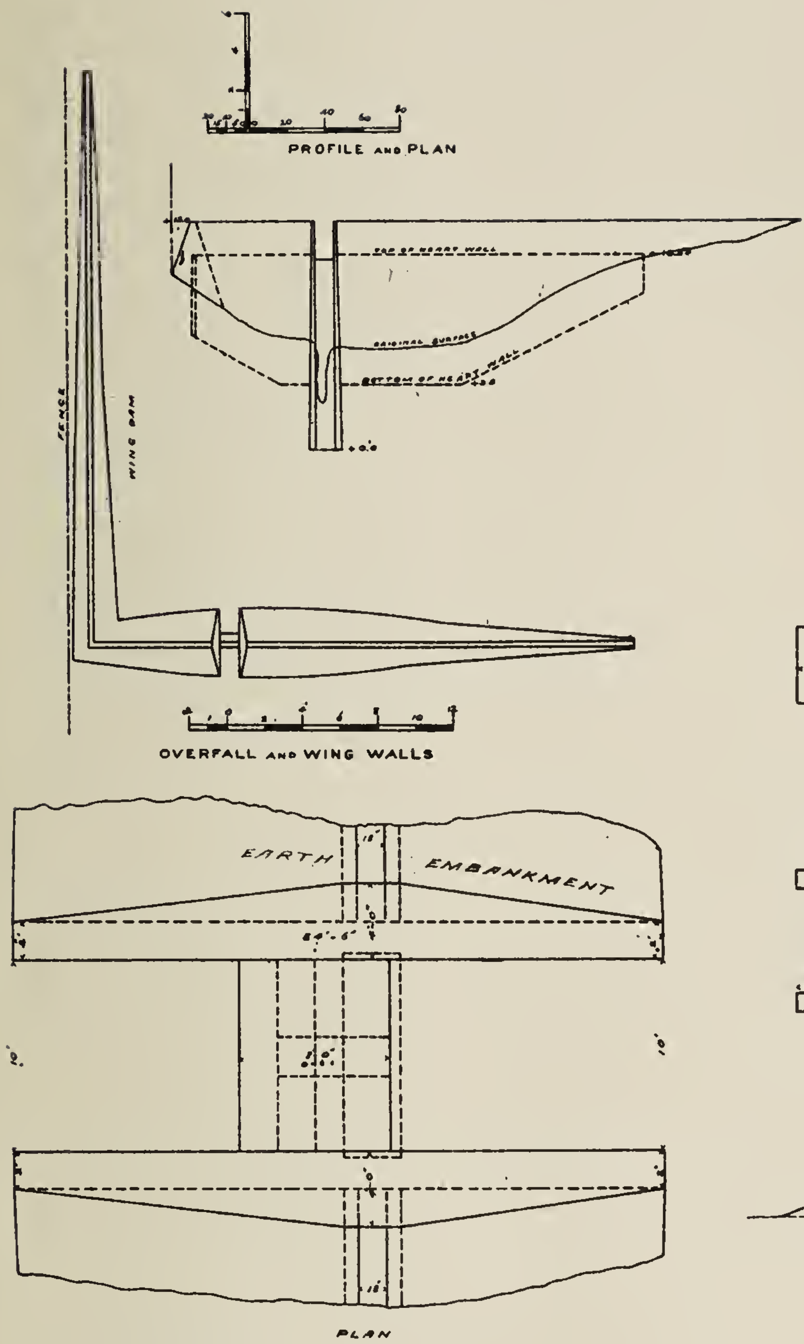
PLATE XIII
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XVI Nº 352.
ODELL ON
LAWRENCEVILLE SCHOOL.

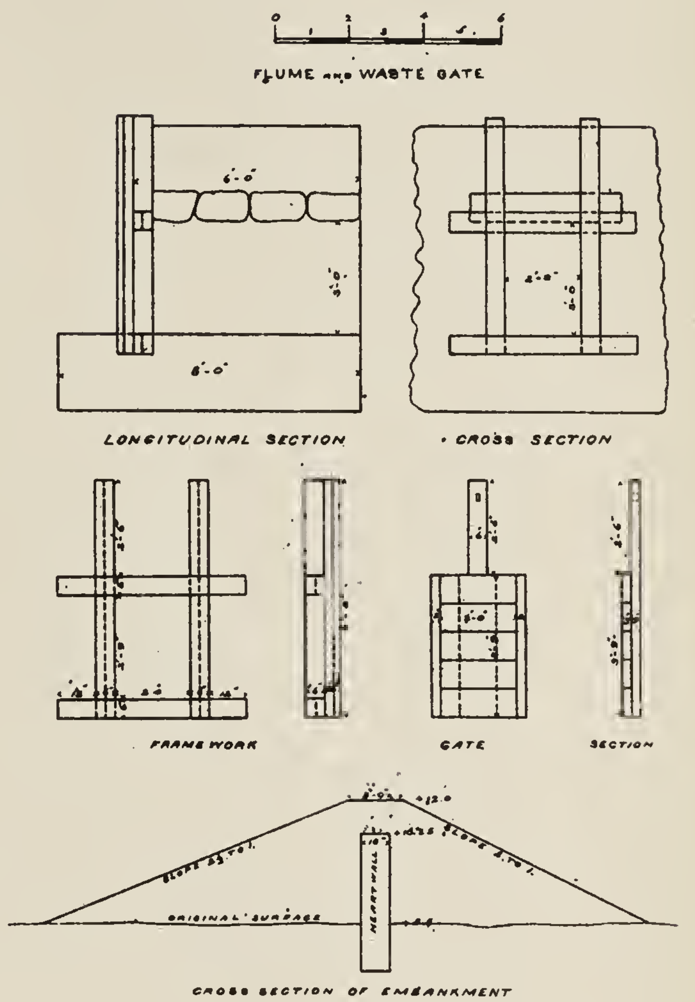
LAWRENCEVILLE SCHOOL

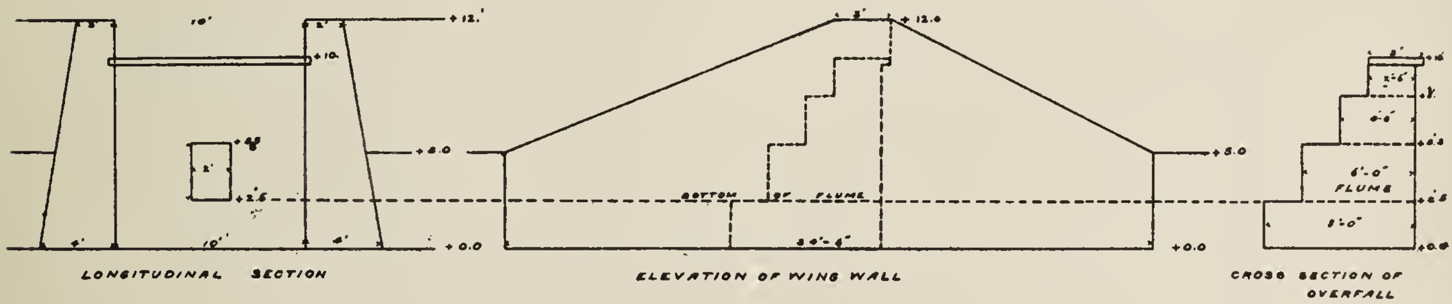
PLAN OF DAM AT POND

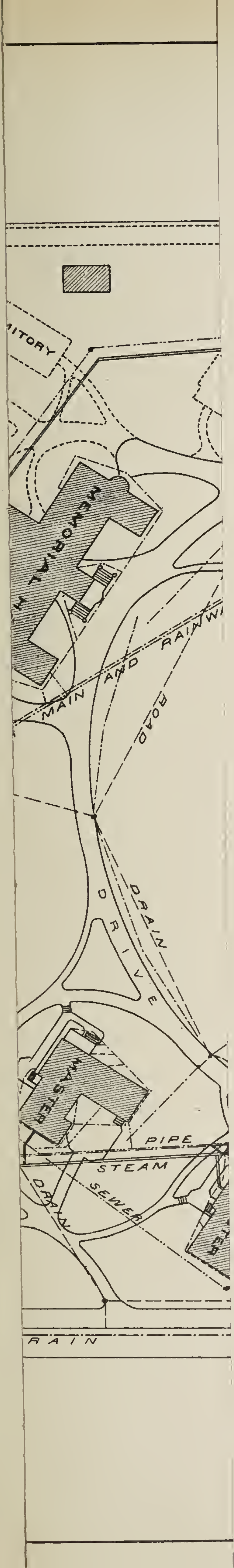
LAWRENCEVILLE, N.J.

1885



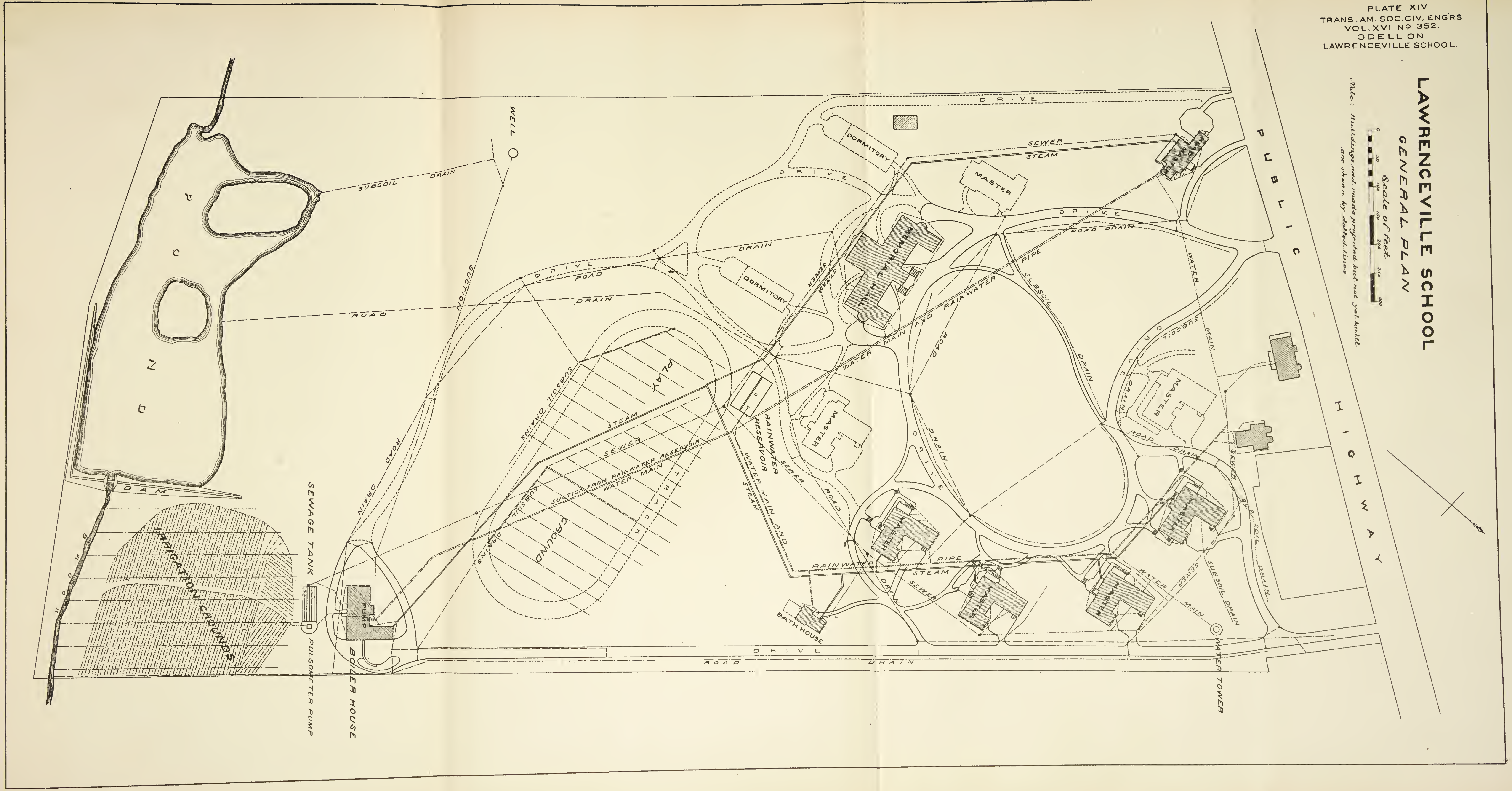






ITORY
MEMORIAL H
MAIN AND RAINW
ROAD
DRAIN
DRIVE
MASTER
PIPE
STEAM
SEWER
DRAIN
TER
RAIN

PLATE XIV
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XVI No 352.
ODELL ON
LAWRENCEVILLE SCHOOL.



AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

353.

(Vol. XVI., March, 1887.)

THE EFFECT OF FREEZING ON CEMENT-MORTAR.

By ALFRED NOBLE, M. Am. Soc. C. E.

READ AT THE ANNUAL CONVENTION, JULY 5TH, 1886.

WITH DISCUSSION.

In the construction of a lock at the St. Mary's Falls Canal, the laying of masonry was discontinued about October 20th of each year on account of the frequent recurrence of freezing weather. On the last day of the work in 1877, mortars of Portland cement and of a good quality of American natural cement were used in adjoining portions of the wall. The same proportions of cement and sand, 1 to 1, were used in both classes of mortar. This masonry was laid during a light rain. The following spring the surface of the Portland cement-mortar was sound, showing perfectly the marks of the rain drops. The natural cement-mortar was disintegrated to a depth of 3 or 4 inches.

In the same locality it was necessary to lay a concrete foundation for a movable dam in February. The weather was extremely cold, generally about zero. The mortar was made with Portland cement. Salt was used freely, but without retarding very much the freezing of the concrete. The concrete was at once covered with a floor of timber and plank on which the masonry abutments were built. Samples of the frozen mortar set properly after being put in a warm place. There was never any settlement of the masonry, and within a few months the concrete sustained a pressure of 15 feet of water without developing any leaks.

In the construction of a bridge across the Clark's Fork of the Columbia River in Northwestern Montana, the caissons were filled with concrete during freezing weather. Portland cement was used. The proportions of cement to sand were 1 to 3. Within a week the laying of stone masonry was commenced on these caissons, and proceeded with as rapidly as possible without apparent injury to the concrete, which had set firmly. In these cases the temperature had risen above the freezing point within two or three days after the concrete had been placed; and it had been permeated to some extent by warm air escaping through leaks from the air-chamber.

Four small piers were built for the St. Louis River Bridge on the Northern Pacific Railroad, near Duluth, in the winter of 1884–85. During the laying of masonry for pier 1, the temperature varied from 0 to 20 degrees; during the building of pier 2, the temperature was about 20 degrees higher, and during the building of the remaining piers the temperature was occasionally above the freezing point. Portland cement was used throughout, the proportions of cement and sand being 1 to 1½ for face stone, and 1 to 2½ for backing. During the extremely cold weather salt was used freely in the mortar and the sand was warmed (not made hot), but with the thermometer at 20 degrees the mortar froze quickly after being spread on the stone; so quickly, indeed, that if the stone, being set, could not be brought to a bearing by a little shaking, it was necessary to raise the stone, scrape off the now frozen mortar, and spread a new bed. In setting the face-stone the mortar was kept back from the face an inch or so to facilitate subsequent pointing. A few weeks later, after there had been milder weather, an examination of the open edges of the mortar beds showed that the mortar used during the coldest weather had set firmly, and no difference could be detected by examination of detached fragments between the mortars in piers 1 and 4; that is to say, between that laid in the coldest and that laid in the mildest weather embraced in the period of construction of these piers.

During the course of tests of cement at the St. Mary's Falls Canal, a few experiments were made relating to the effect of freezing and the use of salt on cement-mortars. They are not submitted as conclusive in any way, but as suggestive, and in the hope that, combined with others, some definite conclusion may be reached.

TABLE A.

Effect of Freezing on Mortars of Portland Cement containing varying amounts of Salt.

Composition of Mortar.

Cement.. 35 ounces.
Water.. 7 "

Salt as in table.

Tensile strength per square inch at seven days.

Treatment.	Salt.								
	0.	⅛ oz.	¼ oz.	⅜ oz.	½ oz.	⅝ oz.	¾ oz.	⅞ oz.	1 oz.
First Series.									
Immersed in test-room when removed from moulds	327	357	375	392	429	402	415	388	402
Exposed to air when removed from moulds and frozen three days; then immersed in test-room four days..............	316	378	411	374	415	405	392	383	409
Second Series.									
Immersed in test-room when removed from moulds	336	422	421	399	394	384	390	356	387
Exposed to air when removed from moulds and frozen six days; then exposed to air in test-room at 70 degrees one day..........................	169	198	167	217	227	215	208	221	239

TABLE B.

Effect of Mixing Salt with Portland Cement-Mortar.

Proportions by measure.

Cement.. 1.
Sand.. 1.

Proportions by weight.

Cement.. 21 ounces.
Sand.. 23 "
Water.. 6 "

Salt as in table.

Means of ten tests.

Salt.	Tensile strength, pounds per square inch.							
	7 days.	30 days.	90 days.	6 mos.	9 mos.	12 mos.	18 mos.	24 mos.
0......................	155	220	289	311	390	382	402	430
⅛ ounce	139	200	246	288	363	364	423	346
½ "	139	192	221	289	352	383	392	326
1 "	128	189	217	288	343	369	350	334

DISCUSSION.

F. Collingwood, M. Am. Soc. C. E.—Mr. Noble said that he found the natural cements did not stand the cold as well as the artificial cements; that he always found the natural cements to be damaged two or three or four inches from the face of the joints after they had been exposed to very severe freezing, and we found something of the same kind in our work on the East River Bridge. On any wide wall where water could collect on top and where it was exposed to the weather all through the winter, we found that we would have to scrape out the joints, but the face joints never troubled us. We used the Rosendale cements.

George S. Morison, M. Am. Soc. C. E.—As bearing immediately upon the subject of freezing, I might mention an accidental experiment that occurred last winter in the work on the Omaha Bridge. I had quite a number of briquettes made of American cements and imported Portland cements, which were exposed to the air twenty-four hours and then left in the customary way in a pail of water. There came on extremely cold weather, and the entire lot became a solid block of ice. When it thawed out the Portland cements were entirely uninjured, but the American cements were entirely ruined, some of them being reduced to mud. Subsequent experiments showed that the cements which stood freezing three days after they were made, would also stand freezing immediately after they were mixed. It has been for some years my practice to use Portland cement exclusively in places where the mortar was likely to freeze before setting.

Robert B. Stanton, M. Am. Soc. C. E.—I have had some experience in laying masonry in very cold weather. In the winter of 1878 and 1879 it was found that a small pier on the Cincinnati Southern Railway was defective. The pier was taken down and rebuilt with Louisville cement, and with the use of salt. The thermometer at the time ranged from 6 to 10 degrees below zero. The iron trestle was put upon the pier, and during the winter no change in the masonry was noticed. When, during the next summer, it became very hot, the pier seemed to sweat; the salt came out and made the sides white, but the cement was as hard as if it had been laid in the summer, and, so far as I have since learned, there has never been any trouble with that pier.

In the winter of 1881, here in Denver, in building the round house and shops of the Union Pacific yards it became cold very suddenly. After waiting for awhile, the weather not getting more moderate, the work was proceeded with and salt was used in lime-mortar, and every night the top was covered with a thick coat of salt; the mortar was not really frozen. The next summer a locomotive got away and struck this masonry between the two windows, tearing away the lower portion and leaving the whole keystone portion above suspended and held up by the mortar, and this masonry hung thus for several weeks: It is possible that the mortar did not freeze during the day, but during the night the temperature was very low.

Eliot C. Clarke, M. Am. Soc. C. E.—Some years ago I made a number of experimental batches of concrete, some of Rosendale cement and some of Portland cement. Of these a portion were made with a large proportion of cement, and some weaker, of each kind. I made them just before freezing weather and left them out, being engaged on work in which some concrete had to be exposed in that way.

They were left for two or three years exposed, and during the first winter the Rosendale concretes without exception began to weather badly on the surface, and from year to year disintegrated; none of the Portland cement concretes were affected at all in three years, though lying right on the surface of the ground in blocks about a foot square.

I remember once talking with Mr. Shanahan, who is the Superintendent of Public Works of the State of New York, about his practice on the Erie Canal, and he told me that he would as willingly build masonry in the winter as in the summer, so far as its durability was concerned. He used Rosendale cement in building masonry, a tolerably strong mortar; that is a large proportion of cement to the sand; and always used in mixing only the strongest brine; that is, water saturated with salt, so that it would foam on top. He said he never knew a case to fail built in that way.

John Bogart, M. Am. Soc. C. E.—I have had occasion to examine recently the masonry referred to by Mr. Clarke, built upon the line of the Erie Canal. This masonry was the retaining wall of the West Shore Railroad where it runs along the canal; where it was first laid in very cold weather, without the proper use of salt, it gave way and got into very bad condition. Directions were then given and carried out, for using a strong solution of salt for mixing the mortar, one barrel of brine being mixed when another was being used for mortar. The result has been very satisfactory, and the masonry is in excellent condition.

J. JAMES R. CROES, M. Am. Soc. C. E., replying to a question as to the use of salt, quoted from a paper presented by him in 1874 on the construction of a masonry dam: "In freezing weather the mortar was mixed with salt water. The rule for proportion of salt was one said to have been used in the works at Woolwich Arsenal some years ago, viz.: Dissolve one pound of rock salt in eighteen gallons of water when the temperature is at 32 degrees Fahr., and add three ounces of salt for every 3 degrees of lower temperature. The masonry laid with mortar thus prepared stood well, and showed no signs of having been affected by the frost."

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354
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IRRIGATION.

By EDWARD BATES DORSEY, M. Am. Soc. C. E.

READ NOVEMBER 17TH, 1886.

WITH DISCUSSION.

One of the first surveys with which the writer was connected was for the purpose of irrigating an extensive district in Northern Peru. During the three years of this survey, he studied, practically, irrigation as practiced there. Since then he has been a close observer of it as practiced throughout South America, Mexico, Colorado, Utah, California and Italy. As his knowledge of it has increased, so has his faith in its great advantage and importance. It could be introduced profitably in most places and in most climates. Perhaps some few meadow and bottom lands would not be improved by it. On the other hand, one frequently sees irrigated meadows in England, notwithstanding its damp climate and frequent rains.

In no part of the world has so much money, talent or attention been given to irrigation as in Italy, yet the climate there is very much the same as Eastern New York, the annual average rainfall being about 39 inches, with about four inches per month in April, May, June and July, the months the crops are growing and maturing. Notwithstanding this heavy rainfall, there has been expended about $200 per acre to bring and distribute the water in some districts.

In India, where the English government has spent so much money in irrigation works—up to 1881, $86 000 000—the annual rainfall is, except in a few small districts, as large as it is here. For example, in the districts irrigated by the Ganges and Jumna Canals it is 38 inches.

In Italy, France and most of India, the rainfall is ample for ordinary crops; irrigation is adopted to increase the yield and to prevent the failure of crops by drought. These reasons will apply equally as well to all other countries; the general impression that irrigation is only necessary or useful in dry countries is entirely incorrect.

One can see at our own door, at the present time, the advantages on plant life of irrigation in this climate—which 55 000 000 of our people have been taught does not require it—the rains being considered all-sufficient. The grass plot in front of this house for some reason had not been watered, and the grass is far from being in a thrifty condition, while other plots on the opposite side of the street have been irrigated, their owners not trusting entirely to Providence. The difference between these adjacent plots is about as great as that between an irrigated Colorado farm and the cattle ranch on the adjoining unirrigated land.

The same can be seen in any village or town. The house that has plenty of water has always green grass and thriving flowers, while in those who do not have abundance of it, the opposite is the case. If water is so necessary to make grass grow for ornament, it is just as necessary to make it grow for hay and pasturage. Commercial vegetation is governed by the same law as ornamental. With a complete system of irrigation, any necessary quantity of water can be placed whenever or wherever desired, thus assuring the largest yield of crops, without the possibility of failure from drought, partial or complete. Many of our Eastern farmers know by sad experience that this frequently happens.

Arid Section of the United States.

Few persons have any idea of the importance of irrigation to the future prosperity of the United States. Heretofore there has been plenty of public land open to settlement at Government prices; now there is but little left that can be farmed profitably without irrigation.

The total area of the United States, excluding Alaska, is 3 547 000 square miles; of this 1 387 000 square miles, or more than one-third, cannot be farmed without irrigation, excepting a few bottoms and meadows comparatively of small extent.

Unless otherwise stated in speaking of the United States, the following remarks will apply to this desert or dry section only.

The annual average precipitation of moisture in the form of rain or snow over the valleys and plains of this section is about sixteen inches, increasing as the elevation of the mountains increase. This moisture is a great help to irrigation, but alone it is not sufficient for crops.

Duty of Water.

This is very variable, being influenced by the soil, climate, crops, price of water; whether the water is bought by so much for irrigating a crop or an acre, or by measurement; and last, but not least, by the intelligence of the irrigator.

The following table has been compiled from the most reliable available authorities, and is believed to be substantially correct.

TABLE No. 1.

Locality.	Crop.	Average Rainfall. In irrigating or crop-growing months, March to September.	Average Rainfall. Per annum.	Duty.
		Inches.	Inches.	Areas.
Jumna Canal, India....	Wheat, maize, etc............	....	38 to 44	306
Ganges " "	" "	....	38 to 56	232
Upper India	" "	....	....	267
Northern "	Cereals	....	38	200
Genil Canal, Spain....	" and vines.............	6	22	240
Valencia, " ...	"	5	16	200
" "	Corn, grass, etc...............	5	16	324
" "	Garden and orchards.........	5	16	162
Northern Peru.........	Corn and cotton...............	0	00	160
" Chili.........	" grain	0	00	190
Lombardy, Italy.......	All crops, including rice.....	22	38	90
Piedmont, "	"	28	38	60
Average all "	"	25	38	67
United States.				
California, San Joaquin Valley	Cereals.......................	2	10	200
Colorado, Denver......	All crops	10	14	56
Utah, Camp Douglas...	"	10	17	60 to 100
Idaho, Boise City......	....	10	17	No reliable data.
Wyoming. Cheyenne...	....	9	10	"
Arizona, Fort Mojave ..	....	2	5	"
Nevada, Winnemucca..	....	3	6	"
Kansas, Dodge City....	....	14	17	"
Montana, Fort Benton.	....	10	13	"
Dakota, Fort Buford...	....	12	17	"
Nebraska, North Platte.	....	15	18	"
New Mexico, Santa Fé..	....	10	14	"
Oregon, Umatilla.	....	4	9	"

The duty of water is calculated upon the usual basis of the number of acres irrigated by one cubic foot of water per second. As before stated, this is a very variable quantity, differing in each locality. The amount put down is an average of what has been reported.

This table is very instructive. In Italy water is abundant and cheap; in fact irrigation is hardly necessary, as the rainfall would be considered sufficient in other countries; consequently the irrigators are very wasteful in applying the water, and it does only one-third the duty it performs in Spain, where it is scarce and dear, and absolutely necessary in farming, owing to the small rainfall. The preceding table shows that the rainfall of our arid section resembles very much that of Spain. Irrigation would probably accomplish the same beneficial results here as there.

In 1873, by Act of Congress, a commission was appointed to examine and report on a system of irrigation for California, This commission consisted of General B. S. Alexander and Colonel George H. Mendell, M. Am. Soc. C. E., United States Engineers, and Mr. George Davidson, United States Coast Survey. After thorough investigation of what was being accomplished in California, Europe and Asia, they reported that a duty of 200 acres per each cubic foot of water might be calculated upon in Central California, where the average annual rainfall is about ten inches.

Under the direction of the United States Government, Mr. George Davidson, of the Uuited States Coast Survey, made a personal examination of irrigation as practiced in India, Egypt and Italy. His examination and report were made in his usual thorough style.

Of irrigation in India he says: "The works projected, and nearly all in progress, are estimated to cost \$169 950 000. With their completion we may safely say that the population will be doubled; famines will be mitigated; the government revenue will certainly be more than doubled." He gives the following as the duty of water in India. One cubic foot per second irrigated

Wheat, maize, etc., in Upper India............	267 acres.
" on East Jumna Canal......	306 "
" Ganges "	232 "

This means an average depth of 32, 28 and 37 inches respectively, exclusive of rainfall.

Login says: "In India, 10 inches of water, distributed in four waterings each of 2½ inches in depth, is sufficient for wheat and similar crops."

Mr. Davidson, in speaking of Central California (where the rainfall will average about ten inches annually), after reviewing what he had seen in India, Egypt and Italy, sums up as follows:

"The above figures indicate that there is a waste of water in some of the districts of India. And yet the more favorable ones approach the best conditions developed in California. The capacity of a canal may therefore be fairly estimated by assuming that 12 inches of water over the surface of the irrigable land will, if properly applied, be amply sufficient for the maturing of one grain crop."

Mr. Charles L. Stevenson, civil engineer, a long resident of Salt Lake City, says: "In former years a cubic foot of water per second only irrigated sixty acres; now it is irrigating over one hundred acres. This applies to the whole territory adjacent to Salt Lake City. More careful husbanding gives better results."

He indorses irrigation in the following strong terms: "With irrigation there is never a failure of crops. It may vary from season to season, but a crop is invariably insured." This is very strong language from an engineer who has been watching it practically for twenty years.

In the eastern portion of the United States the average annual rainfall varies from 30 to 48 inches; let us assume the latter—the maximum. This equals 4 inches per month. The crop growing season is generally estimated at 100 days; let us take it at 4 months. This gives 16 inches as the depth of rainfall necessary for a crop.

With care in distributing water in irrigation, there should be no greater waste, or water running off without producing any useful effect, than in ordinary rain, and not as much as in violent summer showers; assuming it to be the same, then 16 inches distributed over 4 months gives a duty of 180 acres to each cubic foot of water per second. This is probably much greater duty than can be expected in the United States for some time; probably not until labor becomes cheaper and water dearer than they are now. At first our farmers will follow their American idea of farming large tracts of irrigated lands as they have farmed the unirrigated, but they will soon learn that more money can be made on a small farm thoroughly farmed and irrigated, than on a large one, irrigated or not.

The preceding is what can be done with water; what is done is another question.

In Southern California, where water is very scarce, and irrigation has been long practiced and studied, results much better than this are obtained.

In the rainless section of Peru, the writer has seen and calculated a duty of 160 acres per each cubic foot of water per second. In Chili, under similar conditions, 190 acres. In both countries the average would be much less.

Mr. Davidson is well known as a careful observer, of high scientific attainments; and his opinion should be taken as to what can be done. The American is a very apt scholar, especially when he is financially interested. When he finds that water costs money, and any economy made in its use will be to his profit, he will soon get as large a duty from water as any other irrigator.

In most cases where water is sold by measurement, a duty of 120 acres per each cubic foot per second can be expected after a few years' experience and improvement in the farmer's distributing service.

Yield of Water From Water-sheds.

Mr. E. S. Nettleton, State Engineer of Colorado, gives the following useful table on the monthly flow of Colorado streams.

"The Annual Discharge of the Cache la Poudre River.

"Area of water-shed above measuring flume is about 972 square miles.* This gives a 'run-off' equal to 31 054 608 cubic feet per square mile--48 522.8 cubic feet per acre—equal to 13.367 inches, which is a little less than the mean average annual rainfall on the plains east of the mountains.

"The mean annual rainfall on Pike's Peak, 1874 to 1881, inclusive, was 33.6 inches.†

"The percentage of monthly discharge compared to annual discharge is as follows, viz.:

* The elevation of this measuring flume is not given; it is probably about 5 000 feet above tide. The elevation of the water-shed extends from this up to 14 000 feet above the sea.

† Pike's Peak is 14 147 feet high, and about 150 miles south of the water-shed.

January, February, November, December (estimated)	4.59	per cent.
March (estimated)	0.59	"
April	1.86	"
May	22.55	"
June	40.97	"
July	18.74	"
August	6.63	"
September	2.59	"
October (estimated)	1.78	"
Total	100.00	"

This may be taken as a fair sample of the flow of the streams having their origin in the high portions of our mountain ranges.

It appears that in the three months in which the crops are growing, 82 per cent. of the total annual water flows down the stream. This table also shows plainly that no irrigation system is complete, or can work approximately up to the capacity of the stream, without impounding reservoirs to give water for the late, or second, crops. Just how much water, or what percentage of the water, will be required after July, will depend entirely upon the description of crops raised; but there will always be a large demand for water for the corn, hay, and fruit crop; as also for pasturage, commercial and ornamental gardening. This demand for late water will constantly increase, as it is the general tendency of farmers on irrigated land to change from grain, which is generally their first crops, to diversified crops, including fruits; increasing all the time what may be termed ornamental or fancy farming, requiring constant water from frost to frost.

Every engineer in laying out an irrigation system to be fed by rivers of this description, must provide storage reservoirs for the late water that may be required, provided he intends to utilize to the fullest extent the capacity of his stream during the growing season. It will not be necessary to provide all this late water at once, but he should be able to increase it as the demand does.

As the height of the mountains and country supplying the irrigating system with water diminishes, so will the water available for late irrigation decrease. This will oblige the construction of storage reservoirs of greater percentage of the total capacity of the irrigating system.

The proportion of stored water to the whole will be larger in irrigating systems that have their water supply from low elevations or mountains than in those systems that take their supply from high elevations.

Storage Reservoirs.

The cost of storage reservoirs differs very much in different localities and in different geological formations. When Mr. Allen Campbell, M. Am. Soc. C. E., was the Commissioner of Public Works of New York City, he considered the average cost of storage to be $200 for each million of gallons stored. The proposed Quaker Bridge Dam, at the estimated cost, will cost $125 for the same quantity stored. The recent survey for the water supply of Philadelphia makes the average cost in eleven reservoirs, $131.84 for each million gallons stored. A recent survey in Idaho, in exceptional favorable locations, makes the average cost of storage in five reservoirs, 72 135 000 000 gallons, $147 217, or $2.05 for each million of gallons stored. One of these reservoirs will store 52 275 000 000 for a total cost of $50 833, or 98 cents for each million of gallons.

These last are very favorable localities. Basaltic dikes run across the valleys, through which the running water has worn narrow and deep channels, it being only necessary to close up these narrow openings to form large lakes or reservoirs.

Average cost of storing in six reservoirs in Spain, 7 582 000 000 gallons, was estimated at $410 700, or $54.17 each million gallons stored.

At the rate of 180 acres of land irrigated 16 inches deep by 1 cubic foot of water per second, this will require for each acre of land 425 000 gallons for the crop season of four months; say, 500 000 gallons. Where the irrigation is entirely by impounded water the storage facilities must be unusually favorable, or the value of the water exceptionally high, in order to make it pay, but it can generally be used with profit to supplement the running streams late in the season.

The entire investment of the Indian government in irrigation works amounted in 1881 to $86 000 000. The total area irrigated was about 8 000 000 acres. This makes the cost of works per acre irrigated $10.75.

TABLE No. 2.

Cost of Some Irrigating Works.

Name.	Country.	Cost of Works. Per acre irrigated.	Cost of Works. Per cubic foot per second for water used.
High Level Canal	Colorado	$10.83	$549.00
Cajon Canal	California	53.33	1 025.64
Santa Clara Valley Irrigation Company	"	9.68	548.82
Riverside Canal	"	52.75	1 507.14
Mussel Slough	"	7.30	583.65
King's River, north side	"	7.18	277.20
Idaho Mining and Irrigation Company (estimated)	Idaho	2.16	189.38
Carpentras Canal	France	35.67	2 830.19
Verdon Canal	"	81.25	15 330.19
Hanares Canal	Spain	46.66	7 500.00
Ganges Canal	India	14.15	....
Eastern Jumna Canal	"	6.11	....
Western Jumna Canal	"	10.88	....

The average cost of bringing water to the land in seven recent irrigation works near Madrid, Spain, was from $30 to $40 per acre.

The entire canal system of Colorado embraces over 800 miles of large size canals completed; about 150 miles projected; and about 3 500 miles of canals of secondary size. The large canals have cost in construction about $5 000 000; the smaller canals about $3 000 000; and the entire system from $10 000 000 to $12 000 000. Total area of land covered by these canals, 2 200 000 acres. Average cost, $5 per acre.

Italy employs for irrigation a total of 24 000 cubic feet of water per second, which irrigates 1 600 000 acres of land. This gives an average duty of 67 acres for each cubic foot per second.

It is estimated that $200 000 000 has been spent within the last seven hundred years to irrigate 1 000 000 acres in Lombardy, Italy. This gives an average cost of $200 an acre, although the average rainfall is 38 inches, with 22 inches in the irrigating season.

In fourteen districts of the Madras Presidency, India, the English found 53 000 tanks or reservoirs of native construction, with an estimated length of embankment of 30 000 miles. Some of these tanks are of large size. For example, the Ponairy, in Trichinopoly, had an embankment 30 miles in length, and a storage area of about 70 square miles; the Veevanum tank had an embankment of 12 miles in length, and an area of over 30 square miles. These dams or embankments are made of earth without puddle—in one case reaching a height of 108 feet. In the Madras Presidency the average annual rainfall is 35 inches.

Grade and Velocity.

The velocity of the water in the canal should be uniform. It might increase, but in no case should it diminish, as any diminution of the velocity of the current would cause a precipitation of the silt, which would have to be removed from the canal. The engineer should endeavor to deposit as little silt as possible in the canal and all he can on the land, where it is wanted for its fertilizing elements.

The grade should be regulated, in reference to the cross-section of the canal, so as to give a velocity of not less than three, or more than five, feet per second; if less, the silt would be deposited; if more, the water would scour injuriously the banks and bottom of the canal when constructed in light earth or sand. In rock or hard material there is no necessity of limiting the velocity.

Evaporation and Seepage.

This is an ever-varying quantity, owing much to the nature of the ground through which the canals are constructed, and to the care expended on the work. Under most circumstances the evaporation is but a small part of the loss. Mr. Walter H. Graves makes this in Colorado during the irrigating months from .09 to .16 of an inch per day. He thinks that 12 per cent. should be deducted from the carrying capacity in the older canals to allow for seepage and evaporation.

In some of the Colorado canals the loss from these causes is estimated at 50 per cent., which is excessive, and shows that the canal is constructed in bad soil, or that there must be something the matter with the construction. Twenty per cent. ought to be, under ordinary circumstances, a liberal loss from these causes, and this should largely diminish as the banks and bottom of the canal become compact.

Yield of Irrigated Land.

As a sample of the crops that irrigated lands yield, the following extracts are taken from a recent report on the desert lands near Boise City, Idaho, by Mr. A. D. Foote, M. Am. Soc. C. E.

"Mr. I. N. Coston, a member of the Legislature for many years, and one of the most prominent farmers in the Boise Valley, made the following statements to me:

" 'On ten acres of poorest land, with imperfect irrigation, raised 40 tons of red clover hay. Sold 75 000 pounds (1 250 bushels) of onions from 2 acres. Potatoes only gave 200 bushels to the acre. Have raised 1 000 bushels on 2 acres. Have raised 113 bushels of barley on an acre; wheat from 40 to 60 bushels; oats, 100 to 150 bushels; carrots and turnips equally good with potatoes. Connecticut Flint corn will grow well, especially on the higher benches; have raised 60 bushels to the acre in the bottoms. Prunes, the Germans say, grow better than in their own country. Apples, pears, peaches, plums, apricots, cherries, etc., as good, if not better, than in the most favored spots in California. The elm, soft maple, black walnut, locust, etc., make our best shade trees.' "

R. E. Strahorn says: "Idaho valleys cannot be excelled by any region east of California for the production of fruit. Apples, peaches, pears, nectarines, apricots, plums, prunes, grapes, and all the small fruits are produced in the greatest abundance and of a quality unsurpassed. The sage-brush lands, naturally the very emblem of sterility and desolation, are in a few years turned into the finest fruit farms with less trouble than would attend a similar transformation on the wild prairies of Iowa or Nebraska. A prominent fruit-grower estimates that 20 000 large fruit trees have been set out annually for the past 5 years in the valleys surrounding Boise. Several of the orchards in this locality produce from 25 000 to 40 000 bushels of fruit each annually, there having been but one failure in the crop for ten years. General L. F. Cartee, ex-Surveyor-General of Idaho, has 40 varieties of grapes in his vineyard, none of which have ever failed to bear a full crop, save the Catawba. John Krall, in the suburbs of Boise, has 125 acres in fruits (20 000 trees), embracing all the varieties known in this latitude. His production last season was 500 000 pounds. He finds no fruit insects yet, and pears are never troubled with blight or other diseases."

The writer can bear evidence to these statements about Idaho. Last August he examined, professionally, into the advisability and practicability of irrigating a large tract near Boise City. He had never seen anywhere larger crops of hay and all kinds of fruits, especially apples and plums. The wheat had been harvested, but, judging from the stubble, there was no reason to doubt the reported large yield. In fact all kinds of grain, vegetables and fruits of this latitude appeared to grow rapidly and yield abundantly. Boise City was visited with a large fruit merchant from Oregon. Though Oregon is celebrated for its fine apples, this merchant said he had never seen anywhere apple orchards equal to those near Boise City in quality and yield.

All this land produced nothing but sage-brush before it was irrigated.

One of the most extensive and profitable uses of irrigated land will be to grow hay for feeding cattle during winter. Without feed during the winter seasons it is hardly possible for the grazing section of the West to be more heavily stocked than Colorado is at present, but with reserve food in stacked hay for the winter or dry season the stock can be very largely increased.

During a recent trip through the stock range country, stockmen assured the writer that they could afford to pay from $6 to $8 per ton for hay for winter feed and fattening purposes, as the hay can be cut and stacked by contract for one to two dollars per ton. An acre of irrigated land will yield from four to six tons annually. This gives at once a large and profitable business to the farmer. In the northern part of the dry belt, Idaho, Montana and Wyoming, the summer feed is almost unlimited, and with the reserve of winter food the number of stock can be very largely increased.

Profits of Irrigation.

This is very variable, depending upon the crops, soil, climate, market, etc. It can however be safely asserted that crops will average one-half larger on land that can be thoroughly irrigated whenever requiring water, than on the same description of land depending upon the uncertainties of the usual rainfall, say 40 inches annually. This is shown by the great attention paid to irrigation, and the large amount of money spent in France and Italy to bring and distribute the water. Irrigated lands not only produce much larger crops, but the large crops are constant, without failure from drought. During dry seasons, when the farmers on unirrigated lands have short or no crops, the farmer on the irrigated lands has large crops, which he sells at famine prices.

The question is frequently asked how farmers living in the interior of the continent, far from all markets, can afford to pay the annual water rent for irrigating water, and the expense of distributing it. The following are the principal reasons why he can do this.

First.—He is always certain of a large yield.

Second.—He loses no seed, labor or crop by dry or wet seasons.

Third.—The crops are grown and harvested under a bright, clear sky. No loss from storms or rains before or at harvest.

Fourth.—Crops grown under these circumstances are always harvested in good condition, and are generally of superior quality; conseqently bring higher prices.

Fifth.—The railroads passing through this dry section have very wisely adopted low rates on grain and other staple products. This puts the farmer on these irrigated lands in a more profitable position than the Eastern farmer on unirrigated lands. The extra yield of irrigated land, in large unfailing crops of superior quality, more than pays the extra freight.

Sixth.—Irrigation makes the farmer entirely independent of the weather; he can make his crop early or late at will. He can have his land wet or dry, as desired, for plowing, cultivation or harvesting.

Seventh.—Another great advantage is the enriching silt that is brought down and deposited on the land by the water from most rivers. With this it is never necessary to use manure or fertilizers; by this the soil of cultivated lands along the Nile has been kept up, notwithstanding the thousands of years it has been cultivated. In all countries that practice irrigation, there are many instances where farmers pay high prices for water containing silt, when clear water could be had at much less cost. A prominent Idaho farmer said: "I would rather give two dollars an acre water rent for muddy water than one for clear."

In France, notwithstanding there is an average heavy rainfall, irrigated land is worth more than double what the same land would be unirrigated.

In Colorado, land that can be irrigated sells for $15 to $50 an acre, with an annual water rent of $1 50.

William Hammond Hall, M. Am. Soc. C. E., State Engineer of California, says in his last official report: "In California, lands purchasable at $3 to $10 without opportunity or reasonable hope of irrigation, command $50 to $200 per acre when water is brought to them and they have the privilege at hand to receive and pay for irrigation."

Irrigated land in Southeastern Oregon sells for $45 per acre.

TABLE No. 3.

VALUE OF IRRIGATED LAND.

Spain.	Value per Acre.		Annual Rent per Acre.	
	Irrigated.	Unirrigated.	Irrigated.	Unirrigated.
Alcanadre	$375	....	$45	$2 25
Zamora	175	$70	....	....
Near Madrid--				
1st Class Land	640	160	....	....
2d " "	500	100	....	....
3d " "	360	60	....	....
4th " "	300	30	....	....
San Fernando	....	25	25	....
Castillon	700	50	....	....
Valencia	400 to 900	80	....	....

Mr. J. F. Bateman, the celebrated English engineer, in a paper read before the Institution of Civil Engineers, in 1868,* says:

"The fee simple of land in Spain sold from £3 to £8, when not irrigated; the same land, when watered, sold at from £60 to £200 an acre. * * * On one side of a ditch there would be a field producing abundant crops of every sort, and on the other side, for want of water, the land would be a barren waste. * * * The rate to be paid for water distributed from the Henares Canal, amounted, in English money, to 28s. an acre annually. For that the tenants were entitled to twelve irrigations in a year, each irrigation amounting to 2½ inches in depth of water upon the ground, or a total depth of 30 inches in the course of the year.

"That rate was cheerfully paid, and well it might be, because the increased value of the land was at least seven-fold, and he should be nearer the average if he said ten-fold; for land which sold for £6 before irrigation, would sell for £60 after it was irrigated."

This is very strong evidence in favor of irrigation. Perhaps no one is as well posted on it as Mr. Bateman, as he constructed the Henares Canal and many other large works in Spain.

Irrigation and Navigation.

Many of the large canals of India and Europe were built with the intention of combining these. Experience has not shown this to be

* Proceedings of Institution of Civil Engineers, Vol. XXVII, page 512.

good practice, as it complicates the question very much, and at the same time makes the work much more expensive by requiring locks and larger cross-section in order to have less current. This causes the silt to settle in the canal, requiring labor to remove it, and deprives the water of the much-desired fertilizing elements. It is doubtful if the two systems can ever be advantageously combined in this country. This combination has in many cases added very largely to the cost of the Indian and European irrigation works.

Distribution of Water.

Distributing or dividing the water has been one of the most difficult problems connected with irrigation; all have felt the want of a proper measuring instrument. For the want of this, different systems have been followed, such as charging for the water so much an acre, or crop—thus leading to constant trouble and misunderstanding between the buyer and seller; and has made the buyer, as a rule, demand and consume more water than is necessary or desirable. All persons interested in irrigation agree that the water should be sold by measurement, provided a satisfactory apparatus for measuring it could be found. The water-meter invented by Mr. A. D. Foote, M. Am. Soc. C. E., described in a paper rceently read by him before this Society, seems to supply the deficiency, as it works well; by prolonging the weir, it can be made as correct as desired. It can be made by any carpenter from rough planks. It is simple, cheap and accurate.

In Italy and Spain the water is sold by measurement; in India by a fixed price per acre or crop; this last has been found to be the cause of great waste, in some cases being estimated at ten times the necessary quantity.

Riparian Rights and Ownership of Water.

This question has become a very prominent one in California since water has become so valuable for irrigation. From the settlement of the country it had been the custom for the mining ditches to hold absolutely all the water actually appropriated by diversion or used. Later, irrigation ditches followed the same custom, believing that custom made law; a great many millions of dollars had been invested in mining and irrigating ditches. As water became more valuable, and in greater demand, those who claimed riparian rights to the water appealed to the courts to prevent its being diverted to their injury. After long litigation, the case was finally decided last winter by the highest court in favor of the riparian owners, thus reversing at one blow the practice of forty years, where millions had been invested, and on which two of the principal industries, *i.e.*, irrigation and hydraulic mining, had been founded. The situation was so grave that a special session of the Legislature was called to see what could be done under the circumstances. The writer has not fully heard what was done.

Being interested in the matter, he recently obtained an opinion on riparian rights from a prominent legal firm who have very large experience on the subject. As this may be of service to engineers interested in irrigation work in our Western States and Territories, the following full extracts from the opinion are given.

"The provisions of the Idaho statute are recognized and authorized by Sections 2339 and 2343 of the Revised Statutes of the United States The right to sell and dispose of water is given by the act of the Legislative Assembly of Idaho before referred to.

"The laws of Idaho are intended to carry out the legislation of Congress authorizing the appropriation of water for irrigation and other useful purposes to the fullest extent.

"They entirely abrogate the common law doctrine of riparian rights, and subject all the waters running through the public lands of the United States to appropriation.

"In this respect the legislation of Idaho, and many other of the Pacific Territories, has gone beyond that of California and Nevada, and the recent decision of the Supreme Court of California in Lux *v.* Haggin would have been impossible under our statutes. * * * It is plain, from the foregoing summary, that in the State of Colorado, and in the Territories of Montana, Idaho, Dakota, Wyoming, New Mexico, Arizona and Utah, the legislation has wholly abandoned and abrogated all the common law doctrines concerning private property in streams and lakes, and concerning the 'riparian rights' of 'riparian proprietors,' the statutes in express terms apply to all streams, as well those running through public lands as those bordered by the lands of private owners.

"No exception from their operation is made in favor of persons owning lands on the banks of a stream. Under these statutes no proprietor derives any legal benefit or advantages from the fact that his land is immediately adjacent to a stream. Unless he has made an actual appropriation and diversion of its water for the use of his own land he is liable to have perhaps the entire stream appropriated and diverted away for the benefit of a proprietor whose land is situated at any distance from the stream."

From this opinion it appears that California and Nevada have not by special act abrogated or repealed the old common law of riparian rights, while most of the Far West States and Territories have done so.

This legal opinion was obtained, for use in Idaho, from one of the most prominent legal firms there; but it also covers most of our Western States and Territories. This opinion has been confirmed by a New York firm of high legal reputation.

Railroads and Irrigation.

This desert section of the United States is traversed by five transcontinental lines; the aggregate length of railroads in it is about 12 000 miles. Competition has divided up the business and reduced rates so, that now there is but little money in the through traffic. The railroads must look to local business for their revenue; that is, from mining, cattle raising and irrigated farming. The first is uncertain; cattle will always be a good business, but it will not be very large. Colorado is very heavily stocked with cattle, horses and sheep (many say too heavily), yet the annual shipment of these is only about three tons per each square mile. Farming, which can be followed very extensively along some of the lines of railroad by means of irrigation, will soon become a source of great revenue to those roads. Manufacturers will follow farming here as they do in most other places.

The farmers upon irrigated lands are comparatively more prosperous than upon unirrigated lands; they soon become infatuated with the land that always produces large crops regardless of the seasons or rains. All the money that they make goes back to the farm in additional comforts and improvements, all of which must pay tribute to the railroad in freight. The tendency on irrigated lands is towards small farms and dense population. In the irrigated district of Murcia, Spain, there are 2.6 persons to each acre. In many other irrigated districts in Spain there are over one person to each acre. The farmers on irrigated lands are comparatively prosperous, and are good patrons of railroads.

It is estimated that one acre of irrigated land produces, directly and indirectly, over one ton of freight from each acre annually; or over two hundred times more than the same land would if used without irrigation for raising cattle.

DISCUSSION.

T. C. Clarke, M. Am. Soc. C. E.—If Mr. Dorsey's opinion is correct, that "irrigation can be introduced profitably in most places and in most climates," several interesting inquiries suggest themselves.

For instance—why cannot the disposition of sewage be united with irrigation and thus solve one of the vexed questions of modern engineering?

As a safe estimate, assume that one cubic foot of water per second will irrigate 100 acres. One cubic foot of water per second corresponds to an annual discharge of $31\frac{1}{2}$ million cubic feet.

Authorities on sewage state that the amount of sewage discharged per head of population is six cubic feet per day, or 2 200 cubic feet per year. A town with a population of 10 000 would discharge 22 000 000 cubic feet of sewage annually, or enough to irrigate 63 acres of land.

The great difficulty with the disposition of sewage above ground as liquid manure, is the unsanitary condition of the land and the nuisances arising from evil smells. In addition to this, the above statement itself shows why it does not pay, as the land is too limited in amount.

Suppose even the town of 10 000 people dilutes their sewage and treats it not as liquid manure but as water of irrigation. Suppose they mix with it ten times the amount of water, procured as other water is. They can now irrigate 630 acres of land without bad smells or other objectionable results.

The general question will be: Can the additional cost of handling ten times as much water as would remove the sewage be paid for as irrigation of crops and absence of nuisances?

The particular question to be worked out in each separate case is: Will it pay to irrigate —— acres of land, and at the same time properly dispose of the sewage at a cost of $—— for pipes, pumps, pumping, etc.?

If Mr. Dorsey's views are correct, the town of 10 000 people could afford to spend—630 acres at $200 per acre = $126 000 and receive a return in irrigation alone. The amount of water procurable and the population would be the controlling elements.

A. D. Foote, M. Am. Soc. C. E.—Mr. Dorsey, in his valuable paper on irrigation, mentions several cases for the wide variation in its duty of water, as shown by his table following.

It seems to me that by far the most important cause is the method employed in delivering water to the irrigator. Throughout all irrigating countries it will be found that the duty of water is large or small precisely in accordance with the incentive offered to the irrigator for economy in its use. Where water is measured out at so much per unit there the duty will be large; where it is sold by the crop or area watered, there will there be waste and low duty.

Neither climate, soil, crops, rain-fall, nor the intelligence of the irrigator, will in the least account for the vast difference between the duty of water in Spain and Colorado. Spain gets a duty of 140 acres per cubic foot per second, and Colorado one of 56; and the sole reason is that in the early days it was the custom in Colorado to use and waste water at the aforesaid rate. When the later and larger canals were built, they adopted the custom in vogue and encouraged it by selling water by the acre irrigated. At the same time, as if dimly seeing an error in their method, some of the canals measure out their water, but at the rate of $\frac{1}{56}$ of a cubic foot per second per acre.

It was probably difficult to establish an increased duty, as in many cases water was offered as an inducement to buy land. As time goes on it will become more and more difficult to make the change. Changed it must be, or half the agricultural land of Colorado will remain forever barren.

In the arid portions of our own country, a large duty of water will become of great importance, and in constructing new works engineers will save immense trouble in the future if they so plan them as to make it the interest of the individual irrigator to get a reasonable duty for water from the outset.

The storage reservoirs in Idaho spoken of by Mr. Dorsey, were located by me. One very important item in their construction, which accounts in a great degree for their cheapness, and which he omitted to mention, is that in each case a natural waste weir exists. The dams, cheaply constructed of earth (with the proper masonry culverts through them), are high enough to send all waste water over the solid basalt dikes, which are indestructible and require no work or expense.

In the table of various irrigation works given by Mr. Dorsey, I think the wide divergence in costs given by him may be largely attributed to the right of way expenses and to the use of the canals for other purposes than irrigation; not, as is often rather derisively assumed, especially by foreign engineers, because we build "flimsy ditches." In this country the right of way is practically free; the Government gives it over its own land, and lands already occupied, through which our canals are likely to be built, are sufficiently compensated by the water privileges supplied.

As an instance I may mention that in laying out a canal in Idaho, which, with its branches, was several hundred miles in length, only one tract of occupied land was crossed, and the right of way over that was freely given for the water right at the regular price.

Compare this with Baird Smith's picture of Lombardy:

"The whole country is covered with them (minor canals) as by a dense network. At all levels, and by the use of various ingenious devices, they pass over or under or through each other in such ways as to preserve individual rights uninterfered with, though the result, to outward appearance, is a system of such marvelous complexity as to make the observer conclude it must lead to interminable disputes."

It may be well to state that the multiplicity and complexity was brought about by the laws of the country, which caused each landed proprietor to build his own private channel from the main canal, and required him to pay for the right of way and make all crossings. All this could have been avoided if the system had been laid out at one time, as would be the case in the deserts of this country.

The canals of India (and largely those of Europe) were built partially for navigation and water power, and consequently cost much more than if built simply to pass water through.

I quite agree with Mr. Dorsey's opinion in regard to velocities for irrigating channels, and wish he had gone further and given us the manner of determining them.

There seems to be a wide difference of opinion among engineers in regard to the proper formulas in these matters. I well remember being told by a Member of this Society that the formula $V = 90 \sqrt{R\ S}$, which I was using for a large canal, was "all wrong;" the canal would carry a third more than I anticipated. I was somewhat startled, but not convinced.

Captain Allan Cunningham, R. E., in his "Rourkee Hydraulic Experiments," gives the only records known to me of the absolute capacity of a large canal in its ordinary earthen channel. These results have been of great value to me, and I wish he had devoted his whole time to the ordinary channel and let the Solani Aqueduct alone.

Giving his values for R and S in the above formula, I find the resulting velocity agrees remarkably with his observed velocity. Whether this agreement will continue with increase in grade is a question which I hope to see satisfactorily answered. I believe in giving the greatest velocity permissible with safety to an irrigating channel, even if it should occasionally require slope wall or the loss of land in consequence of the extra grade.

The fertilizing silt which swift-running water usually carries is eventually nearly as valuable as the water itself. Without it irrigation in this country would soon be a failure. No land can stand continual production without enriching, and it will be many years before our Far West farmers can afford the ordinary artificial manures. The silt with which our Western rivers is loaded in the spring and summer is so valuable, that the land irrigated by it improves even under the heaviest cropping. I myself know of many instances proving this.

Baird Smith speaks of the great value of silt in Italy, and of the difference in price between the clear and silt-bearing waters. Moncrief gives striking instances of the building up of worthless land entirely from the silt deposited upon it, making it of great value. Wilson, in his "Irrigation in the South of France," speaks of the clear, cold waters of the Sourgues as being refused by the (land) proprietors when they could be had for nothing, and the waters of the muddy Darance bought instead. It is not altogether the silt held mechanically by the water which renders it valuable, however; matter held in chemical solution is sometimes considered the best part of the silt.

There is, I believe, no doubt whatever that irrigation is profitable to the irrigator. Arguments or facts on that point seem to me superfluous and almost absurd; but there is at times serious doubt as to whether irrigating channels ever will pay for their construction, except by indirect returns from the increased prosperity of the country through which they flow.

This has been the case with many foreign canals, which were built with that expectation by royalty or large land-owners.

Canals in this country are built either by individuals or corporations, and are often owned separate from the land. This may be the reason why they are almost invariably large interest-paying properties. Take the average for the whole State of Colorado; the irrigating works costing five dollars per acre, and the average price for water one dollar and a half per acre, gives thirty per cent. yearly interest on the investment, out of which the cost of repairs and distribution must be taken. Ten per cent. is an ample estimate for these, leaving twenty per cent. for the canal owners. Certainly a very good showing, when we consider the number of canals, with their different managements.

On the question of riparian rights there seems to be a settled opinion among Eastern people that it is an outrage to deprive a man of the privilege of having a river "flow unvexed to the sea" by his door. It is difficult, and may be impossible, to change this opinion, which has been the common law of a people for generations, without the aid of object-lessons, so to speak. If any one who thoroughly believes in riparian rights could but walk to the top of a *mesa*, in sight from the desk where I am writing, and look over the country below him for ten minutes, I am quite positive he would admit that riparian rights were not only inapplicable to such a country, but would be most unjust and tyrannical. From that *mesa* he could see the Boise River flowing for sixty miles through a valley only two or three miles wide, between steep *mesas* or banks, scraggy cotton-woods and willows along the river, and scattering farms along the banks, each with its little ditch carrying water to the fields and orchards. In June the turbulent cold flood from the melting snow tears away the feeble gates and dikes, and its brawling, ever-changing channel menaces the farms with sudden overflow. The orchards and farms are green and beautiful, and the crops are enormous; but away to the south and west stretch miles and miles of brown, gently sloping plain, a dreary waste of uninhabited desert, more than half as large as Long Island Sound. Surely it is not arbitrary, unjust or injurious in any sense to take this water out from the little valley, where it is not wanted (except that trifling portion needed by the farmers, and which the law protects them in), spread it over those vast plains, and eventually, like the waters of the Ticive "fertilize and stimulate the soil to such a remarkable degree as to render the region through which it passes one of the most productive and densely populated in the world." But the law itself is not arbitrary or unjust or for the benefit of monopolies. It is very carefully adjusted to the natural laws and wants of the people where it is in force. The riparian right doctrine is entirely abrogated, and it is an absolute necessity that it should be. The other water laws are well defined and distinct, and require of the appropriators of water certain conditions, which, if not adhered to by them, render their appropriation void.

The principle of the law is that the water belongs to the entire community, and the entire community must be allowed the use of it.

Canals are treated in some respects as common carriers. No monopoly can take the water and waste it, nor sell it one season and take it away the next, and thus ruin the irrigator; nor make the price prohibitory; nor do any unfair or unjust thing, simply because it has built a canal to carry water.

As to litigation. In the commonwealths of New Mexico, Arizona, Colorado, Utah, Wyoming, Idaho, Montana and Dakota, there is less litigation to-day, and less fear of it in the future, over water rights than over any other class of property of equal value and interest. In all the above named commonwealths from January 18th, 1884, to October 16th, 1886, there were but seven cases of litigation concerning the use of water, and all these were interpretations of the law, and did not question the law itself.*

The cases were as follows:

1884.	Colorado,	Sieber et al. *vs.* Frink et al.
1885.	"	Golden Canal Co. *vs.* Bright.
"	"	Knoth *vs.* Barclay et al.
1886.	"	Lerimer Co. Reservoir *vs.* People.
"	Idaho,	McCarty *vs.* Boise City Canal Co.
"	Utah,	Lehi Irrigation Co. *vs.* Moyle et al.
"	Arizona,	Clifford et al. *vs.* Larrien.

* In the foregoing I do not wish it to be understood that there were no actions in the lower local courts; there were the usual disputes about this kind of property in these courts, as about other kinds. Any case involving a principle or proper interpretation of the law would go to the supreme courts of the commonwealths which are the courts referred to.

The case of Lux *vs.* Haggin, in California, was decided by four judges against three, and the main points at issue could never arise in any of these other commonwealths.

The following is the syllabus of the case of Clifford *vs.* Larrien, decided in Arizona last summer. This would look as if riparian rights were on ditches rather than on streams.

"The owners of irrigated lands may enjoin one who wrongfully directs water from an irrigated ditch to their injury, when the ditch is the property of another.

"The owners of lands irrigated have rights in water flowing in a ditch, though the ditch be not their property."

In this case other parties were taking the water from the stream above the mouth of the ditch in question which had prior rights. The ditch-owners refused to bring action. The land-owners who bought their water from the ditch-owners then brought suit, with the above result. The court held that the land-owners were the injured party as well as the ditch-owners.

George G. Anderson, C. E.—Probably the most important point raised by Mr. Dorsey in regard to irrigation enterprises, at least as applicable to Colorado, is the necessity of providing storage reservoirs for the late water that may be required. This demand for late water is increasing here, as the average of grass crops that need water from frost to frost increases. Owing to a number of circumstances, it is evident to most observers in Colorado that the period of greatest supply in the rivers is becoming earlier every year. This is due mainly to the fact that the Rocky Mountains have been almost entirely denuded of timber, and the consequence is that, despite the fact that there is always, or almost always, a sufficient snow-fall during the winter, should the spring happen to be open and mild, the snow passes off very rapidly thus unsheltered. The season of 1886 gave very ominous evidence of the disaster this denuding of the forests may entail to the irrigation enterprises, where they depend on river supply only. The spring being mild, the rivers in the northern part of the State were in high water at least three weeks earlier than usual, and before the main supply for irrigation was required. Early in July, at the height of the irrigation season, the rivers were comparatively low. The demand for water was largely in excess of the supply, and had it not been for providential rains, which cannot always be relied on at that season of the year, serious losses might have ensued to growing crops. The Federal Government have now taken steps to prevent, in part at least, this wholesale destruction of timber, when it is probably too late. It would have been well to have enforced lumber-men to plant where they cut, and that can even now be done.

But the building of reservoirs in the mountains to impound the water involves legal as well as financial and engineering questions. It is very doubtful, in the present agitated state of feeling on these questions, if public opinion would permit canal-owners building such reservoirs to be used as aids for individual enterprises, and it is equally doubtful if the State would appropriate any sum to be spent in the construction of such works, even resulting, as it would, in the general good. The Federal Government might be and has been appealed to to make appropriations for such works for States depending on irrigation, but it is apparently impossible to impress upon Eastern legislators the necessity of doing anything of this kind to develop the growth of Western States, not, probably, because they are prejudiced, but because they do not appear able to comprehend the importance of these urgent matters.

As far as Colorado is concerned, it is very doubtful if individuals or corporations—owners of canals—would by themselves venture on such work, considered purely on a financial basis, as at present such enterprises as these are can hardly be classed as revenue-bearing, considered simply as means of conveying and distributing water to farming land.

In regard to the question of evaporation and seepage, Mr. Dorsey regards the estimate of 50 per cent. loss from these causes on some Colorado canals as excessive. It is undoubtedly excessive, and it is as undoubtedly true, in some instances at least. General Pierce, the engineer for the Denver Water Company, an old resident of Denver, and a gentleman who gives close attention to such questions, gave evidence recently in a lawsuit, that on two different occasions he had measured the ditch supplying the City of Denver with water for irrigation and had found on one occasion a loss of 50 per cent., and, on the other, a loss of about fifty-two per cent. The construction of this canal, it may be said, is not of the best. The writer made measurements on the high line canal in the middle of July (1886), and found that where 156 cubic feet per second were passing into the head-gates, only 80 cubic feet per second were passing a point 45 miles from head-gates, and no water was used for any purpose in the intermediate distance. This was during the very hottest and dryest period of an unusually hot and dry summer in Colorado. The soil through which this canal passes is in many places very pervious. There are long stretches of fine sand and in places the canal bottom is on rock badly fissured. The alignment of the canal is very crooked, and no doubt a great loss is experienced from this source. It is to be expected that this serious loss will gradually diminish as the canal bed and sides become compact and puddle naturally. But to estimate a smaller loss from these causes than 25 per cent. would scarcely be wise.

Mr. Dorsey states that the duty of water in Colorado is fifty-six acres per cubic foot per second. This is so at present, but the remarks quoted from Mr. Stevenson, of Salt Lake City, in regard to duty in Utah are applicable to Colorado. It is reasonable to expect that more careful husbanding will greatly increase the duty. There can be very little doubt that in this regard a great deal depends upon the irrigator's knowledge of the economical use of the water, and it is fair to say generally, that the user of water in Colorado is not yet fully alive to this matter. There are exceptions to this, but it must be remembered that the great increase of irrigation is of recent date, and the economic value of water is only appreciated by continued experience. In many instances, too, the farmer is not altogether to blame; there is not much encouragement given him to save water. Though the water is in most instances sold by measurement, it is on the basis of a certain quantity per acre, and the farmer is liable always to take the full amount for which he has paid. It would probably be better to sell and distribute the water by measurement in quantity without any reference whatever to acreage, and thus give farmers an opportunity of economizing. There are, however, on the other side many reasons in favor of the present system of distribution by acreage, at least at the commencement of large enterprises. When it can be clearly demonstrated that the system of measurement by quantity alone will tend to greater economy and be to the mutual advantage of buyer and seller, it will speedily be adopted. And, as a first step to that, a simple, accurate, and inexpensive system of measurement must necessarily be adopted. That has still to be supplied in Colorado.

Mr. Dorsey has ably stated the advantages of farming by irrigation; it would be difficult to do so more tersely.

The value he gives of irrigable lands in Colorado is fully within the limits. Such within a radius of ten miles of Denver rate as high as $75 to $100 per acre, with all water privileges secured.

In the opening paragraph of his paper he says, "perhaps some few meadow and bottom lands would not be improved by it."

This raises another important consideration—the necessity of drainage accompanying irrigation. The writer knows of large tracts of bottom lands which, comparatively dry and valuable before irrigation of the uplands was prosecuted to any extent, are now wet and swampy and considered valueless. They are not really so, however, and only require judicious draining to be made as valuable as the uplands. Several cases are known to the writer where drainage has been followed with most satisfactory results, and the importance of such work will soon be recognized.

Professor EDWARD MEAD (Colorado Agricultural College).—The State of Colorado probably contains the largest and most prosperous irrigated district in the United States. It owes its prominence to several causes, the most prominent of which will be mentioned:

First.—The natural advantages of the country for the building of canals, which have rendered their engineering and construction, as a rule, exceedingly simple and cheap.

Second.—The surprising fertility of the soil, judged from its unpromising appearance before being irrigated.

Third.—The superior local market afforded by the mining camps. The demand for most agricultural products is as yet far greater than the supply, and, as the uncultivated region between the irrigated district of this State and the non-irrigated lands of Kansas affords farmers here a protective tariff in the shape of railway charges, prices have been very remunerative.

Fourth.—The beneficial character of much of the earlier irrigation legislation, which, abolishing the doctrine of riparian rights and making the right to water depend on priority of appropriation and use, has freed us from the dangers and perplexities which now beset California.

Comparatively little engineering skill was required in the construction of the earlier canals east of the Rocky Mountains. The surface of the country has a continuous and comparatively regular slope to the east and southeast, varying from ten to twenty-five feet to the mile, and having been but slightly affected by erosive action, there are no deep ravines or abrupt slopes. But few flumes or embankments, therefore, were required. After leaving the foot-hills the streams flow over shallow beds, whose banks rarely exceed a height of ten feet. The head-gate of the canal is located in some favorable bend, with the bottom of the canal excavated below the bed of the stream, which device obviates the necessity of a dam, or, at most, only a temporary one for low water. The engineering work connected with the construction of some of the larger canals recently built has been both important and difficult. One now being built in the western part of the State diverts the water of the Doloris River into the drainage area of the San Juan by a tunnel one mile in length.

The majority of devices at present employed for measuring water are exceedingly crude and unsatisfactory. The law defining the "statutory inch" is imperfect; the name of the unit is unfortunate, and its use not being compulsory we have almost as many measuring devices as we have canals. The majority are faulty in design, and in many cases worse in matters of construction and location. The use of the term "inch" has led to much confusion, not only among farmers, but judges have rendered decisions in which the term inch only meant the number of square inches in cross section, without regard to the form, size, grade or character of the ditch. One of the great needs of our system at present is the establishment by law of some form of measuring device which shall be reasonably accurate and whose working can be easily understood. The invention of Mr. Foote is an excellent one, but I am at a loss to understand how it could be employed on our large canals. At present all our measuring-boxes are made of wood, but it is to be hoped that masonry and iron will ere long be substituted.

Surface irrigation, either by flooding or by running the water in open furrows, is the universal method of using water. It is universally conceded that sub-irrigation would be a more economical method, but so far the cheapness of land and water, and the high price of pipe material, have prevented its adoption.

What is especially needed in this State is a reform in our present methods of selling water, and laws to protect the users of water against the encroachments and abuses of canal companies. There are at present no adequate laws regulating the price to be charged for water or the manner in which it is sold.

The practice which generally prevails is for ditch companies to sell what are known as "Perpetual Water Rights." The essential features of these contracts are that the ditch company sells to the farmer a perpetual right to enough water from their ditch to irrigate a prescribed area, usually forty or eighty acres, subject to a number of provisions about as follows:

"The amount considered necessary to irrigate the area for which the water right is sold shall in no case exceed a certain specified volume, and the ditch company cannot be required to furnish more than this amount."

"The ditch company shall not be held liable for failure to furnish water by reason of accident to ditch or drouth."

"That the buyer must use the water on the area for which the 'right' was purchased. Hence if the purchaser of an eighty acre 'water right' could, by economy, make it water one hundred acres, without exceeding the maximum volume allowed, he is estopped by his contract from so doing."

The cost of these "rights" has risen steadily with the value of land from three to five dollars per acre under the earlier canals, to from ten to twenty-five dollars per acre at present. This increase in price is not due to increased cost of construction, because in many cases the reverse has been true, but to the fact that ditch companies, in the absence of legal restraints, have been in a position to reap the full benefit of the rise in the value of land. As land without water has a very small market value, and is almost wholly unproductive, the farmer, as a business proposition, can afford to pay almost as much for water alone as his land and water will bring. Accordingly, when land with water sold for ten dollars per acre, water-rights sold for five dollars per acre. When land with water advanced to twenty-five dollars per acre, water rates rose to fifteen, etc. The unlimited license afforded ditch companies in the amount of their charges for water, has made irrigation enterprises a profitable field for investment, of which capital has not been slow to take advantage, the result of which is that canal-building has largely outstripped settlement.

The objections to this system are so obvious as to scarcely need mention. Among the more important may be mentioned:

It is unjust, in case of failure to furnish water, which may result from drouth or mismanagement of the ditch. The farmer loses his crop, while the ditch-owner reaps the proceeds on the money received for the water right.

It offers no incentive to economy in the use of water, but, on the contrary, has been a constant inducement to extravagance and waste.

The high price of "water rights" requires the outlay of a larger sum of money to begin farming than the average settler can command, or the incurring of a greater indebtedness than is prudent in attempting a new system of farming, all of which has operated to retard emigration and settlement.

Of the measures of reform which to me seem necessary, I can only indicate a few.

Instead of a large outlay at first for a perpetual water-right, water should be sold by an annual rental or charge, and the farmer only required to buy or pay for the actual quantity necessary to irrigate his crops. In case a ditch company failed to furnish water, no payment should be exacted. In this way, in seasons of scarcity, both farmer and ditch-owner would bear the burden of failure; as it is the farmer bears it alone.

We should have a standard unit of water measure and a standard measuring device, whose use could be required by either farmer or ditch company.

I am convinced that the "duty" of water for this State, as quoted by Mr. Dorsey, is erroneous, being much below the truth. It was the generally accepted amount in the beginning of irrigation, when the duty was much lower than at present, and, in the opinion of the writer, was too low then, because based on the maximum amount required during the short season of wheat irrigation rather than on the average duty for one hundred days. During the past three years the writer has been employed, through the greater part of the irrigating season, in making measurements for the State of the actual carrying capacity of irrigating canals, and, as a result of his observation and experience, is confident that the majority of farmers do not use more than one-half the amount of their estimated water right.

The daily discharge of the Cache La Poudre has been recorded for the past three years. In 1886, nearly the whole of the discharge was required for irrigation. The amount of land under cultivation on this stream can be closely estimated, and, making no deduction for the portion of the discharge which escaped without being used, or that lost in canals and in the river by evaporation, the duty was one hundred and ninety-three acres to each cubic foot per second. The writer has also observed, in a series of careful experiments now being carried on at the Colorado Agricultural College, that, while evaporation from water surface is much more rapid here than in the Eastern States, the evaporation from soil surfaces scarcely equaled those reported by Beardmore for the rainy districts of Eastern Europe, showing that the soil has to an unusual degree the power of retaining moisture. The average duty of water will not probably fall below one hundred acres, and in many parts of the State is much higher.

It would be a difficult matter to state adequately the benefits which irrigation has conferred on the State. To properly appreciate them one should see the barren, treeless plains above the ditches, and the thriving bounteous fields below. From the room where this is written can be seen fields that have yielded seventy bushels of wheat and nine tons of alfalfa per acre. On the experimental grounds of the Colorado Agricultural College wheat has yielded ninety bushels per acre. These are, of course, exceptional yields, but they are such as any agricultural section can afford to be proud of. And this was once one of the most unpromising portions of the "American Desert." The average yields of small grains and grasses are believed to exceed those of any non-irrigated State, with a certainty of success. No farming community is more contented and prosperous than that of this State as a whole, or more sanguine of future success. Land with water sells from $25 to $150 per acre; without the water it can hardly be said to have a market value.

It was once supposed that fruit could not be grown, but at Fort Collins, in the northern part of the State, at an elevation of five thousand feet, the Hon. W. F. Watrous gives me the following statistics of his yields in fruit culture:

He has gathered one-half ton of grapes from seventy vines four years old; eight bushels of currants from a row of bushes two hundred feet long; also an equally large yield of gooseberries. His largest yield of strawberries, eight thousand quarts per acre; this on heavily-manured prairie soil. He has now thirty varieties of standard apples, and at a recent horticultural exhibit in Denver, one hundred varieties of standard apples grown in the State were exhibited. The success of agriculture here, and the consequent cheapening of its products, has made it possible to more rapidly and successfully develop the mineral resources of the State. Without the home-grown products, the cost of living in mining towns would be so enhanced that hundreds of mines that are profitably worked to-day would be closed.

It has given employment and restored to health hundreds of men who were hopeless invalids in the East.

We are just awakening to a proper realization of the magnitude of this interest, and to the advantages of better practices and better laws. With them will come a more rapid settlement, while the unsurpassed healthfulness of the climate and charm of the mountain scenery will continue to be a potent factor to the same end. And it is confidently predicted that before another half century this State will contain one of the most densely populated and prosperous agricultural districts on the continent,

H. V. Hinckley, M. Am. Soc. C. E.—During the past nine years I have observed irrigation in Central and Southern Colorado and Southwestern Kansas. To ask whether irrigation is a success is to ask whether it is profitable to pay from one dollar to two dollars per acre to insure a full crop in place of one-half or two-thirds of a crop, or sometimes no crop at all. Of course the irrigating privilege per acre may be abused, and too much water is no better than too little. The amount of water and manner of application to produce the best results is a matter to be learned only by experience in each particular case. It is a wonder to my mind that irrigation has not been more extensively practiced in the United States. The first irrigating canal in Kansas was dug in 1880. There are now not less than seven main canals in operation in Southwestern Kansas along the Arkansas River, aggregating possibly three hundred miles in length. A new canal (the Eureka) is just completed from Ingalls, *via* Dodge City, to Kinsley, a distance of 70 miles. The laterals or branches will have several times that mileage. The main canal is 40 feet wide, falls 1⅓ feet per mile, carries 8 feet of water, has cost about eight hundred thousand dollars, and its irrigable territory embraces at least a half million acres of land that now raises nothing for want of water. The average rain-fall is 19 inches, being mostly out of season and unreliable for crops.

The Arkansas River in Kansas is nearly or quite dry during the fall and winter, but through the summer, when water is needed for irrigation, the storms and melting snow on the "Snowy Range" of Colorado swell the river to a turbulent stream.

I quote the following: "Careful estimate of the average of crops grown by irrigation in Western Kansas: Irish potatoes, 300 bushels per acres; sweet potatoes, 300 bushels per acre; onions, 400 bushels per acre; cabbages, 4 000 heads; melons, 8 000; turnips, 1 000 bushels; oats, 75 bushels per acre; spring wheat, 20 to 25 bushels per acre; corn, 40 bushels; sorghum, 15 tons; millet, 4 tons; alfalfa, 8 to 10 tons."

In Kansas "it shall be unlawful for any person or corporation to locate or construct any irrigating canal along or upon any stream of water, or to take and use the water of any stream in such a manner as to interfere with or in any wise hinder, delay or injure any milling or irrigating improvements already constructed or located along or upon any stream of water, or to diminish the supply of water flowing through any established irrigating canal."

A corporation for irrigating purposes, organized under the general laws of the State, acquires a perpetual right to take water from a natural stream, subject to the above limitation. In Colorado the canals upon any stream share the water *pro rata* under the direction of water commissioners and under the general supervision of the State Engineer.

I have seen people wild over their first experiences at irrigation, and I have seen land appreciate wildly from $5 per acre to $200 on the advent of a canal and afterward fall to $20 or $30 per acre where it belonged. Land where full crops cannot be raised with an annual certainty is permanently increased in value by irrigating facilities.

Frederick Eaton, M. Am. Soc. C. E.—The following practical illustrations in support of the claims for the benefits of irrigation are based on experience and personal knowledge.

The Colony Riverside, of San Bernardino County, Southern California, comprises about six thousand acres of land.

It depends entirely for its success on irrigation, like the largest part of the southern part of our State. The soil of the colony is a sandy loam, moderately heavy and of a brown color, caused by oxide of iron; the stratum is eight to twenty feet deep, and rests on a stratum of sandy gravel. The climate is well adapted to the culture of the great majority of fruits.

These lands, before they were settled, were not even considered suitable for grazing purposes, as the average annual rain-fall did not exceed four inches; in consequence the wild grasses seldom attained a growth that would furnish picking for sheep. The lands at that time had therefore hardly any value.

When these lands were put under irrigation some fourteen years ago, they sold for about twenty-five dollars per acre. They have since been steadily advancing.

We find them changing hands now at three and four hundred dollars per acre for uncultivated lands, and from eight to fifteen hundred dollars per acre for lands with matured vines and trees. It cannot be justly charged that these prices are stimulated by social, educational and other such advantages, while the products net the land-owners, annually, two hundred dollars per acre. As further evidence that these prices are entirely due to the influence of irrigation, I will state that a tract in the immediate vicinity of Riverside, having the same character of soil, has recently been made available for cultivation by the construction of the Gage Canal, and the lands which were bought for about ten dollars per acre before the canal was built, are now selling for one hundred and fifty to three hundred dollars.

This land has the same communication facilities and markets as Riverside, and as there are no improvements on them yet, there is nothing but the water to which this increase can be attributed. If the water apportionment to these lands were made on the basis of duty obtained under the Riverside Canal system, the lands would sell for fifty per cent. more. The duty expected from a cubic foot per second under the Gage Canal is two hundred and fifty acres, which is about seventy-five acres more than has been realized under the Riverside Canal. It is possible to obtain this duty for such crops for which the land is intended to be used, provided the distribution is effected through pipes extending over every carrier. This system of distribution would add about fifty dollars per acre to the cost of the land.

Water rights are sold in the Gage Canal at the rate of twenty-five thousand dollars per cubic foot per second, which, on the proposed duty basis, makes the water right for each acre one hundred dollars. The land without a water right is worth ten dollars per acre, and immediately rises to two hundred dollars or more when the water right has been appropriated to it. These figures indicate that land originally worth one thousand dollars, by an investment of twenty-five thousand dollars, became worth fifty thousand dollars. As to the probability of an appreciation in these values, there can be no question so long as the water supply holds out. The experience of Riverside would seem to decide this point.

As to the permanency of the values, it remains with the orange and raisin market to determine.

The foregoing illustration is one in which the argument in favor of irrigation is conclusive. I will produce another where the proof is less striking, but still very clear.

Pasadena, a settlement situated in one of the valleys of Los Angeles County, has an average annual rainfall of about fourteen inches. The peculiarities of the soil are such as to retain moisture well, and the locality is frequently visited by moist fogs. The water supply is very limited, and the most rigid economy is practiced in its distribution.

The duty obtained from a cubic foot per second has been reported as fifteen hundred acres. In order to accomplish this, the entire distribution is done through iron pipes, laid largely in such a manner as to enable the water to be delivered through a hose at each tree. The method of preparing the ground for irrigation, as practiced in this section, and all others where water is scarce, is to hoe up a levee around the tree about six inches high so as to form a basin about eight feet in diameter. This basin is filled with water to the depth of about six inches. A constant discharge of a cubic foot per second will fill 150 000 of these basins eight times a year, which is equal to a flow of four feet over the area occupied by each basin, or a flow of six inches over the entire surface of fifteen hundred acres. This basin system of irrigation is attended with bad results and should never be started. It concentrates the roots around the trunk of the tree, and forces their growth to the surface. The practice of Riverside, and other places where water is plentiful, is to flow between the trees in furrows. This method requires three times the quantity of water used by the basins, but it is the most approved way. There are many improved places in the Pasadena settlement which have never been irrigated, but they do not produce in quantity what the irrigated places produce. It is claimed by those who have no water rights that cultivation is a substitute for irrigation and gives the same results.

This is true in this section as to the "growth," but will not hold in regard to the "yield."

In conclusion, I will say that irrigation will, in every instance, prove beneficial to every part of Southern California.

Henry A. Brainard, M. Am. Soc. C. E.—There is no question about the fact that irrigation has changed the deserts of California and Colorado into fertile fields, and that from being almost worthless they are now worth several hundred dollars per acre in many localities. The land about the thriving City of Los Angeles was little better than a desert, and a tract of land near the town of Riverside, purchased for a few dollars per acre three years ago, and being now supplied with only a very moderate portion of water, is valued at $300 per acre.

Some lands in California seem better adapted to endure the long months of rainless summer than others. On such lands a growth of wild oats springs up in winter, and becomes ripe and dry in June. On desert lands sage brush and cactus are indications of its arid nature.

In what I have to say upon irrigation I will endeavor to confine myself to two subjects which I see are not treated in Mr. Dorsey's paper.

First.—Irrigation by means of artesian wells; and

Second.—The circumstances and conditions when irrigation is detrimental.

I have observed irrigation by artesian wells in a section of country of triangular shape, the base being the southern end of San Francisco Bay, and the apex a point a few miles south of the the City of San José. Within this district artesian water rises from two to twenty feet above the surface. The wells are from four to seven inches in diameter, cased with iron and capped at the top, with regulating stop-cocks or valves by which the supply may be entirely shut off.

Surrounding this district is still another, in which water rises to the surface only, or within distances down to 50 or 60 feet, the wells being from 100 to 200 feet deep.

In the artesian belt the wells vary from 100 to 500 feet. One well irrigates from ten to twenty-five acres of land. The wells are bored along the highest side of the land, the overflowing water being conducted in wooden flumes laterally, and distributed as in any irrigation.

A system of sub-surface irrigation has been attempted by covered perforated pipes, but I have never observed a case of three years' standing that was not a failure; roots fill the pipes, and it is soon abandoned. If it could be used it is more economical of water.

The crops benefited by irrigation are strawberries, small fruits, garden vegetables, garden seeds, alfalfa (for hay and pasture), and late sowed or second-crop fodder corn, which is put in after a crop of barley has been harvested. By means of irrigation we harvest strawberries in

April, and thence every month till January. As I write (December 23d) our markets are plentifully supplied with them for Christmas.

In the district where water does not flow, pumps are used, driven by steam or horse, wherever irrigation is desired. A small engine will irrigate five acres in twenty-four hours, and on the basis of once a week in the application of water, will have a capacity of thirty-five acres.

The advantages of irrigation by artesian wells are: Absolute control of the water as to the amount and also as to the time when it may be used. No complication of riparian rights and appropriation can arise; you use your water when you need it, and just as much as your wells can supply. As artesian wells have multiplied, there has been a reduction of head amounting to from two to four feet within the last twelve years.

Second.—In speaking of the disadvantages of irrigation, I will say that in the valley pastures of Santa Clara County, which I shall use as one location where I have made observations, there is an average rainfall of 12 to 14 inches (21.75 last winter); on the Santa Cruz Mountains, next the ocean, from 40 to 60 inches; and in the coast range proper, on the east side of the valley, 24 to 40 inches. Streams run down from the mountains and sink after the month of June, except the Coyote and Guadalupe Creeks, which carry a small stream during most seasons. Rain falls from November to May inclusive, and there are five months of rainless summer.

This county is largely planted to orchards and vineyards; what is not so occupied is used for grain and hay (hay here consists of grain cut before it is fully ripe). Irrigation has been tried on orchards and vineyards, and is being entirely abandoned by most people.

Grain does not require irrigation. Sowed during winter it ripens in June. Trees grow more rapidly and keep greener for a longer season by irrigation, but they fail sooner. Irrigation causes them to throw out roots near the surface, and they are not well fitted to endure a drouth. Trees never irrigated send their roots downward for moisture, and do not suffer. Fruits raised on irrigated trees grow larger than non-irrigated fruits, but lack in flavor and solidity. Irrigated fruits will not bear transportation; non-irrigated fruits stand transportation to Chicago or New York.

A large crop here consists of French prunes. On irrigated trees four pounds of fresh prunes are required to make one pound of dried ones, while from trees not irrigated two and one-half pounds of fresh fruit make a pound of the dried article, which is better than that from irrigated fruit. The same thing in regard to peaches. Irrigation makes them larger, but watery and flavorless; canners do not like to buy irrigated fruit of any kind, and it is almost worthless for the evaporater. It has been supposed that oranges must be irrigated. One of the best groves I ever saw never had one drop of irrigation, and bears abundant crops.

Vineyards that are irrigated produce grapes that make a poor, crude wine. Table grapes that are irrigated do better, but they will not bear transportation to distant markets. There is ample proof of this. Grapes from the irrigated vineyards of Southern California do not keep half as long as the grapes raised on the foot-hills of Central California. The latter go successfully to New York; the irrigated fruit in most cases proves a loss.

Thorough cultivation, stirring the surface soil, takes the place of irrigation. If the surface for three or four inches is kept stirred and pulverized, there is a capillary attraction that brings the water from the depths, and moisture comes from below instead of from above, as in irrigation.

Another ill-effect of irrigation is malaria, which almost always results from its operations.

In the San Joaquin Valley, and southward, there are many places, formerly healthy, where people now suffer severely from chills and fever and other forms of malarial diseases. These are unknown when irrigation is not followed.

Recognizing the benefits of irrigation to desert lands and to the crops we have heretofore named, we should recommend to every engineer to study carefully the rain-fall and soil of any country before undertaking or advising expensive works for irrigation. If there is an annual rain-fall of ten or twelve inches, irrigation should only be provided for the products heretofore named, leaving grain, fruit and vines, squashes, melons and trees to be provided for by cultivation.

We know of many places where thousands of dollars have been expended for irrigation, and the works finally abandoned as not desirable. We have seen as many dry fields, producing in their natural state nothing but chemisal and sage, and formerly considered dry and worthless, reclaimed by cultivation only, and made to produce grapes and fruits in abundance, and rising in value in four years from $10 to $400 per acre.

C. L. Stevenson, C. E.—In Utah, with an annual rain-fall averaging less than sixteen inches, and which rainfall almost entirely occurs between the 1st of November and the latter part of May, irrigation is absolutely essential to the raising of nearly all the products which go to support life.

Lying in the great basin between the Sierra Nevada and Sierra Madre, are those agricultural valleys and mountain slopes which have become noted for their continued fertility over a period of some forty years.

The alluvial deposits there found, both in valley and on mountain-side originally only growing sage brush and wild grasses, are now being gradually redeemed. with a result that would seem incredible to any not accustomed to the effects produced by the proper application of water.

In this connection I would say that my statement to Mr. Dorsey, that with proper irrigation a failure of crops is unknown, and which he cites as strong language (although my experience for twenty years), is simply one of those statement of facts which would with us in Utah excite no comment.

When some forty years ago Brigham Young and his followers entered the Great Salt Lake and Jordan Valleys, they were told by Bridger, one of the earlier pioneers, that he would give a twenty-dollar gold piece for every bushel of wheat they could raise; it was probably a surprise to both when on the very ground pointed out, fifty-six bushels of wheat were raised to an acre under the crudest methods of applying the waters of the nearest mountain stream.

Even to-day the system of irrigating in Utah (if system it can be called) is of the crudest, with some trifling exceptions.

Unlike some of the adjoining Territories, the custom in Utah has obtained of having the use of "appropriated" waters inure to the land in perpetuity. That is, a small community would take up the entire flow of a stream and accord it to a certain acreage without regard to the real water requirement per acre. This generally was done under the auspices of the Mormon Church, the dominant element then and now in Utah, or rather of the leading men therein.

When other settlers came in to occupy adjoining lands, they found themselves only entitled to "waste water" or a lawsuit. Hence has arisen the singular fact of adjacent lands having first, second and third water rights. Those with first right, say in Jordan Valley, from original cost of $1.25 per acre, at once attained a value of from $15 to $25 per acre, and inside of five years a value of $80 to $100. This last is a small valuation, when we consider the sale value of the crops that can be raised thereon with so little cost to the cultivator.

The waters of irrigation from the mountains annually carry with them fresh fertilizing material, so that practically it costs the average Utah farmer less to keep up his ditches and apply his waters of irrigation than it does the Eastern farmer to manure his land.

One field near Farminton, at first producing some sixty bushels per acre, was kept in wheat for thirty years with no other fertilizer than what was brought by the waters, and there was after the second or third year a general average yield of over forty bushels to the acre.

Not only does the "water right" immediately increase the value of the land to which it attains, but it enhances values to land adjoining, even if such land has no water right.

In Utah, as elsewhere, it is found that proper irrigation tends to a permanent improvement of the soil, and substantially an increasing value more than commensurate with increasing population; at least this is the fact with us in Utah.

Somewhere about the year 1880, Mr. O. J. Hollister, a very careful statistician, and others, as well as myself, made pretty careful investigations as to the "duty" of a cubic foot of water per second when applied in the ordinary way—and right here let me state that the average farmer soon learns not to use an excess of water on his crops, however much he may let run to waste to prevent a "second right" neighbor from getting it. Since then I have verified, in several ways, the results then obtained, and can safely say that while this duty averages one hundred acres, at least one hundred and twenty acres could be just as well cultivated with the same amount of water.

For the first few years, according to soil, the area that can be successfully cultivated with one cubic foot of water per second will not exceed, perhaps, sixty acres, but as the grounds become saturated, the average soon, with us, gets to be about one hundred acres.

I do not think the Utah method of having the water right made a part of the realty by any means a correct one, since it naturally leads to a less acreage cultivated. For the best interests of the community, I consider that the water used for irrigation should be paid for by the cubic foot, delivered to the consumer.

Owing to lack of proper system of distribution and an almost total disregard of engineering economy in construction, I think it safe to say that not one-sixth of the available waters now appropriated is really utilized in Utah.

E. E. Russell Tratman, Jun. Am. Soc. C. E.—In connection with the paper and discussion on "Irrigation," the following extracts from "Irrigation in Spain," written by J. P. Roberts, and published in 1867, may be of interest.

"In some of the immense plains which form the valleys of Spanish rivers, the soil, which is a rich, black, alluvial deposit, is often from 10 to 20 feet in depth; and when the surface becomes exhausted, the farmer replenishes it by digging trenches and turning up the virgin earth, and thus, year after year, crops are grown without putting any manure on the land. When these valleys are irrigated, there seems to be scarcely any limit to what they can produce.

"One cannot but feel surprised at the large amounts which were expended in years past in irrigating comparatively small plains. The Alcavadre Vega, in the valley of the River Ebro (which contains only about three hundred statute acres), is watered by a noria, or undershot wheel worked by the river, and from a careful estimate it has been found that the works for irrigating this land must have cost at the rate of about $175 per acre. This is a great outlay, when the quantity of water pumped up, which is only twelve gallons per minute, is taken into consideration.

"In the Rioja district there are some highly cultivated valleys, which are irrigated in various ways by the waters of the Ebro and other rivers, and they are so much prized by their owners that it is very seldom a plot of ground is to be sold. In the Alcavadre plain the statute acre sells for about $625 to $750, and in other districts which are watered by the rivers Najarilla, Yregua and Ebro, land is rented at from $46.25 to $50.50 per statute acre, while the same quantity of dry land in these districts only produce an annual rent of from $2.16 to $2.88. A report on the effects of irrigation on land adjoining Logrono states as follows: 'The crops are insecure in consequence of the dryness of the climate, and third-class land only produces $2.16 worth of corn to each half acre

(.47) of land. This is at the rate of $1.08 per year, for it is the custom of farmers to till the land only once in every two years, and even this small return is uncertain for want of water. If these lands were irrigated, the produce would increase from half an acre to three and a-half acres of corn for each acre of land, calculating only one crop for every two years; but with the benefit of irrigation the lowest estimate would be one crop for each year, and with good management you could safely calculate on two crops annually, even on third-class land.'

"In the Province of Zamora, in the valley of a river, a part of which was partially irrigated, the watered land sold at from $175 to $205 per acre, while the dry land usually realized from $70 to $90. The difference between the value of watered and unwatered land is not so great there as in other parts of Spain, owing, in some measure, to the climate and want of experience in irrigation, but chiefly to the ignorance and indolence of the farming classes.

"A Spanish engineer who has given considerable attention to the subject of irrigation (Don Juan de Ribera), states that a good supply of water increases the value of land near Madrid from four to ten-fold, and he thus classifies it:

First-class unwatered land, worth $160 per acre, increases to $640 when irrigated.

Second-class, worth $100 per acre, increases to $500 when irrigated.

Third-class, worth $60 per acre, increases to $360 when irrigated.

Fourth-class, worth $30 per acre, increases to $300 when irrigated.

"In the Ampurdan, a fertile plain on the shore of the Mediterranean, the farmers state that the value of land is increased from 100 to 200 per cent. by irrigation. In this valley, which is in the highest state of cultivation and is thickly populated, the best unwatered land sells at from $500 to $600 per statute acre. The climate is hot and dry, but as the land is low, being only a few feet above the level of the sea, the unirrigated ground retains the moisture and often produces abundant crops, though of course much depends on the quantity of rain which falls during the winter and spring. The only part that is properly watered is a large government model farm, and it would be impossible to see more luxuriant crops than those grown on that estate. Although the supply of water is small, it is sufficient to show what the benefits of irrigation are on such rich land. In the valley of the River Tagus it is said that the produce from irrigated land is twelve times greater than that from dry land.

"It may be laid down as a rule that the increase in the value of land consequent upon irrigation is always in proportion to its quality, for while first-class land may be increased one or two-fold, the worst description of Spanish desert, on which goats and sheep wander in quest of a mouthful of herbage, may be brought to produce abundant crops of corn, and thus increase in value from 1 000 to 1 500 per cent.

"The most general and effective system of irrigating is by constructing a weir across the river, and cutting a main canal, with numerous branches or channels for distributing the water. Most Spanish rivers fall rapidly, and as canals are usually made with a very slight grade, they soon get away from the rivers and attain a sufficiently high level to command the plains which it is intended to irrigate. It is not unusual, in cases where rivers have a quick flow, to construct a canal without a weir, commencing above a rapid where the stream is dammed up by some natural obstruction. From the River Ebro, which has a fall of about 1½ per 1 000, several canals have been made in this manner.

"Along the banks of this river numerous norias are in use for watering gardens; they are roughly constructed undershot wheels, with small buckets attached for lifting the water, and are usually manufactured by country carpenters at a cost of from $1 250 to $1 500 each. The expense is very considerable and quite out of proportion to the quantity of land irrigated, which seldom exceeds from 200 to 300 acres. These norias are entirely wrong in principle, for they raise the water from the tail-race of the weir instead of from its head, thereby uselessly wasting power. One of these weirs, which must have cost (including the weir, etc.), at least $30 000, with over 100 H. P. only lifted 11 gallons per minute to a height of 16 feet, which is less than one man could pump up in the same time. The weir was constructed of large blocks of stone piled closely together, and forty-two per cent. of the water in the river passed through, not over it. Small norias worked by horses, for lifting water from wells to irrigate gardens, are much in use all over Spain; many of them are just the same as those used by the Egyptians, being a roughly made timber-wheel with an endless band on which numerous earthenware vessels are fastened; the band is sufficiently long to allow the pots to fill themselves in the well, and they are discharged above when passing over the wheel into timber conduits which lead the water to small reservoirs; these tanks are often half filled with stable manure, and the water is allowed to lie on it for hours before it is used, by which means it not only refreshes but also manures the land. Several well-constructed iron norias have been erected and considerable quantities of water are raised in this manner. Windmills are occasionaly used to work pumps, and water is often lifted from wells by means of a long pole suspended to a tree with a rope and bucket on one end and a counterpoise weight on the other. One of the oldest systems of irrigation was that of collecting the rain-water in large reservoirs formed in the mountain districts.

"The most important canal is the Imperial Canal, which was originally intended for navigation only, but is now almost exclusively used for irrigation, as it waters an immense tract of country near Saragossa; the weir at Tudela is a splendid specimen of masonry, and is a great monument to the talent and perseverance of the priest-engineer, Don Ramon de Lignatelli, who designed and carried out the whole work. It was originally intended to carry this canal to near Caspé, where it was again to unite with the Ebro, but it was completed only as far as Saragossa, from which point a small conduit of more modern date has been made for irrigation alone; this latter is 22.32 miles long, and carries about 70.6 cubic feet of water per second.

"The author records two failures of works constructed during the present century—one where the engineer commenced his canal at a point in the river below the level of the plane to be irrigated, the mistake not being discovered until a considerable part of the canal had been made and several thousand dollars wasted. In the second case the engineer made a mistake, and after about $150 000 had been expended, it was found that the level of the canal was too high, and the water refused to enter it.

"The Urgel Canal, in the province of Lerida, taking its water from the River Segre, was carried out by a Spanish company under a royal concession authorizing them to collect ten per cent. of the produce of the valley in payment for the water. As this system did not work well in practice, the company made arrangements with the proprietors for a payment of $4.62 per acre per annum. The canal irrigates an immense extent of country, and the government contributed considerable subsidies towards its construction.

"At the date of publication (1867) an English company was engaged in constructing two large and important canals, the Henares Canal and Esla Canal. The former required some heavy works, including a large weir and a tunnel about four thousand feet long. The weir is 16.4 feet high and 426 feet long. It rests on a rock foundation, and the interior in of concrete. The first section, 10.5 miles long, irrigating twelve thousand acres, had just been opened, and the total length was to be twenty-nine miles. The latter passes through a flat country, and was nearly completed. The engineers of these canals were Mr. Bateman, of London, and Mr. Higgins, of Madrid.

"Many concessions had been applied for, as no canal taking more than one hundred liters of water per second can be made without a royal concession. These concessions are expensive to obtain, and about two years are generally required to secure one. The period is for ninety-nine years, at the expiration of which the canal becomes the property of the government or land proprietors. The mill falls belong to the canal company in perpetuity. Subsidies of about fifteen per cent. on an estimate certified by government engineers, are occasionally granted, and are paid in three parts: 1, when the earthwork is finished; 2, when the bridges, culverts, etc., are completed; and 3, when irrigation is commenced.

"The weirs for canals and norias on the Ebro are usually from 6 to 9 feet high, constructed roughly of large blocks thrown loosely together; often as much as 40 or 50 per cent. of the water filters through them, In the small rivers the weirs are formed by driving two rows of small piles and filling the space between them with brushwood, sods and stone. Considerable quantities of water run to waste below the weirs, even if they are well constructed, the water finding its way under the weirs through the boulders that form the beds of all the rivers which have rapid falls. The foundations of new weirs are carried down to bed rock or impermeable clay. The canals in the Rioja always follow the contour of the country, utterly regardless of the radius or shape of the curves, but as the fall is very slight the velocity is small. The side slopes are often less than ½ to 1, but stand very well. In every valley in the Rioja a different tariff of water rates is found, as they are arranged according to the expense of the conservation of the canal, etc. The meter in use is usually a flat stone with a round hole, through which the water passes, and so much per hour is charged for the stream, regardless of the head of water in the canal or tank. In the flat plains a small embankment of clay is formed around each field, and when irrigation is required the water is admitted from some of the minor channels through an opening in the bank, usually at the corner of the field, and allowed to flow till the inclosure is covered to a depth of about three inches. On sloping ground the field is usually leveled into a series of horizontal plots, each irrigated as above, or else small cuts and mounds are made on the slope at right angles to the flow of water to check its velocity.

"All the old weirs cross the rivers obliquely, in some cases at an angle of 45 degrees with the bank. Some of the modern canals leave the river nearly at right angles, but the results have not been satisfactory, a dead point occurring at the inside of the curve which the water takes to enter the canal, and this point silts up and causes a bank which gradually blocks the canal.

"While in the north of France, and in some parts of England, large quantities of water are allowed to flow over the fields, the principal object being to manure the land by the sediment thus deposited, in Spain irrigation is only used to moisten the ground and to supply the want of rain; so that a quantity of water which would be considered insufficient in other countries would more than satisfy the wants of a Spanish farmer. Much of course depends on the climate, the soil, and the crops. Corn and olives are usually irrigated from four to six times a year, and fruit, vegetables and green crops are watered at least once a week during the summer and autumn months. Where the supply of water is abundant the farmers irrigate in a slovenly and wasteful manner, while in districts where a little has to go a long way not a drop is allowed to run to waste. In Valencia the average quantity of water used is 1.33 gallons per minute per acre, which is equal to 11.2 irrigations per year. The author is of opinion that the proper average quantity of water for vegetables and green crops is .75 liter. New canals to irrigate small plains are usually constructed to carry 5.34 gallons per minute to each acre, it being assumed that in course of time the whole valley will be laid under cultivation. It would of course be impossible to give such a supply on a very large scale, and where an immense district is to be watered the canals are made to carry one-fourth to one-half that quantity, the amount being regulated by the total volume of water in the river from which the canal is to be supplied.

"The following table shows the quantities of water authorized for new (1867) canals by royal concession:

CANAL.	QUANTITY OF WATER PER MINUTE TO EACH ACRE.	LAND TO BE IRRIGATED.
	Gallons.	Acres.
Cinco Villas..................	1.3	123 550
Tamarite.....................	1.6	256 984
Henares......................	2.2	30 381
Esla..........................	3.2	22 724
Tajo..........................	5.0	8 150
Ebro..........................	5.3	6 422
Isabel II.....................	4.0	4 446

"In the Rioja the irrigation is according to the weather and the crops. When there is a fair amount of rain four irrigations are given to corn, flax, potatoes, olives and vines; in dry seasons six or eight irrigations. Garden vegetables are watered every week from May to October, and oftener in dry seasons, the minimum being twenty irrigations a year. This would give for

	Gallons per minute per acre.
Corn land, say 6 irrigations per year...........	1.41
Meadow land, 8 " " "	1.88
Gardens, 20 " " "	4.70

"The irrigation is calculated at 2¾ inches deep, but in practice they rarely give more than two inches, and often less.

"The following are the prices authorized by the concessions, the prices being per acre for each irrigation, or 154 000 gallons of water: Isabel II, 40 cents; Esla, 38 cents; Henares, 56 cents; Logroño, 50 cents. In Malaga the water rent per annum averages about $4.56 per acre; on the Lobrigat canals from $1.32 to $4.00, and in Aragon from $1 to $6.50. In Cataluña the farmers pay for corn land, $2.88 per acre per year; for clover and artificial grass, $3.60; and for gardens, $3.84.

"The formula for calculating the size of the Isabel II Canal was $V^2 = \frac{L\,H\,I}{0.00025\,(L + 2\,H)}$, but the following is considered to give more correct results, $V = 51\sqrt{IR}$; $V =$ velocity; $I =$ incline; $R =$ hydraulic mean depth. The favorite grade is from $12\frac{1}{2}$ to 19 inches per mile; the best velocity is from $23\frac{1}{2}$ to $27\frac{1}{2}$ inches per second, as that prevents silting and growth of weeds, but does not scour the channel. For such velocities a Spanish engineer states that the radii of curves not protected by pitching should not exceed 492 feet, with a minimum of 328 feet; if pitched, the radius may be reduced to 65 feet. Some of the old canals, with very slight grades, have unpitched curves of 16.5 feet radius.

"In the Rioja the main canals are cleared out once a year, the water being turned off to allow the laborers to work freely. Most of the old canals have large deposits of sediment and growths of weeds. This annual cleaning out costs from $40 to $80 per mile. The cost per acre for irrigating land is estimated at from $30 to $42, according to locality and circumstances; in the case of a canal to be lined with brick the estimate was about $75.

"By the Water Act of 1866, all foreign capital invested in irrigation canals or works is under the protection of the government, and cannot be confiscated in time of war; the minority in a newly irrigated valley must either irrigate or sell the land to the canal company, and under this law the proprietor can, if his demand be exorbitant, be compelled to sell his property; the amount being decided by capitalizing the annual land tax paid to the government and augmenting the product fifty per cent."

Mr. John Westcott (formerly Surveyor-General of Florida).—In Florida, on the peninsula, where there is a well-defined land and sea breeze, and rainy and dry season, drainage first, and then the artificial application of water, adjustable at the proper time and supplied in such quantities as is necessary in the dry season to every branch of agriculture, is cheap, advisable, and profitable. There is hardly a 40-acre lot in Florida where the flat land has not fall enough to take the water off as fast as it falls by constructing at small expense sufficient drains in the rainy season, and artesian wells can be bored from 60 to 250 feet. They will supply any amount of water desired, and so completely under control, that a supply in the dry season can be adjusted in accordance with the natural demand for extensive farming and farm gardening.

Artesian wells can be bored from 2 inches to 12 inches in diameter, flowing any amount of water desired to be conveyed into ditches or furrows, where a rich, teeming soil will soon take it up without causing improper scalding by overflow.

Pine-apples thus cultivated can be forced to weigh from 8 to 15 pounds each. Bananas, tomatoes, onions, potatoes, and most other commercial vegetables, by a proper supply of plant-food and necessary adjustable supply of water in the dry season, can be made to yield a crop several folds greater than with ordinary cultivation.

This is not fancy; it has been done, and can again be accomplished.

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355.

(Vol. XVI.—March, 1887.)

A WATER-METER FOR IRRIGATION.

By A. D. Foote, M. Am. Soc. C. E.

Read October 6th, 1886.

Engineers of irrigating works have long felt the want of a cheap, practical and reasonably accurate method of measuring small volumes of flowing water as delivered to the irrigator.

Selling or dividing water by the acre or area irrigated offers very great inducements for waste, and has many other serious disadvantages.

Selling or dividing water by the methods of measurement now in use causes more vexation and dissatisfaction, to say nothing of legal entanglements and expenses, than all the other work of an irrigating enterprise.

The company by which I am employed proposed to furnish and distribute the water required for irrigating about six hundred thousand acres of land. The legal advisers of the company strenuously opposed selling water by the acre irrigated. They urged (and their long experience in irrigation litigation gave great weight to their opinion) that every settler upon the land would become seriously dissatisfied, and it would be a constant menace to the company to have such a great community with a common cause of complaint against it.

It therefore seemed absolutely necessary for the company to adopt some method of selling water by a measured quantity, and at a certain regular price per unit of that quantity.

The miner's inch is the usual unit of water measurement in this locality, and by custom, and partly by statute, the "4-inch" miner's inch is the particular kind used.

It purports to be the quantity of water which will flow through an inch square orifice in an inch board with a head or pressure of four inches above the center of the orifice, but is in reality the quantity

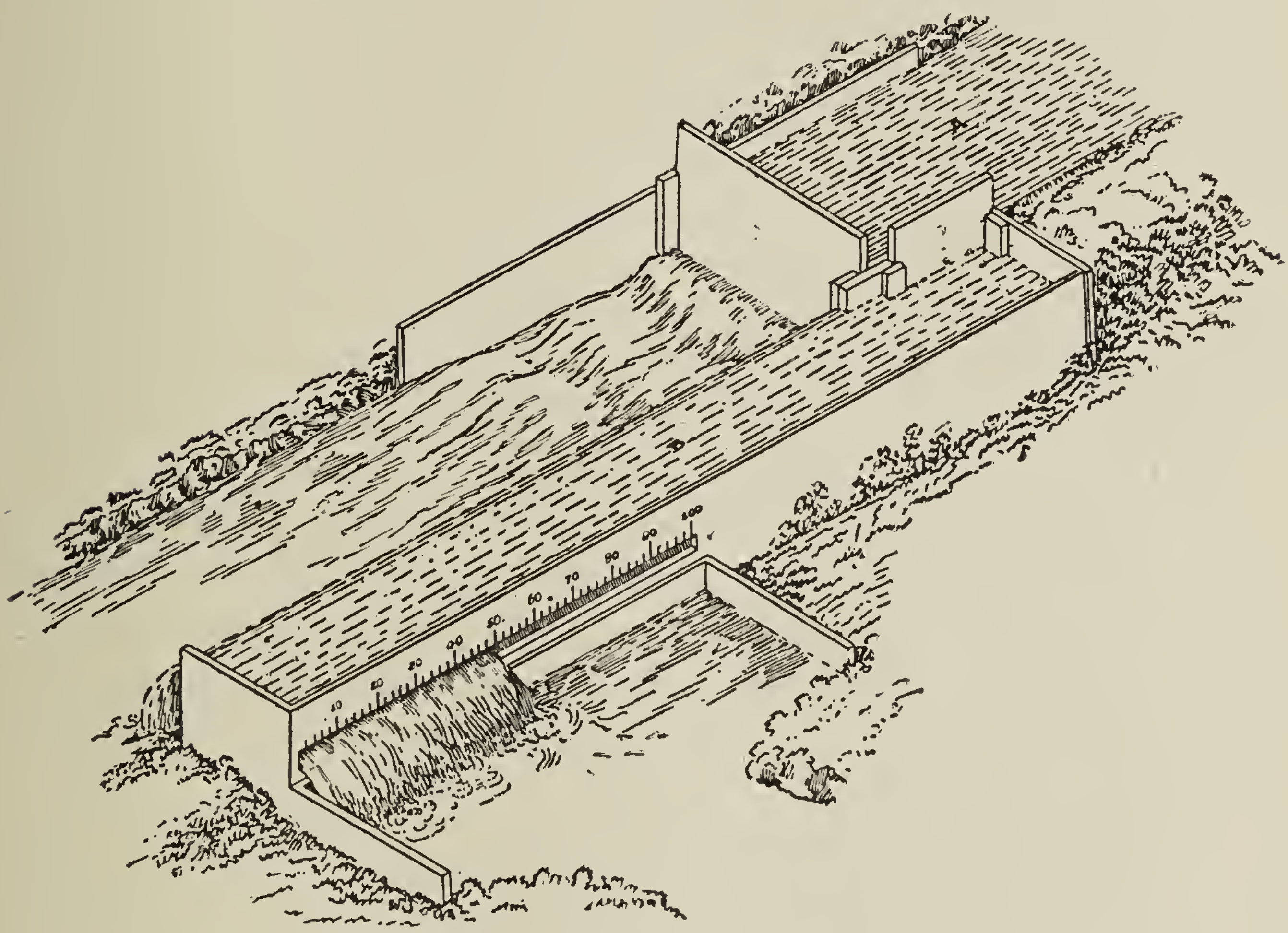

PLATE XV.

of water which will flow though an inch square orifice in an inch board with a head or pressure depending upon the care used in adjusting the gate which admits the water to the orifice, and on the fluctuations there may be in the volume of water flowing in the channel conducting the water to this gate.

It has been described as a "nondescript inch which can scarcely be credited with a remote approximation to correct measurement."

It is called unscientific, inaccurate, unsatisfactory to both seller and buyer of water; and yet it is the best and almost only method used in the whole Western part of our country for measuring flowing water in small quantities. In some parts of Colorado a so-called "head" is used, but it is no more accurate than the inch, and not as convenient.

After much consideration it seemed to me that the miner's inch, although so much derided by engineers, had many valuable qualities. One theoretical inch, as above described, delivers 1 800 cubic feet of water in twenty-four hours. Forty-eight of them equal one cubic foot per second, therefore easily translated into scientific or standard terms. It is small enough not to require subdivision, which is a great convenience.

Last May I constructed a water-meter for measuring 4-inch miner's inches, of which an isometrical sketch is shown, Plate XV. For months it has done its work in a very satisfactory manner, seldom clogging and never varying in its delivery to an appreciable amount.

The apparatus is so simple that the sketch will explain it better than words. The whole value of the meter depends upon the long weir, perhaps better described as an excess or returning weir, which returns all excess of water in the box back to the ditch and thus keeps the pressure at the delivery orifice practically uniform.

I am well aware that the measurement is not absolutely accurate or uniform; but if it is remembered that the variation in delivery is only as the square root of the variation in head, and that, owing to the long excess weir the variation in head is only a small portion of the variation in the delivery ditch, it will be seen that actual delivery through the orifice is very nearly uniform.

There need be but an inch or two loss of grade in the ditch, as but very little more water should be stopped than is delivered through the orifice.

The gate or other obstruction in the ditch should back the water sufficiently to keep the excess weir clear, and at the same time keep say a quarter of an inch of water on its crest, and the surface of the water in the box should then be exactly four inches above the center of the delivering orifice.

The principle of the long excess weir can be used, of course, for delivering water through an open notch or weir, but it is more accurate with a pressure or head, and the greater the head the greater the accuracy, as will readily be seen.

Any one using the meter will naturally adapt it to their own circumstances and desires. It is cheaply constructed and easily placed in position, costing from four to six dollars; quickly adjusted, as the gates do not have to be precisely set; needs no oversight or supervision (if properly locked as they should be) until a change in volume is desired; will deliver a large or small quantity, which is a great convenience, as the irrigator usually wants a small stream continuously and a large stream on irrigating days; is not likely to clog, as floating leaves and grass pass over the excess weir. Half-sunken leaves may catch in the orifice, but as it is to the farmer's interest to keep that clear, he will probably attend to it.

To me, however, the greatest merit which the method possesses (excepting its accuracy) is that the irrigator himself with his pocket-rule can at any time demonstrate to his entire satisfaction that he is getting the full amount of water he is paying for.

I have no patent on this invention, and if any Members of the Society will try the meter and become as well satisfied with it as I am, I shall be amply repaid for placing the design before them. I would like however to have their opinions of it in any case.

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356.

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SOME CONSTANTS OF STRUCTURAL STEEL.

By PALMER C. RICKETTS, Assoc. Am. Soc. C. E.

READ JUNE 2D, 1886.

The comparatively meager existing data with regard to some of the constants of the structural steel made at present in this country, especially that referring to the coefficient of elasticity in compression, caused the writer to undertake some months ago a series of tests on that metal varying in carbon from $\frac{9}{100}$ to $\frac{40}{100}$ of one per cent. It was determined to compare the coefficients of elasticity in tension and compression in adjacent pieces from the same bar, and at the same time to find, when possible, the variation in resistance of specimens from the ends and centers of rods of considerable length, thus determining the homogeneity of the material used. The carbon was also found by the color test from drillings at each of these three places. For these determinations in the Bessemer bars I am indebted to Mr. John M. Sherrerd, the chemist of the Troy Steel and Iron Company. The shearing resistance of the metal was also found, and the ratio between its resistance in double and single shear calculated. On account of the capacity of the 50 000 pounds Olsen testing machine used, and also because a great part of the tests of structural material is made on specimens of about half a square inch in section, the pieces tested were take from three-quarter inch round rods. These bars were in no case turned down, but were tested with the "skin" on. For the Bessemer rods, selected at different times from the stock of the Troy Steel and Iron Company, I take pleasure in thanking Captain R. W. Hunt, M. Am. Soc. C. E., its manager, and for the fifty open-hearth rods, numbered from 1 to 50 in the tables, I am indebted to the Pennsylvania Steel Company, of Steelton, Pa. These last bars were rolled from 14-inch square ingots after several reheatings, and were 40 inches long, twelve being of rivet, twenty-four of tension, and fourteen of compression steel. They were divided into lengths of 20, 6, 2 and 12 inches, the first being used for tension, the next two for compression tests, and the last was sheared. Besides the above material, ten rods of open-hearth steel, about fifteen feet long, from metal used for the rivets of a recent steel bridge, were so cut that tension and compression pieces were obtained from each end and the center.

This was also done with the Bessemer rods of about the same length and with the single rod of "Burden's Best" wrought-iron, which was tested in order to compare the results obtained from the steel used with those from good iron under the same conditions.

The elongations corresponding to increments of stress were measured by a carefully constructed and very accurately divided instrument which reads to $\frac{1}{10000}$ inch, and on which estimations to $\frac{1}{40000}$ inch may easily be made. Through each of its two ring-shaped parts, which encircle the bar to be tested, pass three converging screws with milled heads, whose sharp-pointed ends penetrate the bar at sections whose initial distance from each other is known. Extending vertically upward from the lower ring, and diametrically opposite to each other, are two steel rods which are clamped tightly to the ring at their lower ends, and upon their tops are fitted vulcanite caps bearing insulated metal plates. These are vertically under the pointed ends of two micrometer screws which pass through projections in the upper ring. An electrical contact may thus be obtained, which becomes of great delicacy when a weak current and a sounder are used. The manner in which this instrument is supported on the bar insures the measurement of the strain in a constant distance, which was taken at 8 inches for bars in tension and 4 inches for those in compression. The strains thus measured were used in the determination of the coefficients of elasticity.

A stress of 1 000 pounds was at first put upon the bar, and the micrometer for the first time read. Readings were then taken corresponding to increments of 2 000 pounds of stress, and this was continued until the grip of the instrument upon the bar was loosened, which occurred in tension somewhat beyond the elastic limit and in compression when the piece began to bend. The grip on the bar could be tightened at any time by means of the converging screws with milled heads, but this was not necessary until after the bar had been strained more than the amount used in finding the coefficients, and they were not touched, when once tightened, until after this strain had been reached, care being thus taken to prevent disturbance. For the sake of uniformity in comparison, the strains used in the calculations of all the coefficients were those corresponding to stresses between one and fifteen thousand pounds. This superior limit was taken after an inspection of all the results obtained, as within it the strains corresponding to successive increments of stress were essentially constant for nearly all the bars tested.

As has been stated, there are two micrometers used diametrically opposite to each other, in order to reduce as much as possible the error in strain measurements in the bar. Each micrometer being read, it is read again after an increment of stress has been applied, and the mean of the difference of the readings on each side of the bar is taken as the strain due to this applied stress. That there is bending, or at least unequal straining, is shown by the fact that these differences are seldom exactly equal, in rare cases one of them even being negative.

By dividing the means spoken of by the length in which the strain is measured, that is by eight in tension and by four in compression, there results the strains per inch. These, for the fifty bars of open-hearth steel, are given in Tables Nos. 1 and 2. As estimations to one-quarter of one ten-thousandth of an inch were made, in taking means the figures in the last two places of the decimals are found. Since the readings were first taken at 1 000 pounds, the figures first given are for the increase of stress from 1 000 to 3 000 pounds. A comparison of the strains given show them to be quite regular in each table, not only for the same but for different bars. The greatest variation in Table No. 1 occurs in No. 17, in which 0.000122 and 0.000183 are consecutive decimals. A mean of these gives 0.000152, not far from the average for this specimen. The irregularities in Table No. 2 are somewhat more pronounced, though they are not great. It is noticeable that the greatest deviation occurs in the same bar, No. 17. Here the strain for the stress between 1 000 and 3 000 pounds is 0.000215 inch. As this is the first mean, its cause might be found in a slight deviation from parallelism of the two faces of the specimen, or rather in a slight deviation of one face from a plane surface, the faces becoming more nearly plane and parallel as the applied weight is increased.

The readings, on opposite sides of this bar, from which the mean was derived, would seem to indicate this. In fact the greater deviation from uniformity of the means in the first column than in any of the others of this table tends to show that this happened in a number of the compression pieces. In the last column is given the mean strain on the specimen per inch of length per 1 000 pounds of stress for weights between 1 000 and 15 000 pounds. The strains for three specimens from a bar of "Burden's Best" iron are also given.

Tables Nos. 3, 4 and 5 give the results of the tensile tests. The specimens were 20 inches long with the "skin" on, and were gripped at the ends by wedges in a ball and socket joint. The power was applied by hand, and the breaking was done gradually, the bars remaining in the machine from fifteen to twenty-five minutes. The same numbers belong to the same rod throughout all the tables, those from 1 to 50 belonging to the rods 40 inches long from Steelton, and the ones from 51*a* to 60*c* to the open-hearth rivet bars 15 feet long. Nos. 61*a* to 77*c* are of Bessemer metal from rods also 15 feet long.

The subscripts *a*, *b* and *c*, when attached to the same number, denote pieces from the center and end of the same bar, the letter *b* corresponding to the central piece. The specimens used for tensile and compression tests were always adjacent to each other in the rod. The classification of the open-hearth metal into rivet, tension and compression material conforms to that of the Pennsylvania Steel Company, and color tests by the chemist of that company gave the percentages of carbon and manganese shown.

The areas given throughout the tests correspond to mean diameters, four measurements at each section being taken. The average difference between the greatest and least diameters of the sections measured in the open-hearth bars was about 0.003 inch, and in the Bessemer bars about 0.010 inch, in one or two instances it being as great as 0.015 inch.

A general idea of the error to be expected, under these circumstances, on account of the use of the mean area, is given by comparing the area of an ellipse, with axes say of 0.740 and 0.750 inch, with that of a circle whose diameter is 0.745-inch. In the first case there results an area of 0.4359 square inch, and in the second case the same. If the mean of the four readings were 0.747 inch, the deviation from the elliptic area would be 0.0023 square inch, and for a bar with an ultimate resistance of 30 000 pounds, the difference per square inch would be 360 pounds. The elastic limits given in tension, compression and shear were found by the action of the scale beam, because they are so found in a large majority of the tests of structural metal.

Since some of the 6-inch columns bent before the elastic limit was reached, the corresponding row of figures in Tables Nos. 6 and 7 was filled out by means of results obtained from columns 2 inches long. In these tables, Nos. 51*a* to 77*c* correspond to the long rods, and as only one short column from each one of these was tested, every third line only in this row, between these numbers, is filled. The well-known effect of carbon on tensile resistance is seen in these results; likewise the great variation in this resistance for the same carbon—a variation seen more clearly by an inspection of the following figures:

	Per cent. of carbon.	Ultimate tensile resistance of steel. Pounds per square inch.		
		Maximum.	Minimum.	Difference.
From Table No. 3........	0.20	79 200	68 700	10 500
" " "	0.32	92 500	77 000	15 500
Report on the Bismarck Bridge—Morison	0.20	77 000	64 900	12 100
do.	0.38	98 000	78 700	19 300
do.	0.40	101 600	64 600	37 000
Report of the United States Board for Testing Iron, Steel, etc...............	0.37	100 200	69 600	30 600
do.	0.46	83 100	71 000	12 100
Report of the Naval Advisory Board—Gatewood NorwaySteel—So.Boston	0.14	66 400	57 000	9 400
do.	0.18	69 500	56 600	12 900
Cambria	0.18	69 700	59 000	10 700
Chester............	0.20	73 300	58 400	14 900

Errors in determinations by the color test would account for no such differences; in fact a difference of 30 600 pounds is found in the figures above, taken from the Report of that Committee of the United States Board of which Alexander L. Holley, M. Am. Soc. C. E., was Chairman, and in this steel the carbon was determined by combustion. These figures show the inaccuracy of such statements as, "the increase in ultimate tensile resistance of steel per square inch is almost exactly 1 000 pounds for every 0.01 per cent. carbon, starting with no carbon at 45 000 pounds," and the results here found, as well as those given in the reports above referred to, and others, show the improbability of finding any relation between the ultimate tensile resistance of structural steel, and the percentage of carbon alone, sufficiently close to enable one to be inferred from the other in any particular case with reasonable accuracy. The average of the 0.09 per cent. carbon metal in Tables Nos. 3 and 4 is about 62 000 pounds; of the 0.20, 74 000 pounds; and of the 0.22, 78 000 pounds. No decided conclusions can be drawn from Table No. 3 with regard to the effect of manganese on the metal. In this table, No. 18 was from an imperfect bar with slivers on the surface. It broke at an imperfect weld with a small elongation and reduction of area. No. 47 broke at the point where the heat number was stamped upon it, and had a peculiar fracture with broad planes of shear. The numbers in the column headed Reduction of Area were obtained from measurements of the fractured area.

The variation in ultimate resistance of specimens from the center and ends of the same rod is seen from the last columns of Tables Nos. 4 and 5, the greatest being in No. 57, in which the difference between the greatest and least breaking stress is 3.8 per cent. of the least, though the average for the open-hearth rods is but 1.8, and of the Bessemer but 1.3 per cent.

There is considerable uniformity in the coefficients of elasticity as here determined. In these coefficients in compression it is believed that this is due not alone to the uniformity of the material, but also to the great care exercised in their determination. The difficulty of so confining a long piece of metal subjected to compression, that the true ratio between stress and strain, to a point near the elastic limit, can be determined, is well known. No such attempt was made, but dependence was placed upon an accurate measurement of strain within a short distance. The 6-inch columns used had their bases very carefully faced up parallel to each other. To gain room for the micrometer the specimen was placed on one of the largest faces of a hardened tool steel block 4 x 2½ inches by 1 inch thick, which had been planed and ground on a surface plate until its faces were smooth and parallel. This block rested on the platform of the machine, and after the column had been carefully centered a similar block was placed upon its top between it and the cross-head. When the micrometer was in place, part of it projected beyond the ends of the column on each side of the blocks, and on account of their use the strain could be measured in four inches out of the six. No bending took place until after 15 000 pounds of stress had been recorded, and the coefficients were calculated from stresses within this limit.

From the fact that few measured strains, corresponding to consecutive equal increments of stress, are exactly equal to each other, it follows that the value of the coefficients generally change with the limit of stress taken. Thus, in Specimen No. 1,

	E Equals in	
	Tension.	Compression.
For stress between 1 000 and 15 000 pounds......	30 602 000	30 331 000
" " " " 13 000 "	30 405 000	29 990 000
" " " " 11 000 "	30 197 000	29 767 000

Sometimes, of course, greater differences are shown. Although generally done, and done here, the uselessness of writing significant figures below the tens of thousands, and perhaps below the hundreds of thousands, place is apparent from these numbers.

In Table No. 8 is given the mean coefficients for the steels of different kinds, and also the ratio between these means in tension and compression. It will be noticed, in the first place, that these coefficients in tension and in compression do not vary much with the carbon, being practically the same for the open-hearth rivet, tension, and compression steel.

Secondly, that in tension the average is greater for the open-hearth than for the Bessemer metal by about three per cent., though in compression they are nearly the same. In the third place, that the variation in both cases is greater in the different Bessemer steels than in the open-hearth. It is also particularly noticeable that the average coefficient in tension is greater than that in compression by about 3.5 per cent. for the rivet and 4.3 per cent for the less mild open-hearth steel, and by about 1.4 per cent. for the milder and 2.3 per cent. for the harder Bessemer metal. The low ratio, 1.009, in the average of the fifty-one Bessemer specimens, is partly due to the low value of the coefficient in No. 67*c* in Table No. 5, without which the ratio is 1.014. This piece stretched abnormally, the strain per inch between 5000 and 7 000 pounds being 0.000400 inch. Assistant Naval Constructor Gatewood, in the report of the Naval Advisory Board on the mild steel for the new cruisers, remarks that

"The modulus of elasticity of mild steel in compression is generally taken the same as for tension. This was very closely true in some tests of shaft steel recently made on the Emery machine at the Watertown Arsenal for this Board."

He does not give these results, but further adds:

"Mr. James Christie, from some recent experiments on Bessemer steel of structural quality, gives the value of the modulus under compression much less than under tension. Thus for 0.12 carbon steel he obtained for E in tension 27 030 000 to 32 780 000, with an average value of 30 135 000, while for compression the values were 15 132 000 to 24 490 000, with an average of 20 478 000. This result is anomalous and should be very carefully verified before being accepted."

These figures of Mr. James Christie, M. Am. Soc. C. E., are to be found in Vol. XIII of the Transactions of this Society, and he further there gives as a mean coefficient in tension for 0.36 carbon steel 29 280 000, and in compression 24 570 000.

The coefficients in compression found in this paper are not so small, but the results tend decidedly to show that they are smaller than those in tension. In the open-hearth steel they are larger but in two cases out of the eighty, in No. 12 and in No. 53*c*. In No. 12 the tension piece was slightly bent at first, as a negative reading on one of the micrometers showed, and if the strain between 3 000 and 15 000 pounds be used, $E = 30\,300\,000$. In the Bessemer steel they are larger in fifteen cases out of the fifty-one, when 15 000 pounds is the limit. By taking different limits this number is reduced to eleven, as shown below—although it might be possible, on the contrary, by changing the limits in some of the other bars, to make the coefficients in compression now less than those in tension greater than them. However that may be, the conclusion to be drawn is evidently the same as in the case of the open-hearth steel.

In Table No. 5, attention is called to the following bars: Nos. 62*b* and 67*a*, in which, for the limits 1 000 and 17 000 pounds, E becomes respectively 29 739 000 and 29 535 000 greater than the corresponding values in compression. In No. 64*c* there was one abnormal strain of 0.000250 inch between 7 000 and 9 000 pounds; in No. 67*b* between 1 000 and 17 000 pounds, $E = 28\,842\,000$, still smaller than the result given; and in No. 71*b* the readings are large between 7 000 and 15 000 pounds. Between 1 000 and 17 000 pounds for this bar $E = 27\,757\,000$.

In Table No. 7, for No. 71*a*, $E = 29\ 525\ 000$ between 1 000 and 19 000 pounds, and for No. 72*c*, $E = 28\ 959\ 000$ between 1 000 and 17 000 pounds, both of which values are less than the corresponding ones in Table No. 5. In No. 74*c* the first two readings were small, and there was a negative reading on one of the micrometers. Between 1 000 and 19 000 pounds, $E = 31\ 908\ 000$ for this piece.

The columns, 2 inches long, used to determine the elastic limits in compression, which have already been given, were further compressed, and the stress which would produce a reduction in length of 5 and 10 per cent. was found and recorded in Tables Nos. 9 and 10. At about the last point they began to bend slightly, and no more stress was applied. No. 76 was reduced 8 per cent. in length by 50 000 pounds, the limit of the machine. This corresponds to 111 000 pounds per square inch.

In order to find the shearing resistance of this metal, the instrument shown in Plate XVI was used both for single and double shear. The pieces *A*, made of hardened tool steel, are pressed together by bringing the cover *T* upon the top one by means of the cap screws *C*, and the piece to be sheared when inserted in the orifice *H*, which was made three-quarters of an inch full in diameter, is thus held all around its exterior surface. When the instrument is placed on the platform of the machine and the cross-head is brought down on the upper surface of the block *B*, the shearing is done by the edges of the semicircular groove at the other end of this block, into which the bar fits, and which being also three-quarters of an inch full in diameter half encircles it. The width of this block, which gives the span in double shear, is two inches, and its sides just clear the vertical faces of the pairs of pieces *A*.

When a single surface is to be sheared, the bar is supported on one side only and about one and three-quarters inches projects, although experiments showed that it made no difference whether the projecting distance was more or less than this. For double shear it is pushed into the orifice in the pieces on the other side and hence is supported at each end. By this arrangement the double shear is nearly pure and the single not quite so nearly, as in this case the bar bends a little and its free end slightly leaves the shearing block.

The sheared surface was not quite elliptical in section, but was somewhat pointed at the ends of the horizontal diameter, which remained almost unchanged in length, whilst the vertical was reduced on an av-

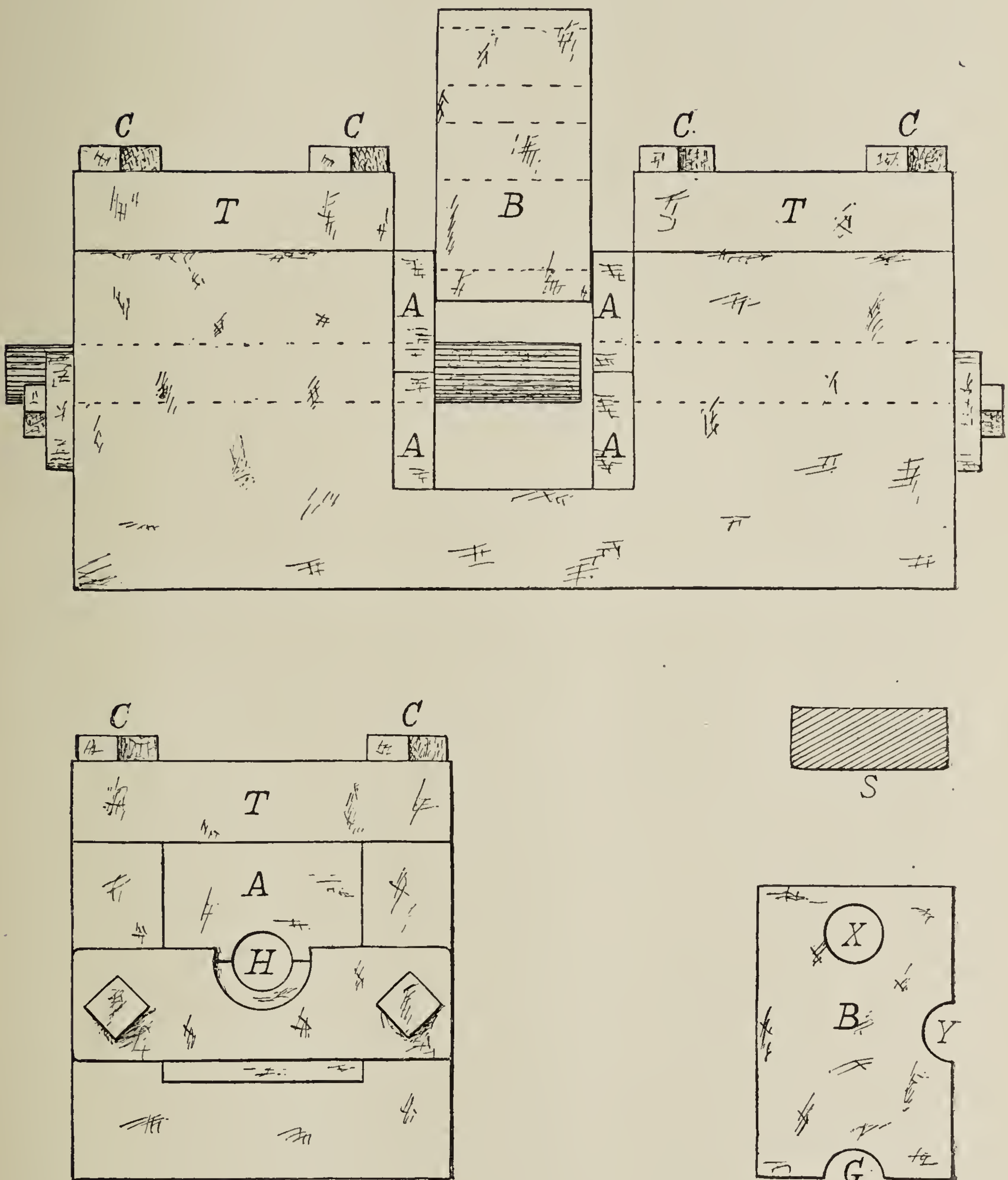

PLATE XVI.

erage to about 0.67 inch in single and 0.69 inch in double shear, giving a reduction of area of about ten per cent. in the first case and of about eight per cent. in the second.

The results of the shearing tests are found in Tables Nos. 11, 12 and 13. One piece from each of the 15-feet long rods, both of open-hearth and Bessemer steel, was tested, three times in single and the same number of times in double shear. For bars numbered 1 to 50, on account of want of material, two single and two double shears only were taken. The quantities in the columns headed "Elastic Limit" give the amount of applied stress when the metal was no longer able to hold up the scale beam. This so-called "elastic limit" is not as determinate a point as the elastic limit, found by the scale beam, of a piece in tension. The capacity of the machine was not great enough to give the double shearing resistance of all the metal tested. For some of the pieces the poise was placed beyond the 50 000-pound limit and the excess estimated. For most of these, and some of the others, it was necessary to allow the stress to remain on the specimen for times varying from ten seconds to three minutes, but all pieces for which the ratio between double and single shear is given were broken, in both, below or at the limit of the machine and without allowing the greatest stress to remain on the bar for any length of time. This ratio is seen to be greater than unity. There is given also the ratio of the ultimate resistance per square inch in double shear to that in tension. This is, of course, found to be about 75 per cent. for the rivet steel, and it is also seen to decrease both in open-hearth and Bessemer metal as the amount of carbon increases.

The shearing resistances given in Tables Nos. 11, 12 and 13 were obtained by using blocks with sharp edges. The impossibility of getting a really pure shear is known. It was the intention, however, to approach this result by means of the apparatus used. In order to reduce the bending to a minimum, the shearing block, at the risk of obtaining somewhat higher results on account of friction, was confined with but small clearance between the pieces holding the rod. In consequence of a suggestion made during the discussion of this paper, some experiments, embodied in Table No. 14, were made to find the ratio of the resistance of the metal when the shearing edges were dull to that when the edges were sharp. This ratio is given in the table, and is found to vary for the steel from 1.153 to 1.236 in single and from 1.109 to 1.141 in double shear, and for the iron to be 1.142 in single and 1.101 in double shear.

As was to be expected, it is considerably greater than unity, and this fact emphasizes the importance of rounding the edges of rivet holes in steel plates after reaming. It is doubtful, however, if these results with round edges should be taken to represent the shearing resistance of rivets in built pieces, even supposing such a mode of rupture without bending were possible, as the edges of the rivet holes would not be uniformly rounded.

The ratio between the resistance of the steel and iron when the shearing edges were sharp, is found to be about the same as when they were rounded, and it is seen generally that, whether the edges be blunt or sharp, if 7 500 pounds be allowed as the shearing resistance of iron, that of rivet steel may be taken from 15 to 20 per cent. greater, or either 8 500 or 9 000 pounds. It may be stated that these edges were blunted only slightly, the curve having a radius of from $\frac{1}{50}$ to $\frac{1}{40}$ of an inch.

For the results found in Tables Nos. 11 to 12, the groove in the shearing block was semicircular, and, as has been remarked, in single shear the rod left the block slightly. In order to find whether this reduced the resistance in single as compared with double shear, a full circular hole was made in the block, and the rod passed into it for single and through it for double shear. As the block is guided during the shear this prevented the rod from leaving the upper surface of the groove. The edges were blunt, and a comparison of the columns in Table No. 14 shows that the ratio between the resistance in single and double shear was not materially altered by confining the end of the rod.

A vertical section *s*, through the axis of the sheared piece is shown. It is seen that the rod is rounded on the lower side near the planes of shear, and this was true whether the edges were blunt or sharp, and whether the half circle or full circle of the block was used. That is, there is a certain amount of bending, and this bending would be found by any method of shearing. The effect of greater bending obtained by spacing the supporting blocks 2⅛ and 2¼ inches apart, while the shearing block remains 2 inches wide, is shown by a few experiments. An examination of the ratios between the resistance per square inch in double and single shear, found in the last four tables of this paper, shows that when the shearing edges were blunt they are less than unity, and when sharp they are greater. The deviation from unity in either case is not large. The larger ratio in the last case is doubtless due to friction.

TABLE No. 1.

Strains per Inch of Length in Round Bars of Open-Hearth Steel due to 2 000 Pound Increments of Tensile Stress.

Number.	Kind.	Per cent. of carbon.	Per cent. of manganese.	Mean diameter Inch.	Area square inch.	Strain in inch corresponding to the increase of the stress from							Strain on Specimen per inch of length per 1 000 pounds of stress. Inch.
						1 000 to 3 000 pounds.	3 000 to 5 000 pounds.	5 000 to 7 000 pounds.	7 000 to 9 000 pounds.	9 000 to 11 000 pounds.	11 000 to 13 000 pounds.	13 000 to 15 000 pounds.	
1	Rivet........	0.08	0.43	0.746	0.437	0.000150	0.000152	0.000161	0.000142	0.000153	0.000145	0.000144	0.0000748
2	"	0.09	0.33	0.732	0.421	0.000159	0.000159	0.000150	0.000143	0.000156	0.000161	0.000145	0.0000745
3	"	0.09	0.35	0.747	0.438	0.000153	0.000158	0.000150	0.000158	0.000150	0.000152	0.000147	0.0000762
4	"	0.09	0.39	0.753	0.445	0.000155	0.000142	0.000147	0.000145	0.000148	0.000150	0.000128	0.0000730
5	"	0.09	......	0.740	0.430	0.000156	0.000148	0.000169	0.000147	0.000155	0.000155	0.000158	0.0000777
6	"	0.09	......	0.743	0.434	0.000155	0.000158	0.000147	0.000155	0.000161	0.000147	0.000139	0.0000758
7	"	0.09	......	0.739	0.429	0.000147	0.000163	0.000153	0.000147	0.000156	0.000148	0.000147	0.0000758
8	"	0.09	......	0.748	0.439	0.000158	0.000147	0.000147	0.000148	0.000147	0.000147	0.000145	0.0000742
9	"	0.09	......	0.747	0.438	0.000142	0.000153	0.000148	0.000153	0.000152	0.000147	0.000144	0.0000742
10	"	0.09	......	0.740	0.430	0.000159	0.000158	0.000147	0.000156	0.000150	0.000159	0.000152	0.0000772
11	"	0.10	......	0.739	0.429	0.000156	0.000153	0.000159	0.000153	0.000139	0.000145	0.000141	0.0000748
12	"	0.10	......	0.741	0.431	0.000172	0.000158	0.000156	0.000153	0.000150	0.000159	0.000142	0.0000770
13	Tension.....	0.19	0.58	0.755	0.448	0.000150	0.000142	0.000153	0.000141	0.000148	0.000153	0 000147	0.0000739
14	"	0.19	0.63	0.746	0.437	0.000164	0.000144	0.000156	0.000155	0.000152	0.000142	0.000164	0.0000769
15	"	0.20	0.57	0.739	0.429	0.000155	0.000150	0.000150	0.000158	0.000153	0.000152	0.000142	0.0000757
16	"	0.20	0.69	0.744	0.435	0.000150	0.000152	0.000152	0.000142	0.000164	0.000153	0.000148	0.0000758
17	"	0.20	0.73	0.743	0.434	0.000155	0.000148	0.000144	0.000155	0.000122	0.000183	0.000144	0.0000750
18	"	0.20	0.74	0.750	0.442	0.000161	0.000134	0.000138	0.000148	0.000148	0.000147	0.000133	0.0000721
19	"	0.20	0.76	0.757	0.450	0.000142	0.000155	0.000138	0.000144	0.000147	0.000148	0.000142	0.0000725
20	"	0.20	0.78	0.742	0.432	0.000152	0.000152	0.000155	0.000142	0.000156	0.000153	0.000152	0.0000758
21	"	0.20	0.81	0.741	0.431	0.000153	0.000148	0.000152	0.000156	0.000159	0.000145	0.000145	0.0000757
22	"	0.20	0.84	0.740	0.430	0.000148	0.000152	0.000155	0.000150	0.000153	0.000153	0.000158	0.0000763
23	"	0.20	......	0.742	0.432	0.000155	0.000156	0.000152	0.000152	0.000155	0.000147	0.000158	0.0000767
24	"	0.21	0.63	0.750	0.442	0.000138	0.000152	0·000147	0.000147	0 000161	0.000147	0.000145	0.0000740
25	"	0·21	0.65	0.742	0.432	0.000152	0.000153	0.000158	0.000144	0.000156	0.000141	0.000150	0.0000751
26	"	0.21	0.78	0.745	0.436	0.000145	0.000152	0.000155	0.000144	0.000147	0.000155	0.000153	0.0000750
27	"	0.21	0.84	0.739	0.429	0.000150	0.000153	0.000156	0.000150	0.000152	0.000145	0.000161	0.0000762
28	"	0.21	......	0.742	0.432	0.000155	0.000150	0.000150	0.000150	0.000150	0.000152	0.000152	0.0000756
29	"	0.22	0.58	0.744	0.435	0.000141	0.000155	0.000148	0.000163	0.000142	0.000153	0.000158	0.0000757

30	Tension.....	0.22	0.65	0.748	0.439	0.000139	0.000153	0.000148	0.000153	0.000150	0.000150	0.000139	0.0000738
31	"	0.22	0.77	0.735	0.424	0.000155	0.000150	0.000158	0.000150	0.000161	0.000153	0.000161	0.0000777
32	"	0.22	0.78	0.743	0.434	0.000150	0.000147	0.000150	0.000150	0.000155	0.000147	0.000145	0.0000746
33	"	0.24	0.82	0.741	0.431	0.000156	0.000152	0.000155	0.000155	0.000147	0.000163	0.000145	0.0000766
34	"	0.25	0.75	0.743	0.434	0.000147	0.000156	0.000152	0.000152	0.000148	0.000153	0.000144	0.0000751
35	"	0.29	0.81	0.734	0.423	0 000155	0.000153	0.000156	0.000156	0.000152	0.000163	0.000144	0.0000770
36	"	0.31	0.68	0.732	0.421	0.000153	0.000156	0.000155	0.000155	0.000159	0.000150	0.000155	0.0000773
37	Compression	0.21	1.03	0.742	0.432	0.000142	0.000150	0.000152	0.000155	0.000155	0.000148	0 000156	0.0000756
38	"	0.31	0.78	0.734	0.423	0.000147	0.000156	0.000153	0.000152	0.000155	0.000150	0.000159	0.0000766
39	"	0.31	......	0.738	0.428	0.000156	0.000155	0.000150	0.000153	0.000147	0.000148	0.000156	0.0000761
40	"	0.32	0.79	0.740	0.430	0.000152	0.000150	0.000147	0.000161	0.000156	0.000148	0.000152	0.0000761
41	"	0.32	0.85	0.733	0.422	0.000152	0.000161	0.000159	0.000152	0.000166	0.000153	0.000152	0.0000781
42	"	0.32	......	0.756	0.449	0.000134	0.000141	0.000145	0.000144	0.000148	0.000152	0.000145	0.0000721
43	"	0.33	0.91	0.742	0.432	0 000145	0.000152	0.000155	0.000153	0.000144	0.000155	0.000152	0.0000753
44	"	0.34	0.60	0.744	0.435	0.000150	0.000145	0.000152	0.000153	0.000150	0.000155	0.000161	0.0000761
45	"	0.36	0.89	0.736	0.425	0.000152	0.000158	0.000153	0.000152	0.000148	0.000161	0.000158	0.0000772
46	"	0.37	0.93	0.740	0.430	0.000156	0.000148	0.000156	0.000147	0.000161	0.000155	0.000145	0.0000763
47	"	0.38	0.89	0.735	0.424	0.000148	0.000152	0.000155	0.000150	0.000152	0.000163	0.000153	0.0000766
48	"	0.39	......	0.739	0.429	0.000148	0.000150	0.000156	0.000158	0.000147	0.000159	0.000147	0.0000761
49	"	0.40	0 87	0.722	0.409	0 000175	0.000150	0.000164	0.000163	0.000158	0.000164	0.000155	0.0000806
50	"	......	......	0.760	0.454	0.000136	0.000147	0.000142	0.000145	0.000142	0.000134	0.000152	0.0000713
78a	"Burden's	......	......	0.762	0.456	0.000148	0.000144	0.000158	0.000141	0.000152	....	....	0.0000742
78b	Best"	......	......	0.761	0.455	0.000144	0.000153	0.000150	0.000145	0.000147	....	....	0.0000739
78c	Iron.	......	......	0.756	0.449	0.000148	0.000150	0.000150	0.000152	0.000153	....	....	0.0000753

TABLE No. 2.

Strains per Inch of Length in Round Bars of Open-Hearth Steel due to 2 000 Pound Increments of Compressive Stress.

Number.	Kind.	Per cent. of carbon.	Per cent. of manganese.	Mean diameter. Inch.	Area square. Inch.	Strain in inch corresponding to the increase of the stress from 1 000 to 3 000 pounds.	3 000 to 5 000 pounds.	5 000 to 7 000 pounds.	7 000 to 9 000 pounds.	9 000 to 11 000 pounds.	11 000 to 13 000 pounds.	13 000 to 15 000 pounds.	Strain on specimen per inch of length per 1 000 pounds of stress. Inch.
1	Rivet........	0.08	0.43	0.746	0.437	0.000147	0.000166	0.000153	0.000150	0.000153	0.000147	0.000141	0.0000754
2	"	0.09	0.33	0.732	0.421	0.000178	0.000153	0.000172	0.000150	0.000169	0.000172	0.000153	0.0000819
3	"	0.09	0.35	0.746	0.437	0.000166	0.000156	0.000156	0.000147	0.000159	0.000156	0.000138	0.0000766
4	"	0.09	0.39	0.753	0.445	0.000153	0.000156	0.000153	0.000156	0.000150	0.000153	0.000138	0.0000757
5	"	0.09	......	0.740	0.430	0.000172	0.000166	0.000153	0.000150	0.000160	0.000153	0.000153	0.0000795
6	"	0.09	......	0.742	0.432	0.000169	0.000150	0.000163	0.000150	0.000156	0.000163	0.000153	0.0000788
7	"	0.09	......	0.740	0.430	0.000159	0.000163	0.000153	0.000159	0.000156	0.000159	0.000169	0.0000799
8	"	0.09	......	0.748	0.439	0.000163	0.000159	0.000159	0.000172	0.000156	0.000147	0.000153	0.0000775
9	"	0.09	......	0.747	0.438	0.000166	0.000156	0.000166	0.000150	0.000163	0.000159	0.000141	0.0000786
10	"	0.09	......	0.739	0.429	0.000169	0.000159	0.000156	0.000150	0.000156	0.000163	0.000166	0.0000799
11	"	0.10	......	0.740	0 430	0.000153	0.000166	0.000159	0.000156	0.000153	0.000163	0.000150	0.0000786
12	"	0.10	......	0.741	0.431	0.000156	0.000156	0.000156	0.000156	0.000153	0.000163	0.000166	0.0000772
13	Tension......	0.19	0.58	0.757	0.450	0.000156	0.000156	0.000153	0.000153	0.000134	0.000163	0.000144	0.0000257
14	"	0.19	0.63	0.747	0.438	0.000163	0.000163	0.000159	0.000150	0.000159	0.000153	0.000147	0.0000781
15	"	0.20	0.57	0.739	0.429	0.000169	0.000159	0.000147	0.000163	0.000166	0.000153	0.000166	0.0000801
16	"	0.20	0.69	0.743	0.484	0.000159	0.000163	0.000153	0.000153	0.000166	0.000153	0.000153	0.0000786
17	"	0.20	0.73	0.744	0.435	0.000215	0.000178	0 000153	0.000156	0.000159	0.000150	0.000156	0.0000835
18	"	0.20	0.74	0.750	0.442	0.000156	0.000150	0.000159	0.000156	0.000153	0.000153	0.000156	0.0000775
19	"	0.20	0.76	0.758	0.451	0.000156	0.000156	0.000147	0.000141	0.000144	0.000159	0.000141	0.0000746
20	"	0.20	0.78	0.742	0.432	0.000150	0.000156	0.000163	0.000147	0.000156	0.000150	0.000163	0.0000775
21	"	0.20	0.81	0.742	0.432	0.000166	0.000159	0.000163	0.000156	0.000159	0.000181	0.000150	0.0000817
22	"	0 20	0.84	0.740	0.430	0.000165	0.000156	0.000156	0.000156	0.000156	0.000159	0.000153	0.0000788
23	"	0.20	......	0.742	0.432	0.000150	0.000169	0.000156	0.000156	0.000147	0.000159	0.000159	0.0000783
24	"	0.21	0.62	0.752	0.444	0.000156	0.000163	0.000147	0.000159	0.000147	0.000156	0.000154	0.0000266
25	"	0.21	0·65	0.744	0.435	0.000172	0.000153	0.000153	0.000166	0.000147	0.000166	0.000156	0.0000795
26	"	0.21	0.78	0.745	0.436	0.000178	0.000150	0.000159	0.000150	0.000153	0.000159	0.000150	0.0000786
27	"	0.21	0.84	0.740	0.430	0.000175	0.000144	0.000166	0.000153	0.000156	0.000166	0.000150	0.0000792
28	"	0.21	......	0.740	0.430	0.000144	0.000169	0.000163	0.000163	0.000169	0.000153	0.000163	0.0000783
29	"	0.22	0.58	0.743	0.434	0.000172	0.000156	0.000147	0.000163	0.000153	0.000153	0.000159	0.0000788

30	"	0.22	0.65	0.747	0.438	0.000172	0.000156	0.000163	0.000156	0·000156	0.000153	0.000156	0.0000795
31	"	0.22	0.77	0.736	0.425	0·000163	0.000153	0.000153	0.000159	0.000166	0.000153	0.000156	0.0000786
32	"	0.22	0.78	0.743	0.434	0.000156	0.000147	0.000156	0.000166	0 000147	0.000150	0.000141	0.0000766
33	"	0.24	0.82	0.741	0.431	0.000175	0.000150	0.000156	0.000156	0.000156	0.000159	0.000156	0.0000790
34	"	0.25	0.75	0.747	0.438	0.000150	0.000159	0.000150	0.000156	0.000153	0.000156	0.000172	0.0000772
35	"	0.29	0.81	0.735	0.424	0.000156	0.000166	0.000156	0.000156	0.000163	0.000141	0.000163	0.0000797
36	"	0.31	0.68	0.734	0.423	0.000147	0.000184	0.000163	0.000150	0.000166	0.000156	0.000144	0.0000797
37	Compression.	0.21	1.03	0.744	0.435	0.000156	0.000156	0.000166	0.000147	0.000156	0.000163	0.000156	0.0000781
38	"	0.31	0.78	0.735	0.424	0.000172	0.000163	0.000144	0.000159	0.000163	0.000156	0.000181	0.0000808
39	"	0.31	......	0.741	0.431	0.000159	0.000156	0.000150	0.000150	0.000163	0.000150	0.000159	0.0000781
40	"	0.32	0.79	0.740	0.430	0.000159	0.000159	0.000159	0.000150	0.000159	0.000156	0.000150	0.0000790
41	"	0.32	0.85	0 733	0.422	0.000166	0.000169	0.000156	0.000169	0.000156	0.000169	0.000163	0.0000808
42	"	0.32	......	0.757	0.450	0.000159	0.000156	0.000159	0.000131	0.000163	0.000153	0.000159	0.0000766
43	"	0.33	0.91	0.744	0.435	0.000141	0.000153	0.000159	0.000156	0.000153	0.000144	0.000147	0.0000759
44	"	0.34	0.60	0.744	0.435	0.000156	0.000156	0.000156	0.000155	0.000159	0.000153	0.000153	0.0000783
45	"	0.36	0.89	0.738	0.428	0.000159	0.000163	0.000159	0.000156	0.000156	0.000159	0.000159	0 0000795
46	"	0.37	0.93	0.740	0.430	0.000175	0.000153	0.000159	0.000147	0.000163	0.000153	0.000150	0.0000786
47	"	0.38	0.89	0.734	0.423	0.000194	0.000159	0.000156	0.000159	0·000150	0.000159	0.000150	0·0000808
48	"	0.39	......	0.741	0.431	0.000175	0.000159	0.000166	0.000153	0.000169	0.000159	0.000156	0.0000810
49	"	0.40	0.87	0.725	0.413	0.000178	0.000166	0.000163	0.000159	0.000175	0.000150	0.000166	0.0000826
50	"	......	......	0.762	0.456	0.000159	0.000147	0.000150	0.000147	0.000141	0.000150	0.000156	0.0000750
78a	"Burden's	......	......	0.761	0.455	0.000166	0.000156	0.000159	0.000144	0.000153	0.000159	....	0.0000778
78b	Best"	......	......	0.758	0.451	0.000153	0.000156	0.000138	0.000169	0.000138	0.000163	....	0.0000747
78c	Iron.	......	......	0.758	0.451	0.000169	0.000156	0.000147	0.000172	0.000159	0.000153	....	0.0000803

TABLE No. 3.

Tensile Tests of Round Bars of Open-Hearth Steel.

Number.	Kind.	Per cent. of carbon.	Per cent. of manganese.	Specimen. Mean diameter. Inch.	Specimen. Area. Square Inch.	Specimen. Elastic limit. Pounds.	Specimen. Ultimate resistance. Pounds.	Specimen. Reduction of area. Per cent.	Specimen. Elongation in 8 inches. Per cent.	Per square inch. Elastic limit. Pounds.	Per square inch. Ultimate resistance. Pounds.	Per square inch. Coefficient of elasticity Pounds.
1	Rivet	0.08	0.43	0.746	0.437	17 750	28 200	59.7	29.8	40 620	64 530	30 602 000
2	"	0.09	0.33	0.732	0.421	19 000	26 900	59.4	25.6	45 130	63 900	30 667 000
3	"	0.09	0.35	0.747	0.438	17 000	27 150	59.8	28.1	38 810	61 990	29 951 000
4	"	0.09	2.39	0.753	0.445	17 750	28 700	60.0	30.1	39 890	64 490	30 787 000
5	"	0.09	....	0.740	0.430	19 000	28 050	57.4	29.8	44 190	65 230	29 938 000
6	"	0.09	....	0.743	0.434	17 500	27 400	53.5	26.9	40 320	63 130	30 405 000
7	"	0.09	....	0.739	0.429	16 750	26 600	60.1	31.1	39 040	62 000	30 760 000
8	"	0.09	....	0.748	0.439	17 750	28 500	62.4	30.2	40 430	64 920	30 692 000
9	"	0.09	....	0.747	0.438	17 500	27 760	65.5	31.1	39 950	63 380	30 762 000
10	"	0.09	....	0.740	0.430	17 500	26 230	63.7	31.2	40 700	61 000	30 112 000
11	"	0.10	....	0.739	0.429	16 250	26 100	62.0	30.4	37 880	60 840	31 173 000
12	"	1.10	....	0.741	0.431	17 250	27 280	61.7	30.0	40 020	63 290	29 783 000
13	Tension......	0.19	0.58	0.755	0.448	20 750	33 500	53.3	28.0	46 320	74 780	30 211 000
14	"	0.19	0.63	0.746	0.437	22 750	33 000	50.3	23.0	52 060	75 520	29 758 000
15	"	0.20	0.57	0.739	0.429	18 750	30 320	49.7	26.0	43 710	70 680	30 805 000
16	"	0.20	0.69	0.744	0.435	19 500	31 600	60.9	25.9	44 830	72 640	30 335 000
17	"	0.20	0.73	0.743	0.434	20 000	32 500	58.1	29.4	46 080	74 880	30 722 000
18	"	0.20	0.74	0.750	0.442	18 750	30 350	35.5	13.2	42 420	68 670	31 380 000
19	"	0.20	0.76	0.757	0.450	21 000	35 620	57.1	25.4	46 670	79 160	30 632 000
20	"	0.20	0.78	0.742	0.432	19 250	31 050	57.4	26.0	44 560	71 880	30 546 000
21	"	0.20	0.81	0.741	0.431	18 500	30 550	60.1	27.9	42 920	70 880	30 662 000
22	"	0.20	0.84	0.740	0.430	19 750	33 350	51.9	27.2	45 930	77 560	30 464 000
23	"	0.20	....	0.742	0.432	19 750	33 100	46.8	24.2	45 720	76 620	30 190 000
24	"	0.21	0.63	0.750	0.442	20 000	32 500	56.6	29.2	45 250	73 530	30 575 000
25	"	0.21	0.65	0.742	0.432	20 750	22 320	50.9	26.7	48 030	74 810	30 818 000
26	"	0.21	0.78	0.745	0.436	21 500	34 580	53.4	26.4	49 310	79 310	30 581 000
27	"	0.21	0.84	0.739	0.429	20 250	33 470	50.6	25.8	47 200	78 020	30 579 000
28	"	0.21	....	0.742	0.432	19 750	33 200	51.2	26 4	45 720	76 850	30 636 000
29	"	0.22	0.58	0.744	0.435	19 750	34 900	47.1	25.7	45 400	80 230	30 380 000

30	"	0.22	0.65	0.748	0.439	21 000	33 490	61.3	26.2	47 840	76 290	30 877 000
31	"	0.22	0.77	0.735	0.424	23 500	34 270	55.4	24.1	55 420	80 820	30 362 000
32	"	0.22	0.78	0.743	0.434	19 250	32 700	52.1	26.6	44 360	75 350	30 [illegible]06 000
33	"	0.24	0.82	0.741	0.431	20 750	35 280	54.8	23.4	48 140	81 860	30 304 000
34	"	0.25	0.75	0.743	0.434	20 900	35 500	51.1	24.2	48 160	81 800	30 676 000
35	"	0.29	0.81	0.734	0.423	21 500	36 420	49.9	25.5	50 830	86 100	30 699 000
36	"	0.31	0.68	0.732	0.421	18 000	33 020	53.4	24.4	42 760	78 430	30 711 000
37	Compression.	0.21	1.03	0.742	0.432	21 000	35 240	59.0	25.6	48 610	81 580	30 636 000
38	"	0.31	0.78	0.734	0.423	20 500	34 050	53.7	24.4	48 460	80 500	30 878 000
39	"	0.31	....	0.738	0.428	21 750	35 690	49.5	24.2	50 820	83 390	30 696 000
40	"	0.32	0.79	0.740	0.430	19 500	33 100	56.7	26.8	45 350	76 980	30 553 000
41	"	0.32	0.85	0.733	0.422	20 000	34 950	51.9	24.5	47 390	82 820	30 332 000
42	"	0.32	....	0.756	0.449	25 000	41 520	51.7	24.1	55 680	92 470	30 891 000
43	"	0.33	0.91	0.742	0.432	22 250	36 450	55.8	25.4	51 390	84 380	30 726 000
44	"	0.34	0.60	0.744	0.435	22 250	36 000	49.9	24.9	51 150	82 760	30 202 000
45	"	0.36	0.89	0.736	0.425	22 750	37 630	55.5	24.4	53 530	88 540	30 465 000
46	"	0.37	0.93	0.740	0.430	21 250	37 500	52.8	23.2	49 420	87 210	30 464 000
47	"	0.38	0.89	0.735	0.424	24 000	39 750	27.8	16.9	56 600	93 750	30 805 000
48	"	0.39	....	0.739	0 429	22 250	39 500	44.8	21.9	51 860	92 070	30 624 000
49	"	0.40	0.87	0.722	0.409	21 250	35 700	46.7	22.4	51 960	87 290	30 342 000
50	"	....	....	0.760	0.454	25 250	38 900	55.9	26.9	55 620	85 680	30 885 000

TABLE No. 4.

Tensile Tests of Specimens from the Center and Ends of Round Bars of Open-Hearth Steel.

Number.	Kind.	Per cent. of carbon.	Specimen.						Per square inch.				Comparison of ultimate resistances in the same bar.		
			Mean diameter. Inch.	Area. Square inch.	Elastic limit. Pounds.	Ultimate resistance. Pounds.	Reduction of area. Per cent.	Elongation in eight inches. Per cent.	Elastic limit. Pounds.	Ultimate resistance. Pounds.	Coefficient of elasticity. Pounds.	Mean of the coefficients of elasticity in the same bar.	Mean of the ultimate resistances per square inch.	Ratio of the mean ultimate resistance to the greatest deviation from the mean.	Ratio of the largest ultimate resistance to the smallest.
51*a*	Rivet.....	About 0.09	0.756	0.449	17 800	28 550	61.7	30.5	39 605	63 600	30 939 000				
51*b*	"	"	0.758	0.451	17 500	28 550	61.7	30.5	38 800	63 300	30 010 000	30 703 000	63 230	0.994	1.010
51*c*	"	"	0.757	0.450	17 000	28 350	60.8	28.9	37 800	63 000	31 160 000				
52*a*	"	"	0.757	0.450	17 000	27 900	65.3	29.6	37 800	62 000	31 063 000				
52*b*	"	"	0.758	0.451	17 400	28 500	65.1	29.4	38 600	63 200	30 471 000	30 499 000	62 670	1.011	1.019
52*c*	"	"	0.758	0.451	17 750	28 250	62.3	29.9	39 400	62 800	29 965 000				
53*a*	"	"	0.760	0.454	16 000	27 500	61.6	30.1	37 400	60 600	30 456 000				
53*b*	"	"	0.760	0.454	16 750	27 850	60.6	29.6	36 900	61 300	30 885 000	29 559 000	61 270	1.011	1.021
53*c*	"	"	0.760	0.454	17 750	28 100	61.8	32.2	39 100	61 900	27 335 000				
54*a*	"	"	0.760	0.454	17 250	28 300	57.9	29.2	38 160	62 500	30 618 000				
54*b*	"	"	0.759	0.452	16 750	28 150	62.4	28.4	37 100	62 300	30 172 000	30 405 000	62 050	1.011	1.018
54*c*	"	"	0.758	0.451	16 500	27 700	61.0	28.2	36 600	61 400	30 424 000				
55*a*	"	"	0.756	0.449	16 000	27 700	65.7	28.6	35 600	61 700	29 696 000				
55*b*	"	"	0.755	0.448	16 500	27 600	64.7	29.0	36 800	61 600	30 075 000	30 047 000	61 800	0.995	1.008
55*c*	"	"	0.754	0.447	16 500	27 750	64.3	29.1	36 900	62 100	30 371 000				
56*a*	"	"	0.757	0.450	16 500	27 550	63.4	27.9	36 700	61 200	30 918 000				
56*b*	"	"	0.758	0.451	17 000	27 900	60.4	30.4	37 700	61 900	30 801 000	30 937 000	61 630	1.007	1.011
56*c*	"	"	0.758	0.451	16 750	27 850	64.3	29.2	37 100	61 800	31 091 000				
57*a*	"	"	0.757	0.450	17 000	28 300	51.7	30.1	37 800	62 900	30 032 000				
57*b*	"	"	0.755	0.448	17 250	28 500	49.4	29.2	38 500	63 600	31 646 000	30 570 000	62 600	1.021	1.038
57*c*	"	"	0.757	0.450	17 000	27 600	51.2	28.1	37 800	61 300	30 032 000				

58*a*	"	"	0.750	0.442	16 000	27 150	62.1	30.9	36 200	61 200	30 166 000				
58*b*	"	"	0.749	0.441	16 250	27 500	60.5	29.6	36 800	62 400	30 415 000	30 272 000	61 870	1.011	1.020
58*c*	"	"	0.751	0.443	16 750	27 450	61.3	31.7	37 800	62 000	30 233 000				
59*a*	"	"	0.752	0.444	16 150	27 700	64.3	29.4	36 400	62 400	30 030 000				
59*b*	"	"	0.754	0.447	16 250	27 600	63.0	29.4	36 400	61 740	30 556 000	30 199 000	62 130	1.006	1.011
59*c*	"	"	0.749	0.441	16 200	27 450	62.3	29.2	36 700	62 240	30 011 000				
60*a*	"	"	0.752	0.444	16 500	27 350	55.1	29.9	37 200	61 600	30 210 000				
60*b*	"	"	0.757	0.450	16 500	27 050	53.7	31.0	36 700	60 100	32 965 000	31 091 000	60 900	1.013	1.025
60*c*	"	"	0.753	0.445	17 500	27 150	53.2	32.0	39 300	61 000	30 097 000				
78*a*	"Burden's	....	0.762	0.456	12 750	23 500	50.9	29.4	27 960	51 540	29 647 000				
78*b*	Best"	....	0.761	0.455	12 500	23 700	53.6	33.5	27 470	52 090	29 738 000	29 652 000	51 400	1.016	1.030
78*c*	Iron.	....	0.756	0.449	12 750	22 700	52.6	28.9	28 400	50 560	29 572 000				

TABLE No. 5.

TENSILE TESTS OF SPECIMENS FROM THE CENTER AND ENDS OF ROUND BARS OF BESSEMER STEEL.

Number.	Per cent. of carbon.	Specimen.						Per square inch.				Comparison of ultimate resistances in the same bar.		
		Mean diameter. Inch.	Area. Square inch.	Elastic limit. Pounds.	Ultimate resistance. Pounds.	Reduction of area. Per cent.	Elongation in 8 inches. Per cent.	Elastic limit. Pounds.	Ultimate resistance. Pounds.	Coefficient of elasticity. Pounds.	Mean of the coefficients of elasticity in the same bar.	Mean of the ultimate resistances per square inch.	Ratio of the mean ultimate resistance to the greatest deviation from the mean.	Ratio of the largest ultimate resistance to the smallest.
61a	0.11	0.748	0.439	18 200	29 250	60.3	28.4	41 460	66 630	28 950 000	....	......	......	......
61b	0.11	0.754	0.447	18 500	29 150	58.3	28.2	41 390	65 210	29 391 000	29 413 000	66 270	1.016	1.027
61c	0.11	0.750	0.442	19 200	29 600	57.0	28.2	43 440	66 970	29 899 000	....	......	......	......
62a	0.12	0.751	0.443	18 400	28 950	59.7	27.4	41 540	65 350	29 186 000	....	......	......	......
62b	0.12	0.750	0.442	18 150	28 800	59.2	28.5	41 060	65 160	29 252 000	29 225 000	65 410	0.995	1.009
62c	0.12	0.750	0.442	18 300	29 050	57.4	27.0	41 400	65 720	29 464 000	....	......	......	......
63a	0.12	0.747	0.438	18 400	28 970	57.3	30.6	42 010	66 140	29 907 000	....	......	......	......
63b	0.12	0.750	0.442	18 500	28 890	62.4	30.1	41 860	65 360	29 899 000	29 692 000	65 620	0.992	1.012
63c	0.12	0.751	0.443	18 300	28 950	57.1	28.7	41 310	65 350	29 270 000	....	......	......	......
64a	0.12	0.754	0.447	18 500	28 650	53.9	28.1	41 200	64 200	29 477 000	....	......	......	......
64b	0.12	0.752	0.444	19 000	28 500	56.8	30.1	42 800	64 190	30 023 000	28 936 000	64 210	1.0003	1.0006
64c	0.13	0.749	0.441	19 000	28 300	56.2	27.4	43 200	64 220	27 308 000	....	......	......	......
65a	0.13	0.763	0.457	22 000	31 700	58.1	26.8	48 140	69 360	29 706 000	....	......	......	......
65b	0.13	0.760	0.454	21 500	31 480	59.5	27.0	47 360	69 340	29 500 000	29 481 000	69 620	0.992	1.011
65c	0.13	0.760	0.454	21 400	31 850	56.4	27.1	47 140	70 150	29 238 000	....	......	......	......
66a	0.13	0.763	0.457	19 300	30 350	59.1	28.2	42 230	65 320	29 439 000	....	......	......	......
66b	0.13	0.760	0.454	19 200	29 800	56.6	27.6	42 290	65 640	29 678 000	29 502 000	65 770	0.991	1.016
66c	0.13	0.756	0.449	19 000	29 790	58.3	27.0	42 310	66 350	29 389 000	....	......	......	......
67a	0.15	0.749	0.441	18 000	29 500	54.7	26.0	40 900	66 900	28 860 000	....	......	......	......
67b	0.15	0.748	0.439	19 000	29 550	56.4	26.5	43 200	67 300	29 157 000	26 730 000	66 940	0.981	1.033
67c	0.16	0.754	0.447	19 000	29 350	56.9	27.4	42 500	65 700	22 173 000	27 984 000	......	......	......
67d	0.15	0.746	0.437	18 000	29 650	56.2	28.7	41 200	67 850	31 447 000	....	......	......	......
68a	0.15	0.753	0.445	18 500	30 650	45.7	24.6	41 500	68 880	30 324 000	....	......	......	......
68b	0.16	0.755	0.448	19 000	30 300	54.2	27.7	42 400	67 630	29 240 000	29 676 000	68 250	0.991	1.018
68c	0.16	0.760	0.442	18 500	30 200	52.0	26.2	41 900	68 230	29 464 000	....	......	......	......

69*a*	0.16	0.747	0.438	18 400	29 900	54.8	28.9	42 010	68 260	30 083 000	....	......	......	......
69*b*	0.16	0.745	0.436	18 200	29 860	55.7	27.6	41 740	68 480	30 266 000	29 930 000	68 440	1.003	1.005
69*c*	0.16	0.745	0.436	17 900	29 900	53.0	27.4	41 060	68 580	29 442 000	....	......	......	......
70*a*	0.17	0.746	0.437	18 400	30 750	56.3	27.1	42 110	70 370	29 375 000	....	......	......	......
70*b*	0.17	0.744	0.435	18 600	30 660	57.2	27.4	42 760	70 480	30 158 000	30 105 000	70 140	1.008	1.013
70*c*	0.17	0.749	0.441	18 300	30 680	55.8	27.1	41 500	69 570	30 784 000	....	......	......	......
71*a*	0.17	0.748	0.439	19 500	31 450	51.9	25.5	44 400	71 570	30 372 000	....	......	......	......
71*b*	0.17	0.746	0.437	19 500	31 450	46.9	23.6	44 600	71 970	26 978 000	28 694 000	71 880	0.996	1.008
71*c*	0.18	0.747	0.438	19 500	31 600	46.8	23.9	44 500	72 110	28 731 000	....	......	......	......
72*a*	0.20	0.759	0.453	20 000	31 800	47.3	25.9	44 200	70 280	30 151 000	....	......	......	......
72*b*	0.18	0.756	0.449	20 000	31 850	52.0	27.5	44 500	70 960	30 987 000	30 161 000	70 510	1.006	1.010
72*c*	0.20	0.756	0.449	20 000	31 550	53.8	26.4	44 500	70 290	29 346 000	....	......	......	......
73*a*	0.22	0.758	0.451	22 000	34 600	49.6	25.0	48 800	76 670	29 741 000	....	......	......	......
73*b*	0.22	0.757	0.450	22 000	34 650	51.9	25.2	48 900	76 980	29 807 000	29 711 000	76 750	1.003	1.005
73*c*	0.23	0.755	0.448	22 000	34 300	51.5	25.2	49 000	76 610	29 586 000	....	......	......	......
74*a*	0.23	0.756	0.449	21 000	32 700	45.1	25.0	46 800	72 850	30 054 000	....	......	......	......
74*b*	0.24	0.757	0.450	22 000	32 850	45.3	24.4	48 900	72 980	31 307 000	31 273 000	72 740	0.995	1.008
74*c*	0.23	0.763	0.457	22 000	33 100	48.6	24.7	48 140	72 400	32 460 000	....	......	......	......
75*a*	0.24	0.754	0.447	21 000	34 100	51.9	25.1	47 090	76 370	30 933 000	....	......	......	......
75*b*	0.23	0.756	0.449	20 500	34 150	50.1	24.6	45 700	76 080	30 987 000	30 878 000	76 110	1.003	1.006
75*c*	0.24	0.759	0.453	21 000	34 350	48.5	25.2	46 400	75 890	30 713 000	....	......	......	......
76*a*	0.36	0.761	0.455	27 750	44 350	40.7	20.5	60 990	97 470	29 045 000	....	......	......	......
76*b*	0.36	0.756	0.449	27 100	44 700	38.5	19.1	60 360	99 550	30 236 000	29 757 000	98 710	1.013	1.021
76*c*	0.36	0.759	0.452	27 000	44 800	39.5	19.4	59 730	99 120	29 989 000	....	......	......	......
77*a*	0.39	0.763	0.457	27 200	43 770	39.0	20.0	59 520	95 780	30 025 000	....	......	......	......
77*b*	0.39	0.762	0.456	27 200	43 860	36.8	19.2	59 650	96 180	30 944 000	30 086 000	95 720	1.005	1.010
77*c*	0.39	0.765	0.460	27 200	43 790	36.7	19.0	59 130	95 200	29 291 000	....	......	......	......

TABLE No. 6.

Compression Tests of Columns of Open-Hearth Steel Six Inches Long.

Number.	Kind.	Per cent. of carbon.	Per cent. of manganese.	Mean diameter. Inch.	Area. Square inch.	Elastic limit. Pounds. Actual.	Elastic limit. Pounds. Per square inch.	Ultimate resistance. Pounds.	Length after test. Inches.	Co-efficient of elasticity. Pounds.
1	Rivet	0.08	0.43	0.746	0.437	17 000	38 720	20 300	5.68	30 330 000
2	"	0.09	0.33	0.732	0.421	20 250	47 870	23 500	5.72	28 995 000
3	"	0.09	0.35	0.746	0.437	17 250	39 380	19 800	5.66	29 888 000
4	"	0.09	0.39	0.753	0.445	18 750	41 950	21 480	5.66	29 697 000
5	"	0.09	....	0.740	0.430	20 500	47 670	21 300	5.71	29 266 000
6	"	0.09	....	0.742	0.432	18 000	41 470	19 740	5.69	29 378 000
7	"	0.09	....	0.740	0.430	17 750	41 370	19 500	5.64	29 102 000
8	"	0.09	....	0.748	0.439	19 500	44 420	21 000	5.65	29 409 000
9	"	0.09	....	0.747	0.438	19 000	43 380	20 000	5.67	29 058 000
10	"	0.09	....	0.739	0.429	17 750	41 180	19 300	5.70	29 170 000
11	"	0.10	....	0.740	0.430	17 250	40 120	19 900	5.54	29 598 000
12	"	0.10	....	0.741	0.431	17 750	41 180	20 400	5.67	30 042 000
13	Tension	0.19	0.58	0.757	0.450	21 750	48 230	24 700	5.66	29 367 000
14	"	0.19	0.63	0.747	0.438	25 000	57 080	24 950	5.80	29 224 000
15	"	0.20	0.57	0.739	0.429	19 500	45 240	23 500	5.69	29 089 000
16	"	0.20	0.69	0.743	0.434	22 000	50 570	22 900	5.72	29 325 000
17	"	0.20	0.73	0.744	0.435	21 500	49 540	23 400	5.71	27 537 000
18	"	0.20	0.74	0.750	0.442	19 500	44 020	24 150	5.71	29 210 000
19	"	0.20	0.76	0.758	0.451	22 500	50 000	27 500	5.71	29 741 000
20	"	0.20	0.78	0.742	0.432	21 000	48 280	21 120	5.67	29 888 000
21	"	0.20	0.81	0.742	0.432	20 200	46 870	23 700	5.67	28 334 000
22	"	0.20	0.84	0.740	0.430	20 500	47 560	25 050	5.72	29 514 000
23	"	0.20	....	0.742	0.432	21 000	48 390	23 750	5.75	29 545 000
24	"	0.21	0.63	0.752	0.444	21 250	47 860	24 100	5.70	29 417 000
25	"	0.21	0.65	0.744	0.435	22 250	51 150	23 080	5.66	28 928 000
26	"	0.21	0.78	0.745	0.436	22 750	52 180	25 600	5.71	29 191 000
27	"	0.21	0.84	0.740	0.430	21 250	49 300	23 800	5.69	29 348 000
28	"	0.21	....	0.740	0.430	21 500	49 090	24 650	5.66	29 683 000
29	"	0.22	0.58	0.643	0.434	21 000	48 280	26 840	5.76	29 242 000
30	"	0.22	0.65	0.747	0.438	21 700	49 510	24 600	5.71	28 731 000
31	"	0.22	0.77	0.736	0.425	24 250	57 060	24 250	5.73	29 946 000

32	Tension......	0.22	0.78	0.743	0.434	20 000	46 080	25 000	5.68	30 095 000
33	"	0.24	0.82	0.741	0 431	22 000	50 930	25 700	5.72	29 356 000
34	"	0.25	0.75	0.747	0·438	22 250	51 030	26 850	5.75	29 562 000
35	"	0.29	0.81	0.735	0.424	22 750	53 150	27 000	5.72	29 597 000
36	"	0.31	0.68	0.734	0.423	19 800	46 810	25 050	5.72	29 667 000
37	Compression	0.21	1.03	0.744	0.435	23 000	52 870	25 250	5.72	29 425 000
38	"	0.31	0.78	0.735	0.424	21 500	50 590	25 100	5.67	29 188 000
39	"	0.31	....	0.741	0.431	23 250	54 070	27 350	5.71	29 698 000
40	"	0.32	0.79	0.740	0.430	20 500	47 560	25 350	5.71	29 431 000
41	"	0.32	0.85	0.733	0.422	20 750	49 050	26 200	5.74	29 326 000
42	"	0.32	....	0.757	0.450	27 000	59 730	30 870	5.71	29 025 000
43	"	0.33	0.91	0.744	0.435	24 250	55 750	26 550	5.72	30 291 000
44	"	0.34	0.60	0.744	0.435	23 800	54 590	26 450	5.72	29 341 000
45	"	0.36	0.89	0.738	0.428	23 500	55 040	28 100	5.75	29 402 000
46	"	0.37	0.93	0.740	0.430	22 000	51 160	27 500	5.72	29 598 000
47	"	0.38	0.89	0.734	0.423	25 250	59 410	30 400	5.79	29 257 000
48	"	0.39	....	0.741	0.431	23 800	55 220	27 500	5.77	28 635 000
49	"	0.40	0.87	0.725	0.413	22 000	53 400	27 100	5.73	29 317 000
50	"	....	....	0.762	0.456	25 500	55 920	28 300	5.68	29 240 000
51*a*	Rivet	About 0.09	From 0.30 to 0.50	0.757	0.450	17 750	39 400	21 040	5.56	29 896 000
51*b*	"	"	"	0.756	0.449	......	......	20 710	5.70	27 113 000
51*c*	"	"	"	0.757	0.450	......	......	20 530	5.64	28 444 000
52*a*	"	"	"	0.757	0.450	18 500	41 100	22 350	5.64	29 110 000
52*b*	"	"	"	0.757	0.450	......	......	20 350	5.57	29 025 000
52*c*	"	"	"	0.758	0.451	......	......	20 400	5.61	29 045 000
53*a*	"	"	"	0.760	0.454	18 250	40 200	20 880	5.62	30 045 000
53*b*	"	"	"	0.760	0.454	......	......	21 210	5.64	28 853 000
53*c*	"	"	"	0.759	0.452	......	......	20 530	5.60	29 411 000
54*a*	"	"	"	0.758	0.451	18 750	41 600	20 920	5.62	30 193 000
54*b*	"	"	"	0.758	0.451	......	......	21 520	5.63	29 302 000
54*c*	"	"	"	0.758	0.451	......	......	20 950	5.62	29 216 000
55*a*	"	"	"	0.753	0.445	17 250	38 600	20 690	5.64	29 013 000
55*b*	"	"	"	0.753	0.445	......	......	20 000	5.64	29 963 000
55*c*	"	"	"	0.754	0.447	......	......	20 530	5.67	29 477 000
56*a*	"	"	"	0.756	0.449	17 250	38 300	20 100	5.60	29 090 000
56*b*	"	"	"	0.757	0.450	......	......	21 350	5.64	29 807 000
56*c*	"	"	"	0.758	0.451	......	......	21 430	5.62	28 960 000
57*a*	"	"	"	0.757	0.450	18 750	41 300	20 940	5.66	29 630 000
57*b*	"	"	"	0.757	0.450	......	......	21 330	5.60	28 940 000
57*c*	"	"	"	0.756	0.449	......	......	20 780	5.70	29 695 000
58*a*	"	"	"	0.753	0.445	17 750	39 900	21 580	5.62	29 437 000
58*b*	"	"	"	0.751	0.443	......	......	19 730	5.65	30 008 000
58*c*	"	"	"	0.751	0.443	......	......	20 300	5.65	28 730 000
59*a*	"	"	"	0.756	0.449	17 750	39 700	20 400	5.68	29 005 000

TABLE No. 6.—*Continued.*

Number.	Kind.	Per cent. of carbon,	Per cent. of manganese.	Mean diameter. Inch.	Area. Square inch.	ELASTIC LIMIT, Pounds. Actual.	ELASTIC LIMIT, Pounds. Per square inch.	Ultimate resistance. Pounds.	Length after test. Inches.	Co-efficient of elasticity. Pounds.
59*b*	Rivet	About 0.09	From 0.30 to 0.50	0.754	0.447	......	......	20 700	5.64	29 740 000
59*c*	"	"	"	0.753	0.445	......	......	21 060	5.66	29 963 000
60*a*	"	"	"	0.752	0.444	17 750	39 700	19 320	5.56	31 433 000
60*b*	"	"	"	0.752	0.444	......	......	20 210	5.60	29 782 000
60*c*	"	"	"	0.754	0.447	......	......	19 750	5.50	29 391 000
78*a*	"Burden's	....	....	0.761	0.455	19 250	42 310	19 450	5.66	28 245 000
78*b*	Best"	....	....	0.758	0.451	20 000	44 350	18 950	5.64	29 687 000
78*c*	Iron.	....	....	0.758	0.451	19 500	43 240	19 100	5.62	27 608 000

TABLE No. 7.

Compression Tests of Columns of Bessemer Steel Six Inches Long.

Number.	Per cent. of carbon.	Mean diameter. Inch.	Area. Square inch.	Elastic limit. Pounds. Actual.	Elastic limit. Pounds. Per square inch.	Ultimate resistance. Pounds.	Coefficient of elasticity. Pounds.
61*a*	0.11	0.751	0.443	18 500	42 000	22 150	28 567 000
61*b*	0.11	0.751	0.443			21 800	29 149 000
61*c*	0.11	0.752	0.444			22 390	28 747 000
62*a*	0.12	0.752	0.444	18 250	41 000	22 780	28 503 000
62*b*	0.12	0.749	0.441			23 010	29 531 000
62*c*	0.12	0.751	0.443			21 900	28 730 000
63*a*	0.12	0.752	0.444	18 750	42 000	21 800	29 162 000
63*b*	0.12	0.750	0.442			22 500	29 210 000
63*c*	0.12	0.754	0.447			21 390	28 635 000
64*a*	0.12	0.750	0.442	20 000	45 500	23 300	28 471 000
64*b*	0.12	0.753	0.445			22 500	30 324 000
64*c*	0.13	0.750	0.442			21 900	28 155 000
65*a*	0.13	0.762	0.456	20 250	44 400	25 150	28 070 000
65*b*	0.13	0.759	0.452			24 340	28 729 000
65*c*	0.13	0.757	0.450			24 240	29 025 000
66*a*	0.13	0.760	0.454	20 000	44 000	22 860	29 281 000
66*b*	0.13	0.758	0.451			23 060	29 830 000
66*c*	0.13	0.759	0.452			23 330	29 324 000
67*a*	0.15	0.749	0.441	24 500	55 600	22 700	29 531 000
67*b*	0.15	0.744	0.435			22 500	29 594 000
67*c*	0.16	0.748	0.439			21 900	29 325 000
68*a*	0.15	0.753	0.445	21 250	47 700	24 000	29 126 000
68*b*	0.16	0.749	0.441			23 800	30 234 000
68*c*	0.16	0.752	0.444			23 800	20 332 000
69*a*	0.16	0.747	0.438	18 000	41 000	21 700	29 812 000
69*b*	0.16	0.744	0.435			21 790	29 341 000
69*c*	0.16	0.748	0.439			22 850	28 666 000
70*a*	0.17	0.749	0.441	18 250	41 300	22 800	28 860 000
70*b*	0.17	0.748	0.439			22 960	29 241 000
70*c*	0.17	0.746	0.437			22 710	29 802 000
71*a*	0.17	0.748	0.439	21 250	48 800	24 400	30 738 000

TABLE No. 7.—*Continued.*

Number.	Per cent of carbon.	Mean diameter. Inch.	Area. Square inch.	Elastic limit. Pounds.		Ultimate resistance. Pounds.	Coefficient of elasticity. Pounds.
				Actual.	Per square inch.		
71b	0.17	0.747	0.438			24 500	30 441 000
71c	0.18	0.747	0.438			24 100	28 412 000
72a	0.20	0.755	0.448	21 000	47 200	23 500	30 488 000
72b	0.18	0.755	0.448			24 900	30 488 000
72c	0.20	0.754	0.447			24 450	29 477 000
73a	0.22	0.755	0.448	23 000	51 200	27 300	30 488 000
73b	0.22	0.754	0.447			27 100	29 477 000
73c	0.23	0.757	0.450			25 700	30 352 000
74a	0.23	0.755	0.448	22 500	50 100	25 000	31 644 000
74b	0.24	0.758	0.451			26 900	29 560 000
74c	0.23	0.758	0.451			25 100	33 112 000
75a	0.24	0.753	0.445	22 000	49 100	26 400	27 960 000
75b	0.23	0.757	0.450			24 750	28 283 000
75c	0.24	0.756	0.449			25 600	28 672 000
76a	0.36	0.752	0.444	24 500	54 300	33 040	29 162 000
76b	0.36	0.757	0.450			34 620	29 454 000
76c	0.36	0.757	0.450			33 550	29 281 000
77a	0.39	0.760	0.454	27 000	58 600	33 610	28 602 000
77b	0.39	0.762	0.456			30 730	28 981 000
77c	0.39	0.760	0.454			32 920	29 281 000

TABLE No. 8.

Comparison of Coefficients of Elasticity in Tension and Compression.

	Per cent. of carbon.	Kind.	Number of tests.	Mark.	Coefficients of elasticity in pounds.						Ratio of the mean coefficient in tension to that in compression.
					In tension.			In compression.			
					Table.	Varies between.	Mean.	Table.	Varies between.	Mean.	
Open-Hearth Steel ..	About 0.09	Rivet.	12	1 to 12	3	29 780 000 and 31 170 000	30 470 000	6	29 000 000 and 30 330 000	29 490 000	1.033
" " ..	0.20 to 0.30	Tension.	24	13 to 36	3	29 760 000 " 31 380 000	30 570 000	6	27 540 000 " 30 090 000	29 310 000	1.043
" " ..	0.30 to 0.40	Compression.	14	37 to 50	3	30 200 000 " 30 890 000	30 610 000	6	28 630 000 " 30 290 000	29 330 000	1.043
" " ..	0.09 to 0.40	....	50	1 to 50	3	29 760 000 " 31 380 000	30 550 000	6	27 540 000 " 30 330 000	29 390 000	1.040
" " ..	About 0.09	Rivet.	30	51*a* to 60*c*	4	27 330 000 " 31 650 000	39 430 000	6	27 110 000 " 30 430 000	29 390 000	1.035
Bessemer Steel	0.11 to 0.13	....	18	61*a* to 66*c*	5	27 310 000 " 30 020 000	29 390 000	7	28 070 000 " 30 320 000	28 970 000	1.014
" "	0.20 to 0.24	....	10	72*c* to 75*c*	5	29 350 000 " 32 460 000	30 490 000	7	27 960 000 " 33 110 000	29 900 000	1.020
" "	0.36 to 0.39	....	6	76*a* to 77*c*	5	20 040 000 " 30 490 000	29 920 000	7	28 600 000 " 29 450 000	29 130 000	1.027
" "	0.11 to 0.39	....	51	61*a* to 77*c*	5	22 170 000 " 32 460 000	29 640 000	7	27 960 000 " 33 110 000	29 370 000	1.009
"Burden's Best" Iron			3	78*a* to 78*c*	4	29 570 000 " 29 740 000	29 650 000	6	27 610 000 " 29 690 000	28 510 000	1.040

TABLE No. 9.

Compression Tests of Columns of Open-hearth Steel Two Inches Long.

Number.	Kind.	Per cent. of carbon.	Per cent. of manganese.	Mean diameter. Inches.	Area. Square inch.	Stress in pounds on specimen at			Stress in pounds per square inch at		
						Elastic limit.	5 per cent. reduction of length.	10 per cent. reduction of length.	Elastic limit.	5 per cent. reduction of length.	10 per cent. reduction of length.
1	Rivet	0.08	0.43	0.748	0.439	17 000	26 540	34 600	38 720	60 450	78 820
2	"	0.09	0.33	0.724	0.423	20 250	25 150	31 800	47 870	59 460	75 180
3	"	0.09	0.35	0.747	0.438	17 250	25 000	33 100	39 380	57 080	75 570
4	"	0.09	0.39	0.754	0.447	18 750	26 000	34 650	41 950	58 160	77 520
5	"	0.09	....	0.740	0.430	20 500	26 000	33 950	47 670	60 460	78 950
6	"	0.09	....	0.743	0.434	18 000	25 550	33 400	41 470	58 870	76 960
7	"	0.09	....	0.739	0.429	17 750	25 250	32 200	41 380	58 860	75 060
8	"	0.09	....	0.748	0.439	19 500	26 000	33 800	44 420	59 230	76 990
9	"	0.09	....	0.747	0.438	19 000	26 000	33 930	43 380	59 360	77 470
10	"	0.09	....	0.741	0.431	17 750	24 890	31 540	41 180	57 750	73 180
11	"	0.10	....	0.740	0.430	17 250	24 000	31 730	40 120	56 810	73 790
12	"	0.10	....	0.741	0.431	17 750	24 980	32 700	41 180	57 960	75 870
13	Tension	0.19	0.58	0.758	0.451	21 750	30 500	41 000	48 230	67 630	90 910
14	"	0.19	0.63	0.747	0.438	25 000	32 700	38 700	57 080	74 660	88 360
15	"	0.20	0.57	0.741	0.431	19 500	27 300	36 700	49 400	63 340	85 150
16	"	0.20	0.69	0.744	0.435	22 000	29 880	37 500	50 570	68 690	86 210
17	"	0.20	0.73	0.743	0.434	21 500	30 000	38 600	49 540	69 120	88 940
18	"	0.20	0.74	0.751	0.443	19 500	28 000	37 900	44 020	63 210	93 220
19	"	0.20	0.76	0.757	0.450	22 500	29 800	42 600	50 000	66 220	94 670
20	"	0.20	0.78	0.744	0.435	21 000	30 750	36 770	48 280	70 690	84 530
21	"	0.20	0.81	0.742	0.432	20 250	28 650	37 750	46 870	66 320	87 390
22	"	0.20	0.84	0.741	0.431	20 500	30 590	39 350	47 560	70 970	91 300
23	"	0.20	....	0.743	0.434	21 000	29 900	39 600	48 390	68 890	91 240
24	"	0.21	0.63	0.752	0.444	21 250	30 790	39 100	47 860	69 350	88 060
25	"	0.21	0.65	0.744	0.435	32 250	29 700	38 750	51 150	68 280	89 080
26	"	0.21	0.78	0.745	0.436	22 750	32 540	42 550	52 180	74 630	97 500
27	"	0.21	0.84	0.741	0.431	21 250	30 500	39 500	49 300	70 760	91 650
28	"	0.21	....	0.744	0.435	21 500	31 400	40 800	49 430	72 180	93 790
29	"	0.21	0.58	0.744	0.435	21 000	32 850	42 520	48 280	75 520	97 750
30	"	0.22	0.65	0.748	0.439	21 750	30 950	39 750	49 540	70 500	90 550
31	"	0.22	0.77	0.736	0.425	24 250	32 500	40 500	57 060	76 470	95 300

32	"	0.22	0.78	0.743	0.434	20 000	29 200	37 750	46 080	67 280	86 980
33	"	0.24	0.82	0.742	0.432	22 000	33 100	41 900	50 939	76 620	96 990
34	"	0.25	0.75	0.745	0.436	22 250	33 850	42 000	51 030	77 640	96 330
35	"	0.29	0.81	0.738	0.428	22 750	33 700	43 300	53 160	78 740	101 170
36	"	0.31	0.68	0.734	0.423	19 180	29 670	38 830	46 810	70 140	91 800
37	Compression........	0.21	1.03	0.744	0.435	23 000	31 800	40 900	52 870	73 100	94 020
38	"	0.31	0.78	0.736	0.425	21 500	31 700	42 600	50 590	74 590	100 270
39	"	0.31	....	0.740	0.430	30 250	33 200	42 860	54 070	77 210	99 670
40	"	0.32	0.79	0.741	0.431	20 500	30 800	39 900	47 560	71 460	92 570
41	"	0.32	0.85	0.734	0.423	20 750	31 800	41 300	49 050	75 180	97 640
42	"	0.32	....	0.759	0.452	27 090	41 000	48 800	59 730	90 710	107 970
43	"	0.33	0.91	0.744	0.435	24 250	35 600	43 800	55 750	81 840	100 700
44	"	0.34	0.60	0.745	0.436	23 800	33 200	43 640	54 590	76 140	100 100
45	"	0.36	0.89	0.737	0.427	23 500	37 500	44 600	55 040	87 820	104 450
46	"	0.37	0.93	0.740	0.430	22 000	34 850	43 660	51 160	81 050	101 540
47	"	0.38	0.89	0.736	0.425	25 250	38 000	46 900	59 410	89 410	110 350
48	"	0.39	....	0.741	0.431	23 800	37 400	46 600	55 200	86 770	108 120
49	"	0.40	0.87	0.724	0.412	22 000	33 800	42 400	53 400	82 040	102 900
50	"	....	....	0.762	0.456	25 000	36 800	47 000	55 920	80 700	103 070
51	Rivet	About 0.09	From 0.30 to 0.50	0.757	0.450	17 750	26 750	33 830	39 440	59 440	75 180
52	"	"	"	0.757	0.450	18 500	27 320	35 550	41 110	60 710	79 000
53	"	"	"	0.760	0.454	18 250	28 000	35 700	40 200	61 670	78 630
54	"	"	"	0.758	0.451	18 750	27 070	35 100	41 570	60 020	77 830
55	"	"	"	0.754	0.447	17 250	27 370	35 490	38 590	61 230	79 400
56	"	"	"	0.757	0.450	17 250	26 760	43 930	38 330	59 470	75 400
57	"	"	"	0.756	0.449	18 750	26 300	35 540	41 760	58 570	70 150
58	"	"	"	0.753	0.445	17 750	26 150	36 170	39 890	58 760	81 280
59	"	"	"	0.754	0.447	17 750	25 830	35 110	39 710	57 780	78 540
60	"	"	"	0.754	0.447	17 750	26 550	32 700	39 710	59 400	73 160
78a	"Burden's Best" iron....	....	....	0.760	0.454	19 250	22 500	30 500	42 400	49 560	67 180
78b	"	....	....	0.756	0.449	20 000	23 000	29 750	44 540	51 220	66 260
78c	"	....	....	0.758	0.451	19 500	23 000	30 000	43 240	51 000	66 520

TABLE No. 10.

COMPRESSION TESTS OF COLUMNS OF BESSEMER STEEL TWO INCHES LONG.

Number.	Per cent. of carbon.	Mean diameter. Inch.	Area. Square inch.	Stress in pounds on specimen at			Stress in pounds per square inch at		
				Elastic limit.	5 per cent. reduction of length.	10 per cent. reduction of length.	Elastic limit.	5 per cent. reduction of length.	10 per cent. reduction of length.
61	0.11	0.747	0.450	18 500	27 470	35 930	42 200	62 800	82 000
62	0.12	0.750	0.442	18 250	27 200	37 080	41 290	61 540	83 890
63	0.12	0.754	0.447	18 750	27 460	35 800	41 950	61 430	80 060
64	0.12	0.748	0.430	20 000	25 390	35 330	45 560	57 840	80 480
65	0.13	0.762	0.456	20 250	30 980	38 640	44 410	67 940	74 740
66	0.13	0.760	0.454	20 000	29 070	37 900	44 050	64 030	83 480
67	0.15	0.749	0.441	24 500	26 500	36 250	55 600	60 100	82 200
68	0.16	0.753	0.445	21 250	27 770	37 000	47 750	62 410	83 150
69	0.16	0.751	0.443	18 000	27 000	35 360	40 630	60 950	79 820
70	0.17	0.750	0.442	18 250	29 230	37 530	41 290	66 130	84 910
71	0.17	0.744	0.435	21 250	29 400	39 090	48 850	67 590	89 960
72	0.20	0.753	0.445	21 000	27 590	39 000	47 190	65 000	87 640
73	0.22	0.756	0.449	23 000	31 860	41 550	51 220	70 960	92 540
74	0.23	0.756	0.449	22 500	31 600	40 900	50 110	70 380	91 090
75	0.24	0.755	0.448	22 000	28 930	40 000	49 110	64 580	89 290
76	0.36	0.758	0.451	24 500	37 130	......	54 320	82 330	......
77	0.29	0.766	0.461	27 000	43 580	50 000	58 579	94 530	108 500

[Re]sistance for each bar. Pounds.	Ratio of ultimate resistance per square inch in double to that in single shear.	Ultimate tensile resistance per square inch. Pounds.	Ratio of ultimate resistance per square inch in double shear to that in tension.
47 950	1.022	64 530	0.743
47 750	1.015	63 900	0.747
47 070	1.018	61 990	0.759
48 240	1.026	64 490	0.748
49 300	1.032	65 230	0.756
48 290	1.024	63 130	0.765
46 890	1.030	62 000	0.756
48 650	1.021	64 920	0.749
48 290	1.017	63 380	0.762
47 130	1.027	61 000	0.773
46 020	1.014	60 840	0.756
47 650	1.036	63 290	0.753
54 650	1.012	74 780	0.731

TABLE No. 11.

Shearing Resistance of Round Bars of Open-Hearth Steel.

Number.	Kind.	Per cent. of carbon.	Per cent. of manganese.	Mean diameter. Inch.	Area. Square inch.	Single shear. Of the specimen. Elastic limit. Pounds.	Single shear. Of the specimen. Ultimate resistance. Pounds.	Single shear. Per square inch. Elastic limit. Pounds.	Single shear. Per square inch. Ultimate resistance. Pounds.	Single shear. Per square inch. Mean elastic limit for each bar. Pounds.	Single shear. Per square inch. Mean ultimate resistance for each bar. Pounds.	Double shear. Of the specimen. Elastic limit. Pounds.	Double shear. Of the specimen. Ultimate resistance. Pounds.	Double shear. Per square inch. Elastic limit. Pounds.	Double shear. Per square inch. Ultimate resistance. Pounds.	Double shear. Per square inch. Mean elastic limit for each bar. Pounds.	Double shear. Per square inch. Mean ultimate resistance for each bar. Pounds.	Ratio of ultimate resistance per square inch in double to that in single shear.	Ultimate tensile resistance per square inch. Pounds.	Ratio of ultimate resistance per square inch in double shear to that in tension.
1	Rivet	0.08	0.43	0.747	0.438	15 000	20 550	34 250	46 920	34 820	46 900	35 500	42 000	39 390	47 950	39 100	47 950	1.022	64 530	0.743
1	"					15 500	20 530	35 390	46 870			34 000	42 000	38 810	47 950					
2	"	0.09	0.33	0.732	0.421	12 500	19 800	29 690	47 030	30 280	47 060	21 500	40 000	25 530	47 510	25 530	47 750	1.015	63 900	0.747
2	"					13 000	19 820	30 880	47 080			21 500	40 400	25 530	47 980					
3	"	0.09	0.35	0.746	0.437	15 500	20 100	35 470	46 000	36 330	46 230	33 000	40 970	37 760	46 880	38 050	47 070	1.018	61 990	0.759
3	"					16 250	20 300	37 190	46 450			33 500	41 300	38 330	47 250					
4	"	0.09	0.39	0.753	0.445	16 250	20 980	36 520	47 150	35 670	47 000	29 500	42 920	33 150	48 220	33 990	48 240	1.026	64 490	0.748
4	"					15 500	20 850	34 830	46 850			31 000	42 950	34 830	48 260					
5	"	0.09	....	0.740	0.430	14 250	20 430	33 140	47 510	33 720	47 770	35 000	42 350	40 700	49 240	41 570	49 300	1.032	65 230	0.756
5	"					14 750	20 650	34 300	48 020			36 500	42 450	42 440	49 360					
6	"	0.09	....	0.743	0.434	15 000	20 400	34 560	47 000	34 560	47 170	26 000	42 250	29 950	48 680	30 820	48 290	1.024	63 130	0.765
6	"					15 000	20 550	34 560	47 350			27 500	41 580	31 680	47 900					
7	"	0.09	....	0.739	0.429	15 250	19 550	35 550	45 570	37 000	45 540	27 000	40 300	31 470	46 970	32 350	46 890	1.030	62 000	0.756
7	"					16 500	19 520	38 460	45 500			28 500	40 150	33 220	46 800					
8	"	0.09	....	0.748	0.439	12 750	21 000	29 040	47 840	29 320	47 670	27 000	42 980	30 750	48 950	30 750	48 650	1.021	64 920	0.749
8	"					13 000	20 850	29 610	47 490			27 000	42 450	30 750	48 350					
9	"	0.09	....	0.747	0.438	12 000	20 690	27 400	47 200	27 970	47 460	24 500	42 300	27 970	48 290	25 970	48 290	1.017	63 380	0.762
9	"					12 500	20 900	28 540	47 720			21 000	42 300	27 970	48 290					
10	"	0.09	....	0.740	0.430	10 000	19 820	23 300	46 090	21 550	45 900	19 000	41 560	22 100	48 330	21 520	47 130	1.027	61 000	0.773
10	"					8 500	19 650	19 800	45 700			18 000	39 500	20 930	45 930					
11	"	0.10	....	0.737	0.427	14 000	19 350	32 790	45 320	33 370	45 380	32 000	39 300	37 470	46 020	38 060	46 020	1.014	60 840	0.756
11	"					14 500	19 400	33 960	45 430			33 000	39 300	38 640	46 020					
12	"	0.10	....	0.740	0.430	15 000	19 650	34 880	45 690	34 880	45 990	27 000	41 170	31 400	47 870	31 690	47 650	1.036	63 290	0.753
12	"					15 000	19 900	34 880	46 280			27 500	40 780	31 980	47 420					
13	Tension	0.19	0.58	0.754	0.447	22 500	24 000	50 340	53 690	50 890	54 020	45 000	48 980	50 340	54 790	50 620	54 650	1.012	74 780	0.731
13	"					23 000	24 290	51 450	54 340			45 500	48 730	50 890	54 510					
14	"	0.19	0.63	0.747	0.438	23 000	23 950	52 510	54 680	52 510	54 450	45 500	48 750	50 800	55 650	50 800	55 450	1.018	75 520	0.734
14	"					23 000	23 750	52 510	54 220			44 500	48 400	50 800	55 250					
15	"	0.20	0.57	0.740	0.430	20 500	21 090	47 670	49 050	48 300	49 300	40 500	43 600	47 090	50 700	48 260	51 100	1.036	70 680	0.723
15	"					21 000	21 310	48 840	49 560			42 500	44 350	49 420	51 500					
16	"	0.20	0.69	0.743	0.434	21 500	22 850	49 540	52 650	49 540	52 620	41 000	46 600	47 230	53 690	48 100	53 950	1.025	72 640	0.743
16	"					21 500	22 820	49 540	52 580			42 500	47 050	48 960	54 200					
17	"	0.20	0.73	0.743	0.434	22 500	23 070	51 840	53 160	51 840	53 310	46 750	47 650	53 860	54 900	53 140	55 000	1.032	74 880	0.735
17	"					25 500	23 200	51 840	53 460			45 500	47 820	52 420	55 090					
18	"	0.20	0.74	0.750	0.442	22 000	23 100	49 770	52 260	50 340	52 250	42 000	46 000	47 510	52 040	49 200	52 370	1.002	68 670	0.763
18	"					22 500	23 090	50 000	52 240			45 000	46 580	50 900	52 690					
19	"	0.20	0.76	0.757	0.450	22 500	25 650	50 000	57 000	50 000	55 170	44 500	51 000	49 450	56 670	50 280	56 670	1.027	79 160	0.716
19	"					22 500	24 000	50 500	53 330			46 000	51 000	51 110	56 670					
20	"	0.20	0.78	0.743	0.434	19 500	22 200	44 930	51 150	45 220	51 500	36 000	45 000	41 470	51 840	42 340	52 160	1.013	71 880	0.726
20	"					19 750	22 500	45 510	51 840			37 500	45 500	43 200	52 480					
21	"	0.20	0.81	0.743	0.434	21 500	21 710	49 540	50 020	49 540	50 100	43 000	44 000	49 540	50 690	49 540	51 040	1.019	70 880	0.720
21	"					19 000	21 780	49 540	50 180			43 000	44 600	49 540	51 380					
22	"	0.20	0.84	0.740	0.430	20 000	23 750	41 190	55 230	45 350	54 940	34 000	47 450	39 530	55 170	40 120	55 120	1.003	77 560	0.711
22	"					21 500	23 500	46 510	54 650			35 000	47 350	40 700	55 060					
23	"	0.20	....	0.744	0.435	21 500	23 750	49 430	54 600	50 580	54 540	42 500	47 900	48 850	55 060	49 710	55 120	1.011	76 620	0.719
23	"					22 500	23 700	51 720	54 480			44 000	48 000	50 570	55 170					
24	"	0.21	0.63	0.752	0.444	22 500	23 070	50 680	51 960	50 680	51 600	45 000	47 050	50 680	52 980	50 680	52 900	1.025	73 530	0.719
24	"					22 500	22 750	50 680	51 240			45 000	46 900	50 680	52 820					
25	"	0.21	0.65	0.743	0.434	22 000	22 800	50 690	52 540	50 120	52 300	44 000	45 680	50 690	52 630	51 560	53 130	1.016	74 810	0.710
25	"					21 500	22 600	49 540	52 070			45 000	46 550	52 420	53 630					
26	"	0.21	0.78	0.744	0.435	22 000	24 940	50 570	57 330	50 570	57 140	43 500	50 000	50 000	57 470	50 290	56 180	0.983	79 310	0.708
26	"					22 000	24 770	50 570	56 940			44 500	47 750	50 570	54 880					
27	"	0.21	0.84	0.740	0.430	21 500	22 800	50 000	53 020	50 000	53 670	44 500	48 000	51 740	55 810	51 740	56 560	1.054	78 020	0.725
27	"					21 500	23 360	50 000	54 320			44 500	49 280	51 740	57 300					
28	"	0.21	....	0.741	0.431	22 000	23 100	51 040	53 600	51 040	52 440	46 600	48 030	53 360	55 720	52 780	55 210	1.053	76 850	0.718
28	"					22 000	22 100	51 040	51 280			45 000	47 150	52 200	54 700					
29	"	0.22	0.58	0.743	0.434	22 500	24 650	51 840	56 800	52 420	56 800	45 000	50 000	51 840	54 150	53 000	55 880	......	80 230	
29	"					23 000	24 650	53 000	56 800			47 000	50 000	54 160	57 600					
30	"	0.22	0.65	0.747	0.438	21 500	24 330	49 090	55 550	49 090	55 000	43 000	48 900	49 090	55 820	49 660	55 680	1.012	76 290	0.730
30	"					21 500	23 850	49 090	54 450			44 000	48 650	50 230	55 540					
31	"	0.22	0.77	0.736	0.425	23 000	23 740	54 120	55 860	54 120	55 810	46 500	49 380	54 710	58 090	55 000	57 900	1.037	80 820	0.716
31	"					23 000	23 700	54 120	55 760			47 000	49 050	55 290	57 710					
32	"	0.22	0.78	0.742	0.432	22 000	22 930	50 930	53 080	51 800	53 270	43 000	46 140	49 770	53 400	50 640	53 880	1.011	75 350	0.715
32	"					22 750	23 090	52 660	53 450			44 500	46 970	51 500	54 360					
33	"	0.24	0.82	0.742	0.432	20 000	24 870	46 300	57 570	46 880	57 460	38 500	50 000	44 560	57 870	47 460	57 760	......	81 860	
33	"					20 500	24 770	47 450	57 340			43 500	49 800	50 350	57 640					
34	"	0.25	0.75	0.744	0.435	22 500	25 160	51 720	57 840	51 720	57 700	43 000	50 000	49 430	57 480	50 580	57 470	......	81 800	
34	"					22 500	25 040	51 740	57 560			45 000	50 000	51 720	57 420					
35	"	0.29	0.81	0.736	0.425	20 000	25 370	51 760	59 690	53 820	59 690	39 000	50 000	45 880	58 720	49 410	58 820	......	86 100	
35	"					23 750	25 370	55 880	59 690			45 000	50 000	52 940	58 820					
36	"	0.31	0.68	0.732	0.421	22 500	23 060	53 440	54 780	53 440	54 810	44 000	47 240	52 260	56 100	53 150	55 750	1.017	78 430	0.711
36	"					22 500	23 090	53 440	54 840			45 500	46 650	54 040	55 400					
37	Compression	0.21	1.03	0.741	0.431	24 000	24 500	55 680	56 840	54 520	57 080	44 500	49 890	51 620	57 880	52 200	57 730	1.011	81 580	0.708
37	"					23 000	24 500	53 360	57 310			45 500	49 630	52 780	57 580					
38	"	0.31	0.78	0.733	0.422	22 500	24 750	53 320	58 410	53 910	58 300	45 500	49 310	52 720	58 420	53 610	58 580	1.005	80 500	0.728
38	"					23 000	24 560	54 500	58 200			46 000	49 570	54 500	58 730					
39	"	0.31	....	0.739	0.429	23 500	24 420	54 780	56 920	54 200	56 900	45 500	50 000	53 030	58 270	54 200	58 270	......	83 390	
39	"					23 000	24 400	53 610	56 880			47 500	50 000	55 360	58 270					
40	"	0.32	0.79	0.741	0.431	23 000	23 800	53 350	55 220	52 780	55 230	43 000	48 230	49 880	55 950	48 720	55 620	1.007	76 980	0.723
40	"					22 500	23 810	52 200	55 240			41 000	47 650	47 560	55 280					
41	"	0.32	0.85	0.734	0.423	22 500	24 540	53 190	58 010	53 780	57 900	45 000	50 000	53 190	59 100	54 080	58 750	1.015	82 820	0.709
41	"					23 000	24 440	54 370	57 780			46 500	49 400	54 960	58 390					
42	"	0.32	....	0.760	0.454	24 500	28 210	53 960	62 140	55 070	62 240	......	......	......	......	......	......	......	92 470	
42	"					25 500	28 300	56 170	62 340											
43	"	0.33	0.91	0.743	0.434	22 000	25 700	50 690	59 220	51 270	58 960	43 000	51 000	49 540	58 760	51 270	58 760	......	84 380	
43	"					22 500	25 470	51 810	58 690			46 000	51 000	53 000	58 760					
44	"	0.34	0.60	0.744	0.435	22 250	25 250	51 150	58 050	48 570	57 800	46 500	50 000	53 450	57 470	52 590	57 470	......	82 760	
44	"					20 000	25 030	45 980	57 540			45 000	50 000	51 720	57 470					
45	"	0.36	0.89	0.736	0.425	23 500	26 000	55 290	61 170	55 880	61 000	46 000	51 000	54 120	60 000	55 890	60 000	......	88 540	
45	"					24 000	25 850	56 470	60 820			49 000	51 000	57 650	60 000					
46	"	0.37	0.93	0.738	0.428	24 500	25 950	57 240	60 630	57 240	60 690	46 000	......	53 740	......	54 030	......	......	87 210	
46	"					24 500	26 000	57 240	60 750			46 500	51 000	54 320	59 580					
47	"	0.38	0.89	0.735	0.424	23 000	27 250	54 940	64 270	55 420	64 220	49 000	......	57 780	......	......	......	......	93 750	
47	"					24 000	27 210	56 600	64 170											
48	"	0.39	....	0.740	0.430	23 500	27 550	54 650	64 070	54 650	64 100	49 000	......	56 980	......	......	......	......	92 070	
48	"					23 500	27 570	54 650	64 120											
49	"	0.40	0.87	0.723	0.411	23 500	25 900	57 180	63 020	56 570	62 230	45 500	50 800	55 350	61 800	55 660	61 930	......	87 290	
49	"					23 000	25 250	55 960	61 440			46 000	51 000	55 960	62 050					
50	"	....	....	0.760	0.454	23 000	27 100	50 660	59 690	50 660	59 580	49 000	......	53 970	......	......	......	......	85 680	
50	"					23 000	27 000	50 660	59 470											

Number.	Kind.	Per cent. of carbon.	Mean diameter. Inch.	HEAR. PER SQUARE INCH. Ultimate resistance. Pounds.	Mean elastic limit for each bar. Pounds.	Mean ultimate resistance for each bar. Pounds.	Ratio of ultimate resistance per square inch in double to that in single shear.	Ultimate tensile resistance per square inch. Pounds.	Ratio of ultimate resistance per square inch in double shear to that in tension.
51	Rivet	About 0.09	0.757	46 390					
51	"	"		46 890	43 610	46 460	1.022	63 230	0.735
51	"	"		46 110					
52	"	"	0.757	47 450					
52	"	"		47 450	38 240	47 450	1.048	62 670	0.757
52	"	"		47 450					
53	"	"	0.759	47 290					
53	"	"		47 420	33 810	47 590	1.034	61 270	0.777
53	"	"		48 060					
54	"	"	0.757	48 720					
54	"	"		48 110	33 520	48 390	1.032	62 050	0.780
54	"	"		48 330					
55	"	"	0.755	46 610					
55	"	"		46 570	34 040	46 590	1.040	61 800	0.754
55	"	"		46 600					
56	"	"	0.757	47 000					
56	"	"		47 500	38 520	47 350	1.062	61 630	0.768
56	"	"		47 560					
57	"	"	0.756	49 000					
57	"	"		48 780	39 350	48 890	1.053	62 600	0.781
57	"	"		48 890					
58	"	"	0.755	47 150					
58	"	"		47 490	35 720	47 210	1.045	61 870	0.763
58	"	"		46 990					
60	"	"	0.756	47 330					
60	"	"		47 330	40 740	47 210	1.024	60 900	0.775
60	"	"		46 980					
78	"Burden's	....	0.757	42 110					
78	Best"			42 170	39 240	41 960	1.035	51 400	0.816
78	wrought-iron.			41 610					
78				41 940					

TABLE No. 12.

Shearing Resistance of Round Bars of Open-Hearth Steel.

Number.	Kind.	Per cent. of carbon.	Mean diameter. Inch.	Area. Square inch.	Single shear.						Double shear.						Ratio of ultimate resistance per square inch in double to that in single shear.	Ultimate tensile resistance per square inch. Pounds.	Ratio of ultimate resistance per square inch in double shear to that in tension.
					Of the specimen.		Per square inch.				Of the specimen.		Per square inch.						
					Elastic limit. Pounds.	Ultimate resistance. Pounds.	Elastic limit. Pounds.	Ultimate resistance. Pounds.	Mean elastic limit for each bar. Pounds.	Mean ultimate resistance for each bar. Pounds.	Elastic limit. Pounds.	Ultimate resistance. Pounds.	Elastic limit. Pounds.	Ultimate resistance. Pounds.	Mean elastic limit for each bar. Pounds.	Mean ultimate resistance for each bar. Pounds.			
51	Rivet	About 0.09	0.757	0.450	18 300	20 500	40 660	45 560			41 000	41 750	45 560	46 390					
51	"	"			17 500	20 690	38 890	45 980	39 630	45 440	40 500	42 200	45 000	46 899	43 610	46 460	1.022	63 230	0.735
51	"	"			17 700	20 150	39 330	44 780			36 250	41 500	40 280	46 110					
52	"	"	0.757	0.450	15 500	20 250	34 450	45 000			33 750	42 700	37 500	47 450					
52	"	"			15 250	20 350	33 890	45 220	34 630	45 260	35 000	42 700	38 890	47 450	38 240	47 450	1.048	62 670	0.757
52	"	"			16 000	20 500	35 560	45 560			34 500	42 700	38 330	47 450					
53	"	"	0.759	0.452	14 000	20 650	30 970	45 690			29 750	42 750	32 910	47 290					
53	"	"			14 250	20 900	31 530	46 240	31 530	46 020	30 500	42 870	33 740	47 420	33 810	47 590	1.034	61 270	0.777
53	"	"			14 500	20 850	32 080	46 130			31 500	43 450	34 850	48 060					
54	"	"	0.757	0.450	14 250	21 050	31 670	46 780			29 500	43 850	32 780	48 720					
54	"	"			14 250	21 350	31 670	46 440	31 670	46 910	30 500	43 300	33 890	48 110	33 520	48 390	1.032	62 050	0.780
54	"	"			14 250	20 930	31 670	46 510			30 500	43 500	33 890	48 330					
55	"	"	0.755	0.448	13 500	20 000	30 130	44 640			29 750	41 760	33 200	46 610					
55	"	"			14 250	20 110	31 810	44 890	31 060	44 780	30 500	41 730	34 040	46 570	34 040	46 590	1.040	61 800	0.754
55	"	"			14 000	20 080	31 250	44 820			31 250	41 750	34 880	46 600					
56	"	"	0.757	0.450	16 500	20 200	36 670	44 890			34 000	42 300	37 780	47 000					
56	"	"			16 500	20 000	36 670	44 450	35 930	44 600	34 500	42 750	38 330	47 500	38 520	47 350	1.062	61 630	0.768
56	"	"			15 500	20 000	34 450	44 450			35 500	42 800	39 450	47 560					
57	"	"	0.756	0.449	15 000	20 600	33 410	45 880			36 000	44 000	40 090	49 000					
57	"	"			15 000	20 850	33 410	46 440	33 780	46 440	35 000	43 800	38 980	48 780	39 350	48 890	1.053	62 600	0.781
57	"	"			15 500	21 100	34 520	46 990			35 000	43 900	38 980	48 890					
58	"	"	0.755	0.448	15 250	20 490	34 040	45 740			31 500	42 250	35 160	47 150					
58	"	"			15 000	20 250	33 480	45 200	33 670	45 190	33 000	42 550	36 830	47 490	35 720	47 210	1.045	61 870	0.763
58	"	"			15 000	20 000	33 480	44 640			31 500	42 100	35 160	46 990					
60	"	"	0.756	0.449	17 000	21 600	37 860	48 110			36 000	42 500	40 090	47 330					
60	"	"			15 500	20 100	34 520	44 770	35 820	46 100	36 500	42 500	40 650	47 330	40 740	47 210	1.024	60 900	0.775
60	"	"			15 750	20 400	35 080	45 430			37 250	42 190	41 480	46 980					
78	"Burden's	....	0.757	0.450	15 750	18 300	35 000	40 670			35 000	37 900	38 890	42 110					
78	Best"				16 750	18 400	37 220	40 890	36 110	40 530	35 750	37 950	39 720	42 170	39 240	41 960	1.035	51 400	0.816
78	wrought-iron.				16 000	17 700	35 560	39 330			35 000	37 450	38 890	41 610					
78					16 500	18 550	36 670	41 220			35 500	37 750	39 440	41 940					

TABLE No. 13.

Shearing Resistance of Round Bars of Bessemer Steel.

Number.	Per cent. of carbon.	Mean diameter. Inch.	Area. Square inch.	Single shear. Specimen. Ultimate resistance. Pounds.	Single shear. Per square inch. Ultimate resistance. Pounds.	Single shear. Per square inch. Mean ultimate resistance for each bar. Pounds.	Double shear. Specimen. Ultimate resistance. Pounds.	Double shear. Per square inch. Ultimate resistance. Pounds.	Double shear. Per square inch. Mean ultimate resistance for each bar. Pounds.	Ratio of ultimate resistance per square inch in double to that in single shear.	Ultimate tensile resistance per square inch. Pounds.	Ratio of ultimate resistance per square inch in double shear to that in tension.
61	0.11	0.751	0.443	21 500	48 530		44 950	50 740				
61	0.11			21 650	48 870	49 210	45 200	51 020	51 000	1.036	66 270	0.770
61	0.11			22 250	50 230		45 400	51 240				
62	0.12	0.751	0.443	23 100	52 140		46 000	51 920				
62	0.12			23 050	52 030	51 470	45 500	51 350	51 470	1.000	65 410	0.787
62	0.12			22 250	50 230		45 300	51 130				
63	0.12	0.749	0.441	21 850	49 550		43 800	49 660				
63	0.12			21 950	49 770	49 740	45 380	51 450	50 940	1.024	65 620	0.776
63	0.12			22 000	49 890		45 500	51 700				
64	0.12	0.752	0.444	21 700	48 900		45 000	50 680				
64	0 12			22 300	50 200	49 470	44 850	50 500	50 540	1.022	64 210	0.788
64	0.12			21 900	49 300		44 800	50 450				
66	0.13	0.758	0.451	22 500	49 890		46 300	51 330				
66	0.13			23 400	51 880	51 000	46 600	51 660	51 510	1.010	65 770	0.783
66	0.13			23 100	51 220		46 500	51 550				
67	0.15	0.747	0.438	21 900	50 000		45 100	51 480				
67	0.15			22 200	50 690	50 500	44 850	51 200	51 260	1.011	66 940	0.766
67	0.15			22 250	50 800		44 800	51 100				
68	0.16	0.753	0.445	22 900	51 460		47 100	52 920				
68	0 16			23 100	51 910	52 280	46 800	52 580	52 840	1.011	68 250	0.774
68	0.16			23 800	53 480		47 200	53 030				
69	0.16	0.749	0.441	22 600	51 250		46 600	52 830				

TABLE No. 13.—*Continued.*

Number.	Per cent. of carbon.	Mean diameter. Inch.	Area. Square inch.	Single shear. Specimen. Ultimate resistance. Pounds.	Single shear. Per square inch. Ultimate resistance. Pounds.	Single shear. Per square inch. Mean ultimate resistance for each bar. Pounds.	Double shear. Specimen. Ultimate resistance. Pounds.	Double shear. Per square inch. Ultimate resistance. Pounds.	Double shear. Per square inch. Mean ultimate resistance for each bar. Pounds.	Ratio of ultimate resistance per square inch in double to that in single shear.	Ultimate tensile resistance per square inch. Pounds.	Ratio of ultimate resistance per square inch in double shear to that in tension.
69	0.16			22 700	51 470	51 280	46 300	52 490	52 550	1.025	68 440	0.768
69	0.16			22 550	51 130		46 150	52 320				
70	0.17	0.747	0.438	23 480	53 610		46 650	53 250				
70	0.17			23 400	53 420	53 260	47 200	53 880	53 390	1.002	70 140	0.761
70	0.17			23 100	52 740		46 450	53 030				
71	0.17	0.748	0.439	22 400	51 020		47 000	53 530				
71	0.17			23 300	53 080	52 010	46 800	53 300	53 380	1.026	71 880	0.743
71	0.17			22 800	51 930		46 800	53 330				
72	0.20	0.752	0.444	22 600	50 090		47 000	52 930				
72	0.20			23 200	52 250	51 530	47 000	52 930	52 870	1.026	70 510	0.750
72	0.20			23 200	52 250		46 850	52 760				
73	0.22	0.757	0.450	24 900	55 330		50 000	55 550				
73	0.22			25 400	56 440	56 150	50 000	55 550	55 550	......	76 750	
73	0.22			25 500	56 670		50 000	55 550				
74	0.23	0.756	0.449	23 900	53 230		49 500	55 120				
74	0.23			23 800	53 120	53 270	49 900	55 570	55 440	1.041	72 740	0.762
74	0.23			24 000	53 450		49 950	55 620				
75	0.24	0.756	0.449	24 800	55 230		50 000	55 680				
75	0.24			25 200	56 120	55 450	50 000	55 680	55 600	1.003	76 110	0.731
75	0.24			24 700	55 010		49 800	55 460				
76	0.36	0.758	0.451	31 850	70 620							
76	0.36			31 870	70 670	70 190	......	......	......	......	98 710	
76	0.36			31 250	69 290							
77	0.39	0.762	0.456	30 800	67 540							
77	0.39			30 930	67 830	67 760	......	......	......	......	95 720	
77	0.39			30 970	67 920							

P AND ROUNDED.

Double shear.				Ratio of ultimate resistance in			
Per square inch.							
Elastic limit. Pounds.	Ultimate resistance. Pounds.	Mean elastic limit.	Mean ultimate resistance.	Double to that in single shear.	Single shear with round edges to that with sharp edges. Half circle.	Double shear with round edges to that with sharp edges. Half circle.	Double shear with round edges to that in tension. Half circle.
5 080	46 510						
2 780	48 720	33 910	47 920	1.024	1.153	1.109	0.857
3 890	48 110						
3 890	48 330						
3 200	46 610						
4 040	46 570	34 040	46 590	1.040	1.236	1.141	0.860
4 880	46 600						
7 250	50 740						
8 960	51 020	37 820	50 510	1.027	1.175	1.123	0.856
9 500	51 240						
5 560	49 040						
....	......	......	......	......	1.148		
890	42 110						
720	42 170	39 240	41 960	1.035	1.142	1.101	0.899
890	41 610						
440	41 940						
330	52 970	33 890	53 150	0.985			
440	53 330						
770	53 370	38 770	53 150	0.960			
770	52 920						
590	56 720	37 590	56 720	0.982			
590	56 720						
740	45 760	34 570	46 220	0.998			
400	46 680						
040	53 070	35 160	53 140	0.954			
270	53 200						
940	52 820	38 940	52 650	0.955			
940	52 480						
010	56 810	37 300	56 820	0.975			
590	56 830						
720	47 120	39 270	46 790	0.988			
820	46 460						
370	50 840	31 810	50 650				
250	50 450						
070	49 600	34 500	49 520				
940	49 430						
190	42 700						

TABLE No. 14.

Comparison of Shearing Resistances of Steel when the Edges of the Shearing Block are Sharp and Rounded.

Number.	Method of shear.	Kind of metal.	Carbon.	Diameter in inch.	Area. Square inch.	Single shear.						Double shear.						Ratio of ultimate resistance in			
						Specimen.		Per square inch.				Specimen.		Per square inch.							
						Elastic limit. Pounds.	Ultimate resistance. Pounds.	Elastic limit. Pounds.	Ultimate resistance. Pounds.	Mean elastic limit.	Mean ultimate resistance.	Elastic limit. Pounds.	Ultimate resistance. Pounds.	Elastic limit. Pounds.	Ultimate resistance. Pounds.	Mean elastic limit.	Mean ultimate resistance.	Double to that in single shear.	Single shear with round edges to that with sharp edges. Half circle.	Double shear with round edges to that with sharp edges. Half circle.	Double shear with round edges to that in tension. Half circle.
54	All edges sharp. Half circle of shearing	Open-Hearth..........	About 0.09	0.756	0.449	14 500	20 750	32 290	46 430			31 500	41 770	35 080	46 510						
	block used, as at *G* in the Figure.			0.757	0.450	14 250	21 050	31 670	46 780	31 830	46 790	29 500	43 850	32 780	48 720	33 910	47 920	1.024	1.153	1.109	0.857
				0.757	0.450	14 250	21 350	31 670	47 440			30 500	43 300	33 890	48 110						
				0.757	0.450	14 250	20 930	31 670	46 510			30 500	43 500	33 890	48 330						
55	do do	"	"	0.755	0.448	13 500	20 000	30 130	44 640			29 750	41 760	33 200	46 610						
				0.755	0.448	14 250	20 110	31 810	44 890	31 060	44 780	30 500	41 730	34 040	46 570	34 040	46 590	1.040	1.236	1.141	0.860
				0.755	0.448	14 000	20 080	31 250	44 820			31 250	41 750	34 880	46 600						
61	do do	Bessemer.............	0.11	0.751	0.443	13 000	21 500	29 350	48 530			33 000	44 950	37 250	50 740						
				0.751	0.443	13 250	21 650	29 910	48 870	30 830	49 170	34 500	45 200	38 960	51 020	37 820	50 510	1.027	1.175	1.123	0.856
				0.751	0.443	14 250	22 250	32 170	50 230			35 000	45 400	39 500	51 240						
				0.748	0.439	14 000	21 530	31 890	49 040			31 500	43 450	35 560	49 040						
76	do do	"	0.36	0.758	0.451	23 000	31 850	51 000	70 620												
						24 000	31 870	53 210	70 670	52 470	70 190	......	......	......	......	......	......	......	1.148		
						24 000	31 250	53 210	69 290												
78	do do	"Burden's Best" iron	....	0.757	0.450	15 750	18 300	35 000	40 670			35 000	37 900	38 890	42 110						
				0.757	0.450	16 750	18 400	37 220	40 890	36 110	40 530	35 750	37 950	39 720	42 170	39 240	41 960	1.035	1.142	1.101	0.899
				0.757	0.450	16 000	17 700	35 560	39 330			35 000	37 450	38 890	41 610						
				0.757	0.450	16 500	18 550	36 670	41 220			35 500	37 750	39 440	41 940						
54	All edges rounded. Half circle of shear-	Open-Hearth..........	About 0.09	0.757	0.450	15 000	24 100	33 330	53 560	33 330	53 950	30 000	47 670	33 330	52 970	33 890	53 150	0.985			
	ing block used, as at *Y* in the Figure.					15 000	24 450	33 330	54 330			31 000	48 000	34 440	53 330						
55	do do	"	"	0.753	0.445	15 000	24 600	33 710	55 280	33 150	55 340	34 500	47 500	38 770	53 370	38 770	53 150	0.960			
						14 500	24 650	32 580	55 390			34 500	47 100	38 770	52 920						
61	do do	Bessemer	0.11	0.748	0.439	14 750	25 300	33 600	57 630	35 030	57 780	33 000	49 800	37 590	56 720	37 590	56 720	0.982			
						16 000	25 430	36 450	57 930			33 000	49 800	37 590	56 720						
76	do do	"	0.36	0.758	0.451	21 000	32 500	46 770	72 380												
						22 500	39 500	50 110	87 970	49 370	80 550										
						23 000	36 500	51 220	81 290												
78	do do	"Burden's Best" iron	....	0.759	0.452	13 500	20 980	29 870	46 420	29 320	46 300	30 500	41 370	33 740	45 760	34 570	46 220	0.998			
						13 000	20 870	28 760	46 170			32 000	42 200	35 400	46 680						
54	All edges rounded. Full circle of shear-	Open-Hearth..........	About 0.09	0.755	0.448	15 000	25 000	33 480	55 800	34 040	55 690	30 500	47 550	34 040	53 070	35 160	53 140	0.954			
	ing block used, as at *X* in the Figure.					15 000	24 900	34 600	55 580			32 500	47 670	36 270	53 200						
55	do do	"	"	0.751	0.443	13 500	24 600	30 470	55 530	31 600	55 140	34 500	46 800	38 940	52 820	38 940	52 650	0.955			
						14 500	24 250	32 730	54 740			34 500	46 500	38 940	52 480						
61	do do	Bessemer.............	0.11	0.748	0.439	16 500	25 550	37 590	58 200	38 160	58 260	32 500	49 880	37 010	56 810	37 300	56 820	0.975			
						17 000	25 600	38 720	58 310			33 000	49 900	37 590	56 830						
76	do do	"	0.36	0.758	0.451	23 000	35 400	51 220	78 840	51 780	78 120										
						23 500	34 750	52 340	77 390												
78	....	"Burden's Best" iron	....	0.759	0.452	15 250	21 680	33 740	47 970	34 020	47 830	35 000	42 600	38 720	47 120	39 270	46 790	0.988			
						15 500	21 550	34 290	47 680			36 000	42 000	39 820	46 460						
54	All edges rounded. } Span, 2⅛ inches...	Open-Hearth	About 0.09	......	......	......	......	......	......	......	......	29 000	45 550	32 370	50 840	31 810	50 650				
	Full circle used. } " 2¼ "											28 000	45 200	31 250	50 450						
55	do " 2⅛ "	"	"	......	......	......	......	......	......	......	......	31 000	43 850	35 070	49 600	34 500	49 520				
	do " 2¼ "											30 000	43 700	33 940	49 430						
78	....	"Burden's Best" iron	....	......	......	......	......	......	......	......	......	30 000	38 600	33 190	42 700						

AMERICAN SOCIETY OF CIVIL ENGINEERS.
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TRANSACTIONS.

357.
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STOPPAGE OF FLOW IN A WATER MAIN BY ANCHOR ICE.

By James B. Francis, Past President Am. Soc. C. E.
Read December 15th, 1886.

WITH DISCUSSION.

Carleton, a district of St. John, New Brunswick, is supplied with water from Spruce Lake by means of a cement lined sheet-iron pipe 12 inches in diameter and about seven miles long.

This pipe discharges into a distributing reservoir in the outskirts of the town, which has a depth of water, when full, of 18 feet, and a capacity of about one and three-fourth million imperial gallons. This pipe was discharging to its full capacity, or at the rate of about three hundred thousand gallons in twenty-four hours, with the water in the reservoir drawn down about ten feet, leaving a depth of water and ice of about eight feet.

A plan and section of the reservoir, showing also the inlet and outlet pipes, is given on Plate XVII.

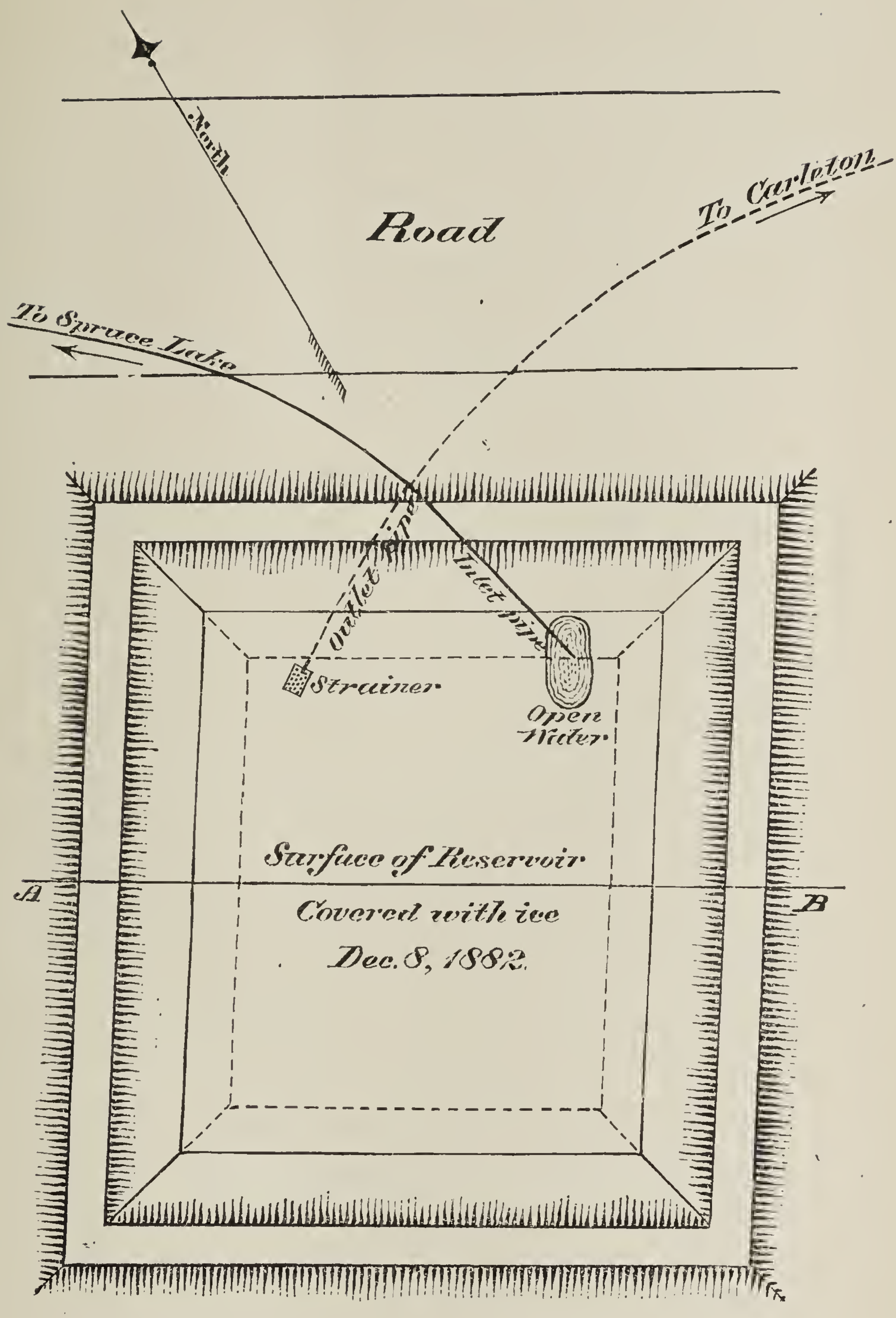
On the evening of December 8th, 1882, the supply of water to the district suddenly ceased, and so continued for a short time until other connections were opened. On the following morning a hole was cut in the ice, which was about six inches thick, immediately over the outlet pipe, where a mass of ice was found, which is described as a kind of slush or minute particles of congealed water; on prying upon it, it floated up, and the bottom of it was exactly the shape of the strainer of the outlet pipe, showing that it rested upon it and completely closed up the outlet. The strainer is a copper rose perforated with holes about one-fourth of an inch in diameter, the marks of which appeared upon the ice. The whole column when taken out, or as much of it as would hold together, was about the size of a barrel, and was composed of minute particles of ice, all standing on end, firmly adhering to each other. On removing this ice the water commenced to flow to the town as usual.

The temperature of the air had fallen suddenly from 43 degrees above zero at 8 P.M. of the 7th to 10 degrees above at 8 A.M. of the 8th, with a mean for the day of 12.3 degrees. The sky was clear and had been so for the greater part of the day, and the wind was blowing a strong gusty breeze (twenty to thirty miles an hour) from the northwest.

Spruce Lake was entirely frozen over, and the reservoir was covered with ice, except over the inlet pipe, where there was an area of two or three hundred square feet clear of ice.

The temperature of the water in Spruce Lake, reservoir or mains, was not observed at the time, but from observations at other times it is stated that the usual winter temperature of water drawn from the mains is about 35 degrees.

The City of St. John (East) is supplied with water in a similar manner to Carleton (or St. John West), but from Little River Reservoir, by means of three mains, one of 12 inches and two of 24 inches diameter, and about five miles long. The reservoir has an area of 37.5 acres, and was completely frozen over from November 27th, 1882, to May 5th, 1883, and from December 1st, 1883, to April 22d, 1884. Records are kept of the temperatures of the air and water at the gate-house at Little River Reservoir and as drawn from the mains in the city, and are given in the annual reports of the Superintendent of the Water-works. I find in these reports the data for the temperatures in the following table, which are the means for the respective months in the years 1882, 1883 and 1884:



SECTION THROUGH AB.

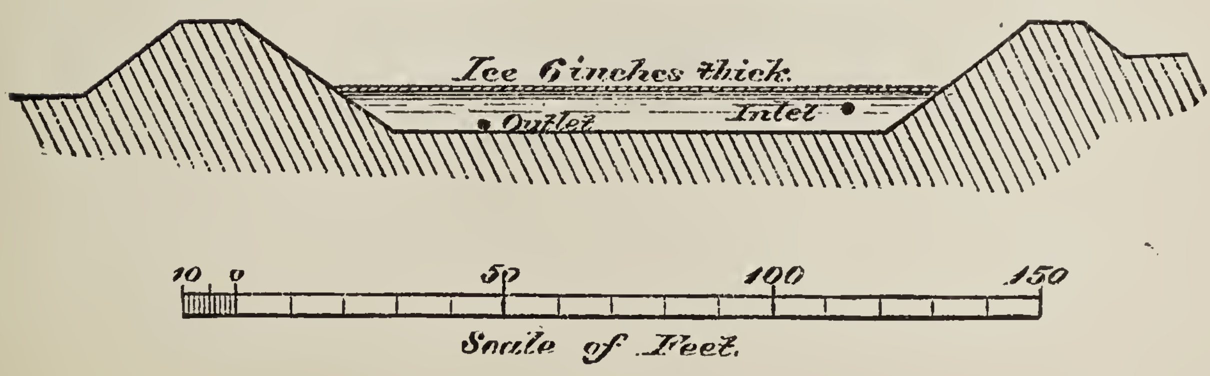


PLATE XVII.

	December.	January.	February.	March.
	Degrees.	Degrees.	Degrees.	Degrees.
Mean temperature of the water in the gate-house 2 feet below the surface	34.34	34.10	33.90	33.84
Mean temperature of the water as drawn from the mains in the city .	36.28	35.28	34.47	34.13
Difference........	1 94	1.18	0.57	0.29
Mean temperature of the air for the three years.	22.79	15.80	20.42	28.92
Mean temperature of the air for twenty-four years, 1861 to 1884....	22.02	18.13	21.11	27.35

In the absence of similar observations for the Carleton Water-works, the localities and arrangements being so nearly alike, I think we may safely infer from the above table that the temperature of the water entering the Carleton Reservoir December 8th, 1882, when the stoppage occurred, was not below 36 degrees Fahr., or 4 degrees above the freezing point.

The discharge of the inlet pipe being 300 000 imperial gallons in twenty-four hours, the mean velocity of the water entering the reservoir was about 0.71 feet per second, and the center of the pipe was about 3.5 feet below the surface of the water. This must have produced an agitation in the part of the reservoir near the inlet pipe in the form of eddies in various directions, some of them extending to the surface, and the temperature of the entering water being above the freezing point, the result was the open water of two or three hundred square feet over and near the inlet pipe.

Anchor ice, according to my observation, is originally formed on the surface of water which is agitated, either by a current or the wind; it is formed in minute particles or needles, which, as they differ little from the specific gravity of the water, are carried by currents or eddies in any direction, and when they come in contact, either with one another or with other substances of the same temperature, freeze together, or, as described by Faraday, who appears to have been the first to observe the phenomenon, but in larger masses of ice, *regelate.* According to this theory, the ice which closed the strainer formed on the open water over the inlet pipe, was carried under the ice by eddies and currents, and continued in motion until it reached the strainer.

The quantity of water in the reservoir when drawn down as above stated, was about five hundred and sixty thousand imperial gallons, and as the daily consumption of water was about three hundred thousand gallons, the entire contents must have been changed, on the average, every 44.8 hours. The mean temperature of the day immediately preceding the stoppage having been 12.3 degrees, the mean temperature of the water in the reservoir must have been reduced somewhat, but to what extent I have no means of estimating. The water in immediate contact with the ice would of course be reduced to 32 degrees, and the ice would be increasing in thickness. The great mass of the water, not being near the influent or effluent pipes, would be very quiet. The water below the stratum in contact with the surface ice would remain at the temperature at which it entered the reservoir, which we find must have been near 36 degrees, unless cooled from the surface; this could take place only to a very small extent, except by means of a vertical circulation in the mass of water, but as the specific gravity of water at the temperature of 32 degrees is less than at the temperature of 36 degrees, or at any temperature between that temperature and 32 degrees, there could be no such tendency. From these considerations I infer that the temperature of the great mass of the water in the reservoir did not get reduced to the freezing point, and that the particles of ice should not thaw, they must have floated very near the surface ice, and so continued until they reached a point over the strainer on the outlet pipe, arriving there with the water from all directions, and accumulating in a mass, which gradually extended downwards until it reached the strainer. The constitution of the mass of ice which was found on the strainer, as described above, does not appear to be inconsistent with this explanation of its mode of formation.

The reservoir has been in use since its construction in 1874, and although it has frequently been as low in as cold weather, this has been the only occasion when the strainer has been obstructed by ice. What the peculiar conditions were to cause the trouble at this time, and not at others, do not appear to be known, but I think it probable that there was much less open water, if any, on the other occasions.

The information on which this paper is based was communicated to the writer by Mr. George F. Harding, Chairman of the Board of Water Commissioners for Carleton, and Mr. Gilbert Murdoch, M. Am. Soc. C. E., Superintendent of the Water-works and Sewerage of St. John, N. B.

DISCUSSION.

Charles B. Brush, M. Am. Soc. C. E.—I had an experience very similar to that just described on the 2d of January, 1885. The circumstances were almost identical. There was a rapid fall of temperature during that day, from 27 degrees to 11 degrees Fahr. At 20 o'clock needle ice appeared in the reservoir of the Hackensack Water Company, reorganized, at Weehawken, N. J. It first appeared on the surface, and was then drawn down into the wet well of the gate-house through the outlet pipe of the reservoir. The capacity of the reservoir is 15 000 000 gallons, and on the above date the surface of the water was 10 feet above the bottom of the 20-inch outlet. The water was flowing into the reservoir at the rate of about three feet per second, and out at about three and one-half feet per second. The needle ice, on reaching the wet well of the reservoir, commenced immediately to form a thin coating against the screens, thus effectually shutting off the flow of water through them in a very short time. We immediately opened the by-pass, and set men at work with long poles breaking the ice on the screens, and stirring it up in the well. The whole force of the current in the by-pass was then thrown directly on the reverse side of the screens, and after working a couple of hours the trouble ceased. The needle ice was simply driven back into the reservoir and disappeared.

B. S. Church, M. Am. Soc. C. E.—I will mention an experience with anchor ice on a somewhat larger scale which occurred at the entrance of the conduit which supplies water to the City of New York. On the 29th of December, 1872, I received a notice (as engineer in charge of the Croton Aqueduct) from one of my keepers, that the depth of water in the conduit at Sing Sing was rapidly decreasing. Suspecting the cause, I drove at once to Croton Dam and found anchor ice forming in Croton Lake around the mouth of and choking the flow of water in the aqueduct.

The usual depth of seven feet was reduced to five feet and finally to four, by 10.30 A. M.

The thermometer at 7 A.M. stood at 0 degrees Fahr. and at 2 P.M. at + 25 degrees Fahr. in the open air.

The water in the aqueduct 400 feet from its mouth, and from where anchor ice had formed, was at a temperature of + 33½ degrees Fahr.

The mouth of the aqueduct was of the horse-shoe form, 7½ feet wide and 10 feet 8 inches high in the clear. The anchor ice took the form of a dome twelve or thirteen feet in diameter resting against the vertical masonry face of the aqueduct entrance, and three or four feet below the surface at the upper end of its vertical diameter.

A hot-water pipe-coil and small boiler used for another purpose were fortunately on hand and available. This coil was lowered into the body of the mass of spongy ice and heat applied. Then by the aid of poles and planks the ice was forced through the conduit by five men, after about three hours' labor, which restored again the full flow of water in the aqueduct.

Theodore Cooper, M. Am. Soc. C. E.—I have no special information to add to the subject of anchor ice. But in this connection it may be interesting to state an anomalous experience in the use of a mixture of glycerine and water in hydraulic machinery exposed to the low temperatures of this climate.

It has been generally accepted that a mixture of about four parts of glycerine to six of water would not freeze when subject to our usual winter temperatures.

It is also an accepted belief that under similar conditions the freezing and melting temperature of liquids is identical.

During the winter of 1885–86 some hydraulic machinery put up by me worked satisfactorily throughout the colder days of the winter, using the above proportions of glycerine and water, without freezing. But in the later part of the winter, during much milder weather, the machinery was disabled by freezing of the pipes. The assistant in charge of the work having been troubled this way several times without discovering the cause, finally tried the solution with a hydrometer and found the top of the liquid in the tank to be nearly pure water. He found on emptying the tank a congealed mass, about the consistency of ice-cream, at the bottom. He stated that though at this time the temperature of the air had been above the freezing point for some days, the temperature of this congealed mass was about zero Fahr.

This appeared so anomalous that I doubted the exactness of his observations, until I found the following statement in regard to the peculiarities of glycerine:

"It is soluble in all parts of water; when exposed to air it gradually absorbs water; cooled *rapidly* it only becomes viscid without congealing even when a temperature of — 40 degrees C. (— 40 degrees Fahr.) is attained; but if kept for some time at a temperature of about 0 degrees C. (32 degrees Fahr.) it gradually forms hard deliquescent crystals which melt only at 22 degrees C. (72 degrees Fahr.). This fact is now used as a means of concentrating and purifying glycerine."

As the discussion on the formation of anchor ice tends to show special conditions of the atmosphere at the time, there may be some special case in the formation of this needle ice not heretofore well recognized.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
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TRANSACTIONS.

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358.

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DETERMINATION OF THE SIZE OF SEWERS.

By ROBERT E. McMATH, M. Am. Soc. C. E.

READ DECEMBER 15TH, 1886.

The quantity of water which may be expected to flow off from a given area, the time or rate of its discharge, and the size of conduit required to convey that quantity at the rate necessary to avoid disastrous flooding, are questions that often arise, and to which answers of some sort must be given by engineers.

Such answers are and must always be largely matters of judgment, based upon an estimate of local conditions and experience in similar cases.

That trained judgment may have the best guide possible under the circumstances, a method of ascertaining the results of experience free from personal bias or the influence of unproved theory is desirable. Such a method in practical form I aim to present in this paper. The method is of general application; the results indicated by the figures given are applicable only to the locality which furnishes the experience. The data I give in detail, in the hope that similar data from other localities may also be published, and so material for a better discussion be made available.

The particular problem whose solution I undertake, is the determination of the size of a storm-water sewer, under the conditions that it shall carry off the water of the great storms of the locality, and that no excess of size and cost be allowed.

The engineer usually has, as positive data, the area to be drained and the gradient of the proposed sewer; he also knows, or should know, approximately the rainfall in inches during storms; the intensity of the rainfall, or the rate of fall during the period of heaviest rain; the duration of such period of great intensity; and the condition of surface as to slopes and permeability.

The importance of all these factors has not always been recognized, and sewers have been built without their consideration. Some of the sewers already built have proven adequate, of which some may be unnecessarily large; others have proven insufficient. If now we can make a comparison of actual sewers and separate out those known to be insufficient, we may arrive at a standard below which we must not go, and, if the experience is extended, we obtain a standard of capacity beyond which we need not go. If this could be done with data drawn from every part of the world, the result might be a safe general rule. But, for the present at least, it is safer for each locality to seek its rule of proportioning sewers from its own experience or from that of places subject to like meteorological and surface conditions.

The City of St. Louis has had a sewer experience of about thirty-five years, during which her sewer system has gradually extended to

In 1861	31.5	miles.	
1865	38.6	"	
1870	111.1	"	
1875	163.4	"	
1880	196.4	"	
1885	232.1	"	
December, 1886	258	"	nearly.

Of the present mileage about thirty-five miles of sewers are taken into consideration in the following discussion, mostly larger than two feet by three. These include all the known cases of serious flooding.

The proportioning of sewers in St. Louis has from the beginning proceeded on the assumption that one inch of rainfall per hour would reach the sewers, or practically one cubic foot per second from each acre of drainage area.

In the earlier days no close proportioning seems to have been attempted, and in some important cases the area to be ultimately drained was not taken into consideration. Later the proportions have been determined by Weisbach's formula for discharge, assuming that the sewers were to run three-fourths full when discharging one cubic foot per second per acre. The formula, as adapted to computation by Robert Moore, M. Am. Soc. C. E., is $d = \left(\frac{A^2}{10.33\ s}\right)^{\frac{1}{5}}$

in which d = diameter of sewer in feet.

A = area drained in acres.

s = grade of sewer in feet per hundred.

The Weisbach formula gives a discharge for the larger sewers materially less than that by the now generally accepted Kutter formula. The arbitrary assumption of three-fourths full is also an under-estimate of the capacity of the sewers.*

In a paper published in the *Journal of the Association of Engineering Societies*, August, 1886, Robert Moore and Julius Baier show that c in Kutter's formula may, for practical use, be made independent of slope. Using this modification I compute the column headed "Moore b," of the following Table No. 1, the column headed "Moore a" being computed by the formula given above. Comparison of the formula is made by the ratio $\frac{\text{Moore } a}{\text{Moore } b}$, which ratio it will be noticed is not affected by slope. Using "Moore a" as abscissas and "Moore b" as ordinates, I have the broken line shown on "Diagram of volume reaching sewers;" but since in the use of Moore a in St. Louis the discharge of one cubic foot per acre per second has been assumed, I call the abscissas acres instead of cubic feet, hence the broken curve represents the former St. Louis rule of sewer capacity computed according to the newer and better formula. (Plate XVIII.)

As a matter of experience no St. Louis sewer of size greater than 2 feet by 3 has proved seriously insufficient which has been proportioned by the rule, but the probability of an over-allowance in the larger sizes is suggested by the fact that the curve of volume expected under the rule is concave to the axis of ordinates.

Table No. 2 (see page 187) presents data taken from actual sewers, from which Table have been plotted the column of "Capacity of sewers" as ordinates to the "Areas drained" as abscissas in (Plate XVIII) diagram of "Volume reaching sewers," and the cases of known insufficiency have been distinguished by inclosing the points.

* The maximum discharge of a circular conduit is computed to occur when the free arc is 6J degrees, depth .93[3] per cent., and hydraulic mean depth 0.2914 of the diameter.

TABLE No. 1.

Diameter of sewer.	Discharge of sewer if slope = 0.50 in 100.			Discharge of sewer if slope = 1.0 in 100.			Discharge of sewer if slope = 2.0 in 100.			
	Moore *b*.	Moore *a*.	Moore *a*. / Moore *b*.	Moore *b*.	Moore *a*.	Moore *a*. / Moore *b*.	Moore *b*.	Moore *a*.	Moore *a*. / Moore *b*.	
Ft.										
1	2.86	2.27	0.79^{4}	4.05	3.21	0.79^{2}	5.73	4.55	0.79^{4}	Pipe sewers. n in formula for discharge = 0.015.
1.25	5.27	3.98	0.75^{5}	7.46	5.62	0.75^{3}	10.55	7.94	0.75^{2}	
1.50	8.67	6.26	0.72^{2}	12.64	8.86	0.70^{1}	17.35	12.52	0.72^{2}	
1.75	13.34	9.21	0.69^{0}	18.87	13.12	0.69^{5}	26.68	18.41	0.69^{0}	
2.0	19.15	12.85	0.67^{1}	27.09	18.18	0.67^{1}	38.31	25.71	0.67^{1}	
2.5	29.95	22.46	0.75^{0}	42.35	31.76	0.75^{0}	59·90	44.92	0.75^{0}	Brick sewers. n in formula for discharge = 0.013.
3.0	50.02	35.43	0.70^{8}	70.80	50.10	0.70^{8}	100.10	70.85	0.70^{8}	
3.5	75.0	52.1	0.69^{4}	106.1	73.7	0.69^{4}	150.1	104.2	0.69^{4}	
4.0	109.9	72.7	0.66^{2}	155.4	102.9	0.66^{2}	217.0	145.5	0.67^{0}	
4.5	152.3	97.6	0.64^{1}	215.4	138.1	0.64^{1}	302.0	195.3	0.64^{6}	
5.0	203.2	127.0	0.62^{5}	288.4	179.7	0.62^{3}	406.4	251.7	0.61^{9}	
5.5	260.3	161.2	0.61^{9}	368.1	228.0	0.61^{9}	....	....	....	
6.0	329.4	200.4	0.60^{9}	465.9	283.4	0.60^{8}	....	....	....	
6.5	407.1	244.8	0.60^{1}	575.7	346.2	0.60^{1}	....	....	....	
7.0	494.8	294.6	0.59^{5}	699.8	416.7	0.59^{5}	....	....	....	
8.0	704.1	411.4	0.58^{4}	995.8	581.8	0.58^{4}	....	....	....	
9.0	958.6	552.3	0.57^{6}	1 356	781.0	0.57^{6}	....	....	....	
10	1 264	718.7	0.56^{9}	1 788	1 017	0.56^{9}	....	....	....	Brick sewer. n = 0.013.
12	1 984	1 134	0.57^{1}	2 806	1 603	0.57^{1}	....	....	....	
12 x 14	2 228	1 430	0.64^{2}	3 152	2 023	0.64^{1}	....	....	....	Stone side walls and invert. n = 0.012.
15 x 18	4 000	2 513	0.62^{8}	5 656	3 554	0.62^{8}	....	....	....	
15 x 20	4 901	3 203	0.65^{3}	6 931	4 530	0.65^{3}	....	....	....	Timber bottom.

Some cases of over-charged sewer are exceptional, in that they are not due to want of capacity at the place where overcharge was observed, but to the transmitted effect of insufficiency at some other part of the sewer. Gorging in other cases arises from improper intersections: two streams meet in such a way as to destroy their velocity, hence the velocity below the junction is much less than that which the grade should give, and the tributary sewers are also choked. Setting aside these cases, which are indicated by asterisks in Table No. 2, a curve can be drawn which leaves all the inclosed points below its trace. A few uninclosed points are also found below, but these are cases of sewer not yet tested, since the areas they drain are mostly unimproved. The curve of the diagram is

$$Q = 0.75 \times 2.75 \sqrt[5]{15\ A^4}$$

Q being the quantity of water reaching the sewer per second or the required capacity of the sewer, and A the area drained. In symbols this equation would be written

$$Q = c\ \gamma \sqrt[5]{s\ A^4} = A\ c\ \gamma \sqrt[5]{\frac{s}{A}},$$

in which c = the proportion of rain-fall that will reach the sewers; that is, it makes allowance for loss by evaporation, absorption and retention. Its value for any locality is a matter of judgment after taking into consideration the season at which the heaviest rain-fall occurs; the condition of surface paved or naked; the soil, porous or impenetrable; the use of ground, urban or suburban, park, lawn, etc. For St. Louis the proportion is three-fourths of the rain-fall.

The symbol γ stands for the number of cubic feet of water falling upon an acre of surface per second during the period of greatest intensity of rain. Practically it is the same as the rate of rain-fall in inches per hour. St. Louis is liable to have rains at the rate of $2\frac{3}{4}$ inches per hour, and γ is taken to be 2.75 cubic feet per second.

The mean surface grade in feet in a thousand is represented by s, and, as has been stated, area is represented by A.

The product of the factors not under the radical represents the quantity of water which the sewer must take, and the factor under the radical is the rate of arrival.

The form is like the well-known Burkli-Zeigler formula,

$$q = c\,\gamma\,\sqrt[5]{\frac{s}{A}},$$

in which q is the quantity per acre reaching sewers, hence

$$Q = A\,q = c\,\gamma\,\sqrt[4]{s\,A^3}.$$

For comparison I have made Table No. 3, in which the second column is the actual allowance made by the St. Louis rule; the third column, the Burkli-Zeigler formula; and the fourth the preferred curve, $Q = 0.75 \times 2.75\,\sqrt[5]{s\,A^4}$, for St. Louis conditions.

TABLE No. 3.

Acres.	Old Rule St. Louis.	Burkli-Zeigler.	New Rule St. Louis.	Acres.	Old Rule St. Louis.	Burkli-Zeigler.	New Rule St. Louis.
5	6.7	13.6	12.8	1 100	....	775.3	961
10	13.9	22.8	22.9	1 200	....	827.6	1 030
15	20.4	30.94	30.94	1 300	....	878.8	1 098
20	26.8	38.4	38.9	1 400	....	929.0	1 165
25	33.4	45 4	46.5	1 500	....	978.3	1 232
30	40.4	52 0	53.9	1 750	....	1 098	1 393
40	55 0	64.5	67.8	2 000	....	1 214	1 550
50	70.0	76.3	81.0	2 250	....	1 326	1 704
60	85.7	87.5	93.8	2 500	....	1 435	1 853
70	101.5	98.2	106.1	2 750	....	1 541	2 000
80	118.3	108.6	118.0	3 000	....	1 645	2 145
90	133.6	118.6	129.7	3 250	....	1 747	2 286
100	150	128.4	141.1	3 500	....	1 847	2 426
125	192	151.7	168.7	3 750	....	1 945	2 564
150	234	174.0	195.2	4 000	....	2 042	2 699
175	278	195.3	220.8	4 250	....	2 137	2 833
200	321	215.9	245.7	4 500	....	2 230	2 966
250	410	255.2	293.7	4 750	....	2 323	3 097
300	501	292.6	339.9	5 000	....	2 414	3 227
350	588	328.4	384.5	5 500	....	2 592	3 483
400	676	363.0	427.8	6 000	....	2 767	3 734
450	765	396.6	470.1	6 500	....	2 938	3 981
500	855	429.2	511.4	7 000	....	3 106	4 224
550	946	461.0	552.0	7 500	....	3 271	4 463
600	1 038	492.1	591.8	8 000	....	3 433	4 700
700	....	552.4	669.4	8 500	....	3 593	4 933
800	....	610.6	744.9	9 000	....	3 750	5 165
900	....	667.0	818.5	9 500	....	3 906	5 392
1 000	....	721.8	890.5	10 000	....	4 059	5 619

The Burkli-Zeigler formula was derived from a comparison of discharge from comparatively small areas. For areas less than fifty acres it would apply to the St. Louis experience, but its failure is apparent when large areas, 1 000 acres or more, are considered. The St. Louis rule it will be noticed makes smaller allowance for discharge than the Burkli-Zeigler for areas less than sixty acres, and less than the new rule up to eighty acres, and yet but few cases of deficient size of sewers draining small areas appear in the table. This may be accounted for by the fact that the tops of small sewers lie, as a rule, deeper below the cellars adjoining than do the tops of large sewers, and consequently they may run under a head and no one be inconvenienced; also it must be remembered that very few small sewers appear in the table. If opportunity occurs I hope to extend the study to the smaller sizes also.

In connection with such studies the query often arises: Does the velocity in sewers attain the value indicated by formulas which depend upon slope? Some doubt that it does, and claim that angular blocks of stone would be moved by velocities such as formulas call for, and that there is no evidence that such blocks are moved. In 1883 an examination of Mill Creek sewer found several large rocks containing as much as ten cubic feet, which could not be accounted for, except on the supposition that they had been carried to the place where found by the current. They were sledged into pieces of moderate size, which were not found at a subsequent examination. In Rocky Branch sewer two rocks 36 x 36 x 9 inches were found, in 1883, which were not broken up; one is not now in the sewer, the other has moved 23 feet. The rocks were limestone, weighing about 150 pounds per cubic foot. Again, while putting in a granite invert in Camp Spring sewer, a storm came when a section of invert was unfinished, the whole unfinished section of paving disappeared. The blocks were found afterwards in an alternate branch about 1 200 feet from the spot they started from. The sewer was 7 x 8 feet and its gradient .0124. The maximum velocity under the conditions would, by formula, be 21.5 feet per second. The Rocky Branch sewer may have had a velocity of 19.8 feet per second, and Mill Creek 17.6, according to formula, and the movement of the rocks seems to show that some such velocity must have been reached.

Table No. 4 gives particulars as to form, also the hydraulic factors for the several sewers used in St. Louis. The hydraulic factors are for the sewer at its maximum discharge without head.

By the factors given, a table of capacities for various grades was computed, which I have platted as diagrams of "Capacities of Sewers." The use of the diagrams needs no explanation. (See Plate XVIII.)

TABLE No. 4.

Size.	Form data.			Hydraulic factors.					
	Rad. arch.	Rad. invert.	Rad. sides.	Area.	Wet perimeter.	r	$\sqrt{r}$.	n.	c.
Pipe sewers.				Sq. ft.	Ft.	Ft.	Ft.		
6"	....	....	....	.1905	1.309	.1457	.3817	.012	83.2
8"	....	....	....	.355	1.745	.1942	.4407	.012	88.4
9"	....	....	....	.430	1.963	.2185	.4674	.012	93.0
10"	....	....	....	.530	2.356	.2428	.4938	.012	95.4
12"	....	....	....	0.763	2.618	.2914	.5398	.012	98.3
15"	....	....	....	1.192	3.272	.3642	.6035	.012	103.7
18"	....	....	....	1.716	3.927	.4371	.6611	.012	108.1
21"	....	....	....	2.336	4.582	.5099	.7141	.012	113.1
24"	....	....	....	3.051	5.236	.5828	.7634	.012	116.3
Brick sewers.									
2' × 3'	1' 0"	0'9"	4'0"	4.693	6.874	.683	.826	.013	109.2
2'6" × 3'6"	1' 3"	1'0"	4'2¾"	6.829	8.199	.833	.913	.013	113.5
3' × 4'	1' 6"	1' $\frac{5}{16}$"	3'9"	9.249	9.551	.968	.984	.013	116.6
3'6" × 4'6"	1' 9"	1'5"	4'3"	12.313	11.042	1.115	1.056	.013	119.5
4' × 5'	2' 0"	1'8⅝"	4'9"	15.516	12.104	1.282	1.134	.013	122.4
4'9"	2' 4½"	....	....	17.211	12.435	1.384	1.176	.013	123.8
5'0"	2' 6"	....	....	19.069	13.090	1.457	1.208	.013	124.8
5'3"	2' 7½"	....	....	21.025	13.744	1.593	1.237	.013	125.7
5'6"	2' 9"	....	....	23.073	14.398	1.603	1.266	.013	126.6
5'9"	2' 10½"	....	....	25.217	15.053	1.675	1.294	.013	127.5
6'0"	3' 0"	....	....	27.459	15.708	1.748	1.322	.013	128.3
6'3"	3' 1½"	....	....	29.796	16.362	1.821	1.349	.013	129.0
6'6"	3' 3"	....	....	32.226	17.017	1.894	1.376	.013	129.8
6'9"	3' 4½"	....	....	34.754	17.671	1.967	1.402	.013	130.5
7'0"	3' 6"	....	....	37.374	18.326	2.040	1.428	.013	131.1
7'3"	3' 7½"	....	....	39.594	18.980	2.113	1.454	.013	131.8
7'6"	3' 9"	....	....	42.904	19.635	2.185	1.478	.013	132.4
7'9"	3 10½"	....	....	45.810	20.289	2.258	1.503	.013	132.9
8'0"	4' 0"	....	....	48.813	20.944	2.331	1.527	.013	133.5
8'3"	4' 1½"	....	....	51.916	21.598	2.409	1.551	.013	134.0
8'6"	4' 3"	....	....	55.109	22.252	2.477	1.574	.013	134.5
8'9"	4' 4½"	....	....	58.394	22.899	2.550	1.597	.013	135.0
9'0"	4' 6"	....	....	61.782	23.559	2.623	1.619	.013	135.5
9'3"	4' 7½"	....	....	65.258	24.210	2.695	1.642	.013	136.0
9'6"	4' 9"	....	....	68.833	24.653	2.768	1.664	.013	136.5
9'9"	4' 10½"	....	....	72.504	25.520	2.841	1.685	.013	136.9
10'0"	5' 0"	....	....	76.274	26.180	2.914	1.707	.013	137.3
10'3"	5' 1½"	....	....	80.131	26.832	2.987	1.728	.013	137.7
10'6"	5' 3"	....	....	84.087	27.327	3.060	1.749	.013	138.1
11'0"	5' 6"	....	....	92.287	28.776	3.205	1.790	.013	138.9
12'0"	6' 0"	....	....	109.829	31.414	3.497	1.830	.013	139.6
Brick and stone.			Batter.						
12' × 14'	7' 0"	12'	1 in 12	132.665	35.419	3.745	1.935	.015	122.8
15' × 18'	9' 0"	15'	1 in 12	208.120	44.72	4.654	2.157	.015	126.0
15' × 20'	10' 0"	Flat.	1 in 12	249.204	51.60	4.830	2.197	.015	126.6
			Radius.						
15' × 15'	7' 6"	"	12' 6"	177.009	41.534	4.262	2.064	.015	124.7
			Batter						
10' × 15'	4' 0" and 10' 0"	"	1 in 12	130.461	37.12	3.515	1.875	.015	121.8
10' × 11'	5' 0"	"	1 in 12	94.004	31.55	2.979	1.727	.015	119.7

TABLE No. 2.

Data from St. Louis Sewers.

Length of sewer. Feet.	Size. Feet.	Mean grade. Feet per 1 000.	Area to be drained in acres = A.	Capacity of sewer in cubic feet per second. $Q = a\,c\,\sqrt{r\,s}$. (Kutter.)	Capacity required = Q'. $Q' = 0.75 \times 2.75\,\sqrt[5]{15\,A^4}$.	
			Mill Creek Sewer.			
770	12'×14'	2.50	1 500	1 550	1 225	Not built, but under contract.
3 656	15'×18'	1.25	3 549	1 980	2 225	Built, but not in use. The drainage from 1 200 acres will be diverted.
2 119	15'×20'	1.30	4 156	2 500	2 800	Receives Compton ave. sewer.[1]
3 984	15'×20'	2.30	5 099	3 320	3 275	Receives Joab and Ohio avenue sewers.
4 639	15'×20'	2.50	6 204	3 450	3 840	Receives Camp Spring and 13th street sewers, and is overcharged.
4 615	15'×20'	5.60	6 311	5 180	3 890	Elevation at mouth, — 28'.3. Highest water in river, + 7'56. Back-water in spring.
			Compton Avenue Sewer.			
1 570	5'×6'	10 70	247	380	295	
1 350	5'×6'	5.90	284	253	330	Often overcharged.
2 078	7'3"	12.10	400	830	435	
			Joab Street Sewer.			
969	3'6"	16.00	66	133	101	
2 423	4'×5'	16.30	208	270	255	
			Ohio Avenue Sewer.			
803	$5'.\frac{8}{10}$	3.00	93	230	132	
650	6'	3.00	111	256	153	
1 302	7'	5.40	243	540	288	
520	7'25	9.00	253	720	294	
2 766	7'.5"	12.10	436	920	459	
			Camp Spring Sewer.			
1 106	3'×4'	8.70	55	100	87	Sewer overcharged.*
1 257	4'×5'	13 50	161	248	207	"
1 064	6'×7'	23.40	197	870	245	
1 333	6'5×7'5	13.20	327	802	364	
1 299	7'×8'	12.40	396	932	423	
929	8'5"	4.00	516	733	524	" *
			Branch to Camp Spring.			
497	2'×3'	23.0	21	64	41	
424	2½'×3½'	14.6	49	86	80	Sewer overcharged.*
			Thirteenth Street Sewer.			
1 051	3'5"×5'	13.30	66	220	101	
600	5'	13.30	102	333	142	
404	6'	13.30	125	538	167	
1 805	6'	5.30	164	356	210	
1 055	6'×7'	16.00	207	729	254	

Ninth Street Sewer.

2 547	3½'×4½'	7.00	89	130	129	
420	5'5"×7'	5.00	121	370	163	

Biddle Street Sewer.

640	2'5"×3'5"	5.00	17	51	35	
2 265	3'×4'	3.40	82	60	120	Sewer overcharged.
3 650	4'×5'	10.00	100	215	140	
250	5'×7'	4.40	237	308	284	
950	7'×8'	3.50	441	496	462	" " *
680	9'	2.00	458	605	475	" " *
1 444	12'	1.74	657	1 165	640	" " *
604	12'	26.80	668	.....	650	

Northwestern Sewer.

2 547	3'×4'	9.40	113	102	154	Sewer overcharged.
754	3'5"×4'5"	13.20	138	178	183	" "
282	4'2"×5'6"	13.20	147	390	193	

Branch to Northwestern Sewer.

2 180	2'×3'	8.70	20	40	40	
651	2'6"×3'6"	11.50	23	73	44	

Cass Avenue Sewer.

2 158	3'×4'	2.30	61	49	96	Sewer overcharged.
990	2½'×3½'	14.70	60	86	95	
1 325	4'×5'	5.00	94	151	134	" " *
910	5'×7'	5.00	159	330	204	
928	4'2"×5'6"	4.60	190	180	237	" "
1 087	8'	2.00	219	443	265	

Chambers Street Sewer.

640	2½'×3½'	14.50	51	85	83	
2 145	4'×5'	5.00	102	152	141	
1 894	4'×5'	28.10	162	362	207	

Benton Street Sewer.

1 085	5'6"×6'10"	3.70	123	320	164	
1 365	4'×5'6"	10.90	155	163	200	Sewer overcharged.
1 772	4'×5'6"	18.80	188	340	233	

Grand Avenue Sewer.

387	2'×3'	11.00	...	45	...	
530	2'6"×3'6"	6.50	12	56	24	
2 937	3'6"×4'6"	6.00	138	122	182	Sewer overcharged.
1 260	4'×5'	4.00	198	138	246	" "
1 197	5'×6'6"	4.00	236	266	283	" "
817	5'×6'6"	6.00	271	325	312	" "
880	5'×6'	6.00	304	256	343	" "

Elliott Avenue Sewer.

2 361	2'×3'	15.00	31	52	55	Sewer overcharged.
1 064	4'×5'	15.00	91	265	131	
1 300	5'×6'	11.40	185	392	231	
1 380	6'6"×8'6"	10.00	289	840	330	
1 193	7'×9'	10.00	623	995	610	

Davis Street Sewer.

635	2'6"×3'6"	5.00	43	48	73	Area mostly parked. (Fair grounds.)
1 679	3'×4'	5.00	62	75	96	
937	3'6"×4'6"	5.40	108	114	150	
2 143	4'×5'	11.70	155	231	200	

Farrar Street Sewer.

1 231	4'6"×5'6"	10.00	111	290	152	

Rocky Branch Sewer.

3 735	10′6″	4.40	1 084	1 290	950	
1 400	10′6″	5.50	1 723	1 506	1 520	
3 049	10′6″	6.00	2 168	1 575	1 660	
940	12′	7.00	2 201	2 350	1 680	
437	12′	20.00	2 256	....	1 720	
2 573	12′	7.00	2 413	2 350	1 830	

Branch on Broadway.

932	2′×3′	7.80	33	40	58	Sewer overcharged.
540	2′×3′	10.00	46	43	77	" "
706	2′×3′	16.60	51	55	83	" "
960	2′6″×3′6″	16.00	66	90	102	" "

Salisbury Street Sewer.

770	2′×3′	19.00	49	60	81	Sewer overcharged.
1 498	3′×4′	8.40	91	98	131	" "

Bremen Avenue Sewer.

698	2′6″×3′6″	15.00	54	87	87	
853	3′6″×4′6″	10.00	87	155	128	
1 889	4′×5′	6.00	129	168	172	

Ferry Street Sewer.

1 590	5′6″	7.70	216	323	263	
1 179	5′6″	14.00	251	...	296	
935	5′×7′	20.00	277	...	317	
1 245	5′×6′	5.50	338	274	373	Sewer overcharged.
2 130	8′×10′	4.20	496	876	507	

Gingrass Creek Sewer.

366	2′6″×3′6″	3.00	30	56	54	
1 062	3′6″×4′6″	4.00	39	100	69	
780	4′×5′	4.00	89	133	130	

Rutger Street Sewer.

1 188	3′×4′	16.10	32	133	56	
2 140	3′6″×4′6″	23.00	88	235	127	
1 035	4′×5′	7.10	119	180	163	
979	4′×5′	14.00	128	252	172	

Miller Street Sewer.

315	2′×3′	42.5	25	90	48	
1 046	2′×4′	11.0	42	73	71	
1 287	3′×4′	10.70	62	108	97	

Carroll Street Sewer.

2 069	2′6″×3′6″	15.70	44	87	76	
616	2′6″×3′6″	9.30	52	68	84	
1 228	3′×4′	27.00	70	173	106	
165	3′6″×4′6″	34.80	74	288	112	
2 480	4′×5′	12.70	149	241	196	

Trudeau Street Sewer.

1 040	2′6″×3′6″	16.00	120	112	163	Sewer overcharged.
2 351	4′×5′	27.60	192	360	240	
2 832	5′×6′	8.00	244	330	288	
869	6′×7′6″	16.70	258	815	302	

Barton Street Sewer.

1 599	2′6″×3′6″	36.30	44	132	76	
370	3′×4′	18.00	49	140	82	
2 058	4′6″	7.00	98	168	136	
805	4′9″	18.20	109	330	150	

Louisa Street Sewer.

1 516	2′6″×3′6″	27.30	54	114	88	
2 272	4′×5′	6.80	183	175	230	
962	4′9″	10.80	198	273	245	

Arsenal Street Sewer.

1 718	4′	10.30	96	156	136	
1 180	5′	9.00	183	273	202	
1 967	6′	6.00	245	360	290	
1 926	7′6″	4.10	548	533	552	
3 099	8′0″	15.70	710	1 160	680	

Illinois Avenue Sewer.

630	2′×3′	9.20	32	42	57	
657	4′0″	8.70	72	147	108	
660	5′6″	7.40	165	319	212	
661	6′0″	5.00	181	329	226	
141	7′×8′	5.00	288	592	328	

Potomac Street Sewer.

517	2′6″	26.00	21	69	42	
495	3′0″	12.50	28	78	52	

Cherokee Street Sewer.

614	2′6″×5′6″	18.50	40	98	69	
1 352	3′6″	20.00	53	150	86	

Wyoming Street Sewer.

972	2′×3′	27.00	47	70	78	
951	2′9″×3′9″	10.00	54	83	87	
658	4′0″	16.10	64	200	100	
991	4′6″	16.10	92	272	133	

Branch to Arsenal Street North of Lynch Street.

872	2′×3′	13.60	36	50	62	Sewer overcharged.
214	3′0″	5.00	49	48	82	" "

Southern Sewer.

634	4′×5′	10.20	122	214	165	
1 484	5′×7′	7.00	217	390	265	
2 669	10′×11′	6.00	766	1 508	720	
1 556	10′×11′	13.30	816	2 200	750	

Vandeventer Avenue Sewer.

329	2′6″×3′6″	5.50	37	50	66	These sewers are under construction. The sizes were proportioned to drainage areas strictly according to the former St. Louis rules and so built, except the last 3 356 feet, where grade was increased from 4 to 5 feet per thousand after the size was determined.
344	3′×4′	5.50	48	79	80	
314	3′6″×4′6″	6.00	57	122	92	
425	4′×5′	5.50	69	161	106	
516	4′9″	4.00	99	162	140	
436	5′3″	4.00	129	208	173	
402	5′6″	4.00	152	233	197	
575	6′0″	4.00	196	295	243	
655	6′3″	4.00	214	328	262	
1 490	6′6″	4.00	230	365	276	
595	7′9″	4.00	379	576	410	
361	8′0″	4.00	419	628	444	
508	8′3″	5.00	442	768	464	
2 848	8′6″	5.00	572	825	570	

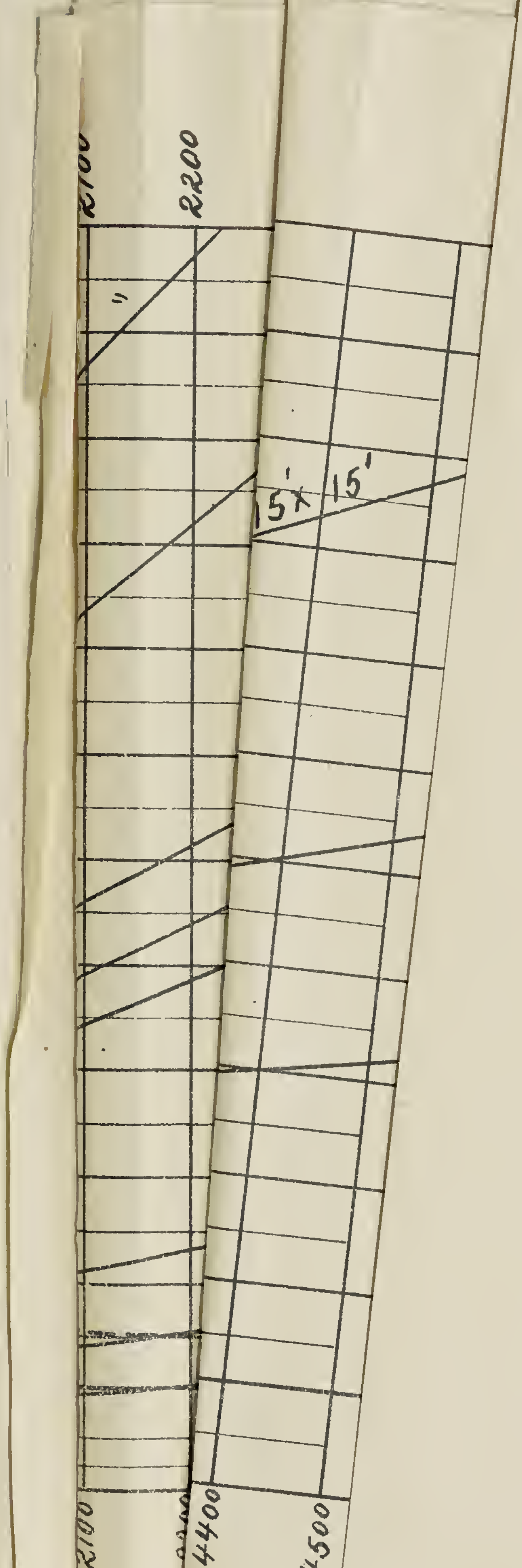
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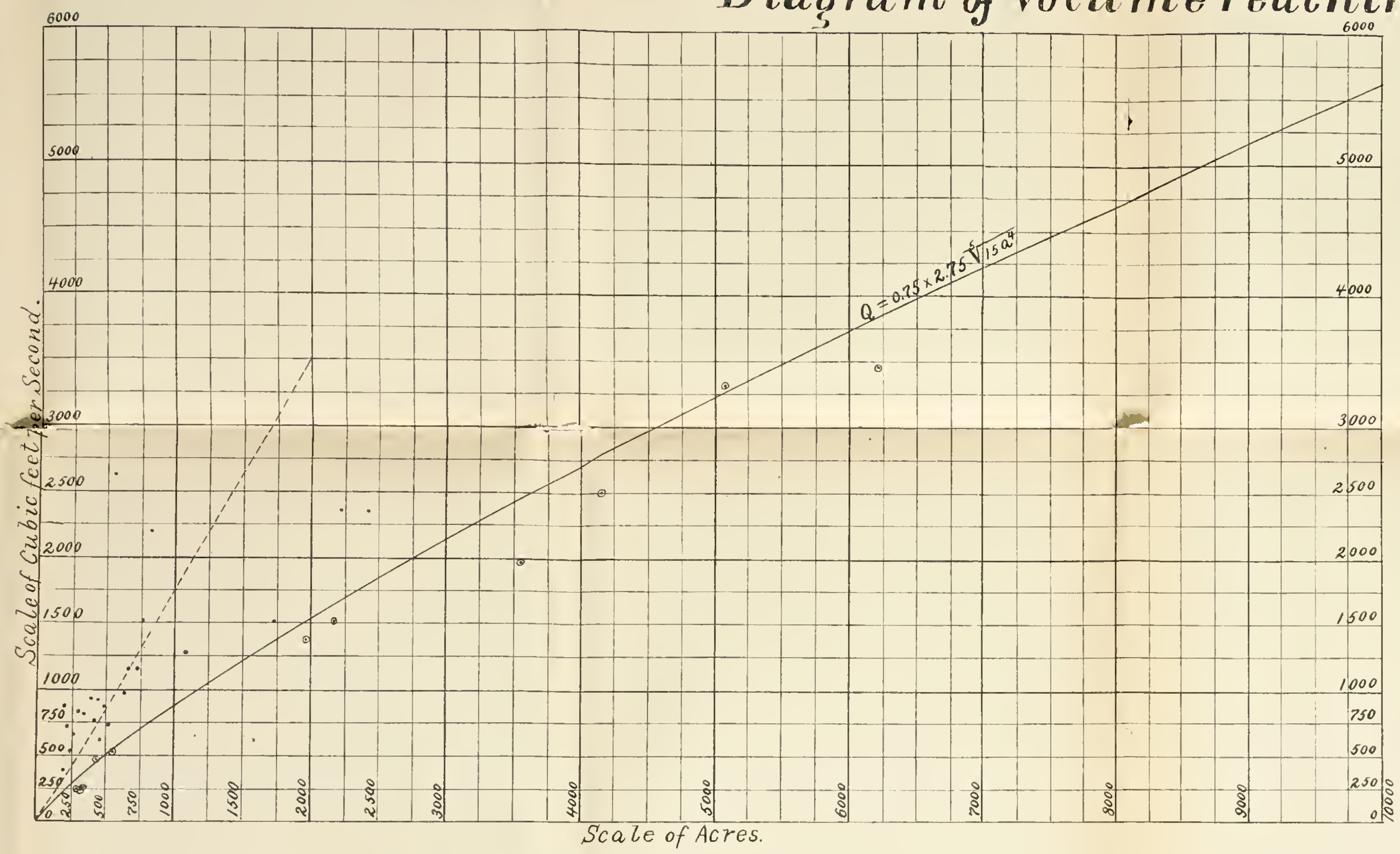
PLATE XVIII
TRANS. AM. SOC. CIV. ENGRS.
VOL. XVI No 359
McMATH ON
SIZE OF SEWERS.

Diagram of Volume reaching Sewers.



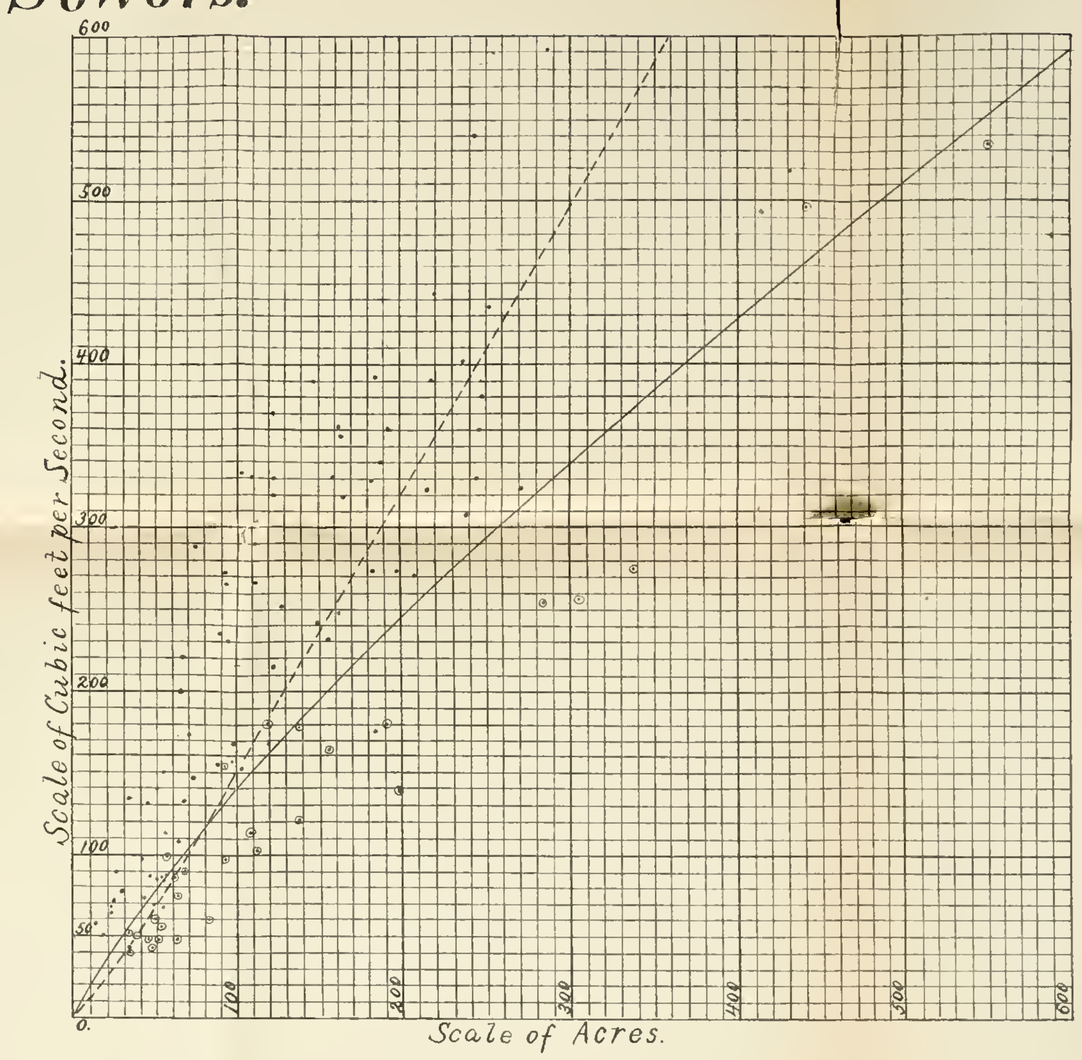
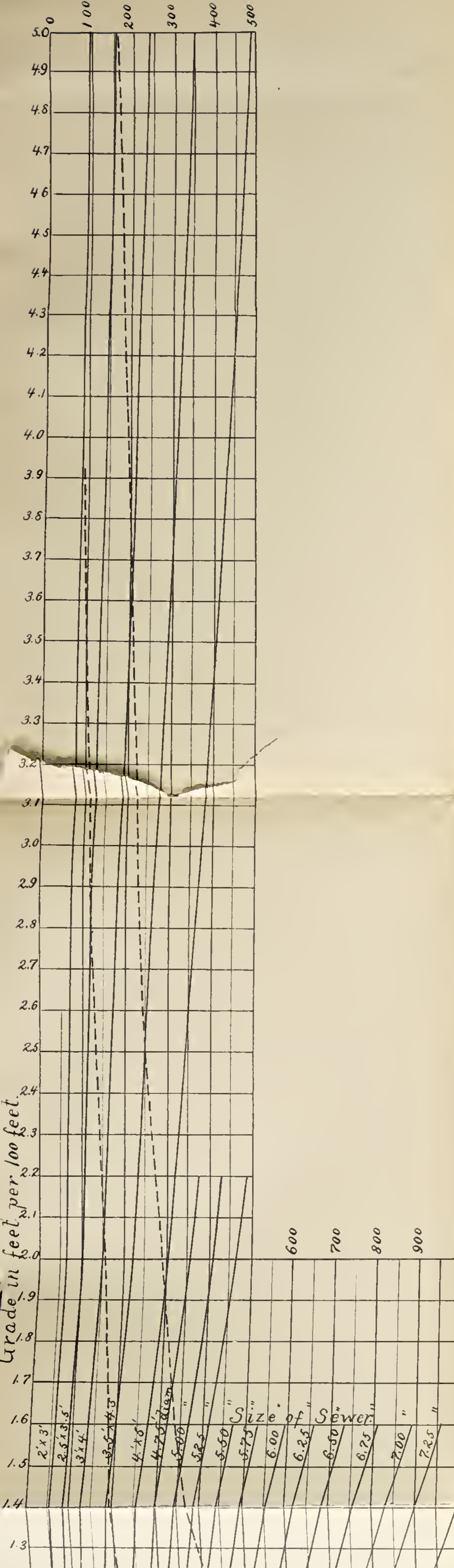
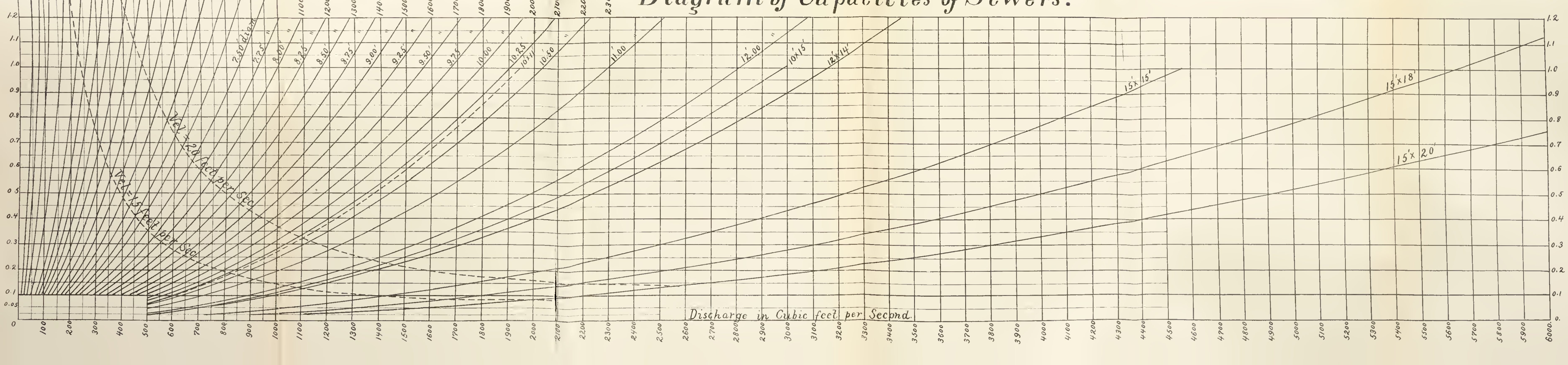


Diagram of Capacities of Sewers.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

359.
(Vol. XVI.—May, 1887.)

FORMULAS FOR THE WEIGHTS OF BRIDGES.

BY PROFESSOR A. J. DUBOIS, JUN. AM. SOC. C. E.
READ MAY 18TH, 1887.

WITH DISCUSSION.

In the Society Transactions for February, 1886, Vol. XV, page 85, Mr. George H. Pegram, M. Am. Soc. C. E., presented a set of empirical formulas which excited considerable discussion and interest. The object of the present paper is to present a series of rational formulas, which accommodate any specifications and design, and which not only give accurately the weight of iron for any given style, but also are of value in deciding the best depth and length of panel in any given case. We shall first give these formulas and illustrate their use and application, and afterwards give their demonstration, so that their basis and rational form may be clearly recognized. If the method and formulas here given are what the writer at present is disposed to think them, they merit discussion, as they seem to give better results as to weight, over a larger range, than any heretofore proposed; and if thus accurate, owing to their rational form, they ought to be reliable guides in questions of practice. No other formulas known to the writer admit of such close determination of weight, give the best depth and panel length for any case, and between different styles of truss, best dimensions being taken in each case, enable one to choose the best.

Floor System.

We shall first consider the floor by itself. The chief defect hitherto, in all attempts to deduce formulas for weight, has been in not keeping the floor system separate. The floor we consider as composed of the rails, ties, planking, etc. (or in the case of highway bridges, of the roadway, etc.); and of the stringers and cross-girders. The weight of the rails, ties, planking, etc., is tolerably constant for all railroad bridges, while for highway bridges the planking, roadway, etc., must be estimated for the case in hand.

For railroad bridges we take the weight of rails, ties, planking, etc., at 400 pounds per lineal foot for single track. We take this as not far from the average simply for illustration, so that we may fully explain our method of procedure and the application of our formulas.

This portion of the floor being then settled upon for any case, we next consider the stringers and cross-girders. If the stringers are of wood they are easily estimated and need not be considered here. If, as is most always the case, we have iron plate-girders of uniform depth, we can at once estimate the weight by the following formula:

$$\left.\begin{array}{l}\text{Weight of a plate-girder}\\ \text{in pounds.}\end{array}\right\} = \frac{12\,W\,l^2 + 2\,R\,l\,d^2}{1.2\,R\,d - 12\,l^2} \qquad (1)$$

where l = span in feet; d = depth in inches; R = the average flange stress in pounds per square inch; W = the total external load in pounds, including allowance for impact.

This formula gives very close results, and was first given by the writer in the February number of the Transactions referred to. Mr. Pegram there tested it by actual weights, with the following results for $R = 9\ 000$ pounds.

Span in Feet.	Depth in Inches.	W.	Formula Weight.	Actual Weight.
20	24	68 700	2 110	2 645
30	36	96 000	4 594	4 871
40	42	110 000	7 772	7 818
40	48	110 000	7 554	7 824
50	54	130 000	11 786	11 835
50	60	130 000	11 550	11 882
60	60	144 000	16 714	16 432
60	66	144 000	16 307	16 614
70	72	156 870	21 962	21 117
80	72	172 000	29 446	28 535
80	84	172 000	28 097	28 114

These results, as Mr. Pegram remarks, are "all that could be asked." Such differences as there are can be as well attributed to a change in the value of R as to any defect in the formula.

From formula (1) we have directly the best or economic depth.

$$\text{Economic depth in inches} = \frac{10\,l^2}{R} + \sqrt{\frac{6\,W\,l}{R} + \left(\frac{10\,l^2}{R}\right)^2} \qquad (2)$$

From formula (2) we can find the best depth in inches, d, of a plate girder, when the total external load, W, in pounds, length in feet, l, and working stress in pounds per square inch, R, are known. The weight, as shown by (1), will vary but little for values of d, differing not much from the best depth.

These two formulas are not difficult or laborious of application, but such labor as may be involved in their application may be greatly lessened by tables constructed from them in accordance with standard specifications.

We shall, for the sake of complete illustration, give such tables. The live-load system we assume for this purpose, consists of two typical locomotives with tenders, followed by train, as given in "Strains in Framed Structures," second edition, Wiley & Sons. We have there given tables by which moments and equivalent uniform loads can be at once and accurately determined for the actual weights and distances. We give this system below. It will be seen that it is somewhat in excess of Mr. Pegram's typical consolidated Class T, which he states "is now coming into use, and will probably be the loading for some time to come."

LIVE-LOAD SYSTEM ASSUMED FOR ILLUSTRATION.

LOCOMOTIVE.		LOCOMOTIVE.		CAR.	
15 000		15 000		12 000	
	8 feet.		8 feet.		5 feet.
25 000		25 000		12 000	
	5 "		5 "		10 "
25 000		25 000		12 000	
	5 "		5 "		5 "
25 000		25 000		12 000	
	5 "		5 "		5 "
25 000		25 000			
	7 "		7 "		
TENDER.		TENDER.		CAR.	
15 000		15 000		12 000	
	5 "		5 "		5 "
15 000		15 000		12 000	
	5 "		5 "		10 "
15 000		15 000		12 000	
	5 "		5 "		5 "
15 000		15 000		12 000	
	9 "		4 "		5 "
				etc.	

For impact we take 30 per cent. increase of external load for all spans below 25 feet and $40 - \frac{2}{5} l$ per cent. increase for all spans above 25 feet ($l =$ span in feet). We take rails, ties, etc., at 200 pounds per foot per stringer, or 400 pounds per foot per track, and R at 8 000 pounds per square inch. If these data are considered satisfactory, those who wish to use our method may in general use the following tables, and will seldom need to calculate at all.

TABLE No. 1.

Iron-plate Stringers of Uniform Depth.

Panel Length in Feet.	Equivalent Uniform Live Load per Stringer in Pounds.	Total External Load W, including Allowance for Impact and Flooring at 200 pounds per foot.	Economic Depth in Inches.	Weight in Pounds.
10	25 000	35 100	16.4	545
11	27 272	38 314	18	657
12	33 333	46 453	20.6	828
13	36 538	50 879	22.5	974
14	39 286	54 711	24.2	1 130
15	41 666	58 066	25.8	1 292
16	43 750	61 035	27.4	1 460
17	45 588	63 684	28.8	1 634
18	47 222	66 068	30.3	1 816
19	48 685	68 230	31.6	2 000
20	50 000	70 200	33	2 197
21	52 380	73 554	34.6	2 421
22	54 545	76 628	36.2	2 652
23	56 521	79 457	37.7	2 900
24	58 333	82 073	39.2	3 133
25	60 000	84 500	40.7	3 382
26	61 537	86 491	42	3 633
27	63 518	89 042	43.4	3 904
28	65 355	91 390	44.8	4 180
29	67 068	93 562	46.2	4 463
30	68 832	95 784	47.5	4 756

From this table we can take at once the weight of stringer for any panel length, in accordance with the live-load system assumed, and the allowance for impact and unit stress. In like manner, and based upon the same data, we may calculate from our formulas (1) and (2) the best depth and weight of cross-girders, assuming that each cross-girder carries two stringers as given by Table No. 1.

We give here two tables for cross-girders. The first gives the equivalent live load and total external load W, and the second, the corresponding weights for single and double track, with best depths. Again we call attention to the fact that considerable deviations from these depths will not much affect the weights.

TABLE No. 2A.

Iron-plate Cross-Girders of Uniform Depth.

Live load and total external load, W, for single track. For double track, take double values. Rails, ties, etc., 400 pounds per foot.

Panel length in Feet.	Equivalent Uniform Live Load.	Total External load W, including Live Load, weight of two Stringers and Allowance for Impact.	Panel length in Feet.	Equivalent Uniform Live Load.	Total External Load, W, including Live Load, weight of two Stringers and Allowance for Impact.
10	50 000	71 620	21	87 619	131 400
11	54 545	78 340	22	90 226	135 630
12	58 332	84 220	23	93 260	140 740
13	62 690	90 790	24	96 041	145 480
14	66 428	96 575	25	98 400	150 040
15	69 666	101 725	26	100 960	154 220
16	72 500	106 370	27	103 147	158 280
17	75 000	110 590	28	105 714	162 860
18	78 055	115 560	29	108 103	167 220
19	81 578	121 140	30	110 333	171 400
20	84 750	126 290			

TABLE No. 2B.

Iron-plate Cross-Girders.

Weight and economic depth for single track, 15 feet wide, and double track, 25 feet wide. Rails, ties, etc., 400 pounds per foot. $R = 8\ 000$; allowance for impact, 30 per cent.

Panel length in Feet.	Single Track, 15 feet wide.		Double Track, 25 feet wide.		Panel length in Feet.	Single Track, 15 feet wide.		Double Track, 25 feet wide.	
	Depth in Inches.	Weight in Pounds.	Depth in Inches.	Weight in Pounds.		Depth in Inches.	Weight in Pounds.	Depth in Inches.	Weight in Pounds.
10	29	1 433	52.6	4 384	21	38.7	1 937	71	5 915
11	30	1 500	55	4 582	22	39	1 967	72	6 000
12	31	1 553	57	4 748	23	40	2 000	73.4	6 060
13	32	1 612	59	4 886	24	40.7	2 036	74.6	6 160
14	33	1 662	61	5 080	25	41.4	2 068	75.8	6 260
15	34	1 706	62.5	5 212	26	42	2 096	76.8	6 345
16	35	1 744	64	5 328	27	42.4	2 124	77.8	6 427
17	35.5	1 772	65	5 400	28	43	2 154	79	6 564
18	36.3	1 811	66.5	5 551	29	43.6	2 172	80	6 664
19	37.2	1 860	68.2	5 682	30	44.2	2 209	81	6 748
20	38	1 898	69.6	5 743					

If the widths are very different from those assumed, it may be necessary to use formulas (1) and (2). In general, for highway bridges, these formulas must be used. But for ordinary railroad practice, Tables Nos. 1 and 2 will give at once weight of stringers and cross-girders.

A good rough and ready rule for depth of cross-girder for single track is: depth of cross-girder in inches, = panel length in feet, + width of roadway in feet. For double track about 1.6 times this depth.

Our two tables, or the formulas (1) and (2) from which they are derived, thus enable us at once to estimate readily the weight of iron in the floor system.

We are now able to estimate the total weight of iron, as well as to determine the best depth for any *plate-girder bridge.*

Plate–Girder Bridges.

For this purpose we need only know the equivalent uniform live load and the weight of wind bracing.

The weight of wind bracing is given by the empirical formula

$$\text{total weight of wind bracing} = N\,(540 + 3.6\,l)$$

where N is the number of panels, and l = span in feet.

For spans under 30 feet, the equivalent uniform live load for the whole bridge may be found from Table No. 1 by taking double the values there given. Above 30 feet we have the following.

TABLE No. 3.

Equivalent Uniform Live Load for Plate-Girder Bridges.

Span in Feet.	Equivalent Uniform Live Load.	Span in Feet.	Equivalent Uniform Live Load.	Span in Feet.	Equivalent Uniform Live Load.	Span in Feet.	Equivalent Uniform Live Load.
30	137 670	43	184 580	56	217 300	69	244 750
31	140 310	44	186 940	57	218 770	70	246 770
32	143 130	45	189 300	58	222 040	71	248 530
33	152 700	46	192 860	59	224 900	72	250 470
34	154 950	47	195 190	60	227 770	73	252 380
35	157 000	48	198 420	61	229 860	74	253 940
36	158 900	49	200 340	62	231 820	75	256 070
37	163 000	50	203 500	63	233 700	76	258 470
38	170 000	51	205 270	64	235 520	77	260 760
39	172 800	52	207 970	65	237 280	78	262 770
40	176 280	53	209 790	66	239 130	79	264 490
41	179 190	54	212 480	67	240 850	80	267 120
42	181 890	55	213 960	68	242 500		

Let us now illustrate the preceding by an example:

Example.—Required the best depth and weight of iron for a single track through plate-girder bridge, 63 feet long and 15 feet wide, center to center. Distance between floor beams, $15\frac{3}{4}$ feet. Iron-plate stringers and floor beams.

As there are four panels, the wind bracing is $4\,(540 + 3.6 \times 63) =$ 3 067 pounds, or 1 533 pounds for each girder, if there are two. Each stringer is $15\frac{3}{4}$ feet long, or, allowing for thickness of cross-girders, nearer 15 feet long, and from Table No. 1 we have for the weight of such a stringer 1 292, or, say, 1 300 pounds. As there are eight such stringers, the total weight is 10 400 pounds, or 5 200 pounds for each girder. The weight of cross-girders from Table No. 2 is about 1 700 pounds. There are five of these, and hence 4 250 pounds for each girder. The rails, ties, etc., we take at 200 pounds per foot for each girder, or 12 600 pounds. The live load from Table No. 3 gives 116 850 pounds per girder. Total, 140 433. Allowance for impact, $40 - \frac{2}{5}\,l = 14.8$ per cent., or 20 784. Hence $W = 161\,217$ pounds. (If we take 14.8 per cent. of the equivalent live load only, we should have $W = 157\,700$ pounds.)

We have then from formula (2) for the best depth, taking $R = 8\,000$,

$$d = \text{about } 92.5 \text{ inches} = 7.7 \text{ feet},$$

and from formula (1), using this depth,

$$\text{weight of one girder} = \text{about } 19\,400 \text{ pounds}.$$

The total weight of iron is then $30\,383 \times 2 = 60\,766$ pounds. The static load for each girder is $30\,383 + 12\,600 = 42\,983$, and the girders can now be designed. If the stringers were wood, we should estimate their weight and proceed as above.

It will be seen from this example that the amount of calculation required is slight and quickly made. The estimate is close, and we have also determined the best depth. As to this latter, a considerable change in depth has but slight influence on the weight. Thus, for 60 inches depth, instead of 92.5, we have 21 400 pounds for the weight, instead of 19 400. We believe, however, that careful designing will show our depth to be the best. Our method can be adapted readily to any specifications and loading. It gives not only a close value for weight, but is an aid in designing. Our tables, we believe, give results in accord with good practice.

We can now pass on to

Bridge Trusses.

Let w_1 = the equivalent uniform load per foot per truss due to the assumed live-load system.

w_2 = load per foot per truss due to the cross-girders, stringers, and rails, ties, planking, etc.

w_3 = load per foot per truss due to the wind bracing.

These three, $w_1 + w_2 + w_3$, give the total external load per foot per truss.

For our assumed load system, we have the following values of w_1 and w_2:

TABLE No. 4.

EQUIVALENT UNIFORM LOAD, w_1, PER FOOT PER TRUSS, ON THE BASIS OF ASSUMED LOAD SYSTEM FOR SINGLE TRACK. FOR DOUBLE TRACK, TAKE DOUBLE VALUES.

Span =	60	65	70	80	90	100	110	120	130
w_1 =	1 845	1 782	1 723	1 636	1 623	1 625	1 643	1 637	1 625
Span =	140	150	160	170	180	190	200	210	220
w_1 =	1 610	1 588	1 560	1 538	1 511	1 486	1 460	1 435	1 408
Span =	230	240	250	260	270	280	290	300	310
w_1 =	1 386	1 370	1 348	1 329	1 310	1 292	1 275	1 260	1 245
Span =	320	330	340	350	360	370	380	390	400
w_1 =	1 230	1 215	1 200	1 185	1 170	1 155	1 140	1 125	1 110

These equivalent uniform loads can be easily checked from our tables in "Strains in Framed Structures," second edition. The table, it will be noticed, gives w_1 for one truss, on the assumption of two trusses to the bridge.

Taking the weight of rails, ties, etc., at 200 pounds per truss, and making use of Tables 1 and 2 for the weight of stringers and cross-girders, we have the following values for w_2. (See Table No. 5.)

Finally, for the load in pounds per foot per truss due to wind bracing, w_3, we have the following empirical formulas:

Depth below 12.5 feet or pony trusses,

$$w_3 = \frac{N\,(270 + 1.8\,l)}{l}$$

Depth between 12.5 and 24 feet, upper and lower horizontal bracing,

$$w_3 = \frac{N\,(336 + 3.2\,l)}{l}$$

Depth above 24 feet, upper and lower horizontal and vertical sway bracing,

$$w_3 = \frac{N\,(7.5\,l - 180)}{l}$$

where N = number of panels and l = span in feet. These formulas

TABLE No. 5.

Load w_2 per Foot per Truss for Single and Double Tracks.

Panel Length in Feet.	Single Track.				Double Track.			
	Weight of One half Cross-girder.	Weight of One Stringer.	Weight of One-half Flooring, etc.	w_2	Weight of One-half Cross-girder.	Weight of Two Stringers.	Weight of One Floor, etc.	w_2
5	507	188	1 000	339	1 555	376	2 000	786
6	582	248	1 200	338	1 783	496	2 400	780
7	631	315	1 400	335	1 935	630	2 800	766
8	679	386	1 600	333	1 992	772	3 200	745
9	695	463	1 800	327	2 135	926	3 600	740
10	716	545	2 000	326	2 192	1 090	4 000	728
11	750	657	2 200	328	2 291	1 314	4 400	727
12	776	825	2 400	333	2 374	1 650	4 800	735
13	806	974	2 600	337	2 443	1 948	5 200	738
14	831	1 130	2 800	340	2 540	2 260	5 600	743
15	853	1 292	3 000	343	2 606	2 584	6 000	746
16	872	1 460	3 200	345	2 664	2 920	6 400	748
17	886	1 634	3 400	348	2 700	3 268	6 800	751
18	906	1 816	3 600	351	2 775	3 632	7 200	755
19	930	2 003	3 800	354	2 841	4 006	7 600	760
20	949	2 197	4 000	357	2 871	4 394	8 000	763
21	968	2 420	4 200	361	2 957	4 840	8 400	771
22	983	2 652	4 400	365	3 004	5 304	8 800	777
23	1 001	2 900	4 600	369	3 030	5 800	9 200	783
24	1 018	3 133	4 800	373	3 080	6 266	9 600	789
25	1 034	3 382	5 000	376	3 128	6 764	10 000	795
26	1 048	3 633	5 200	380	3 172	7 266	10 400	801
27	1 062	3 904	5 400	384	3 213	7 808	10 800	808
28	1 077	4 180	5 600	387	3 282	8 360	11 200	816
29	1 086	4 463	5 800	391	3 332	8 926	11 600	822
30	1 104	4 756	6 000	395	3 373	9 512	12 000	829

are for width of 15 feet. If width is greater, multiply by $\frac{\text{width}}{15}$

The last formula holds for deck bridges also.

We can thus find, for any case, the value of w_1, w_2 and w_3 for railroad bridges. For highway bridges w_3 is the same, but w_2 and w_1 will be different according to construction and case. No trouble in any given case will be found in finding w_1 and w_2. Now the total external load, $w_1 + w_2 + w_3$, being thus known, our formula for the weight per foot of one truss, w_4, not including bed plates, rollers and end shoes, is

$$w_4 = \frac{w_1 + w_2 + w_3}{\dfrac{3.6\,\mu\,d}{A + \dfrac{\mu\,(45\,p^2 + 202\,d^2)}{(w_1 + w_2 + w_3)\,p}} - 1} \tag{3}$$

In this formula, w_1, w_2 and w_3 have already been defined and their values given; $d=$ depth of truss in feet; $p=$ panel length in feet; $\mu=$ the numerator of the strut formula used. Thus the strut formula specified by Mr. Pegram, and generally used, is for iron, $\dfrac{8\,000}{1+\dfrac{(\text{length})^2}{a\,r^2}}$, where length is in inches, r is the least radius of gyration in inches, and a is a constant depending upon the end conditions. In such case μ in formula (3) stands for 8 000. In general μ stands for the numerator of the strut formula used in the design. The only remaining quantity in our formula is A. This is a theoretic quantity depending on the style of truss, number of panels, depth, and panel length, as follows:

For single intersection Pratt:

$$A=p^2\left(2\,N^2+3\,N-2\right)+3\,d^2\left(2\,N-4+\frac{11}{N}\right)$$

For double intersection Whipple:

$$A=2\,p^2\left(N^2+3\,N-10+\frac{12}{N}\right)+3\,d^2\left(N-2+\frac{16}{N}\right)$$

For Warren girder:

$$A=p^2\,(2\,N^2+1.5\,N-2)+6\,N\,d^2$$

and so on. For every style of truss, A can easily be worked out. We present for discussion only these three types.

Our formula (3) is rational in form. The strut formula is included in it, *so that account is taken of the material required for stiffening.* The quantity 3.6 is a result of the theoretic discussion. The only empiric quantities are 45 and 202. These may vary somewhat with individual design, and have been determined by comparison with two cases where the weight and dimensions were known. The method by which the formula has been deduced, we may say here, is similar to that adopted by Charles Bender, C. E., in "Principles of Economy in the Design of Metallic Bridges." Wiley & Sons, 1885. Our value of $\dfrac{A}{d}$ is his "strain length," but in formula (3) we have, as we have said, *introduced the strut formula,* and thus have a variable unit stress. The fault of all formulas for weight has been the adoption of a constant unit stress for all members. We shall notice this point more at length later on. We confine ourselves here to explaining and illustrating the use of the formula. For ready application, we give in the following table the value of A for different numbers of panels, N.

TABLE No. 6.

VALUES OF A FOR DIFFERENT TRUSSES.

N.	A. SINGLE INTERSECTION.	A. DOUBLE INTERSECTION.	A. WARREN GIRDER.
2	$12\,p2 + 16.5\,d2$	$12\,p2 + 24\,d2$	$9\,p2 + 12\,d2$
3	$25\,p2 + 17\,d2$	$24\,p2 + 19\,d2$	$20.5\,p2 + 18\,d2$
4	$42\,p2 + 20.25\,d2$	$42\,p2 + 18\,d2$	$36\,p2 + 24\,d2$
5	$63\,p2 + 23.6\,d2$	$64.8\,p2 + 18.6\,d2$	$55.5\,p2 + 30\,d2$
6	$88\,p2 + 29.5\,d2$	$92\,p2 + 20\,d2$	$79\,p2 + 36\,d2$
7	$117\,p2 + 34.714\,d2$	$123.428\,p2 + 21.857\,d2$	$106.5\,p2 + 42\,d2$
8	$150\,p2 + 40.125\,d2$	$159\,p2 + 24\,d2$	$138\,p2 + 48\,d2$
9	$187\,p2 + 45.666\,d2$	$198.666\,p2 + 26.333\,d2$	$173.5\,p2 + 54\,d2$
10	$228\,p2 + 51.3\,d2$	$262.4\,p2 + 28.8\,d2$	$213\,p2 + 60\,d2$
11	$273\,p2 + 57\,d2$	$290.1818\,p2 + 31.3636\,d2$	$266.5\,p2 + 66\,d2$
12	$322\,p2 + 62.666\,d2$	$342\,p2 + 34\,d2$	$304\,p2 + 72\,d2$
13	$375\,p2 + 68.538\,d2$	$397.846\,p2 + 36.69\,d2$	$355.5\,p2 + 78\,d2$
14	$432\,p2 + 74.357\,d2$	$457.714\,p2 + 39.4285\,d2$	$411\,p2 + 84\,d2$
15	$493\,p2 + 80.2\,d2$	$521.6\,p2 + 42.2\,d2$	$470.5\,p2 + 90\,d2$
16	$558\,p2 + 86.0626\,d2$	$589.5\,p2 + 45\,d2$	$534\,p2 + 96\,d2$
17	$627\,p2 + 91.941\,d2$	$661.412\,p2 + 47.8235\,d2$	$601.5\,p2 + 102\,d2$
18	$700\,p2 + 97.833\,d2$	$737.333\,p2 + 50.666\,d2$	$673\,p2 + 108\,d2$
19	$777\,p2 + 103.737\,d2$	$817.263\,p2 + 53.5263\,d2$	$748.5\,p2 + 114\,d2$
20	$858\,p2 + 109.65\,d2$	$901.4\,p2 + 56.4\,d2$	$828\,p2 + 120\,d2$

We can now readily apply formula (3).

The following tabulation gives the results of our formula as applied to the cases given by Mr. Pegram in the February, 1886, number of the Transactions, taking $\mu = 8\,000$:

Span in Feet.	Depth in Feet.	Panel Length in Feet.	$w_1 + w_2 + w_3$ as given.	Formula Weight of One Truss.	Actual Weight of One Truss.	Difference.	Per Cent. of Difference.
104 S. I.	24	$17\frac{1}{3}$	1 820	23 525	23 658	— 133	$-\,0\frac{56}{100}$
			2 000	24 726	24 458	+ 268	$+\,1\frac{9}{100}$
			2 090	25 330	25 074	+ 256	$+\,1\frac{2}{100}$
150 D.I.	25	$16\frac{2}{3}$	1 675	46 000	46 032	— 32	$-\,0\frac{7}{100}$
			1 844	48 688	49 628	— 940	$-\,1\frac{88}{100}$
			1 950	50 388	52 627	— 2 239	$-\,4\frac{25}{100}$
$201\frac{1}{2}$ D.I.	28	16′ $9\frac{1}{2}$″	1 565	87 499	85 131	+ 2 368	$+\,2\frac{78}{100}$
			1 776	94 872	93 626	+ 1 246	$+\,1\frac{33}{100}$
			1 946	100 879	100 283	+ 596	$+\,0\frac{59}{100}$
320 D.I.	34	20	1 605	286 445	273 671	+ 12 774	$+\,4\frac{66}{100}$
			1 798	312 694	299 718	+ 12 976	$+\,4\frac{33}{100}$
			2 088	351 475	302 206	+ 49 269?	?
$255\frac{1}{2}$ D.I.	29	$18\frac{1}{4}$	1 943	186 392	185 558	+ 834	$+\,0\frac{4}{100}$
	38	$18\frac{1}{4}$	1 943	169 721	174 945	— 5 224	$-\,2\frac{98}{100}$

The last case of the 320-foot span is probably a misprint. It is scarcely possible that a rational formula, which agrees so closely with practice through such a wide range, should suddenly drop out on a single case while agreeing well with the other two cases of the same example, and with all the other examples. We expect to hear from Mr. Pegram that there is some misprint or some misconception on our part as to data or construction. The variation above and below the actual is no more than would be due to variations in design and fluctuating unit stress. An absolutely exact formula, if such were possible, might be expected to show such variations. The column of differences is a better criterion than that of percentages. About one ton is the greatest error up to 320 feet span, and there about six tons only, while the smallest difference runs down to thirty-two pounds.

The formula follows the change of depth in the last example, and also follows the change of loading in all the examples. It will be interesting to see how near, by the use of our Table No. 5 and the formulas for wind bracing, we can check Mr. Pegram's *total weight of iron.* Our formulas for wind bracing are based upon very liberal practice, and give results too large in many cases of light bracing which are so common.

Thus in the first example, 104-foot span, $17\frac{1}{3}$-foot panel lengths, we have from Table No. 5, $w_2 = 349$. Subtract 200 for rails, etc., and we have 149 pounds per foot per truss of iron, or $149 \times 2 \times 104 = 30\,992$ pounds for stringers and cross-girders. For wind bracing our allowance is $77 \times 104 = 8\,008$. For truss we have already 23 525, or 47 050 for both trusses. Total weight of iron is then 86 050 pounds, or, adding 3 600 pounds which Mr. Pegram gives for bed-plates, end-shoes and rollers, 89 650 pounds. The actual weight given by Mr. Pegram is 90 555 pounds, or about one per cent. of difference only. In this way we have the following tabulation:

Span	Total Weight of Iron by Formula.	Actual Weight.	Differences.	Per Cent. of Difference.
104	89 650	90 555	— 905	— 1
	92 052	93 050	— 998	— $1\frac{7}{10}$
	93 260	96 957	— 3 697	— $3\frac{9}{10}$
150	157 050	154 427	+ 2 623	+ $1\frac{6}{10}$
	162 426	163 302	— 876	— $0\frac{5}{10}$
	165 826	177 190	— 11 364	— 6
$201\frac{1}{2}$	276 100	267 410	+ 9 690	+ 3
	290 846	286 055	+ 4 791	+ $1\frac{6}{10}$
	302 860	304 459	— 1 599	— $0\frac{5}{10}$
320	788 610	778 552	+ 10 058	+ 1
	841 108	837 387	+ 3 721	+ $0\frac{4}{10}$
	918 670	946 031	— 27 361	— 2
$255\frac{1}{2}$	506 801	508 415	— 1 614	— $0\frac{3}{10}$

Of course the different loading in the several cases affects the floor system, and as we take our own floor system in every case, some fluctuation is to be expected. Still the results are very close, and serve to inspire confidence in our formulas. We believe that such deviations as occur in designing, which a formula cannot follow, would account for our variations. In every example there is at least one case where our deviation from actual amount is not much over one to two tons.

Mr. W. M. Hughes, M. Am. Soc. C. E., has given in the Discussion in the February number for 1886, a list of executed examples. It is difficult to make a satisfactory test of them, because the value of μ used, and the value of $w_1 + w_2 + w_3$ actually employed are unknown. We shall assume $\mu = 8\,000$, and take w_2 and w_3 as given by our Table No. 5 and formulas for wind bracing. For w_1 the values we take are

Span =	50	61	75	84	105	120	135
w_1 =	1 700	1 476	1 200	1 200	1 000	1 000	1 000
Span =	165	184	200	150	142		
w_1 =	1 000	1 000	1 000	2 000	2 000		

The last two spans, 150 and 142 feet, are double track. All are single intersection except the three spans 165, 184 and 200 feet, which are double intersection and wood stringers. These values of w_1, I judge from the formulas, ought to be near those taken by Mr. Hughes. We have then for $w_1 + w_2 + w_3$, the values given in the following tabulation, taking for the wood stringers 50 pounds per foot. If it should happen that these are near the actual values, the test would be very satisfactory, as it would show that from our formulas for weight we have been able to hit the actual loading. The values of w_1 given above are small for present practice, but conform to the old rule of a ton a foot for each track for spans over 100 feet, and may, therefore, we suspect, be not far from correct. If we are right in this, it is because of these light loadings that Mr. Hughes' cases differ so much from the results of Mr. Pegram's formula. We would call attention to the close agreement of the double-track spans, for which Mr. Hughes *found* 51 *and* 59 *per cent. deviation* from Mr. Pegram's formula. The weight for the double intersection spans 165, 184 and 200, includes the wood stringers.

Span in Feet.	Depth in Feet.	No. of Panels.	Panel length in Feet.	$w_1 + w_2 + w_3$	Formula Weight of one Truss per Foot.	Formula Total Weight.	Actual Weight.	Difference.	Per cent. of difference.
50	$6\frac{1}{4}$	4	$12\frac{1}{2}$	2 064	147	31 100	31 360	— 260	$-0\frac{8}{10}$
61	8	4	$15\frac{1}{4}$	1 844	153	39 235	39 294	— 59	$-0\frac{15}{100}$
75	21	5	15	1 581	168	52 350	51 157	+ 1 193	$+2\frac{3}{10}$
84	21	6	14	1 583	192	63 000	62 875	— 125	$-0\frac{2}{10}$
105	20	7	15	1 388	208	83 160	78 476	+ 4 684	$+5\frac{9}{10}$
120	22.5	8	15	1 391	245	104 640	103 094	+ 1 546	$+1\frac{5}{10}$
135	22.5	9	15	1 404	281	130 950	124 015	+ 6 935	$+5\frac{6}{10}$
165	27	11	15	1 276	315	162 030	165 725	— 2 695	$-1\frac{6}{10}$
184	28	12	$15\frac{1}{3}$	1 287	358	200 560	198 351	+ 2 209	$+1\frac{11}{100}$
200	28	13	15.385	1 293	397	236 000	240 241	— 4 241	$-1\frac{76}{100}$
150	28	7	$21\frac{3}{7}$	2 815	420	250 500	242 930	+ 6 570	$+2\frac{6}{10}$
142	27	7	$20\frac{2}{7}$	2 806	399	228 620	235 700	— 7 080	-3

The agreement is very close, but, as we have said, we can base no conclusion upon it, unless it should appear that the values of $w_1 + w_2 + w_3$ are very nearly those actually used in dimensioning. Any change in our formula (3) due to individual design should be made in the values of 45 and 202, or in the value μ, or both. We would ask Mr. Hughes to test our formula by the actual data, and see whether, with such change, if necessary, it will not check his results.

Professor J. A. L. Waddell, M. Am. Soc. C. E., in "Memoirs of the Tôkiô Daigaku," No. 11, has given a table of weights of bridges of various lengths. The strut formula used by him is that of C. Shaler Smith, M. Am. Soc. C. E., the numerator of which, instead of being constant and equal to 8 000, is variable and given by

$$\frac{40\ 000}{4 + \frac{1}{20}\ \frac{\text{length}}{\text{least diameter}}} \quad \text{or} \quad \frac{10\ 000}{1 + \frac{1}{80}\ \frac{\text{length}}{\text{least diameter}}}$$

It is difficult to satisfactorily test our formula by Professor Waddell's results, without knowing in each case the actual value of μ adopted, and it may be that with these actual values we should need to change 45 and 202. We must leave the test therefore to him. We have tested our formula by his results, however, by assuming for the ratio of $\frac{\text{length}}{\text{least diameter}}$, the expression $\frac{\sqrt{p^2 + d^2}}{\frac{62}{80}}$ for spans less than 200 feet, and $\frac{\sqrt{p^2 + d^2}}{\frac{75}{80}}$ for spans above 200 feet. This gives for μ the values

$\dfrac{10\ 000}{1 + \dfrac{\sqrt{p^2 + d^2}}{62}}$ and $\dfrac{10\ 000}{1 + \dfrac{\sqrt{p^2 + d^2}}{75}}$. With this single change in our formula, intended to hit, as near as may be, the value of μ actually used, we have the following results :

Span in Feet.	Depth in Feet.	No. of Panels.	$w_1 + w_2 + w_3$ As Given.	w_4 By Formula.	w_4 As Given.
S.I. 80	16	5	1 617	169	169
" 100	20	5	1 616	205	205
" 120	21	6	1 522	248	235
" 140	23	7	1 449	295	294
" 160	24	8	1 399	349	349
" 180	27	8	1 345	395	394
D.I. 180	30	8	1 423	380	392
S.I. 190	28	9	1 323	438	437
D.I. 190	32	9	1 434	423	430
" 210	34	10	1 432	490	486
" 230	38	10	1 387	546	524
" 270	43	12	1 326	673	678
" 300	46	13	1 135	756	798

If it should turn out that we have hit reasonably near the actual values of μ used, and also that where the largest deviations occur, the actual value μ gives better results, the value of our formula would be very strongly established. Here, at all events, is a general formula entirely rational, which checks very closely Professor Waddell's results. As the same formula also gives Mr. Pegram's results, and apparently Mr. Hughes' also, it would seem to be in accord with fact. Professor Waddell will, we hope, give a few hours to the adjustment of the formula to his data, and report his results.

In his report on the "Iron Railway Bridge Across the Mississippi," Van Nostrand, 1869, Mr. T. C. Clarke, M. Am. Soc. C. E., has given in detail the weights of several spans. These spans are of old construction, upper chords cast-iron, short panel lengths, small live load, light floor and wind bracing. They are all double intersection, Phœnix post and chords.

Our weights ought then to be in excess of actual for long spans. It will be interesting, however, to find how close we can check. In all the spans, $w_1 = 1\ 250$, $w_2 = 220$, $w_3 = 32$. The stringers are wood, and weight of stringers with track, ties, etc., 150 pounds per foot per truss. The weights given are iron-work only. We take $\mu = 8\ 000$.

Span.	Depth.	No. of Panels.	Panel Length.	$w_1 + w_2 + w_3$.	Formula Weight of one Truss per Foot.	Formula Total Weight.	Actual Weight.	Differences.	Per cent. of Difference.
154	22	14	11	1 500	367	147 224	148 592	— 1 368	$-0\frac{92}{100}$
$197\frac{1}{3}$	24	16	$12\frac{1}{3}$	1 500	487	233 248	224 400	+ 8 848	$+3\frac{9}{10}$
247	26	19	13	1 500	656	375 440	347 126	+ 28 314	$+8\frac{15}{100}$

The constants 45 and 202 and μ could easily be changed so as to give the actual weights more accurately. The check under the circumstances is, however, perfectly satisfactory.

In contrast with these results let us take one of the 200-foot spans of the Plattsmouth Bridge, single intersection, span, 200 feet, $d = 30$, $N = 8$, $p = 25$. We have from Table No. 4, $w_1 = 1\,460$, from Table No. 5, $w_2 = 376$, $w_3 = 52$. Then from our formula, for $\mu = 8\,000$, we have $w_4 = 488$. The weight of track, etc., is 230 pounds per foot per truss. Hence, total weight of iron = 274 400. The actual weight is 270 387. Difference, + 4 013 pounds, or $+ 1\frac{46}{100}$ per cent.

We are, we think, justified by the preceding, in asserting that it is possible to produce a formula, based on theoretical considerations alone, of practical value. That such a formula is not necessarily very complicated, and the constants it contains are easily supplied. That such a formula checks well on actual weights, and the influence of the form of truss upon the weight is properly taken into account. We may, we think, go still further. If our formula is as accurate as these comparisons would seem to indicate, then, since it is rational in form, it ought to be of service in determining *best depth and panel length*, as also in deciding between trusses of different types.

Economic Depth.

We see from Table No. 6 that A consists in general of a coefficient multiplied by p^2 plus another coefficient multiplied by d^2. Let us call these coefficients α and β, so that in general $A = \alpha p^2 + \beta d^2$, where the values of α and β are given for any number of panels by Table No. 6. Thus for five panels, for single intersection, $\alpha = 63$ and $\beta = 23.6$, and so on.

Then we have at once from formula (3),

$$\text{economic depth in feet} = \frac{l}{N}\sqrt{\frac{\alpha + \dfrac{45\,\mu}{(w_1 + w_2 + w_3)\,p}}{\beta + \dfrac{202\,\mu}{(w_1 + w_2 + w_3)\,p}}} \tag{4}$$

where l = span in feet, N = number of panels, μ = numerator of the strut formula used, p = panel length in feet.

w_1 = equivalent uniform load per foot per truss due to the live load.

w_2 = load per foot per truss due to stringers, cross-girders and floor.

w_3 = load per foot per truss due to weight of wind bracing.

α and β are the coefficients of p^2 and d^2 in Table No. 6.

By the use of formula (4) and Tables Nos. 5 and 6, we may then determine, in any case, not only the best depth, but also the best number of panels, or the best panel length.

Let us take as an illustration the first example of Mr. Pegram, viz.: Through span = 104 feet, single intersection, $\mu = 8\,000$. From Table No. 4 we have $w_1 = 1\,633$; for the wind bracing we have $w_3 = 6.43\,N$; for depth between 12.5 and 24; and $w_3 = 5.77\,N$ for depth above 24. The value of w_2 we take from Table No. 5, according to the panel length.

Thus for $N = 4$, we have $p = 26$, $w_2 = 380$, w_3 = about 26, and $w_1 + w_2 + w_3 = 2\,039$. Then from formula (4) we have $d = 25.5$, and from formula (3) $w_4 = 207$. For four panels then, each truss weighs 207 pounds per foot. But if we subtract $w_1 = 1\,633$ from 2 039, we have $w_2 + w_3 = 406$, and if we subtract from this 200 pounds for the rails, etc., we have 206 for iron in the floor, for one truss. The total weight of iron then is 207 + 206 = 413 pounds per foot per truss, or 826 pounds per foot in all.

If we make the calculation thus, for five and six panels we have the following results: Span, 104, single intersection.

				w_4.	Total Weight of Iron per Foot.
$N = 4$	$p = 26,$	$w_1 + w_2 + w_3 = 2\,039$	$d = 25.5$	207	826
$N = 5$	$p = 20.8,$	$w_1 + w_2 + w_3 = 2\,026$	$d = 24$	216	818
$N = 6$	$p = 17\frac{1}{3},$	$w_1 + w_2 + w_3 = 2\,020$	$d = 20$	234	842

We see at once from this example, that as the number of panels diminishes, or the panel length increases, the truss grows lighter, but the floor system heavier. There is, then, a best number of panels, in this case five, for which the total weight is a minimum.

Mr. Pegram has taken for this case six panels, and a depth of 24 feet. We find the same depth, but a longer panel length, viz., 20.8 instead of $17\frac{1}{3}$ feet. If we were to take six panels, as Mr. Pegram does, then according to our formulas, a depth of 20 feet would be better than 24 feet, and this, we think, will be substantiated by actual design.

Our formulas, therefore, not only furnish us with an accurate estimate of the weight, but are a valuable aid in designing. The best depth for the same span, 104 feet, if double intersection, for 5 panels is $d = 23.5$ feet, while for Warren girder, 5 panels, we have $d = 20$ feet. The corresponding weights are $w_4 = 208$, and $w_4 = 214$, and total weight, 802 and 814 pounds per foot. For this case and loading then, we see that the double intersection comes out the lightest, and Warren and single intersection about the same, for best dimensions. It would seem then that an intelligent application of our formulas will enable us not only to find the best dimensions and weight for any given style of truss, but to choose between different styles.

For practical and constructive reasons. it would appear best to limit the length of panel to about 25 feet, in case our formulas should lead to a greater length, and for the same reasons to limit the depth for double intersection with posts pinned at center, to 50 feet. Within these limits our method agrees well with the best practice, and confirms the tendency to long panels which is so marked a feature of the latest practice.

Thus, let us take for further illustration Mr. Pegram's second example: through span = 150 feet, double intersection, $\mu = 8\,000$.

We have for

	w_4	Total Weight of Iron per Foot.
$N = 9,\ p = 16\frac{2}{3},\ w_1 + w_2 + w_3 = 1\,992;\ d = 27.8$	327	1 062

Mr. Pegram takes $d = 25$ and gets $w_4 =$ about 325 for a load of 1 844, a result which is also given by our formula.

Careful designing will, I think, justify 27.8 feet as a better depth if 9 panels are used. But we may go further, and say that a still better result would be obtained *by greater panel length.*

Thus we have for

	w_4	Total Weight of Iron per Foot.
$N = 7,\ p = 21^3,\ w_1 + w_2 + w_3 = 1\,994;\ d = 31.8$	301	1 014
$N = 6,\ p = 25,\ w_1 + w_2 + w_3 = 2\,002;\ d = 34.4$	286	1 000

It would seem then that six panels and depth of about 34 feet would be better.

A change in the depth will not much affect the weight, however. Mr. Pegram insists upon this, and our formula confirms it. Thus, if we take $d = 28$, instead of 34.4, we have for $N = 6$, $w_4 = 293$, and total weight $= 1\,014$ instead of 1 000. This is better than Mr. Pegram's proportions, and we can say quite confidently that a longer panel length in this case would be better. In opposition to Mr. Pegram then, we would say that a change in panel length does affect the total weight. If it did not, the tendency to long panels of our present practice would have no justification. The depth also is not a matter of entire indifference, except within somewhat narrow limits.

Our formula indicates also that the depth should be greater for double than for single track. Thus we have for double track

	w_4	Total Weight of Iron per Foot.
$N = 6$, $p = 25$, $w_1 + w_2 +_3 = 4\,034$; $d = 40$	458	1 832

If we take 28 feet depth in the first case, we should take about 33 in the case of double track. The formula depths are 34.4 in one case and 40 in the other

For single track, single intersection, and $N = 6$, we have $d = 31$ and $w_4 = 307$, total 1 042. The single intersection should be of less depth than the double, and the total weight is greater.

For Warren girder, single track, $N = 6$, we have $d = 28$ and $w_4 = 308$. This comes out as before, about the same as single intersection, best dimensions taken for both. The economic depth for the Warren is less than for either single or double intersection.

The influence of a change in panel length only is shown by the following, for double intersection, single track:

				w_4	Total Weight of Iron per Foot.
$N = 5$	$p = 30$	$w_1 + w_2 + w_3 = 2\,014$	$d = 34.4$	270	992
$N = 6$	$p = 25$	$w_1 + w_2 + w_3 = 2\,002$	$d = 34.4$	286	1 000
$N = 7$	$p = 21\frac{3}{7}$	$w_1 + w_2 + w_3 = 1\,994$	$d = 34.4$	304	1 020

We see that while the weight of truss diminishes as the number of panels diminishes, the floor system increases. If we limit the panel length to 25 feet, as we have recommended, three trials, as in the case of the 104-foot span, will in any case determine the best number of panels and best depth. If this depth runs too great, it may be somewhat reduced without affecting the result appreciably, but the best number of panels will hold good.

Again, for Mr. Pegram's 320-foot span, double intersection through truss, he takes 16 panels and a depth of 34 feet. For his loading and dimensions, our formula, as we have seen, closely gives his weight. But if we try for 15 and 16 panels, we have

$N = 15$	$p = 21\frac{1}{3}$	$w_1 + w_2 + w_3 = 1\,753$	$d = 53$
$N = 16$	$p = 20$	$w_1 + w_2 + w_3 = 1\,753$	$d = 51.3$

Limiting the depth, however, to 50 feet, we have for

				w_4	Total Weight of Iron per Foot.
$N = 14$	$p = 22.857$	$w_1 + w_2 + w_3 = 1\,750$	$d = 50$	866	2 372
$N = 15$	$p = 21\frac{1}{3}$	$w_1 + w_2 + w_3 = 1\,753$	$d = 50$	835	2 316
$N = 16$	$p = 20$	$w_1 + w_2 + w_3 = 1\,753$	$d = 50$	842	2 330

We have then 15 for the best number of panels instead of 16, and as to the depth, we venture to say that most any depth between 45 and 50 will be found better than the depth of 34 feet assumed by Mr. Pegram. Mr. Pegram's weight of truss is $w_4 = 937$.

We have given enough of illustration to show that if our method and formulas are at all reliable, they will go much further than merely to afford a good estimate of weight alone. Being rational in form, they show the relative influence of all the data which enter into the problem of weight, and serve to settle the important points of proper proportions and best design. The formulas can be applied under any specifications and loading, to any style of truss for which A can be made out.

Mr. Charles Bender, in his recent work, "Principles of Economy in the Design of Metallic Bridges," has made out the value of $\frac{A}{d}$ for many cases, and from the general discussion of minimum values of this quantity, deduces several important relations and rules.

Following this lead, and determining A in similar manner, I have been able to take more or less perfectly into account the *influence of a varying unit stress in the various members, and the influence of the stiffening material required by long struts.* The apparent neglect of these important elements would be the main criticism we should offer upon the very neat and interesting discussion in the work alluded to. Thus Mr. Bender's formula for weight, as we understand it, would take the form

$$w_4 = \frac{w_1 + w_2 + w_3}{\frac{a\ R\ d}{A} - 1}$$

where a would be a coefficient of design and R the *mean unit stress for all members.* We have been unable from this formula to get any results which would check actual designs through any wide range, while the depths given by it are entirely inadmissible, wholly, as we believe, owing to the influence of long struts, and the varying unit stress they call for. Comparing this formula with our formula (3), it will be seen that in the term $\frac{\mu\,(45\,p^2 + 202\;d^2)}{(w_1 + w_2 + w_3)\,p}$, lies the influence of these elements. Any one wishing to test our formulas upon designs of his own, should simply adjust it in the coefficients 45 and 202 to suit his practice, using for μ and $w_1 + w_2 + w_3$, the exact values actually employed in the design.

Limiting Length.

It will be of interest to point out the significance of the entire term in our formula (3)

$$\frac{3.6\,\mu\,d}{A + \frac{\mu\,(45\,p^2 + 202\;d^2)}{(w_1 + w_2 + w_3)\,d}}$$

If we denote by L the limiting length of girder, or that length for which it will just support its own weight, w_4, we have

$$w_4\,L = (w_1 + w_2 + w_3 + w_4)\,l$$

or

$$w_4 = \frac{w_1 + w_2 + w_3}{\frac{L}{l} - 1}.$$

Comparing this with formula (3), we see at once it is identical, and we have the limiting length of girder

$$L = \frac{3.6\,\mu\;d\,l}{A + \frac{\mu\,(45\,p^2 + 202\;d^2)}{(w_1 + w_2 + w_3)\,p}}$$

Thus, in Mr. Pegram's first case, span 104, $d = 24$, $N = 6$, single intersection, we have for $w_1 + w_2 + w_3 = 1\,820$, $L =$ about 940 feet. For Mr. Pegram's 320-foot span, $N = 16$, $d = 34$, double intersection, we have for $w_1 + w_2 + w_3 = 1\,605$, $L = 894$. If we should assume that the limiting length were constant for any style of truss, say not far from 940 feet for single and double intersection, we should have at once the very simple formula

$$w_4 = \frac{w_1 + w_2 + w_3}{\frac{940}{l} - 1}$$

How far such a formula would go in checking Mr. Pegram's results, the following tabulation will show:

Span.	$w_1 + w_2 + w_3$	Formula Weight of One Truss.	Actual Weight of One Truss.	Per Cent. of Difference.
	1 820	23 505	23 658	$-0\frac{64}{100}$
104	2 000	25 896	24 458	$+5$
	2 090	27 040	25 074	$+7$
	1 675	47 700	46 032	$+3$
150	1 844	52 500	49 628	$+5$
	1 950	55 500	52 627	$+5$
	1 565	85 637	85 131	$+0\frac{5}{10}$
201.5	1 776	97 123	93 626	$+3$
	1 946	106 593	100 283	$+6$
	1 605	265 920	273 671	-2
320	1 798	297 920	299 718	-6
	2 088	345 920	302 206	?
255.5	1 943	185 237	185 558	$-0\frac{17}{100}$

Here, then, is a single, simple formula made "off-hand," which satisfies well the entire range of all Mr. Pegram's examples quite as well in fact as his own series of formulas, with their carefully determined constants. Probably a little pains would determine a value for L which would give even better results. The formula as it is, judged by agreement alone, is about as good as Mr. Pegram's series, and with two or three values of L to match his coefficients, would easily be better. As it is, if no great accuracy is required, it is good enough for general purposes, as, for instance, for an estimate of weight preliminary to calculation of strains. It is also rational in form so far as it goes, but its accuracy depends upon the assumption that the limiting length is practically constant. Only in so far as this assumption is correct, is this formula reliable.

The comparison just given shows how far this assumption is practically correct. It is questionable whether, in the close competition of the day, any engineer would base a bid entirely upon a formula, and we doubt whether Mr. Pegram would so use his own formulas, in view of the variations shown by his tables of results. A formula which is liable to vary by 4 per cent. or more, would be of little value for such purpose. But that same close competition renders of great interest the questions of best depth and panel length, and all points bearing upon good design. A sound, rational formula, which gives reliable estimate of weight, ought to have a valuable bearing upon such questions, while empirical formulas are, for such purposes, worthless; it is in this that we consider the main value of our formulas to lie; it is from this point of view that the present article is written; and from this point of view that we offer the present method and formulas for discussion.

Demonstration of the Formulas.

The deduction of formulas (1), (2), (3), and (4) is simple, and will take but little space in conclusion.

As the weight depends upon the areas, and these are actually determined by statical calculation, any rational formula must be based at bottom upon such statical calculation, and a general statical calculation ought to furnish a reliable formula.

Let us first deduce formulas (1) and (2) for plate girders.

Let the total external load be represented by W, and let W_1 be the weight of the girder itself. Let $l =$ the span in feet, and $d =$ the depth in inches. Let $R =$ the working stress per square inch in the flanges.

Then, since the total load is $W + W_1$, we have for the moment at the center $\frac{(W + W_1) \times 12\, l}{8}$ inch pounds.

If we divide this by the depth in inches, d, we have the flange strain. Dividing the flange strain by R, we have the flange area. The area of both flanges then will be $\frac{(W + W_1)\, 12\, l}{4\, R\, d}$. If we take the thickness of the web at t inches, its area is $d\, t$ square inches. We have then for the sectional area of the girder $\frac{(W + W_1)\, 12\, l}{4\, R\, d} + d\, t$ square inches. If we multiply this by $\frac{10}{3}$ we have the weight of one foot in length. The weight of l feet then is

$$\mathrm{W}_1 = \left[\frac{(W + W_1)\, 12\, l}{4\, R\, d} + d\, t\right] \frac{10}{3}\, l$$

From this, taking the thickness t at $\frac{1}{2}$ inch, we have

$$W_1 = \frac{12\, W l^2 + 2\, R\, l\, d^2}{1.2\, R\, d - 12\, l^2} \qquad (1)$$

Differentiating, and putting first differential equal to zero, we have

$$\text{economic depth in inches} = \frac{10\, l^2}{R} + \sqrt{\frac{6\, W l}{R} + \left(\frac{10\, l^2}{R}\right)^2} \qquad (2)$$

These are formulas (1) and (2) already given for plate girders.

Let us now deduce (3) and (4).

Take, for illustration, a Warren girder having N panels, and consider it loaded at every lower panel point with the full panel load $(w_1 + w_2 + w_3 + w_4)\,p$. Since there are $N - 1$ such loads, the reaction is $\frac{(w_1 + w_2 + w_3 + w_4)\,(N-1)\,p}{2}$.

The strain in the first lower panel is the reaction multiplied by the half-panel length and divided by the depth. If this strain is divided by the stress per square inch for tension R_t, we have the area in square inches. Multiply this area by $\frac{10}{3}$ and by the panel length p, and we have the weight of the first lower panel.

We have then for the weight of the first lower panel

$$\frac{10\,(w_1 + w_2 + w_3 + w_4)\,p_3}{12\,R_t\,d}\,[N-1].$$

In a similar way we find the weight of each lower panel, and thus obtain the following values:

Weight of 1st lower panel		$\frac{10\,(w_1 + w_2 + w_3 + w_4)\,p^3}{12\,R_t\,d}$	$[N-1]$
" 2d " "		"	$[3\,(N-1) - 2]$
" 3d " "		"	$[5\,(N-1) - 8]$
" 4th " "		"	$[7\,(N-1) - 18]$

and so on.

Summing up by series, we can easily find the weight of N lower panels, or the whole lower chord:

$$\frac{5\,(w_1 + w_2 + w_3 + w_4)\,Np^3\,(N^2 - 1)}{18\,R_t\,d}$$

Since $Np = l =$ the span, the weight per foot per truss of the lower chord is

$$\frac{5\,(w_1 + w_2 + w_3 + w_4)\,p^2\,(N^2 - 1)}{18\,R_t\,d}$$

In a precisely similar manner we find for the weight of the upper chord per foot per truss:

$$\frac{5\,(w_1 + w_2 + w_3 + w_4)\,p^2\,(N^2 - 1)}{18\,R_c\,d}$$

where R_c is the stress per square inch for compression. We thus, it will be observed, keep the stress per square inch for tension and compression separate.

For the braces the secant of the angle they make with the vertical is

$$\sec.\Theta = \frac{\sqrt{\frac{p^2}{4} + d^2}}{d} = \frac{\sqrt{p^2 + 4d^2}}{2d}$$ We have then for full loading the strain in the first tie, $\frac{p\,(w_1 + w_2 + w_3 + w_4)\,\sqrt{4d^2 + p^2}}{2d}\,[N-1]$

Dividing by R_t for the area, and multiplying by the length $\frac{\sqrt{4d^2 + p^2}}{2}$ and by $\frac{10}{3}$, we have:

Weight of 1st tie $\frac{10\,(w_1 + w_2 + w_3 + w_4)\,(4d^2 + p^2)\,p}{12\,R_t\,d}\,[N-1]$

"	2d	"	"	$[(N-1)-2]$
"	3d	"	"	$[(N-1)-4]$
"	4th	"	"	$[(N-1)-6]$

and so on.

The weight of N ties is then $\frac{5\,(w_1 + w_2 + w_3 + w_4)\,(p^2 + 4d^2)\,N^2\,p}{12\,R_t\,d}$

and the weight per foot of the ties per truss is

$$\frac{5\,(w_1 + w_2 + w_3 + w_4)\,(p^2 + 4d^2)\,N}{12\,R_t\,d}$$

In similar manner for the struts we have

$$\frac{5\,(w_1 + w_2 + w_3 + w_4)\,(p^2 + 4d^2)\,N}{12\,R_c\,d}$$

where R_c is the stress per square inch for compression.

The total weight per foot is, then,

$$w_4 = \frac{5\,(w_1 + w_2 + w_3 + w_4)}{18d}\left[\frac{(N^2-1)\,p^2}{R_t} + \frac{(N^2-1)\,p^2}{R_c} + \frac{1.5\,N\,(p^2 + 4d^2)}{R_t} + \frac{1.5\,N\,(p^2 + 4d^2)}{R_c}\right]$$

For the sake of brevity let us write

$$w_4 = \frac{5\,(w_1 + w_2 + w_3 + w_4)}{18d}\left[\frac{T}{R_t} + \frac{C}{R_c} + \frac{S}{R_s}\right]$$

where T refers to the lower chord and ties, or all tension members, C to the upper chords, and S to the struts; hence $T = (N^2 - 1)\,p^2 + 1.5\,N\,(p^2 + 4d^2)$, $C = (N^2 - 1)\,p^2$, and $S = 1.5\,N\,(p^2 + 4d^2)$.

We have then

$$w_4 = \frac{\dfrac{w_1 + w_2 + w_3}{3.6\,d}}{\dfrac{T}{Rt} + \dfrac{C}{R_c} + \dfrac{S}{R_s}} - 1$$

Now the strut formula in common use is, for the upper chords,

$$R_c = \frac{\mu}{1 + \dfrac{p^2}{c\,r_1^2}}$$

and for the struts

$$R_s = \frac{\mu}{1 + \dfrac{p^2 + 4\,d^2}{4\,c\,r_2^2}},$$

where $\mu = 8\,000$ for iron, and r_1 and r_2 are least radii of gyration of the cross-section, and c a constant depending upon end conditions.

We have then, inserting these values of R_c and R_s,

$$w_4 = \frac{\dfrac{w_1 + w_2 + w_3}{3.6\,\mu\,d}}{\dfrac{\mu}{R_t}\,T + C + S + \dfrac{Cp^2}{c\,r_1^2} + \dfrac{S\,(p^2 + 4\,d^2)}{4\,c\,r_2^2}} - 1$$

Now R_t is, on the average, about 9 000 pounds, and $\mu = 8\,000$ pounds. We shall make but slight error in taking $\dfrac{\mu}{R_t} = 1$. Such slight error as there is, is partially balanced by the fact that we have found the weight of web for full load instead of partial live load and full dead load. If then we put $\dfrac{\mu}{R_t} = 1$ and $T + C + S = A = p^2\,(2\,N^2 + 3\,N - 2) + 12\,N\,d^2$, we have

$$w_4 = \frac{\dfrac{w_1 + w_2 + w_3}{3.6\,\mu\,d}}{A + \dfrac{C\,p^2}{c\,r_1^2} + \dfrac{S\,(p^2 + 4\,d^2)}{4\,c\,r_2^2}} - 1$$

Now for the upper chords the square of the radius of gyration, r_1^2, should increase as C increases, and also as the panel load increases, and diminish as μ increases. So also for r_2^2. We ought to have then, approximately,

$$r_1{}^2 = \frac{(w_1 + w_2 + w_3)\, p\, C}{a^1\, \mu}, \quad r_2{}^2 = \frac{(w_1 + w_2 + w_3)\, p\, S}{b^1\, \mu}$$

where a^1 and b^1 are empirical constants. This step in the development is the one to be specially criticised. Upon it depends the accuracy of our formula. The results of the application of the formula are the best justification of the accuracy of this reduction, by which the strut formula is introduced. Inserting these values, we have finally, for the weight per foot per truss

$$w_4 = \frac{w_1 + w_2 + w_3}{\dfrac{3.6\, \mu\, d}{A + \dfrac{\mu\,(a\, p^2 + b\, d^2)}{(w_1 + w_2 + w_3)\, p}} - 1} \tag{3}$$

where a and b are constants to be determined by actual cases and individual design, and for Warren girder, $A = p^2\,(2\,N^2 + 3\,N - 2) + 12\,N\,d^2$. We find by comparison with actual examples $a = 45$, $b = 202$.

We can easily determine A for other types of truss, as given in the text.

Replacing A by its general value $\alpha\, p^2 + \beta\, d^2$, differentiating and putting first differential equal to zero, we have

$$\text{economic depth} = \frac{l}{N}\sqrt{\frac{\alpha + \dfrac{45\,\mu}{(w_1 + w_2 + w_3)\, p}}{\beta + \dfrac{202\,\mu}{(w_1 + w_2 + w_3)\, p}}} \tag{4}$$

DISCUSSION ON
FORMULAS FOR THE WEIGHTS OF BRIDGES.

J. A. L. Waddell, M. Am. Soc. C. E.—It is to me a matter of deep regret that I am unable to spare the time to discuss Professor DuBois' valuable paper in the manner that he suggests.

Nothing but sheer necessity would prevent me from complying with such a request; but at this season there is no time to spare in the office of a bridge engineer, the great call for bridges making it necessary to work day and night.

However, as far as my table of weights is concerned, further adjustment of Professor DuBois' formula seems unnecessary, judging by the accuracy of most of the results in his table.

Professor DuBois' method of dividing bridge weights into those for floor system, lateral system and trusses, is the most rational that could be employed; for the weights of these portions are almost independent of each other.

The weight of floor system depends upou the panel length and the amount and distribution of weight on engine wheels.

The weight of lateral system depends upon the panel length and the wind pressure assumed.

The weight of trusses depends upon the panel length and the combined live and dead loads. The system of concentration of wheel loads affects the truss weight but slightly.

There is, however, another very important factor affecting the values of these weights, viz., the specifications by which the designs are prepared. Until there is greater uniformity in general specifications for railroad bridges it is useless to hope for accuracy in formula for weights. The fact that this want of uniformity exists is no credit to our profession; it simply shows that bridge engineers do not sufficiently discuss the subject of bridge-designing. Each one works too much by himself, and consequently fails to benefit by the experience of the rest. In my opinion there ought to be brought about, either through the American Society of Civil Engineers or otherwise, a convention of bridge engineers, comprising all those who are known to have given the subject of designing much study. Plenty of time should be allowed to prepare for this convention, topics for discussion being sent to the committee months beforehand, and published in the engineering journals. Several conventions would probably be necessary in order to arrive at definite conclusions. At the first no decision should be attempted, but the discussions should be taken down in full, published and circulated. At the next meeting the discussions might be continued and recorded as before; then, before adjournment, a committee should be appointed to draft one or more sets of complete general specifications. Finally, at another meeting, these could be discussed, voted upon and adopted with the necessary modifications.

Such a method of procedure would cost both time and money, but the results would be well worth all the trouble and expense.

A. Gottlieb, M. Am. Soc. C. E.—I have read with much interest the paper of Professor DuBois, about determining the weights of bridges, finding the most economical depth of trusses, panel lengths, etc.

I cheerfully give credit to him for the neat, concise, and, I might say, perfect deductions of his formulas, and the great accuracy achieved by them in finding the weight of bridges.

I am sorry to say that the time at my disposal did not permit me to test the formulas myself, on data in my possession, but I accept Professor DuBois' comparisons with Mr. Pegram's tables of weights, and other executed bridges, as undoubtedly correct. The result proves, if anything, that Professor Du Bois has not only a clear understanding of the subject, but has also taken great pains in elaborating it in a scientific manner, with the employment of the least possible number of empirical coefficients or factors.

The questions that present themselves to my mind, and undoubtedly to others, are: What is the practical value of these formulas, or will they ever be used to any great extent? Is there any shorter and more convenient way to obtain the same results?

I think there is, at least as to determining the weights of bridges. For what purpose do we wish to determine these weights? For an approximate estimate to determine the cost of structures the formulas given will do very well indeed, although for truss bridges they are too complicated for such a purpose, and there are probably other ready means on hand for every engineer to use. For actual competition for work, Mr. DuBois himself states few engineers would be willing to use them at the close competition existing. There remains, therefore, principally their usefulness in aiding to find the correct dead weight to be used in calculation of strains in bridges to resist certain given live loads. As far as plate-girders, or cross-girders in truss spans and stringers, are concerned, the formulas are simple enough, but are nothing more or less than the calculation of the actual strains, sections and weights for the girders for a given live load and an assumed general dead load W. For short girders this formula is accurate enough, but must be corrected for long girders, the flange section in the center being assumed to be uniform throughout, which, of course, is not actually the case. The increased amount of stiffness, and perhaps also thickness of web at ends, does not compensate for the deficiency in section of flanges at the ends of long girders, say over fifty feet.

As to the formulas for trusses, it is certainly a very laborious task to find the value of A for each kind or style of truss, which in itself will deter engineers in practical life from using it, The simpler, quicker, and fully as accurate plan which I would suggest, is to figure the strains for the live load separate from those for the dead load, which latter may be assumed near enough from previous experience; or, if tried by an inexperienced party, by assuming a dead load *ad libitum*, proportion the parts separate for dead-load and live-load strains, and compute the weights of the structure thereby. If the result does not check the assumed dead weight, make the necessary correction, which is very quickly done. The weight of floor, cross-girders and stringers, of course can be ascertained easily beforehand, as suggested by Professor DuBois.

As to the finding of the most economic depth or height of trusses, panel lengths, etc., the formulas are valuable. But I submit for consideration the fact, that ultimately that design is the most practicable which, when performing the same duty, is the cheapest, not always the lightest.

While the saving of material is the first object desired, with a view to proper economy, we must consider also the amount of work required for different designs, and the practicability of obtaining the material of such dimensions as the theoretic formula may prescribe.

Palmer C. Ricketts, Assoc. Am. Soc. C. E.—It will be admitted that rational formulas deduced for purposes of generalization are more liable to give correct results within the limits considered than those which are purely empiric; and that of rational formulas that one will, within limits, probably be the most satisfactory for general application which takes account of the most of the variable conditions existing. The purpose of such formulas may however be defeated by their complexity, and one purely empiric, or nearly so, may, of course, be more advantageous for general use. The remarks on the plate-girder formula of Professor DuBois are suggested by its application to a number of actual weights of girders built in accordance with first-class modern bridge practice for engines given by the following figures:

14 000	8 feet.	22 000	5 "	22 000	5 "	22 000	5 "	22 000	10 "	13 000	4 "	13 000	6 "	13 000	4 "	13 000

Although the reason for the use of an equivalent uniform load is apparent in the deduction of the formula, and although tables giving the loads for all spans and engines may be computed, yet its use was found to be comparatively cumbrous in this case, having no such tables constructed; as whatever may be thought of the use of wheel concentrations for truss spans of considerable length, the simplicity of the calculations, and the advisability of their application to bridges of this character, would seem to render the use of an equivalent uiform load in the actual design at least unnecessary. It will be noticed that this engine, although differing from that given as Class C by Mr. George H. Pegram in his paper published in Vol. XV of the Transactions of this Society, approaches it more nearly than it does to any of the others for which he gives formulas, and it is proposed to compare the weights obtained by the use of his corresponding equation with the ones found from that discussed here. In making the comparison, it is remembered that the formula considered is more general, but the three given by Mr. Pegram differing by a constant would seem to cover most engines used in specifications at present. The following table giving, for plate girder-spans of from twenty to fifty feet, the actual weights and those calculated by the two equations, seems to show that the use of that of Professor DuBois would not be warranted in this case. The length, center to center of bed plates was used in both cases.

PLATE GIRDERS.

Span C to C.	Depth.	Weight.		
		Actual.	DuBois.	Pegram.
Ft. In.	Ft. In.			
20 0	2 0	7 119	6 123	6 903
26 6	2 3	9 480	9 418	10 616
33 6	3 0	15 000	13 826	15 291
36 0	3 0	16 640	15 748	17 064
42 6	3 6	21 944	20 859	22 090
53 6	5 0	31 650	30 538	31 656

A considerable number of other actual weights of spans from fourteen to fifty feet were also compared with those obtained by the use of Mr. Pegram's equation, and were found to agree about as well as those given.

With regard to the claim for accurate economic depth, it seems to me that a formula, deduced as this one is by the use of an equivalent load, and of a cross-section of flange not necessarily the mean, and in which the assumption of a thickness of web greater than that generally used helps to make up for the weight not considered at the ends, can hardly be considered to give an exact economic depth, especially as Professor DuBois truly remarks that a considerable variation in depth makes slight difference in the weight. I regret that I have not sufficient data at hand to apply the formula for truss bridges, but similar remarks would apply to it also. It is not intended here to disparage the results of Professor DuBois, whose ability and knowledge of the subject is well known, but the criticisms apply on account of the difficulty of including, in formula sufficiently simple to render their use general, many of the conditions which enter into the proper design of a bridge.

Charles J. Morse, M. Am. Soc. C. E.—I have found Professor DuBois' paper very interesting, and think his formulas more closely approximate the analytical solution of the much vexed problem of the weights of bridges than any I have seen. I think his method of making separate calculations of floor and bracing preliminary to finding the weights of the trusses is the correct method of procedure for determining an accurate formula, although more tedious of application.

The formula he obtains is, however, only an approximation and not a solution. It comes nearer a solution because it includes, or attempts to include, one more condition of the many involved, viz., the strut formula or varying unit strain.

His formula for stringers is simple and accurate for the flanges and web. The "details," such as end connections, ribs and fillers, rivets, etc., are provided for by assuming the web to be one-half an inch thick for all depths and lengths. The same formula applies to floor beams by changing the uniform live load to suit their conditions of loading and to plate girders, deck or through, after first calculating the bracing and floor systems. Whether this web assumption accurately provides for the details, we have not had time to determine.

The formula for the weight of truss is derived by summing the expressions for the weights of each principal part, but takes no account of details whatever until the strut formula is introduced.

This strut formula is introduced by assuming a value for the square of the radius of gyration, in terms of the panel loads, number of panels, unit strains, etc. The assumption seems a reasonable one, but it may or may not be correct. With the strut formula is introduced a constant to be determined empirically; or rather two constants, one for upper chord-strut formula and the other for intermediate posts. These constants have to provide for not only the special peculiarities of design, but for all details as well, including pins, lattice and tie bars, pin plates, rivets, nuts, etc. It seems to me a clear case of overloaded constant.

I believe the soundness of the formula is vitiated by assuming that the details (which by the way sometimes amount to 25 per cent. of the whole) can be provided for by these constants or that details are a constant factor of the weight. I know in fact that the details are a *varying* percentage of the weight of the principal parts, varying inversely as their weights.

The author derives from his formula for weights of girders and trusses, by the method of maxima and minima, the most economic depths, etc.

This "most economic depth" is based simply on "least weight of material," irrespective of the relative cost of material or shop construction, of good proportioning, or other considerations.

It is interesting and valuable as far as it goes, but unfortunately it stops far too short to be of much practical use. I think such formulas as Professor DuBois has ingeniously devised are useful for an analytical discussion of general laws governing weights, depths, etc., for hints and suggestions they furnish the designer; but of little practical value for every-day service, because they are not accurate enough for final estimates, and require too much labor for simply finding dead weights to use in calculating strains.

The economic depths determined by the formulas are excessively large, as might be expected. I do not agree with Professor DuBois that 7.7 feet is a proper depth for a 63-foot plate girder, even though that depth gives the least weight.

Considerations of relative prices of materials, of lateral strength, of shop cost, etc., tend to reduce this depth considerably.

To get some some idea of the effect of prices of material in modifying economic depth, I have taken Professor DuBois' formula for simple stringers and introduced constants for the prices of angles, plates, etc. Using his notation, his formula for weight of simple girder is

$$\left[\frac{(W + W_1)\, 12\, l}{4\, R\, d} + d t\right] \frac{10}{3} l$$

the first term of which gives the weight of the flanges and the second the web and details.

Let a = price per pound for angles (flanges assumed to be all of angles).
" p = price per pound of web plates.

Since in assuming $t = \frac{1}{2}$ inch, the weight of all end connections, rib and fillers, rivets, etc., are included with the web, it will be a close approximation to assume that the proportion of plate to other materials included with web is as 3 to 1, and that the average price of the details, viz., ribs, fillers, rivets, etc., will be that of angles.

Hence let $$\tfrac{3}{4} p + \tfrac{1}{4} a = m = \frac{3p + a}{4}$$

Making $t = \frac{1}{2}$ inch and multiplying each term by its price per pound, we get

$$\text{Total cost} = \frac{10\,(W + W_1)\, a\, l^2}{R\, d} + \frac{5\, m\, d\, l}{3}$$

assuming that (W_1) is constant, since it varies but little for small changes of depth. Differentiating and placing equal to zero, most economical depth for minimum cost of material,

$$\frac{6\, a\, l\, (W + W_1)}{R\, m}$$

Applying this formula and using the values given in Professor DuBois' Table No. 1, and assuming for trial that the price of plates is ten per cent. greater than angles, we have $a = 1$, $p = 1.10$, $m = 1.075$, and obtain the following results:

l. Panel length in feet.	W. Uniform live load per stringer.	d. Economic depth for least weight.	d. Economic depth for least cost.	Difference per cent.
10	35 100	16.4 inches.	15.7	—4.27
15	58 066	25 8 "	24.9	—3.50
20	70 200	33.0 "	31.8	—3.63
25	84 500	40.7 "	39.2	—3.68
30	95 784	47.5 "	45.9	—3.37

The application of the same formula to an 80-foot plate girder, under same loading, calling (a) the average price of the plates and angles comprising the flanges, and again assuming the wide web plates to cost ten per cent. more than this average, gave the economic depth 101.8 inches, instead of 103.4 inches as given by Professor DuBois.

These results show that the relative price of material as far as girders are concerned, does not cut a very large figure in modifying economic depth, but amounts to enough to be taken into account in an analytical formula. If now an expression for "good practice" could be introduced into the formula, and another variable for "shop cost," and another for "cost of erection," a resulting "best depth" might be obtained. All these conditions must be fulfilled in economical designing, and therefore ought to be contained in any analytical formula proposing to give "the most economical depth."

H. C. Jennings, M. Am. Soc. C. E.—Before proceeding with the discussion of the application of Professor Du Bois' formulas to the determination of the actual weights of plate-girder bridges and their economic depths, I would like to mention a few facts in regard to the original of the 104-foot span upon which the author has based considerable of his comparison of Mr. G. H. Pegram's formula with his own, and to the design of which he takes exception.

The original of this 104-foot span is on the line of the Chicago, Milwaukee and St. Paul Railway, having been built about ten years ago, and was probably one of the first of a style of through truss bridge which has since been extensively adopted by engineers of this country, especially for spans under 200 feet. The point made in the design, and for which especial excellence was claimed, is the riveting of the floor-beams to the posts, above the bottom chord, thus forming, with the top struts and knee-braces, a rigid resistance to any overturning effort at each panel point. In order to accomplish this, 24 feet was the minimum depth of truss permissible to allow the proper clear height for safe passage of trains. It will be seen from this explanation that there were other considerations than that of the least amount of iron in the structure that determined the proper depth of truss to be used.

I will now proceed to the plate-girder formulas, and would present as a basis for what I have to say the following tables.

TABLE No. 1.

Length of girder.	Width C. to C.	Style.	Weight of girders.		Weight of bracing.		Weight of bed plates and bolts.	Total weight.		Per cent. of difference.
			Actual.	By formula.	Actual.	By formula.	Actual.	Actual.	By formula.	
Feet.	Feet.									
41	14	Through	17 035	15 104	3 505	2 750	888	21 428	17 854	—$16\frac{7}{10}$
$49\frac{1}{2}$	14	"	22 476	22 328	3 832	3 591	997	27 305	25 919	— $5\frac{1}{10}$
60	14	"	33 576	33 324	5 201	4 536	1 053	39 830	37 860	— 5
63	14	"	36 694	35 768	5 387	4 601	1 269	43 350	40 369	— 7
68	15	"	44 160	43 422	7 537	4 709	1 353	53 050	48 131	— $9\frac{2}{10}$

The depths used in the formula were the actual depths as built, and were as follows: For 41-foot span, 42 inches; $49\frac{1}{2}$-foot span, 50 inches; 60-foot span, 60 inches; 63-foot span, 66 inches; and 68-foot span, 70 inches.

TABLE No. 2.

	Length.	Width.	Depth.	Style.	Weight of girders.		Weight of bracing.		Weight of bed plates and bolts.	Total weight.		Per cent. difference.
					Actual.	By formula.	Actual.	By formula.	Actual.	Actual.	By formula.	
	Feet.	Feet.	In.									
a	45	$6\frac{1}{2}$	48	Deck	20 236	16 122	1 263	2 808	....	21 599	18 930	—$12\frac{3}{10}$
b	30	$6\frac{1}{2}$	48	"	8 816	7 396	885	1 994	....	9 701	9 340	— $3\frac{7}{10}$
c	45	9	54	"	20 664	17 020	2 820	2 808	900	24 384	19 828	—$18\frac{7}{10}$
d	30	9	42	"	11 284	8 082	2 150	1 944	600	14 034	10 026	—$28\frac{5}{10}$

Spans *a* and *b* were figured for flange sections based on the moment of inertia of the net section, with a unit strain from Weyrauch's formula, 10 000 pounds per square inch; spans *c* and *d* were figured with depth back to back of angles and *r* 9 000 pounds per square inch.

Table No. 1 shows a comparison of the actual weights and the formula weights of a few plate-girders built for the Chicago, Milwaukee and St. Paul Railway some years ago, when the live load in use for bridges of this class was 4 000 pounds per lineal foot. The type of these bridges was a through girder with wooden floor-beams supported at their ends by an angle-iron shelf riveted to the lower part of the web.

This floor, with the rails and guard timbers, is assumed to weigh 700 pounds per foot for width, center to center of girders, of 14 feet, and 800 pounds per foot for width of 15 feet.

In Table No. 2 I have shown a comparison with spans proportioned in accordance with our present practice, in which the live load is given by Mr. G. H. Pegram in his Class T, with a percentage added for momentum of 30 per cent. for spans under 25 feet, and, in proportion to length of span, up to 8 per cent., for 80 feet. In all the above girders the web is considered to have no part in resisting flange stress. It will be seen that the formula weights are less than the actual in all cases. Let us now proceed to an analysis of the formulas and see if we can expect from them reliable results.

Considering first the weight of the girder only, it may be said to be composed of the following items, viz.:

First.—Weight of material in the flanges.

Second.—Weight of material in the web.

Third.—Weight of stiffeners, splices, bearing plates, and for girders with wooden floor, shelf-angles.

Professor DuBois by assuming the first item to be uniform throughout the entire length of the girder, and the second considerably in excess of the average practice, presents a formula for the total weight of the girder in which the third item is entirely disregarded. It would appear from this that the reliability of the results from the formula depends on the balancing of excess and deficiency. Can such a formula be termed rational, or relied upon to give accurate weights.

The empirical formula for the weight of the bracing would seem, from the fact that it is entirely independent of width or style of bridge, to be only adapted to some one style of design. This can be illustrated by an example showing the actual weights of bracing for two plate-girder bridges, length of each 60 feet, one deck and the other through, both with wooden floors.

Bracing for 60-foot deck weighs 3 605 pounds.

Bracing for 60-foot through weighs 5 201 pounds.

Again, it will be seen on reference to the application of formula to the 63-foot span in Professor DuBois' paper, that he makes the weight of bracing for that span with iron floor-beams and stringers to be 3 067 pounds; in the table above presented is a 63-foot span, with wooden floor, in which the bracing weighed 5 387 pounds, of which 4 115 pounds was the weight of cross-struts, which would be replaced by the floor-beams in a design with iron-floor system, leaving but 1 272 pounds for the weight of diagonals. Here again it would seem that the method employed by Professor DuBois is but an approximation, and cannot give close results. Were we to consider the total weight of the structure, another item of weight would seem to be neglected, that of bed-plates, expansion rollers (if used), and anchor bolts. In the 60-foot span referred to above, bed-plates and bolts weighed 1 053 pounds.

While agreeing with Professor DuBois in the theory that the formulas for weights of bridges should be rational, I disagree with him in the method of deriving such formula. To produce reliable results by any such process as used by Professor DuBois, the much discussed problem of proper thickness and stiffening of web plates must be introduced. The absence of data on this point on which we can base conclusive deductions, leaves this question open to individual constructions, giving rise, as at present, to considerable variation of opinion as to both the thickness of web and number of stiffeners necessary to a properly designed plate-girder. Now, as this as yet unsolved problem appears to be a stumbling block in the way of a correct formula based on Professor DuBois' methods, can we not find some equally as rational a method that will not call into play this obstacle. Adopting as a method for computing live-load stresses a uniformly distributed load which will produce a center moment equal to that from the worst position of wheel loads, and from this center moment proportioning the material in the flanges according to the law for uniformly distributed loads, the weight of the material in the flanges of a girder should bear a constant relation to the bending moments, which latter can be expressed by a parabolic curve. The weight of the web, stiffeners, bracing and other component parts can be expressed by an equation of a line founded on the summation of the proper functions of length of span, depth of girder, width of bridge and assumed wind load. Having now all the component weights of the structure represented by the equations of two lines, which may, from the analysis of actual bridges, be accurately platted, it but remains to combine the equations into one in which all conditions affecting the weight are represented. I claim no originality for this method of determining bridge weights, but feel convinced that if ever a reliable formula is deduced it will be by this method.

Edwin Thacher, M. Am. Soc. C. E.—As the object of this paper is to furnish formulas which will be of service to bridge-builders in determining the weights of bridges in advance of detailed calculations, and which will give results agreeing closely with such calculations, it is hoped that the author will kindly consider the following remarks.

I am a little in doubt as to what he intends his formula (1) to represent. Judged by his demonstration it represents the weight of the flanges and web of a plate-girder, to which must be added the weight of the miscellaneous iron-work necessary to complete the structure; but in his example of a 63-foot through girder he would appear to make it represent the entire weight of iron-work, exclusive of wind bracing, and perhaps masonry plates, and we presume this latter is his intention.

It is expecting too much of any one formula to be applicable to all specifications, and I can see no reason why the one under consideration should be applicable to any. It assumes:

First.—Flanges of uniform section.
Second.—Half-inch thickness of web.
Third.—Effective depth of girder identical with depth of web.
Fourth.—Length between bearings identical with total length.
Fifth.—No loss from rivet-holes in tension flange; and
Sixth.—No allowance for the effect of web in flange area.

The first assumption is not often correct except in light girders; the second only in very heavy or very shallow ones; the third only in heavy girders; the fourth only for track stringers and floor-beams with riveted connections; the fifth is never correct; and as for the sixth, this is governed by specification. But the writer has never yet been able to understand how a plate can be securely riveted between two angles and not be strained equally with the angles. A formula to be useful must be simple; must consider the conditions about as they are; and must meet the specifications. In the table following formula (1) are compared the formula weights and what are said to be actual weights of girders for spans up to 80 feet in length. I do not understand how these actual weights can agree so closely with the formula, unless either the same methods were used in the calculation as were used in making the formula, or by the chance balancing of errors. It appears, however, to agree surprisingly well in some comparisons made by the writer with actual estimates when the web is omitted in flange area and a liberal provision is made for stiffeners, and it will undoubtedly answer the purpose for approximate estimates or for assuming dead load preliminary to estimating, and this, I believe, is all that the author claims for it.

There is no great difficulty in making simple formulas for the weight of plate-girders, using the same data in about the same way as it would be used in careful estimates, and which will give results agreeing as closely with such estimates as those made by different computers will agree with each other, and if weight and cost only are required, further estimating can be dispensed with.

We will proceed to give such formulas, using the following notation:

l, length of girders from center to center of bearings.
l_1, length of span over all.
p, length of panel.
x, distance from center of girders to center of stringers in through girders.
d, depth of girder between centers of gravity of flanges.
d_1, depth of web.
t, thickness of web.
S, mean stress per square inch in flanges.
A, area in square inches of two angles in either flange, and such flange plates, if any, as extend the full length of girder.
W, weight supported per lineal foot of rail.
w, weight per lineal foot of flanges necessary to support W.
w_1, weight per lineal foot of girder due to web.
w_2, weight per lineal foot of girder due to stiffeners.
a, weight per lineal foot of a pair of stiffeners, including fillers if used.

w_3, weight per lineal foot of span due to all iron-work, exclusive of flanges, web and stiffeners.

N, number of panels.

Thickness of Web.

This is usually governed by the specifications for rivet-bearing, thus:

For longitudinal girders,

$$t = \frac{W\,l \times \text{pitch}}{24\,d \times (\text{diameter of rivet}) \times (\text{bearing per square inch})} \tag{1}$$

For cross-girders,

$$t = \frac{W\,p \times \text{pitch}}{12\,d \times (\text{diameter of rivet}) \times (\text{bearing per square inch})} \tag{2}$$

Very few specifications allow less than 12 000 pounds per square inch diametrical bearing, and assuming $\frac{7}{8}$-inch rivets with a minimum pitch of 3 inches, the required thickness of web in inches will be

For longitudinal girders .... $t = \dfrac{W\,l}{84\,000\,d}$ (3)

For cross-girders .... $t = \dfrac{W\,p}{42\,000\,d}$ (4)

t will rarely exceed $\frac{3}{8}$-inch in thickness.

Flanges.

Plate-girders may be divided into two classes:

First.—Girders having flanges of uniform section.

Second.—Girders having flange angles of uniform section, and flange plates of uniform strength.

In either class the web may be omitted in flange area or be allowed its proper value. The first class is used for track-stringers, floor-beams, and spans up to about 35 feet in length, and the second class for longer spans. The longitudinal section of flange plates of uniform strength is a parabola, and to provide for the usual extension of plates beyond the theoretic point, this will be considered to have a length equal to that of the girders between bearing points. Allowing 12½ per cent. loss of area in bottom flange due to rivet-holes, and 3⅓ pounds as the weight of 1 square inch of iron 1 foot long, we obtain the following:

Weight per lineal foot (w) of flanges:

Longitudinal girder with flanges of uniform section $w = \dfrac{.8854\,W\,l^2}{d\,S}$ (5)

Longitudinal girders with flange plates of uniform strength,

$$w = 2.361\,A + \frac{.5903\,W\,l^2}{d\,S} \tag{6}$$

Cross-girders with flanges of uniform section, $w = \dfrac{7.083\,W\,p\,x}{d\,S}$ (7)

Weight per lineal foot (w_1) of girder due to web:

1.—Web omitted in flange area for girders of uniform section or of uniform strength.

As no part of the web is included in flanges, the whole must be added, or $w_1 = 40\,t\,d_1$. (8)

2.—Web considered in flange area for girders of uniform section.

As $\frac{1}{3}$ of web is included in flanges, $\frac{2}{3}$ only must be added, or $w_1 = 26.67\, t\, d_1$ (9)

3.—Web considered in flange area for girders with flange plates of uniform strength.

As the formula for flanges considers the angles only of uniform section and the balance of uniform strength, and as the web is of uniform section, $\frac{2}{3} \times \frac{1}{3} = \frac{2}{9}$ of web is provided for in flanges and $\frac{7}{9}$ must be added, or $w_1 = 31.11\, t\, d_1$ (10

Weight per lineal foot (w_2) of girder due to stiffeners. (Omit in floor-beams and track stringers.)

The following formulas provide for extra stiffeners at the inside edge of end bearing plates and double allowance at ends of girders:

1.—Average distance apart, 1.5 × depth of girder,

$$w_2 = \left(\frac{2}{3} + \frac{5\, d_1}{l_1}\right) a \tag{11}$$

2.—Average distance, 1.0 × depth of girder,

$$w_2 = \left(1 + \frac{5\, d_1}{l_1}\right) a \tag{12}$$

3.—Average distance, 0.75 × depth of girder,

$$w_2 = \left(\frac{4}{3} + \frac{5\, d_1}{l_1}\right) a \tag{13}$$

4.—Keystone specifications governed by shearing stress,

$$w_2 = \left(\frac{d_1}{4} + \frac{5\, d_1}{l_1}\right) a \tag{14}$$

Weight per lineal foot (w_3) of main girder due to all iron-work exclusive of flanges, web, and stiffeners.

1.—Deck plate-girders, with cross-ties resting on the top flange of girder. For bottom end and masonry plates, web splice plates, cover plates, anchor bolts, track bolts, rivet-heads, end bracing, cross-section bracing, one system of lateral bracing, and all connections.

$w_3 = \begin{cases} 22.0 \text{ pounds for girders having no flange plates.} \\ 26.5 \text{ pounds for girders having flange plates.} \end{cases}$

Add 6.0 pounds for girders braced top and bottom.

2.—Through plate-girders, with cross-struts, cross-ties resting on the bottom flange of girder. For bottom end and masonry plates, web splice plates, cover plates, anchor bolts, track bolts, rivet-heads, lateral struts, lateral rods, and all connections.

$w_3 = \frac{260 \times (N+1)}{l_1} + 20.0$ pounds for girders having no flange plates.

Add 5.0 pounds for girders having flange plates.

3.—Through plate-girders, with iron floor-beams and wood or iron track stringers.

A.—Track Stringers.

For packing bolts, track bolts, spikes, washers, and rivet-heads.

$$w_3 = \begin{cases} 5.0 \text{ pounds for wooden stringers.} \\ 3.5 \text{ pounds for iron stringers.} \end{cases}$$

Add 5.5 pounds if stringers have flange plates.
Add 4.5 pounds if stringers are braced laterally.

B.—End Struts.

(Omit if floor-beams are used at ends.)

For end struts, stiffeners, rivets, and all connections of end struts to main girders and track stringers.

$$w_3 = \begin{cases} \dfrac{990}{l_1} \text{ for wooden stringers.} \\[2ex] \dfrac{1\,030}{l_1} \text{ for iron stringers on top of floor-beams,} \\[2ex] \dfrac{950}{l_1} \text{ for iron stringers on side of floor-beams.} \end{cases}$$

C.—End Floor-Beams.

(Omit if struts are used at ends.)

For end stiffeners, rivets, and all connections of end floor-beams to main girders and track stringers.

$$w_3 = \begin{cases} \dfrac{560}{l_1} \text{ for wooden stringers.} \\[2ex] \dfrac{600}{l_1} \text{ for iron stringers on top of floor-beams.} \\[2ex] \dfrac{530}{l_1} \text{ for iron stringers on side of floor-beams.} \end{cases}$$

D.—Intermediate Floor-Beams.

For stiffeners, rivet-heads, and all connections of intermediate floor-beams to main girders and track stringers

$$w_3 = \begin{cases} \dfrac{280 \times (N - 1)}{l_1} \text{ for wooden stringers.} \\[2ex] \dfrac{295 \times (N - 1)}{l_1} \text{ for iron stringers on top of floor-beams.} \\[2ex] \dfrac{330 \times (N - 1)}{l_1} \text{ for iron stringers on side of floor-beams.} \end{cases}$$

Add 3.0 pounds if floor-beams have flange plates.

E.—Main Girders.

For bottom end and masonry plates, web splice plates, cover plates, anchor bolts, lateral bracing and rivet-heads.

$$w_3 = \begin{cases} 19.0 \text{ pounds for girders having no flange plates.} \\ 24.0 \text{ pounds for girders having flange plates.} \end{cases}$$

The preceding formulas and values are applicable to any specification, not only as to unit stresses, but other conditions of calculation, and will give the weights of material with the correctness of careful estimates. The values of w_1, w_2 and w_3 can be easily tabulated and require no calculation, and the formulas for the value of (w) are certainly simpler and require less labor in calculation than formula (1), given by Professor DuBois.

Economic Depth of Girders.

The economic depth is governed principally by the material in the web and flanges. The slight effect due to end stiffeners, and in some specifications to intermediate stiffeners, will, for the sake of simplicity, be neglected.

We will consider six general cases, as follows:

1.—Longitudinal girders of uniform section, web omitted in flange area,

$$(w + w_1) = \frac{.8854\ W l^2}{d S} + 40\ t d_1 \quad (15), \quad d = \sqrt{\frac{.8854\ W l^2}{40\ t S}} \quad (21)$$

2.—Longitudinal girders of uniform section, web considered in flange area,

$$(w + w_1) = \frac{.8854\ W l^2}{d S} + 26.67\ t\ d_1 \quad (16), \quad d = \sqrt{\frac{.8854\ W l^2}{26.67\ t S}} \quad (22)$$

3.—Longitudinal girders with flange plates of uniform strength, web omitted in flange area,

$$(w + w_1) = 2.361\ A + \frac{.5903\ W l^2}{d S} + 40\ t\ d_1 \ (17)\quad d = \sqrt{\frac{.5903\ W l^2}{40\ t S}} \quad (23)$$

4.—Longitudinal girders with flange plates of uniform strength, web considered in flange area,

$$(w + w_1) = 2.361\ A + \frac{.5903\ W l^2}{d S} + 31.11\ t d_1 \ (18)\quad d = \sqrt{\frac{.5903\ W l^2}{31.11\ t S}} \quad (24)$$

5.—Cross-girders of uniform section, web omitted in flange area.

$$(w + w_1) = \frac{7.083\ W p x}{d S} + 40\ t d_1 \ (19), \quad d = \sqrt{\frac{7.083\ W p x}{40\ t S}} \quad (25)$$

6.—Cross-girders of uniform section, web considered in flange area,

$$(w + w_1) = \frac{7.083\ W p\ x}{d S} + 26.67\ t d_1 \quad (20), \quad d = \sqrt{\frac{7.083\ W p x}{26.67\ t S}} \quad (26)$$

The economic depth (d) is obtained in all cases by making the first differential of $(w + w_1) = 0$.

If $t = \frac{3}{8}$ inch and $S = 9\ 000$, we have following values:

$$(21) = \sqrt{\frac{W l^2}{152\ 500}} \qquad (22) = \sqrt{\frac{W l^2}{101\ 600}} \qquad (23) = \sqrt{\frac{W l^2}{228\ 700}}$$

$$(24) = \sqrt{\frac{W l^2}{177\ 900}} \qquad (25) = \sqrt{\frac{W p x}{19\ 000}} \qquad (26) = \sqrt{\frac{W p x}{12\ 700}}$$

from which it will be seen that the depth varies greatly with the conditions assumed in the calculation. Eor example take a 60-foot span $W =$ 2 310 pounds per lineal foot, t and S as above.

$$\text{Case 2, } d = \sqrt{\frac{2\ 310 \times (60)^2}{101\ 650}} = 9.0 \text{ feet.}$$

$$\text{Case 3, } d = \sqrt{\frac{2\,310 \times (60)^2}{228\,700}} = 6.0 \text{ feet.}$$

The author's formula (2) therefore, which is based on only one condition of calculation, and that one seldom used, except for short spans, is very misleading.

The formulas 21 to 26 inclusive should also be used with caution, for the lightest girder is not necessarily the cheapest one, which we can best illustrate by an example: Take a 60-foot span, Case 4, data as above. The most economical depth by formula (24) is 6.8 feet. This gives slightly less material in web and flanges than any other depth, but when the cost of workmanship is considered it is found to be much too deep. For depths of about four feet and under, the cost of workmanship may be considered practically independent of depth, but for greater depths the increase in cost for punching, assembling, riveting and handling, is quite considerable. This will vary in different shops, but the average may be taken about as given in the following table, showing the effect of workmanship on the economic depth. It will be seen that although a depth of 6.8 feet is economical in material, a depth of 5.5 feet is most economical all things considered.

Depth in feet.	Material per lineal foot of span.			Increase of cost of work due to depth.	Relative cost per lineal foot of span.
	Weight in pounds.	Rate.	Cost.		
4.0	512.8	2.5	$12.82	....	$12.82
5.0	483.7	2.5	12.09	0.13	12.22
5.5	476.6	2.5	11.91	0.25	12.16
6.0	472.5	2.5	11.81	0.42	12.23
6.5	471.0	2.5	11.78	0.62	12.40
7.0	471.7	2.5	11.79	0.87	12.66

The most economical depth of girder when known cannot always be used, as the depth is very often governed by other considerations more important. Professor DuBois in his example of 63-foot through plate girder uses a track stringer 25.8 inches deep. Such depth is seldom allowed to exceed 18 inches, and it is frequently found necessary to limit the depth to 10 or 12 inches, so practically formulas for economic depth appear to be of very little service.

A careful estimate of the 63-foot through plate-girder span taken as an example by Professor DuBois, using the same loads, impacts and unit stresses, gives results as follows :

Track stringers and track bolts	179.8	pounds.
Floor-beams and connections	186.5	"
Main girders and laterals	574.8	"
Total iron-work per lineal foot of span..	941.1	"

The results by the writer's formulas are as follows :

			Pounds.
Track stringers and track bolts.	Flanges ....(5)=54.2		
	Web ....(8)=32.2		
	Balance ....(w_3)= 3.5		
		$=89.9 \times 2$	=179.8
Floor-beams and connections.	Flanges ....(7)=59.8		
	Web.. ....(8)=56.6		
		$=\dfrac{116.4 \times 15 \times 5}{63}=138.5$	
	End floor-beams (w_3)=	$\dfrac{530}{63} \times 2$ .... = 16.8	
	Int. " (w_3)=	$\dfrac{330 \times 3}{63} \times 2$ .... = 31.4	
			=186.7
Main girders and laterals.	Flanges........(6)=124.4		
	Web...........(8)=115.5		
	Stiffener ....(13)= 22.9		
	Balance........(w_3)= 24.0		
		$=286.8 \times 2$............	= 573.6
	Total iron-work per lineal foot of span...................		940.1

The results obtained by Professor DuBois are as follows :

Track stringers	165.1	pounds.
Floor-beams..................................	134.9	"
Main girders	615.9	"
Laterals.....................................	48.6	"
Total iron-work per lineal foot of span ..	964.5	"

The total weight given by Professor DuBois agrees quite closely with the estimate, and if his lateral bracing had been put in at about twenty pounds per lineal foot, its actual weight, the agreement would have been still closer, but it will be seen by an examination of the weights in detail that this is due to a fortunate balancing of errors, which may not often happen, as follows :

	Actual.	Prof. DuBois.	Difference.
Track stringers...........	179.8	165.1	—14.7
Floor-beams..............	186.5	134.9	—51.6
Main girders and laterals..	574.8	664.5	+89.7
Total.............	941.1	964.5	+23.4

To make convenient use of the writer's formulas, the values of w_1, w_2 and w_3 should be tabulated, then the only ones requiring calculation are those giving values of w (5, 6 and 7). In these, W, which represents the total weight supported per lineal foot of rail, may, in addition to live load, impact, timber and rail, be taken as follows:

For track stringers........................	100	pounds.
For floor-beams............................	200	"

For main girders W may be made to include weight of iron-work, if this is known with sufficient exactness, or it may be made to include any assumed weight of iron-work, or the weight exclusive of iron-work, as may be preferred, and if the assumed weight varies materially from the actual weight, corrections can be made as follows:

Let W = Load assumed.
Let F = Weight of flanges due to W.
Let D = Difference between assumed and actual weight due to W.
Let C = Correction in weight of iron-work.

Then $\pm\ (D + C)$ = correction of assumed load, and $\pm\ (D + C)\ \frac{F}{W}$ = correction in weight of iron-work, or $C = \pm\ D\ \frac{F}{(W - F)}$.

As this correction can be applied so easily, it is a matter of little importance whether the load W be assumed correctly or not.

I regret that I will not be able to comment on formulas (3) and (4) for bridge trusses given by Professor DuBois, as the time and space occupied in the consideration of his formulas (1) and (2) for plate-girders is much greater than contemplated.

William M. Hughes, M. Am. Soc. C. E.—I have tested the formula given by Professor DuBois for obtaining the weight of iron bridges with a result as given below.

These bridges, except the two double-track spans, were calculated to carry a load consisting of two 72-ton consolidation engines, followed by a uniform load of 2 000 pounds per foot, and the double-track spans for a load of 2 400 pounds per foot for each track, including engine load.

For w_1 w_3, and $w_1 + w_2 + w_3$, the values are as follows:

Span	=	50	61	75	84	105	120
w_1	=	1 600	1 550	1 475	1 450	1 430	1 415
w_3	=	16	15	24	26	28	29
$w_1 + w_2 + w_3$	=	1 923	1 886	1 800	1 784	1 765	1 773
Span	=	135	165	184	200	142	150
w_1	=	1 400	1 350	1 325	1 300	2 400	2 400
w_3	=	30	41	43	45	60	60
$w_1 + w_2 + w_3$	=	1 728	1 736	1 717	1 698	3 165	3 157

For w_2 I have used the actual weight of the cross-girders and stringers, which vary somewhat with the same panel length, owing to the fact that more or less was allowed for impact according to the location of the bridge, with reference to the liability of running at a high or low rate of speed. I have taken the floor, track, etc., at 300 pounds per foot, which is slightly in excess for the style of floor used on these bridges.

I have also used for the value of w_3 the actual weights, which are considerably less than the formula would give. In applying the formula for girders I found it to give 1 700 pounds for the weight of a cross girder, whereas the actual weight was 2 200 pounds. In applying this formula it requires entirely too much labor to make it of any practical value. This also seems to me an objection to the formula for obtaining the weight of the trusses; almost any engineer who makes a practice of figuring bridge work has, or can easily obtain, the weight of structures already built, and can thus assume dead loads sufficiently close that he will scarcely ever be obliged to go over the calculations the second time.

Span.	Kind of truss.	Stringer.	Depth.		Number and length of panels.			Weight, less shoes and rollers.	Weight by formula.	Difference per cent.
			Ft.	In.	No.	Ft.	In.			
50	Pratt.	Iron.	6	3	4	12	6	29 860	28 000	$-6\frac{64}{100}$
61	"	"	8	0	4	15	3	37 894	39 405	$+3\frac{83}{100}$
75	"	"	21	0	5	15	0	48 557	47 025	$-3\frac{26}{100}$
84	"	"	21	0	6	14	0	59 675	59 470	$-0\frac{34}{100}$
105	"	"	20	0	7	15	0	75 276	80 430	$+6\frac{41}{100}$
120	"	"	22	6	8	15	0	99 894	106 650	$+6\frac{26}{100}$
135	"	"	22	6	9	15	0	120 515	133 110	$+9\frac{47}{100}$
165	Whipple.	Wood.	27	0	11	15	0	161 825	148 830	$-8\frac{73}{100}$
184	"	"	28	0	12	15	4	193 251	192 096	$-0\frac{6}{10}$
200	"	"	28	0	13	15	$4\frac{1}{2}$	236 640	232 800	$-1\frac{65}{100}$
142	Pratt.	Iron.	27	0	7	20	$3\frac{1}{2}$	229 700	229 200	$-0\frac{22}{100}$
150	"	"	28	0	7	21	5	237 930	247 200	$+3\frac{75}{100}$

The 142-foot span weighed somewhat more than it should, owing to the fact that the same section was used for the floor-beams and stringers as for the 150-foot span.

George H. Pegram, M. Am. Soc. C. E.—I desire first to correct Professor DuBois in his statement that in double-track spans "Mr. Hughes found 51 and 59 per cent. deviation from Mr. Pegram's formula."

The facts are these, that for double-track spans I suggested adding 90 per cent. to the weight given by the formula for single track. Mr. Hughes showed the addition as 51 and 59 per cent., making the deviation 20 and 16 per cent. respectively; quite bad enough, however, if true. In replying to his discussion, I endeavored to show that his conclusions were unreasonable on the face, and that the weights on which they were based showed discrepancies among themselves sufficient to destroy their value as standards. Professor DuBois has doubtless been led into this error through Mr. Hughes' table, in which the additions for double track are put in the column of "differences" for single track.

In commenting upon his formula, based upon a constant limiting length of span, Professor DuBois says:

"Here, then, is a simple formula made off hand which satisfies well the entire range of all Mr. Pegram's examples quite as well, in fact, as his own series of formulas with their carefully determined constants."

Now the only formulas which I presented were for the entire weight of the bridge, while Professor DuBois' formula is for one truss. Comparatively a simple matter and quite a different thing.

The full text of my indorsement of his girder formula is this: "The results for girders are all that could be asked, * * * but neither the girder nor truss formula gives proper variations for different depths and loads."

Professor DuBois seems to have entirely abandoned the truss formula referred to, and my following remarks will be confined to a discussion of the formula which he substitutes for it, after first calling attention to a serious fault in his proposed live load, which he says "is somewhat in excess of Mr. Pegram's typical consolidation, Class T."

The average weight of his locomotives per foot of track is 3 398 pounds, and of his train, 1 920 pounds. In the "Class T" loading these weights are respectively 3 276 and 3 000 pounds.

While his engine loading is slightly heavier, his train load is very much lighter, in fact it is but little more than half that now running on some roads. The result is that in small spans the difference will be very small, but it will rapidly increase with the span; for example, at 400 feet the equivalent load for Class T will be 800 pounds (about 36 per cent.) heavier. Professor DuBois is quite right with regard to my 32-foot span; the disputed weight should be 352 206 pounds, which agrees almost exactly with his formula weight.

The special merit which Professor DuBois claims for his formula for the weight of one truss is that it is rational and takes into account the panel length, depth of truss and style of truss, and is a reliable guide in deciding upon these elements.

He gives a number of examples to show the accuracy of the formula in giving total weights, which are quite satisfactory as far as they go, but equally good results over the same range could be obtained with a purely empirical formula. There are no examples of different styles of trusses, none showing the effects of different panel lengths, and only one for different depths, the 255½-foot span. The weights of one truss of this span are given for depths of 29 and 38 feet, all other conditions being the same. It should therefore be a good check.

The difference between the formula weights, it will be seen, is 16 671 pounds, and that between the actual weights 10 613 pounds, making the formula 57 per cent. in error.

It will be interesting to check the application of the formula for different panel lengths and for the combined effects of different depths and panel lengths, and to this end is submitted a study of a 255½-foot span with twelve combinations of panel length and depth. The conditions are practically the same in all these spans, only such changes being made in the construction as would occur in building them.

The live load assumed in calculations is two 86-ton Pennsylvania Railroad consolidation engines followed by a train weighing 3 000 pounds per foot, with the usual additions for impact. The stresses are 10 000 pounds in tension and 8 000 pounds in compression, reduced in chords and posts by the usual Bouscaren-Rankine formula. The wind pressure is taken at 30 pounds per square foot on twice the surface of one truss, together with a moving train surface averaging ten feet high. Wind strains are taken at 50 per cent. greater stress than given above.

Regarding the construction: The trusses are Whipple; the diagonal ties of the 14-panel spans for depths of 29 feet and 32 feet are in one

WEIGHTS OF 255-FOOT 6-INCH SPANS OF DIFFERENT DEPTHS AND PANELS.

DEPTH.	29 feet.	32 feet.	35 feet.	38 feet.
Ten panels of 25.55 feet:				
Top chord	137 698	125 968	116 380	108 186
Bottom chord	90 934	81 548	74 746	68 666
End posts	34 139	34 771	35 979	37 750
End-post struts	3 288	3 376	3 484	3 607
Com. posts and suspenders*	29 684	33 525	37 146	41 178
Diagonal ties	59 681	61 452	60 112	58 734
Pins	10 000	10 000	10 000	10 000
Trusses	365 424	350 640	337 847	328 121
Stringers	75 380	75 380	75 380	75 380
Floor-beams	25 275	25 275	25 275	25 275
Portal and lateral struts	10 201	13 651	14 591	15 476
Lateral rods	10 334	11 416	11 500	11 612
All other iron	12 500	12 500	12 500	12 500
Total span	499 214	488 862	477 093	468 364
Twelve panels of 21.3 feet:				
Top chord	138 664	126 793	117 222	108 673
Bottom chord	90 720	82 199	75 265	69 174
End posts	30 708	31 990	33 233	34 343
End-post struts	3 180	3 294	3 413	3 525
Com. posts and suspenders*	37 776	42 857	47 900	54 740
Diagonal ties	59 656	61 321	60 532	59 831
Pins	12 000	12 000	12 000	12 000
Trusses	372 704	360 454	349 565	342 286
Stringers	62 416	62 416	62 416	62 416
Floor-beams	28 626	28 626	28 626	28 626
Portal and lateral struts	11 867	16 057	16 997	17 882
Lateral rods	11 060	11 870	11 924	12 800
All other iron	12 500	12 500	12 500	12 500
Total span	499 173	491 923	482 028	476 510
Fourteen panels of 18.25 feet:				
Top chord	133 733	122 181	113 154	105 948
Bottom chord	91 307	82 875	73 310	69 220
End posts	28 083	29 820	31 634	32 414
End-post struts	3 090	3 219	3 341	3 452
Com. posts and suspenders*	44 752	51 200	57 218	63 609
Diagonal ties	59 591	59 129	61 572	61 753
Pins	10 560	10 560	13 500	13 500
Trusses	371 116	358 984	353 729	349 890
Stringers	51 748	51 748	51 748	57 748
Floor-beams	31 333	31 333	31 333	31 333
Portal and lateral struts	13 533	18 463	19 333	20 288
Lateral rods	11 986	12 990	13 065	13 234
All other iron	12 500	12 500	12 500	12 500
Total span	492 216	486 018	481 708	478 993

* The hip suspenders are made stiff, similar to posts.

length; those of the other spans are in two lengths, but the pin does not pass through the post. The top struts are lattice girders of the depth of the chord. In the 29-foot depths they are connected with the posts by angle-iron knee braces, in the other depths there are struts between the posts and diagonal rods between these and the top struts. The item "all other iron" is made up of bed-plates, 4 524 pounds; end shoes, 4 256 pounds; rollers, 1 220 pounds; and bolts, washers, loose rivets, etc., 2 500 pounds. A prototype of one of these spans actually built has been used as a check.

Some interesting relations are shown in these weights. It is evident that the panel length has practically no effect upon the total weight. It is also evident that changes in depth affect the weight less as the generally assumed economical angle of inclination of the ties (45 degrees) is approached.

The following table gives the formula and actual weights of one truss of the four extremes of the above table. The first two are those of Professor DuBois' tables, previously referred to. In all cases $(w_1 + w_2 + w_3) = 1\,943$ for one truss.

WEIGHT OF ONE TRUSS.

	Formula.	Actual.
14 panels, 29 feet deep	186 392	185 558
14 " 38 "	169 721	174 945
10 " 29 "	171 619	182 712
10 " 38 "	148 164	164 060

We have seen that with 14 panels, and depths of 29 and 38 feet, the formula is in error 57 per cent. in giving the difference in weight. For a depth of 38 feet, and 10 and 14 panels respectively, it is in error 100 per cent., and for a depth of 29 feet, with 10 and 14 panels, the error is too large to consider. Comparing the 14-panel truss, 29 feet deep, with the 10-panel truss, 38 feet deep, the formula is in error 78 per cent. With such results we are led to inquire whether the formula is really a rational formula.

Professor DuBois has rendered an answer to this quite easy by inviting special criticism upon his equations for the value of the square of the radius of gyration, upon which he says the accuracy of his formula depends.

It will be necessary, I think, to consider but one of these vital equations, viz.:

$$r_1{}^2 = \frac{(w_1 + w_2 + w_3)\, p\, C}{a^1\, \mu}.$$

This equation has no rational basis. It is an assumption in justification of which Professor DuBois appeals to the results of the application of his formula. Such an appeal would be admissible in a purely empirical formula, but certainly not in the vital step of a rational formula. The equation has a definite meaning, however, and can be readily checked. a^1 is a constant and we can substitute the values of the other factors and see how "constant" it is. I shall take a few spans that have been used in the paper under discussion.

I make C for

$$\text{Pratt truss} \ldots = p^2 (N - 2) \left(N + \frac{7}{2} - \frac{6}{N}\right)$$

$$\text{Whipple truss} = p^2 (N - 2) \left(N + 5 - \frac{12}{N}\right)$$

The 104-foot and 150-foot spans are Pratt, and the 255½-foot spans, Whipple. In all examples $\mu = 8\,000$, and $r_1{}^2$ is taken from the strain sheet.

The following values of a^1 are obtained in solving the equation :

Span.	N	p	$w_1 + w_2 + w_3$	$r_1{}^2$	a^1
104 feet....................	6	$17\frac{1}{3}$ feet	2 090	15.5	2 984
150 "	9	$16\frac{2}{3}$ "	1 950	22.5	4 152
$255\frac{1}{2}$ "	14	$18\frac{1}{4}$ "	1 943	52	6 179
$255\frac{1}{2}$ "	10	$25\frac{55}{100}$ "	1 943	52	8 600

Such variations in the value of a^1 would seem sufficient to seriously affect the rationalism of the general formula. Its aim is results, and I cannot see that its value is impaired by its empiricism provided proper results can be obtained. It already has a wide application, as Professor DuBois has shown, and I hope it will be modified to give the nice differences which he desires.

A formula which would take into account variations in panel lengths and depths would be very interesting and valuable, but would probably never be used to determine the proportions of bridges. There are too many other considerations than weight entering into the problem. It is fair to assume that the proportions adopted in practice, which are the outgrowth of sharp competition, will always be the best criteria of economy, although they may not be of excellence, and the engineer who has to decide upon these most important elements in the design of a bridge might naturally be expected to know what is being done in practice.

The weight, although the largest factor in the cost of a bridge, cannot be taken as a measure of economy. It is quite probable that the heaviest span in the table of 255½-foot spans (the one with 10 panels, 29 feet deep) would prove the cheapest. It would also be the best, except for the flat inclination of the diagonal ties, to which there seems to be a growing objection.

I can see no "practical and constructive reasons" limiting the panel to 25 feet, as Professor DuBois suggests. Within the past few years some very excellent bridges have been built with 30-foot panels. In a measure I agree with him in limiting the depth.

Mr. T. C. Clarke, M. Am. Soc. C.E., in his discussion of Mr. Joseph M. Wilson's paper, "On Specifications for Strength of Iron Bridges," in Society Transactions for June, 1886, gives an excellent epitome of "the principal improvements in bridges," from which I quote:

"V. Improvements in design.

"1. Less depth of truss.

"2. The general use of long panels."

These two lines contain the essence of goodness in modern bridges.

Henry B. Seaman, Jun. Am. Soc. C. E.—In the development of his formula, Professor DuBois has calculated his weights upon a theoretical basis, and, it appears, omits the consideration that posts and eye-bars are invariably longer than he has provided for, the posts extending a foot or more longer than assumed, and the eye-bars requiring from 16 inches to 6 feet additional material for the formation of the eyes. Does he not omit to provide for the excess at pin joints altogether?

The percentage of increase due to this I think would be quite appreciable in so accurate a formula.

Professor DuBois' method for finding uniform loads for spans, say 20 feet and under, will not produce "equivalent" loads; they are equivalent in weight, but not in moment.

G. H. Thomson, M. Am. Soc. C. E.—An old formula is $(fL + C) =$ weight per lineal foot of single-track girder in pounds where

L is the extreme length of girder.

C is a constant.

f is a factor, variable with loading and unit stresses allowed.

C in deck plates covers lateral and vertical sway, including ends, and is uniform for $L = 20$ to 80 feet. In through plates it covers laterals, knees and floor; or it can cover laterals only, and the floor-beams and track-stringers can be numerically added, as the floor is more or less variable in weight, depending upon the specification for loads, and lengths of floor-beams and track-stringers.

For heavy plate work I use $C = 60$ pounds for deck-plate and about three hundred and eighty pounds for through plates and the girders stock, with weight correspondent to the following values of f:

For $L = 25$ feet, $f = 16.0$ decreasing 0.2 per lineal foot for each foot increase of L, making $f = 11.0$ at $L = 50$ feet.

After $L = 50$ feet, f decreases at the rate of 0.1 for each foot increase of L, becoming 9.0 at $L = 70$ feet.

General formulas for weights of truss bridges are misleading, and fail because they are not based on correct principles.

A. J. DuBois, Jun. Am. Soc. C. E.—I regret that Professor Waddell has been unable to test my formula as applied to his examples, and especially that it has not occurred to him to state whether I am correct in asserting that the numerator of the strut formula used by him was taken with a sliding factor, in accord with the expression I have used for it. If this point had been decided favorably, I should be justified in claiming the agreement shown in the comparison of the formula with his results. As it is, I can at present merely call attention to the agreement as more or less corroborative.

The other points alluded to by Professor Waddell are certainly of sufficient importance to warrant discussion, but not in the present connection. It was in the hope that my formulas might furnish a reliable basis for the discussion of some of the important points of bridge design, such as depth, panel length, etc., that I presented them.

The main points to be decided are the reliability, scope and rationality of the formula. I do not present them, as several seem to suppose, as being specially quick of application, although I do not think they will be found so tedious or time-consuming as to render them unavailable. My object was to present formulas which should be, so far as possible, rational in form, and which, if proved to be accurate, might serve as guides in questions of practice. As to ease of application, if the other points are satisfactory, there is no trouble, with the aid of proper tables, in using the formulas at all comparable to the labor which would otherwise be requisite to properly settle the same questions.

The point made by Mr. Gottlieb and others, that the "best depth," properly so called, is that which is cheapest, not necessarily that which is lightest, is too sound and obvious to need comment. But it seems to me that the proper basis for settling upon the one, must involve the determination of the other. Such a process as Mr. Morse has employed, would indicate that the modification of depth due to price is but little for plate girders, and moreover that it is a nearly constant percentage. If such a result could be satisfactorily established, a formula which goes directly the one depth would be a practical solution of the other. I think, however, that Mr. Morse has not satisfactorily taken into account the item of labor, and that such a method as that employed by Mr. Thacher is more to the point, where the increase of cost due to depth is considered. Here again the basis of the solution is a knowledge of the "lightest depth;" and a formula which will give weight with sufficient accuracy for different depths is therefore very desirable.

Let me first discuss this point of accuracy so far as the various comparisons enable me to do so.

Professor Ricketts has given a comparison of results of the formulas for plate-girders. Unfortunately, not enough is given to make the comparison in any sense decisive, or to justify his conclusions. Only the length, depth, shop weights and locomotive system appear to have been certainly known. The other data required by the formula are, I understand, more or less assumed, and may or may not be in accord with the actual cases. Of course I presume that they are assumed tolerably close, but so are the results also; and the main point is still undecided, as to whether the results are not as close as they ought to be. Professor Ricketts has taken the wind-bracing as given by my formula, not the actual weight. This alone might well make all the difference, as is shown by Mr. Jenning's comparisons. Where the wind-bracing may vary for same span, and within 6 inches of same depth, from 885 pounds to 2 150 pounds, it is essential in testing the formula to know what it actually did weigh. So for the weight of floor Professor Ricketts has taken 400 pounds. This may, of course, be correct, but for deck plate-girders it would seem rather small. If a formula depending on many data is not tested for the actual values of all the data, the results cannot be expected to exactly coincide. Had there been such coincidence, under the circumstances it would be pretty clear evidence not for, but against the formula. As it is, judging from the results of others, I am inclined to believe that the formula results represent very well, not the actual weights, but what would have been the actual weights if all the data assumed had been actually used in the designs.

Mr. H. C. Jennings has given two tables which are of value. In both the weight of bracing, as given by my empirical formula, has been used, instead of the actual weight. In other respects, with perhaps one very important exception, the actual data have been inserted in the formula. If we use the actual weight of bracing, and leave out the bed-plates and bolts, we have the following results:

Span.	Weight of girders.		Total weight.	
	Actual.	Formula.	Actual.	Formula.
41	17 035	15 138	20 540	18 643
49.5	22 476	22 342	26 308	26 174
60	33 576	33 370	38 777	38 571
63	36 694	36 440	42 081	41 827
68	44 160	43 576	51 697	51 113

The agreement here is as close as could be reasonably expected of any formula. The variations, and especially the fact that all the weights of girders alone are less than the actual, would seem to indicate variations in R and a somewhat smaller actual value for R than that used in the formula. The value of R used in the formula was 9 000 pounds, the same value as used by Mr. Jennings. The only differences are using the actual weight of wind-bracing and omitting the bed-plates and bolts. The clear depths are also used in the formula; using the effective depth would give somewhat greater values and make the agreement even closer.

My formula was the clear depth instead of the effective, and disregards the web, on the supposition that in actual designing the web is not disregarded and the effective depth is used, the two errors tending to balance. The web is taken at ½ inch in the formula to allow for splice-plates, stiffeners, etc. As the girders in the preceding table were actually designed disregrading the web, and probably for effective depth, we may expect to find a slight excess over the formula. The formula results are, in fact, a little small. If the girders had been designed taking the web into account, the formula results would be a little large. In either case the formula would give very good results.

In Mr. Jennings' Table No. 2, the girders were figured for the moment of inertia of the net flange section, with average value of R, 10 000 and 9 000 pounds. It will be noticed that the formula results for girder alone, as given by Mr. Jennings, are much too small. I fail to see why a formula which gives correctly the weight of a girder, with constant flange section and ½ inch web, web disregarded in flange strain, should give a less result than the same girder, probably for varying flange section, and certainly not more than ⅜-inch web. It would seem that the test is not sound in some particular. I think the trouble is to be found in the value of R used. It is also to be noted that, through a misconception, the equivalent load for the locomotive system assumed was not used by Mr. Jennings, but only the total load occupying the span. It would seem that if the moment of inertia of the flange section is used with R 10 000 and 9 000, then in the formula we should take for a fair test somewhat more than $\frac{2}{3}$ R, or, say $\frac{5}{6}$ R, or 8 450 and 7 500. Our formula is made out for our method of figuring, and allows for that method an excess for stiffeners, splice-plates, etc. To compare it with another method of figuring it should be tested for equivalent values. It is hard to see how it can possibly give much less than actual weight under any system of design. Taking R at 8 450 and 7 500, and the equivalent loads, and using the actual values of the bracing, and omitting bed-plates and bolts, we obtain the following values:

TABLE No. 2.

Span	Weight of Girders.		Total Weight.	
	Actual.	By formula.	Actual.	By formula.
a 45	20 236	20 248	21 599	21 511
b 30	8 816	9 128	9 701	10 013
c 45	20 664	21 296	23 484	24 116
d 30	11 284	10 034	13 434	12 184

A slight change in the uncertain value for R would, it seems to me, remove such discrepancies as appear.

I do not wish to seem to put more stress upon the accuracy of the formula results than they will bear, but when the results of Mr. Pegram's comparison, given in my paper, are thus apparently confirmed by Mr. Jennings' and Mr. Thacher's, as we shall see presently, and I may add by numbers of cases of my own, I cannot but feel confident that Professor Ricketts' results would show as well if he or I only knew just what data to use. It also appears probable that the accuracy is sufficient to admit of confidence in the conclusions derived from the formula as to "least weight-depth," if I may be allowed the expression, to distinguish from least cost or "best depth," provided the formula itself is sufficiently rational.

Mr. Thacher gives for the weight of flanges and web of a longitudinal girder the expression, using his notation,

$$w + w_1 = \frac{a\,W l^2}{d\,S} + b\,t\,d,$$

where a and b are constants, depending upon whether the flanges are of constant cross-section or uniform strength, and whether the web is disregarded or not in figuring the flange area.

He recognizes that in this expression W really contains the dead weight itself, and gives a method of trial and correction by means of which it may be correctly assumed. Would it not be as well to insert it at once in the expression and solve?

Thus, $$w + w_1 = \frac{a\,(W + w + w_1)\,l^2}{d\,S} + b\,t\,d,$$

and hence $$w + w_1 = \frac{a\,W\,l^2 + b\,t\,d^2\,S}{d\,S - a\,l^2}$$

This expression is identical with my own. I do not find it necessary to change the constants to fit different methods of designing. This of course is strictly an error. If it is considered proper to do so it can be easily done, but the results of comparisons seem to indicate that for our purpose it is hardly worth while. The errors made in my expression are well summarized by Mr. Thacher himself, and it is hardly necessary to say that I admit them all. But how about their effect? By assuming flanges of constant cross-section, I am correct for short girders and get too much for long ones. By taking the web $\frac{1}{2}$ inch I always get too much; by taking the total length for effective length I get too much; by not allowing for effect of web I get too much; by disregarding rivet-holes I get too little, and so also by taking the depth instead of effective depth. I thus have a surplus amounting to but little for light girders, and increasing, as it should, with span and depth for heavy ones, which goes far to cover connections, splice and cover plates, stiffeners, etc., To claim exact compensation is of course absurd. To claim equal accuracy with special formula designed to cover special cases is equally absurd; but I think I can claim partial compensation, and an accuracy quite satisfactory. Mr. Thacher's comparison well illustrates this.

In his estimate for the 63-foot girder, there is, I think, a slight error in his weight of the flanges of the stringers. Taking my tables, the load is 58 066 pounds, or 3 871 pounds per foot, and adding say 160 for dead weight, we have 4 031 pounds. By Mr. Thacher's formula (5) we then have 49 pounds instead of 54.2.

Also in the web of the floor-beams, 56.6 pounds would correspond to a thickness of ½ inch. Taking the thickness at ⅜, we should have only 42.45 pounds.

Making these corrections, I would make the comparison thus:

		Thacher.	Formula.
Track stringers and track bolts.	Flanges and web	=162.4	165.1
	Balance (w_3)	= 7.	
Floor-beams and connections.	Flanges and web	=121.7	134.9
	Connections, etc	= 48.2	
Main girders and laterals.	Flanges and web	=479.8	615.9
	Laterals, etc.,	= 48.0	48.6
	Stiffeners, etc.,	= 45.8	

Now here is evident not a fortunate balance of errors, but an allowance or surplus for stiffness and connections, which, if not exact, at least varies, as it should, with all the dimensions, and gives in this case, as in all the others already examined, a close result. For the track stringers the formula gives a close result. The excess due to web of ½ goes far to balance the allowance for connections, and would go farther if the web were considered in flange area. The same holds true for the floor beams. For the main girder we have also an additional excess due to taking the flanges as of constant cross-section. This excess is of course liable to vary more or less, but it seems to me that after providing for stiffness, splice plates, etc., the small residue can never be a source of great error, and this conclusion seems justified by the results of the comparisons of formula with actual results, and explains the deviations, never very large, which appear.

In his remarks upon "least weight" depth, Mr. Thacher gives the depth for (23) as $d = \sqrt{\dfrac{.5903\ W l^2}{40\ t S}}$. This seems to me slightly in error. It assumes the term 2.361 A as unaffected by d. But really the constant flange angles support a certain proportion of the whole load, say $a\ W$, and this would give us $d = \sqrt{\dfrac{.295\ W l^2\ (2 + a)}{40\ t S}}$, and this again is not strictly correct, because the dead weight is itself included in W. We should strictly have

$$d = \frac{.295\,(2 + a)\,l^2}{S} + \sqrt{\frac{.295\ W\,l^2\,(2 + a)}{40\,t\,S} + \left[\frac{.295\,(2 + a)\,l^2}{S}\right]^2}$$

where W does not include the dead weight. The effect of both corrections would be to rèduce the difference in depth between case (2) and case (3). It seems to me then that I am not without justification in avoiding a change of constants to fit different cases, though, if thought necessary, this can easily be done. With such change, and instead of our formulas for wind-bracing, usual to suit different styles, my formula would become essentially the same as Mr. Thacher's method. Unless actual tests show this to be imperative, I should dislike to do it. All tests thus far given do not, it seems to me, indicate such a necessity.

Whatever may be thought of the accuracy of the formula and method as applied to plate-girders of large span, as far as regards track stringers and cross-girders, the results, it seems, ought to be quite good. If so, then at least the floor system can in any case be accurately estimated, and by the aid of tables similar to those given in my paper, very speedily estimated also.

It remains only to discuss the truss formula. With the values for A once made out and tabulated, this is ready enough of application. Its accuracy seems very thoroughly established if the comparison with Mr. Pegram's cases and the results of Mr. Hughes are considered, so far at least as the scope of these comparisons extend. From the latter we see that in twelve spans of all sizes, of different styles, and built according to different specifications, aggregating 1 521 017 pounds in all, the formula gives an aggregate of 1 524 216 pounds, or only about 3 200 pounds in excess. The fluctuations, almost without exception, are such as can be attributed to fluctuations in μ, or to the excess of actual sections over those required. Even an accurate, perfectly unexceptionable, rational formula must show such variations above and below the actual weights. That the formula should be entirely rational is too much to expect. That it is mainly rational, and in form wholly so, I believe.

In my criticisms of the 104-foot span alluded to by Mr. Jennings, I did not of course imply that the depth chosen was not the best as determined by the special circumstances, but simply used it as an illustration of how the effect of change of depth could be easily studied in connection with change in panel length.

Mr. Pegram is quite correct as to my misunderstanding of the 51 and 59 per cent. deviations. It was an inadvertence on my part. As Mr. Hughes gives these same spans with a deviation by my formula of 0 and + 3¾ per cent., I should be loth to consider them as "unreasonable on their face." They may indeed not conform to certain standards, but, what seems of more importance, the formula conforms itself to their construction, as a rational formula should do with any span, standard or not, when actual data are inserted.

In the remark quoted by Mr. Pegram as to the "simple formula made off-hand," I fail to see the force of his reply. I had already in my paper given a formula for truss weights, which agreed well with actual weights, and shown that by its aid and my tables total weights were also given with equal accuracy. It seems to follow that if the simple formula alluded to also gives the truss weights closely, precisely the same use of it as of the other would likewise closely give total weights. It is only necessary to add to the truss weight per foot, $w_2 + w_3 - 200$ to get half total weight per foot.

This simple formula is indeed thoroughly rational as far as it goes. It is well known in the history of the subject. For any given system and standard specifications the limiting length may be taken as practically constant, and I am convinced that with a series of values for L to match Mr. Pegram's coefficients it will give truss weights quite accurately, and, combined with any good method of estimating floor, will give with equal accuracy total weights.

In the partial quotation to which he objects, where he indorses the girder formula, it did not occur to me as necessary to quote the remainder, because it referred in part to another truss formula not given in my present paper; and that part of it which referred to the girder did not seem to me, in view of the causes of variation I have already alluded to, entirely justified by the comparison. It is, however, quite in order that Mr. Pegram should call attention to the limitation of his indorsement, and if my statement of it is likely to give rise to misconception, it is unintentional on my part, and I am glad to be corrected.

In the live-load system referred to as somewhat in excess of Mr. Pegram's, I had in mind the locomotive only. The car loads which follow are designedly light. In the tables referred to in this connection in my paper, I found it just as well to take concentrated car loads, and to take them light. By doing so an increase of tabular values is easily figured out to suit any given specifications. In all my comparisons I have taken the loads as given, and therefore it does not seem to me that the fault is very "serious."

I do not understand the statement that there are no examples of different styles of trusses and none showing the effects of different panel lengths. In the table of Mr. Pegram's cases are two styles, single and double intersection. In Mr. Hughes' there are two styles and also different specifications for these styles. I have noticed in my paper the effect of double track and have given a case of Warren girder, and also given several examples of the effect of change in panel length both for constant and varying depth. It is quite true that I have given no actual weights of different styles myself for comparison with the formula, and it is quite proper that I should not. I wish the formula results to be compared with the actual weights furnished by others, not by myself, and if Mr. Pegram or any one else feels the lack of further examples for comparison, they are invited and desired to furnish them. That is the object of this discussion.

Mr. Pegram in his comparison of the 255.5-foot span with the results of formula, speaks of percentages of error in the differences of weight. The curve represented by my formula, for the depth variable, is an hyperbola. His comparison shows that at different points there is not exact correspondence, and also that at these points the inclination of the tangent to the curve is not the same as for the actual curve. This may well be; it is even to be expected, and yet there can be substantial agreement for all that. If the results of formula are found to vibrate, with but small error, from one side to the other, and yet within a large range of actual comparison give accurate results, it can be assumed that the formula represents the facts. This is the point to be established, and it seems to me that this is the method by which to test it, viz., by actual comparison. I see no object in Mr. Pegram's method of comparison except it be desired to obtain as large percentages of errror as possible. This method might easily be made to seemingly discredit an exceedingly accurate formula. The percentages are large while the actual errors are easily covered by all the attainable fluctuations in design. More tests of just such results with a large number of spans, so that we may see, not the percentage of error in differences in any one particular case, but the errors in actual amounts for a large variety of cases, are just what is needed. Such tests are furnished to some extent by the tables I have given of Mr. Pegram's and Mr. Hughes' cases, and I am desirous to obtain many more.

The results and the construction of the formùla lead me to believe that it is mainly rational, and to be depended upon for the effect of changes in the data within a sufficient range, provided the best constants can be determined. It may well be that this I have not done. But thus far I have no data upon which to base better ones, and the results of actual comparison are certainly very good. It will be observed that there are but two empirical constants, 45 and 202. If I wished then to fit the formula to Mr. Pegram's results, I could at most only make it fit any two, and it would have to take its chances with the rest. Yet, out of Mr. Pegram's fourteen cases of five groups, it is almost exactly right in one case in every group through the whole range. The last case of 320-foot span turns out to have been correctly given by the formula. It is under two per cent. in four cases more, and its greatest variation in the rest is less than 5 per cent. In the twelve cases given by Mr. Hughes, it hits almost exactly three cases, and is below 4 per cent. in all but five cases. Such variations as there are might be owing to slight variations in loading and unit stress, even in an exact formula, and especially to excess of actual sections over those required. This is certainly a good showing, as the cases cover a large range, include different styles, and are built under different specifications.

Mr. Pegram is quite right in giving special attention to the expression for r_1^2, the square of the radius of gyration for the upper chords. I especially invited discussion on this point, and considering its importance, I could have wished that Mr. Pegram's treatment had been somewhat more thorough. When he speaks of it as a mere assumption and having no rational basis, I hardly know how to interpret the words. Does he mean that it is pure assumption to say that r_1^2 increases with the weight of the chord or with the loading, and that such assumptions are irrational? It would seem that the basis is as rational as can be desired. The form only is to be criticised.

Mr. Pegram proceeds to test this expression. To do this he shows that putting for r_1^2 its actual value, the constant in the expression is not constant. Now, it may appear paradoxical to state that if the value of r_1^2 as given by the invention of a constant for a^1 had checked exactly the true values of r_1^2, that the fact would have proved the expression to be incorrect. But such is the case. The value of r_1^2 given by the expression in question is not the actual value for the chords as found from the strain sheet, nor is it meant to be. If it were, then it would itself need to be reduced by a varying amount to allow for details, lattice bars, pin-plates, etc. Mr. Morse has very properly noticed this point, and if the expression as it stands had really stood Mr. Pegram's test and closely agreed with his values, it would have shown a clear case of "overloaded constant," no provision being made for details, and Mr. Morse's objection would have been sustained.

Now, it seems to me no pure assumption, but to have a sound rational basis, to assert that the actual value of r_1^2, as found from the strain sheet, ought to increase with $(w_1 + w_2 + w_3)\ C$, which is proportional to the area, and ought to vary inversely as μ and N. We have then for the strain sheet values,

$$r_1^2 = \frac{(w_1 + w_2 + w_3)\ C}{a\ \mu\ N.}$$

If we test this as Mr. Pegram has done, to see how constant a is, we have the following results.

Span.	N.	p.	$w_1 + w_2 + w_3$.	r_1^2.	a.
104	6	$17\frac{1}{3}$	2 090	15.5	28.7
150	9	$16\frac{2}{3}$	1 950	22.5	29.6
$255\frac{1}{2}$	14	$18\frac{1}{4}$	1 943	52	24 2
$255\frac{1}{2}$	10	25.55	1 943	52	33.5

A value of a not far from 30 would it seems give a good average value for r_1^2. It is not stated whether the values of r_1^2 given are the greatest values or average for the span. The variation of a is slight and it can be practically taken as constant. Perhaps if average values of r_1^2 were used in testing, the variations of a would be still less. As it stands, it seems to me that the expression just given for r_1^2 will give very good values for r_1^2, if a is taken about thirty.

Now, as Mr. Morse points out, the details are a varying percentage. If we multiply numerator and denominator by p, we have

$$r_1^2 = \frac{(w_1 + w_2 + w_3)\,p\,C}{a\,\mu\,l}$$

If we take as our varying percentage $\frac{l}{a^1}$, which gives us just such an allowance as Mr. Morse insists on, we have

$$r_1^2 = \frac{(w_1 + w_2 + w_3)\,p\,C}{a^1\,\mu}$$

which is precisely the expression given in my paper. It will now of course no longer check the actual values of r_1^2, and just because of this it does take account of details, and it seems to me the constant is not "overloaded."

As with this single exception everything in the formula is strictly deduced, and the form is thus rational, it would seem that if this single exception is in accord with reason and meets Mr. Morse's objection, and at the same time Mr. Pegram's, an appeal to the results of the application of the formula is quite in order. All such appeals thus far have been quite satisfactory. If the error in differences noticed by Mr. Pegram is considered important, I should expect it to be easily remedied by a better choice of constants, thus bringing the formula curve into close accord with the actual. The main point is whether the form and character of the formula curve is correct. If so, its course can be easily guided. More studies like Mr. Pegram's, of the same span with different depths are needed. If the formula can be fitted to these and still give as good results as at present, it would certainly be a reliable guide in questions of proportion.

The point made by Mr. Seaman, that the theoretical weight does not take account of full length of posts or of additional length of eye-bars required for heads, is well taken. Two ways of meeting it are open. Either to insert the addition as a percentage of increase to the theoretical value for A, or to take account of it in the expression

$$\frac{a\,p^2 + b\,d^2}{(w_1 + w_2 + w_3)\,p}$$

I find that this expression, with the constants 45 and 202 as given, seems to cover both the material required for long struts and this also. The same remarks which cover the objections of Mr. Pegram and Mr. Morse as to value of r_1^2 and "overloaded constant" apply here. The expression is of the same form as the theoretical value of A, and the results seems to establish that a and b are practically constants.

As to Mr. Seaman's remark upon my method of finding uniform load, I do not catch the force of it. I should certainly in finding "equivalent load," find that uniform load which gives the greatest moment caused by the actual loading. So far as the live-load system is concerned, I do this. To it I add the loading due to floor system. For a span of 20 feet, or any other span, I have intended to give for the uniform loading the equivalent load for the load system adopted, increased for impact, and add to it the floor system. In all my comparisons I have of course taken the load as given by those who furnish the example.

The statement of Mr. Thomson, that general formulas for the weight of truss bridges are misleading, and fail because they are not based on correct principles, seems to me to be itself misleading by reason of its generality. He would hardly assert that the weight of any special span, with given data and specifications, cannot be correctly estimated. But in the derivation of my formula I have followed very closely, step for step, the method which would be pursued in any special case. The principles which hold for the one are applied in the other. The strain in every member is found, its area and volume and weight determined, and then the weights added. Of course it does not follow that a problem which admits of special solution in any given case can always be solved generally, but if not, a good reason can usually be given. Certainly, incorrect principles in the present case cannot be put forward consistently as a reason by any one who uses identical principles in the solution of special problems. My formula is based throughout upon accepted statical principles, and with the single exception of extra material for stiffening, etc., is accurately rational. The allowance for this extra material seems to me also rational in its form, and the results seem thus far to justify it.

I give on opposite page a tabular statement of results obtained by using my formula and tables, which may be of interest. All the spans are double intersection. The "least weight depth" is given in each case.

Span.	No. of panels.	w_1	w_2	w_3	Depth.	Total weight of iron.
100	4	1 625	376	26	25	76 500
	5	1 625	357	33	22.5	78 000
	6	1 625	347	39	20	80 400
	7	1 625	341	46	17.5	84 200
110	5	1 643	365	31	25.5	90 860
	6	1 643	352	38	23	92 180
	7	1 643	344	44	21.5	95 580
120	5	1 637	373	30	28.5	95 500
	6	1 637	357	36	26	96 720
	7	1 637	348	42	24	97 920
130	5	1 625	380	38	32	117 000
	6	1 625	364	41	29	124 280
	7	1 625	353	43	26.5	128 180
140	6	1 610	370	37	31.5	134 400
	7	1 610	357	43	29	139 160
	8	1 610	350	50	27	141 680
150	6	1 588	376	38	33	150 000
	7	1 588	363	44	32	152 100
	8	1 588	353	50	30	157 200
160	7	1 560	368	45	34.5	168 960
	8	1 560	357	51	32	173 440
	9	1 560	350	57	30	178 560
170	7	1 538	374	45	37	186 660
	8	1 538	362	51	35	191 420
	9	1 538	354	58	32.5	197 880
180	8	1 511	367	52	37	208 080
	9	1 511	357	59	35	216 360
	10	1 511	351	65	33	222 120
190	8	1 486	372	52	39.5	228 000
	9	1 486	361	59	37	234 460
	11	1 486	354	66	35	242 440
200	8	1 460	376	53	42	246 800
	9	1 460	362	59	40	252 400
	10	1 460	357	66	38	261 600
210	8	1 435	381	53	44.5	267 540
	9	1 435	370	60	42	275 200
	10	1 435	361	66	39.5	283 080
230	9	1 386	378	60	47	317 400
	10	1 386	369	67	44.5	329 360
	12	1 386	354	80	40	349 600
250	10	1 348	376	68	49	377 500
	12	1 348	360	81	44.5	400 500
	14	1 348	351	95	40.5	429 000
270	10	1 310	384	68	50	428 760
	12	1 310	367	82	49	456 300
	14	1 310	355	96	44.5	487 620
300	12	1 260	376	83	50	549 000
	14	1 260	363	97	50	588 000
	16	1 260	353	110	47	625 800

As to Mr. Thomson's formula for plate girders, it takes the form for total weight $(21 - 0.2\,l)\,l^2 + b\,l$ for spans under 50, and $(16 - 0.1\,l)\,l^2 + b\,l$ for spans between 50 and 80, where $b = 60$ for deck and 380 for through girders.

Mr. Thomson speaks of this as if the first term represented the girder proper and the last term the sway bracing, etc. The formula has no such meaning. It is simply an application of the familiar expression

$$a + b\,l + c\,l^2 + d\,l^3 +, \text{ etc.}$$

which has been made the basis of so many empirical formulas in the sciences. If the quantities to be represented form a straight line, the first two terms are used. If a curve, three or more. Mr. Thomson's formula is but the application of this, and as it has practically six constants for the small range from 20 to 80 feet, it can be fitted accurately in six places to any series of values, and will of course give good intermediate values. The constants thus determined, however, are good only for girders of a certain style and for personal practice, and worthless for any one else.

Thus, take the deck-plate girders given by Mr. Pegram in the February Transactions, 1886, for Class C. If we fit Mr. Thomson's formula to these cases at 20, 50 and 80 feet we have for the weight $\left(7.4 + \frac{l}{290}\right) l^2 + 192\,l$. This formula is very different in the constants from Mr. Thomson's, but gives exceedingly close results for Mr. Pegram's cases, without any change for spans over 50. I presume it would not fit Mr. Thomson's practice, however, while on the other hand Mr. Thomson's formula does not at all fit Mr. Pegram's practice, as will be seen from the following tabulation :

Span.	Formula.	Thomson.	Actual weight.
20	6 827	8 000	6 841
30	12 510	15 300	12 732
40	19 740	23 200	19 283
50	28 530	30 500	28 561
60	38 880	39 600	38 590
70	50 876	48 300	49 983
80	64 448	56 000	64 561

If the problem be merely to reproduce the results of special practice by an empirical formula, this is easily done, and Mr. Thomson's form is a very old and favorite one for all such purposes. It can be fitted to Mr. Pegram's results with much better accuracy than the form he has adopted, as I have satisfied myself by actual trial and comparison through the whole range. The result of any such formula is at best but a tolerable estimate, varying more or less from the actual according as the case in hand departs more or less in dimensions, loading, etc., from the practice upon which the formula is based.

I believe that a better empirical formula for trusses than any thus far proposed can be given by the form already alluded to, viz.: $w_4 = \dfrac{w_1 + w_2 + w_3}{\dfrac{L}{l} - 1}$, where the limiting span L may be given by a simple empirical formula. Such a formula would have the advantage of a rational form, and might therefore be expected to follow better the actual curve. It would also take account of loading and depth as well as span. Even if we take L as constant, the result as we have seen is very good.

I would therefore propose for $\dfrac{L}{l}$ the form $\dfrac{\mu\, d}{a\, l^2 + b\, d^2}$, as coinciding with the theoretic form already deduced. Determining the values of a and b from spans 104 and 320 as given by Mr. Pegram, we have for $\mu =$ 8 000, $a = 0.635$ and $b = 25$. Here then is an empiric formula of wider scope, which follows change of depth and loading with considerable accuracy, as shown by the following tabulation. Better values for the constants a and b may undoubtedly be found. The preceding are hastily formed from two extreme cases only.

Span.	Depth.	No. of panels.	$w_1 + w_2 + w_3$	Formula weight of one truss.	Actual weight.	Difference.	Per cent. of difference.
104	24	6	1 820	23 649	23 658	— 9	0
			2 000	26 000	24 458	+ 1 542	$+ 6\frac{3}{10}$
			2 090	27 170	25 074	+ 2 096	$+ 8\frac{35}{100}$
150	25	9	1 675	44 250	46 032	— 1 782	$- 3\frac{87}{100}$
			1 844	48 750	49 628	— 878	$- 1\frac{77}{100}$
			1 950	51 600	52 627	— 1 027	$- 1\frac{95}{100}$
201.5	28	12	1 565	80 197	85 131	— 4 934	$- 5\frac{8}{10}$
			1 776	90 675	93 626	— 2 951	$- 3\frac{15}{100}$
			1 946	99 742	100 283	— 541	$- 0\frac{53}{100}$
320	34	20	1 605	271 680	273 671	— 1 991	$- 0\frac{73}{100}$
			1 798	304 320	299 718	+ 4 602	$+ 1\frac{53}{100}$
			2 088	353 280	352 206	+ 1 074	$+ 0\frac{35}{100}$
255.5	29	14	1 943	183 193	185 558	— 2 365	$- 1\frac{27}{100}$
255.5	38	14	1 943	170 163	174 945	— 4 782	$- 2\frac{73}{100}$
255.5	32	14	1 943	176 550	179 616	— 3 066	$- 1\frac{7}{10}$

There are undoubtedly other elements than that of weight alone entering into the problem of proportions. But so long as the weight is the basis of all discussion, the effect upon weight of change of dimensions must be of importance. The outgrowth of practice under sharp competition is to be relied upon, but such a formula ought to aid such growth and give valuable hints to practice. The conclusion of modern practice as to long panels, for instance, is justified by the formula, and such a formula in the beginning might well have facilitated, and even hastened, the conclusion.

If I cannot then altogether agree that the present attempt has been as thoroughly tested and discussed as it should be, or that its value has been thus far conclusively settled, it is not because I am willfully hard to be convinced. Nor would I be thought to treat with scant ceremony the conclusions of those whose opinions are justly entitled to weight as those of acknowledged experts. I trust it will not be considered obduracy if I still ask for further tests.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
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TRANSACTIONS.

(Vol. XVI., June, 1887.)

DISCUSSION ON FORMULAS FOR THE WEIGHTS OF BRIDGES.*

WILLIAM H. BURR, M. Am. Soc. C. E.—In this paper Professor DuBois has undoubtedly framed a formula of very considerable accuracy, provided the quantities which enter into it and which depend upon other formulas are accurately computed. A formula which gives the weight of iron in any railway truss bridge must have its value based upon two considerations, one of which is its accuracy and the other its ease of application; such a formula would have value in either one of two uses.

In the first place, if it were sufficiently accurate to be used as a basis of a tender, any reasonable amount of complication would not be of importance. On the other hand, if the formula were not sufficiently accurate to give a reliable weight on which the contract price of a bridge is to be founded, any complication in its form or application would render it of little value for approximate purposes.

I believe that there are few engineers who would be willing to hazard an important tender on any formula which has yet been devised, including the one under consideration. The great variety in the specifications now used by railroad companies and their engineers, leads to an equally great variety in the weights of the iron for a span of a given length, whether of a single or a double-track structure.

If it can be shown that the formula of Professor DuBois will give the

* This is a continuation of the Discussion on the Paper on Formulas for the Weights of Bridges, by A. J. DuBois, Vol. XVI, page 191, May, 1887.

weights of iron in spans of different lengths and for different specifications within two or three per cent., it may be considered as sufficiently accurate for use in making tenders in close competition, but not otherwise. It is safe to say that neither this nor any other formula which is or can be devised with reasonable simplicity will fulfill this condition. But if a formula is not sufficiently accurate for the purposes of close estimate, it still may possess considerable value in making those approximate estimates of cost which are so frequently required in ordinary practice. Such a formula, however, should be very simple, and that under consideration does not meet this requirement.

In the first place, we find that in order to obtain a result from the application of Equation 3 in Professor DuBois' paper, at least four quantities which enter it must be computed by other formulas; and we find, furthermore, that one of them, w_1, involves a very considerable amount of labor, and after it is obtained it is entirely an imaginary quantity, and one which possesses no real existence.

After having found these four quantities therefore, with considerable labor, from other formulas, they are to be inserted in a formula of considerable complication, and one from which a numerical result cannot very readily be obtained. It does not appear therefore that the formula can be used for purposes of an accurate estimate, nor yet be quickly and simply used for the purposes of an approximate estimate. I believe it is true that the claim of Professor DuBois for increased accuracy over other formulas is well founded. It is probably by far the most nearly accurate formula which has yet been devised. I only wish to point out the facts that it is not sufficiently accurate to form the basis of a tender in ordinarily close competition, nor is it sufficiently simple to admit of quick use for the purposes of an approximate cost.

The use of an equivalent uniform moving load is a matter of which much has lately been said, and it would seem to appear that it is supposed that there is a real uniform load per foot, which is in some sense or other equivalent to any given system of concentrated moving loads; but such is not the case. There is no such thing whatever as a uniform moving load which is equivalent to a given system of concentrated loads for any length of span, long or short. It is very true that there may be found a uniform load per lineal foot which will produce the same bending moment at any section of a truss as a given system of concentrated loads, or which will give the same shear as that system of moving loads in any given web member; but that so-called equivalent moving load will not give the same moment, or the same shear, in any other parts of the structure.

Nothing can militate against the accuracy of any formula, therefore, with greater certainty, than this assumption of the use of an equivalent moving load, especially if, as in the present paper, the definition of that equivalent load is omitted.

A formula for the weight of iron in a given truss, and under a given system of moving loads, of sufficient accuracy to take the place of a detailed estimate, is very much like an accurate long-column formula; both are absolutely impossible.

A. J. DuBois, Jun. Am. Soc. C. E.—Professor Burr justly remarks that variety in specifications leads to equal variety in the weight of a given span; but he does not recognize that my formula accommodates itself to different specifications and thus actually gives the weight in accordance with the specifications adopted.

If it can be shown that the formula gives weights within two or three per cent., it may be used in making tenders. This condition Professor Burr thinks it safe to say neither this nor any other formula can ever fulfill with reasonable simplicity. Why? The problem is of easy solution in any special case. It rests mainly upon static calculation, which can be perfectly generalized. Why should not the generalized calculation be as reasonably simple as the special solution? Of course, as I have already admitted, it does not always follow that because a special problem can be solved, therefore the problem can be generalized; but it generally does, and when it does not the reasons why can always be clearly stated. If such valid reasons exist in this case they have not so far been clearly stated in this discussion. Indeed Professor Burr's admission that this is "by far the most nearly accurate formula which has yet been devised," would seem to indicate that these reasons are not insuperable. The accuracy is simply the result of following, step by step, in the generalization, the successive operations of the special solution. That the attempt to do this even imperfectly results in the "most nearly accurate formula yet devised," seems to me significant. What *prima facie* valid reasons are there why this may not be still more perfectly done and a still more accurate formula result? With a margin of three per cent. to cover minor irregularities, I am of opinion, after considerable studying of the subject, that this can be done.

Professor Burr over-estimates the labor of using the formula. The four quantities he alludes to can all be tabulated in accordance with any specification, once for all. Any one using the formula in practice would provide first his tables. Any bridge engineer has the data for such tables at hand. These tables once formed, the application of the formula is about as simple and easy as any of the far less accurate and less rational ones now in use. The same objection might as reasonably be made to one proposing logarithms as an aid in calculation. The logarithm must first be computed at considerable labor; but how many compute the logarithms they use? Properly fitted for use therefore, the formula will admit of quick use for approximate purposes, and it is admitted to have greater accuracy. In addition it fits any specifications and takes account of style of truss. It appears to me therefore that it is worth the trouble to any engineer to furnish the tables which thus render it available.

The error due to equivalent uniform load is committed in the interests of simplicity. Its effect in militating against the accuracy of the formula can only be determined by trial. Such comparisons as have thus far been made certainly do not show that the effect is great.

I believe that the Professor is too scientific to pronounce any problem "absolutely impossible" unless there are valid reasons in the nature of the problem itself which justify the assertion; the problem of perpetual motion, for instance, or squaring the circle. If I am laboring upon such a problem I hope, for the sake of our common humanity, he will spare enough time either privately or publicly to enable me, if not unfortunately too far gone already, to get out of the toils.

To compete with a detailed estimate by any method less complete and thorough is, I admit, "impossible" in the nature of things. But with a margin of three per cent., and a similar method at bottom in both cases, it would seem to me that the question is not closed.

Professor Burr neglects to mention the aid which a rational formula as accurate as this ought to be in determining the influence of variation in dimensions, etc. This, it seems to me, should not be entirely ignored.

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NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

360.

(Vol. XVI.—June, 1887.)

A MASONRY DAM.

By JOHN W. HILL, M. Am. Soc. C. E.

READ SEPTEMBER 15TH, 1886.

WITH DISCUSSION.

Eden Reservoir is the principal settling and distributing basin of the Cincinnati Water-works; contains two divisions, one of 57 000 000 gallons, and the other of 43 000 000 gallons nominal capacity; and receives a daily pumpage from the Ohio River of 15 000 000 to 20 000 000 gallons.

The site of the reservoir was a natural ravine, across which were built two masonry walls, one forming the division wall between the two sections or basins, and the other the main retaining wall, or dam, placed lower down in the course of the ravine.

The ravine was filled with earth, and the filling covered with a concrete floor 12 inches thick (in both sections of reservoir), to a level within 29 feet of the crest of the division wall, and within 35 feet of the coping of the main retaining wall.

In addition to filling the ravine, there was considerable excavation of the slopes of the upper basin to increase the capacity of the reservoir, and the slopes (2 horizontal to 1 vertical) were all paved with limestone blocks, set in cement-mortar.

The natural soil in the ravine was excavated to the rock and marl strata; leveled off and stepped up the sides of the natural slopes as shown in Plates XX and XXI, and the walls built of lime-stone obtained from the neighboring hills.

The work was commenced in January, 1866, and the division wall and basin above it completed during 1872; but no use was made of it until October, 1874. The main retaining wall and the lower basin were finished and put in service late in 1878.

As shown by the plan of the reservoir (Plate XIX) the basins are of irregular form, and the slopes follow the natural contours of the ravine, excepting where the excavation in the upper basin somewhat changed these to increase the storage room.

Under the floors of the reservoir, sewers were laid in the natural soil to intercept and conduct away the subterranean drainage; these united in a single outfall sewer of stone masonry of 5 by 6 feet diameters, which conducts the drainage into the Ohio River.

The division wall was constructed with an influent chamber at one end and an effluent chamber at the other, between which the length of wall at the crest is 307 feet.

This wall (Plate XX) is trapezoidal in section; has a width at crest under coping stones of 10 feet; a height at center of 67.5 feet, and a base width at same point of 30 feet.

The division wall has a batter alike on both sides, and is of uniform stability and strength when subjected to the lateral pressure of the water in either section of the reservoir, with the opposite section empty. Under ordinary conditions of service the depth of water in the reservoir is 30 feet, or 1 foot over the crest of division wall.

That portion of the division wall above the floors of the reservoir is of uniform section throughout its length, and has a top width of 10 feet, a bottom width of 19 feet, and a height equal depth of water of 29 feet.

Estimating the weight of the masonry at 144 pounds per cubic foot, and the coefficient of friction as 0.60, the vertical pressure on the horizontal joints; then the frictional stability of the upper 29 feet of wall, with one section of the reservoir full and the other empty, is

$$\frac{10+19}{2} \times 29 \times 144 \times 0.60 = 36331.2 \text{ pounds}$$

per unit of length, and the horizontal thrust of the water is

$$\frac{29^2 \times 62.5}{2} = 26281.25 \text{ pounds,}$$

and the ratio of frictional stability to horizontal thrust of the water, neglecting the cohesion of the mortar in the joints, is

$$\frac{36331.2}{26281.25} = 1.38.$$

Neglecting the vertical component of the prism of water sustained by the batter of the wall, and taking the center of gravity in the vertical center of the wall, the leverage resistance is

$$\frac{10 + 19}{2} \times 29 \times 144 \times 9.5 = 575\ 244 \text{ pounds,}$$

and the overturning moment,

$$\frac{29^2 \times 62.5 \times 29}{2 \times 3} = 254\ 052 \text{ pounds,}$$

and the ratio of leverage resistance to overturning moment =

$$\frac{575\ 244}{254\ 052} = 2.225.$$

Of course the vertical pressure of the water on the battered surface of the wall, and the cohesion of the mortar in the joints, have an actual effect to increase the stability of position as well as to increase the frictional stability.

According to the method of the French engineers, which neglects the frictional stability and stability of position, but fixes the strength of the wall at any bed joint by the pressure developed at the extremity of that joint remote from the thrust, we have under same conditions of loading the following result.

Vertical component of the prism of water resting upon the batter of the wall, 4 078.125 pounds.

Horizontal position of the center of gravity of the prism of water from vertical line cutting the wall 29 feet from the crest = 1.5 feet.

Weight of the masonry in wall (above joint) 29 feet below crest, 60 552 pounds.

Horizontal ordinate of center of gravity of the section of wall,

$$x = \frac{1}{3}\left(a + b - \frac{ab}{a+b}\right)$$

where a = width of wall at top; b = width of wall at given joint, and x = horizontal ordinate of center of gravity. Inserting values and working,

$$x = \frac{1}{3}\left(10 + 19 - \frac{10 \times 19}{10 + 19}\right) = 7.483 \text{ feet.}$$

Vertical ordinate =

$$Y = \frac{H}{3}\left(\frac{2a + b}{a + b}\right)$$

where H = vertical depth, or depth of wall to given joint, and Y =

vertical ordinate from given joint to center of gravity. Inserting values and working,

$$Y = \frac{29}{3}\left(\frac{2 \times 10 + 19}{10 + 19}\right) = 13.000 \text{ feet.}$$

Horizontal distance at depth 13 feet from floor of reservoir, from face of wall to vertical line bounding the water prism,

$$\frac{4.5 \times 13}{29} = 2.017 \text{ feet,}$$

and, horizontal distance of center of gravity of section of wall from same vertical line =

$$7.483 + 2.017 = 9.5 \text{ feet.}$$

Horizontal distance of center of gravity of the combined prism of water and section of wall from vertical line bounding the water prism =

$$x = \frac{w\,x_a + W\,x_v}{w + W}$$

Where w = weight or vertical component of the prism of water,
" W = weight of the section of masonry above given joint,
" x_a = horizontal ordinate of center of gravity of water prism from its vertical boundary,
" x_v = horizontal ordinate from same vertical to center of gravity of section of masonry,
and x = horizontal ordinate of center of gravity of combined prism of water and section of masonry;
inserting values and working,

$$\begin{array}{rr} 4\,078 \times 1.5 = & 6\,117 \\ 60\,552 \times 9.5 = & 575\,244 \\ \hline 64\,630 \qquad & 581\,361 \end{array}$$

and,

$$x = \frac{581\,361}{64\,630} = 8.995 \text{ feet,}$$

and horizontal distance of center of pressure from center of gravity =

$$x_1 = \frac{P\,h}{3\,W}$$

Where P = horizontal component of the water pressure at face of wall,
" h = depth of water,
" W = weight of masonry,
and x_1 = horizontal distance from center of gravity to center of pressure;
inserting values and working,

$$x_1 = \frac{26281.25 \times 29}{60\,552 \times 3} = 4.195 \text{ feet.}$$

The center of pressure is distant from the edge of the join

$$u = 19 - (8.995 + 4.195) = 5.81 \text{ feet},$$

and the resultant pressure per square foot at edge of joint is obtained from one of the two expressions,

$$P_y = \frac{2}{3}\left(\frac{W}{u}\right) \qquad (A)$$

or,

$$P_y = 2\ (2t - 3u)\frac{W}{t^2} \qquad (B)$$

Where $W =$ weight of masonry above joint,
" $u =$ distance from center of pressure to extremity of joint,
" $t =$ width or length of joint from face to back of wall,
and $P_y =$ pressure per square foot at extremity of joint.

$$(A) \text{ applies when } u < \frac{t}{3}$$

$$(B) \text{ applies when } u > \frac{t}{3}$$

Substituting values we have, for

$$P_y = \frac{2}{3} \times \frac{60552}{5.81} = 6948. \text{ pounds,}$$

or 48.25 pounds per square inch.

The limiting pressure, according to French authority, is 92.5 pounds per square inch, although this pressure is largely exceeded in many existing works.

This wall has frequently heretofore, and is at the present time, required to act as a dam with the upper section of the reservoir in service and the lower section empty.

The pressure at the base of the wall, including the thrust and counter-thrust of the earthfill, taking the angle (φ) which the plane of fracture of the earthfill makes with a vertical cutting the base of wall—at 25^0, the weight of the material in the fills at 100 pounds per cubic foot, and the vertical depth of a mass of earth the base load of which, per unit of area, equals the vertical pressure of a column of water 29 feet high or deep, as $\frac{29 \times 62.5}{100} = 18.125$ feet, and depth of fill assumed for calculation $= (67.5 - 29) + 18.125 = 56.625$ feet.

Then the horizontal component of the earth-fill on the water side of the wall is,

$$P_3 = \frac{h_1{}^2 \times tan.^2\ \varphi \times 100}{2} - \frac{h_2{}^2 \times tan.^2\ \varphi \times 100}{2}$$

Where $h_1 =$ vertical depth of actual and assumed fill of earth $=$ 56.625 feet.

Where $h_2 =$ depth of assumed fill above actual fill $= 18.125$ feet, and $P_3 =$ the horizontal component of the actual fill, with 29 feet of water in section of reservoir upon one side of division wall; substituting values and working,

$$P_3 = \frac{56.625^2 \times 0.466^2 \times 100}{2} - \frac{18.125^2 \times 0.466^2 \times 100}{2} = 31\,247.35$$

pounds.

The point of application of P_3, above base of wall will be $Y_3 = \frac{2}{3} \times \frac{h_1{}^2 + (h_1\,h_2) + h^2{}_2}{h_1 + h_2}$ = vertical distance from surface of actual and assumed fill to point of application of P_3; or,

$$Y_3 = \frac{2}{3} \times \frac{56.625 + (56.625 \times 18.125) + 18.125^2}{56.625 + 18.125} = 40.68.$$

and 56.625 feet — $Y_3 = 56.625 - 40.68 = 15.945$ feet.

The horizontal component of the water pressure is 26281.25 pounds, and the point of application $67.5 - \left(\frac{2}{3} \times 29\right) = 48.17$ feet above base of wall, and the point where the combined pressures meet,

$$x_0 = \frac{P_0\,Y_0 + P_3\,Y_3}{P_0 + P_3}$$

Where P_0 is taken as the horizontal component of water pressure.

" P_3 as the horizontal component of earth pressure.

" Y_0 = the vertical distance from base of wall to point of application of horizontal component of water pressure, Y_3 = vertical distance from base of wall to point of application of horizontal component of earth pressure, and x_0 as the vertical distance from base of wall where the combined components meet; or,

$$x_0 = \frac{(26281.25 \times 48.17) + (31247.35 + 15.945)}{26281.25 + 31247.35} = 30.66 \text{ feet.}$$

The counter-pressure of the fill on the opposite side of wall will be

$$P_3 = \frac{38.5^2 \times 0.466^2 \times 100}{2} = 16093.5 \text{ pounds,}$$

and the point of application above base $= \frac{38.5}{3} = 12.83$ feet; and the unbalanced thrust of the components of the earthfill, and water pressure upon side of wall next to the full basin becomes,

$$P = 26281.25 + 31247.35 - \frac{16093.5 \times 12.83}{30.66} = 50794.1 \text{ pounds.}$$

The weight of the wall at center per unit of length is 194 400 pounds, and the weight of the water and earth prism sustained by the batter on one side is 24 634.3 pounds, and of the prism of earth fill on the opposite side 10 587.5 pounds, and total weight on base = 229 621.8 pounds.

The center of gravity of the section of wall combined with the prism of earth and water upon one side, and the prism of earth on the other, has been taken as 14.33 feet from a vertical intersecting the base of wall upon the water side; and the horizontal distance from center of gravity to center of pressure,

$$x_1 = \frac{50794.1 \times 30.66}{229621.8} = 6.78 \text{ feet},$$

and from center of pressure to extremity of joint, or base,

$$u = 30 - (14.33 + 6.78) = 8.89 \text{ feet},$$

and pressure per square foot at extremity of joint,

$$Py = \frac{2}{3} \times \frac{229621.8}{8.89} = 17\,219 \text{ pounds},$$

or 119.6 pounds per square inch.

The feature of special interest, however, in the construction of this reservoir, is the main retaining wall; this is an elaborate specimen of rubble masonry and a handsome piece of heavy architecture, although possessing, in the judgment of the writer, several defects when viewed from an engineering standpoint.

The retaining wall or dam, at the deepest part of the ravine which it spans, has a vertical height or depth of 118.66 feet, a base width of 48.75 feet, and a uniform width of top, not including the arches or buttresses, of 15 feet. Of the maximum height of wall, 3.85 feet is represented by the invert under the outfall drain, and the net vertical height of wall, for purposes of calculation, is taken as 114.8 feet, with a base width of 47.5 feet.

The distance from center to center of the projecting buttresses is 68 feet, and the clear span of arch 48 feet.

The wall, not including buttresses or arches, at any point in its height, is of uniform section throughout its length, and calculations of the section at the center of the ravine will apply to any section of the wall of corresponding depth from coping.

At the water face of the wall the maximum depth of fill is 83.66 feet, and at the back of the wall the fill is about 58.43 feet deep, leaving an exposed face of wall subject to water pressure of 35 feet, and an unsupported depth of back of 60.23 feet.

The face of the wall is vertical, and the back has a batter of 4.25 inches to one foot for a height of 53.43 feet from base; and for the remaining 65.228 feet of height, it has a batter of 2.4 inches to 1 foot.

The projecting piers or buttresses which support the arches at back of wall have a batter of 4 inches to the foot for a height of 49.52 feet from base, a batter of 1.3 inches for an additional height of 29.69 feet to the springing line of the arch, and from this point to top of wall the buttresses are vertical. The construction of the wall, buttresses and arches will be readily understood by reference to Plate XXI, which gives a section of the main wall at the center of the ravine and a partial elevation of the back.

The chambers in the body of the wall shown in the drawing of section are longitudinal in direction, and were calculated to receive and conduct to the outfall drain at the bottom of the wall any leakage from the reservoir through the wall, the surface water from the roadway on the top of the wall and to serve as drains for water from the soil at the extremities of the wall.

In construction these chambers (according to report) were allowed to fill with rubbish and offal from the work, and their functions as drains either limited or wholly destroyed.

In plan the dam is a vertical segmental arch, with the concave side to the water, the chord of which is 840 feet, and the lineal measurement of the four principal segments 925.56 feet. At the southwest end of the wall it joins the effluent pipe chamber of the division wall with a vertical arch of 36 feet radius and 52.5 feet chord upon its soffit, and at the northeast end it joins the paved slope on the side of the hill with a vertical arch of 85 feet radius and 175.5 feet chord, measured upon its soffit.

During January of 1886 the writer's attention was drawn to the discharge of water through the outfall drain under the main wall, which was entirely too large to be accounted for by subterranean drainage; and as the back of the wall was cracked and exuded water at a point near the bottom of the reservoir and nearly over the outfall drain, it was deemed advisable to draw the water from the basin next the wall and examine its condition. Whereupon it was discovered that the wall was cracked upon its face from a point about the usual water line down to and under the concrete floor.

Upon removing the concrete, the earthfill below was found considerably saturated with water ; at some points it had settled away from the concrete several inches, and was so soft as to render excavation quite difficult, for the purpose of exploring the crack in the wall. The crack itself extended as deep as the examination was made, and sensibly widened from the top downwards, from which it was inferred that the break was due to vertical settlement of the masonry on its foundation.

By referring to the plan of the reservoir, Plate XIX, the position of the chamber "A" and tunnel leading from this to the influent chamber "B," at the east end of the division wall, will be noted. The chamber is of rubble, and the influent tunnel leading from it to the influent chamber is of brick masonry. Two 40-inch pipes with flange ends were built in the wall opposite the tunnel at a point 64 feet below the coping, one of which was completed (as shown by dotted lines) through the tunnel to the influent chamber at the east end of division wall. The tunnel rises with an unknown grade from the chamber "B." The chambers "A" and "B" and the influent tunnel were calculated to be impervious.

For several years the reservoir was supplied through the 40-inch influent pipe in the tunnel, until it was discovered, in 1881, that the water stood in what was intended to be a dry chamber ("A") at the same level as the water in the basin, when the influent pipe and tunnel and influent chamber "B" were abandoned, and the delivery of water to the reservoir made through the effluent chamber "C," at the southwest end of the division wall.

Subsequent investigation showed that the influent pipe had broken at the first joint inside the main dam or retaining wall, the barrel of the pipe separating from the flange, and leaving a space all around of about $\frac{5}{8}$ inch through which the water passed into the tunnel and the two chambers.

Recent investigation by the writer shows the arch at the bottom of chamber "A" and the arch of the influent pipe tunnel to be cracked and the joints open, through which water has probably passed under pressure of 18 to 25 pounds per square inch into the earthfill over the tunnel and around the chamber.

This infiltration through the fill has diminished the power of the earth to support the concrete floor, and as a result many cracks have occurred in the latter through which the water has passed downwards from the basin into the fill below.

In addition to the vertical crack in the principal wall, the concrete floor and the fill below were found to have sunk around the chamber "A," and from the quantity of mud in the chamber and the damaged condition of its arch, it was believed that a portion of the earthfill had found its way into the chamber and tunnel.

In the design of the reservoir it was assumed that in its use no water other than subterranean drainage would be found under the floor of the basins, and the ramifying sewers and drains were supposed to be adequate to the interception and removal of this water, and the prevention of any serious saturation of the earth fill below the concrete.

In brief, it was expected that none of the water pumped into the basins would ever pass below the concrete floor.

No measurements were made of the flow through the outfall drain previous to emptying the section of reservoir next the main wall, owing to the difficulty and danger at that time of working in the sewer; but from comparison of the pumpage before and after the basin was abandoned, it is probable that the average loss of water was upwards of 3 000 000 gallons per diem.

Part of this loss was through the cracks in the back of the masonry wall, but the larger portion passed through the crack in the face of the wall into the drainage chambers, and into the outfall drain, and through the openings in the floor of the basin through the fill into the intercepting drains, and finally into the same outfall drain through the base of the wall.

The use of the basin has been abandoned since January, 1886.

Returning to a consideration of the principal wall or dam, the base loads on the foundation and the stability have been calculated as follows, estimating the weight of the masonry at 144 pounds per cubic foot:

Volume of masonry in wall per unit of length, exclusive of arch, cornice and belt courses and projecting buttresses, 3587.5 cubic feet.

Volume of masonry in arch, cornice and belt courses, pilasters and projecting buttresses per unit of length, 251.13 cubic feet.

Average load per square foot of base =

$$Wo = \frac{3587.5 + 251.13}{47.5} \times 144 = 11637.1 \text{ pounds.}$$

Estimating the load upon foundation for a length of wall corresponding to the base length of buttress (20 feet), and assuming the weight of arches to be entirely sustained by the buttresses, and that such section of wall is capable of independent vertical motion, then the volume of masonry would be 17076.97 cubic feet, or 853.85 cubic feet per unit of length of section, and the average pressure per square foot of base,

$Wo = \dfrac{853.85 + 3587.5}{47.5} \times 144 = 13464.3$ pounds; and similarly for a section of the wall if the projections on the back were removed,

$$= Wo = \frac{3587.5 \times 144}{47.5} = 10875.8 \text{ pounds per square foot.}$$

These average loads are not excessive, and excepting water of the subterranean drainage or from the basin of the reservoir has passed downwards below the base of the wall and softened the marl strata, or the clay seams between the underlying strata of lime-stone, no settlement sufficient to break the wall should have occurred.

From a comparison of the present pumpage into the reservoir per diem, with the pumpage of the past four or five years, it would appear that the crack in the wall and its corresponding settlement, and the damage to the floor of the basin were not of recent occurrence.

Several years ago (perhaps at the time when the breaks in the wall and concrete floor occurred) the consumption of water, as exhibited by the pumpage into this reservoir, showed a considerable and unwarranted increase over previous and similar periods of time; to correct which and keep down the average consumption per capita, the water department attempted an increase of population of over six per cent. per annum, when the natural increase per annum is 1.5 per cent.

(The increase of population perpetrated by the water department must not be taken in a literal sense).

In estimating the strains which may be applied to the wall from the water side, we have the horizontal pressure due the earthfill below the basin in its normal condition, combined with the horizontal pressure of the water in the basin.

Should this earthfill become saturated with the leakage from the basin, and the fill become a mixture of earth and water, with an angle of repose of 0 or nearly 0, and a specific gravity of 1.6, then the horizontal component of the pressure of the earthfill below the basin, combined with the water above the fill, would be greatly increased, as is well known. Of course, in the design of the basin, no such condition as a thorough saturation of the earthfill was contemplated; and in fact (as it was supposed), ample provision was made to conduct away water from whatever source which should find its way into or through the fill.

But sewers and drains sometimes become clogged and fail to discharge their functions as conduits; and since these drains are deep in the ground, difficult of access and seldom explored, is it not possible that a work of disintegration might go on through years of time, which eventually would diminish the carrying capacity of the subterranean sewers, or close them entirely; in which event not alone the water of leakage from the basin, but the subterranean drainage, would accumulate in the earthfill above the wall, and subject it to pressures far beyond the expectation of its constructors.

In the construction of the reservoir it was believed that the bottom could be made water-tight, and that the drains would be required to conduct away only the subterranean and such surface drainage as was calculated to pass downwards through the channels for the purpose constructed in the wall; but in this respect the work has signally failed, and for some years the fill has been open to saturation by the leakage from the basin.

To subject this wall, however, to a strain far in excess of that contemplated by its builders, it is not necessary to saturate the entire fill or any large portion of it; if the earth next the wall be gradually softened by leakage through the joint made between the concrete and the wall, until this extends down or nearly down to the footing courses, a pressure may be applied equal to or greater than that due a column of water of same height.

The earth support at the back of the wall, as shown in the section, Plate XXI, is fill also; but although loosely deposited (as the writer is informed), is probably, from its free exposure to the action of the elements, equal in compactness and natural stability to the fill at the face of the wall, which is supposed to have been rolled and tamped more or less during its introduction.

In the following calculations the general assumptions are made:

First.—That the pressures upon the wall, from materials in front or behind it, are developed in horizontal planes.

Second.—That the mortar has no cohesive effect, and the horizontal component of any vertical load of earth or water is limited to the joint in the masonry for which such load is estimated.

Third.—That the horizontal thrusts of the earth or water have no effect either to increase or diminish the vertical loads of the wall, and that the pressure or load on any joint cannot exceed, nor be less than, the weight of masonry above that joint.

The data for quantities of material have been estimated from measurements on the original drawings.

Section of wall between buttresses or counterforts.

Section of wall opposite water, 35 feet deep; top width, 15 feet; bottom width, 22 feet; volume per unit of length, 647.5 cubic feet; weight on joint, 35 feet from top = 93 240 pounds.

Section of wall above earthfill at back, 60.23 feet from top; top width, 15 feet; bottom width, 27.25 feet; volume per unit of length =

PLATE XIX
TRANS. AM. SOC. CIV. ENGRS.
VOL XVI Nº 360
HILL ON
A MASONRY DAM

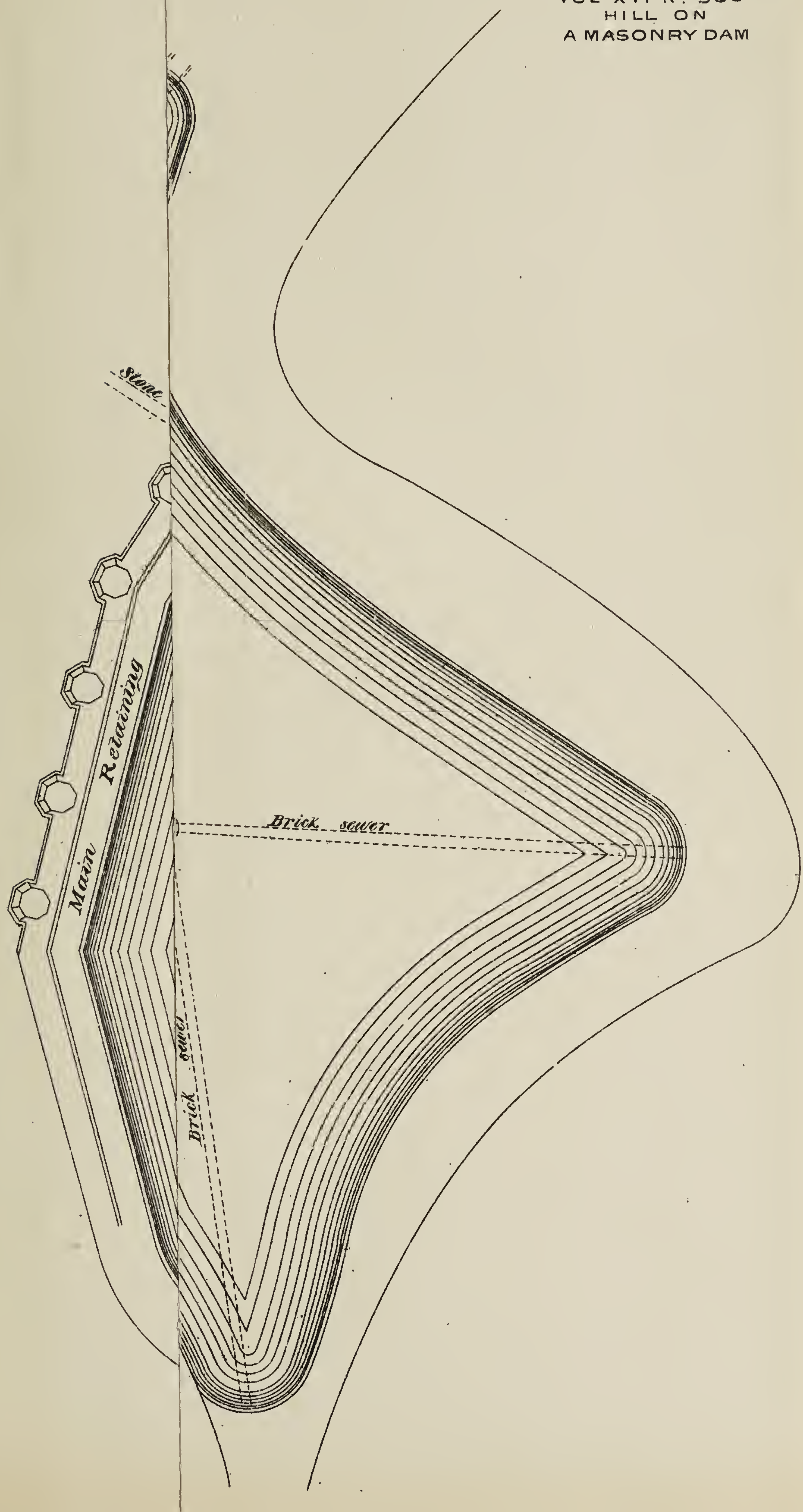


PLATE XIX
TRANS. AM. SOC. CIV. ENGRS.
VOL XVI No 360
HILL ON
A MASONRY DAM

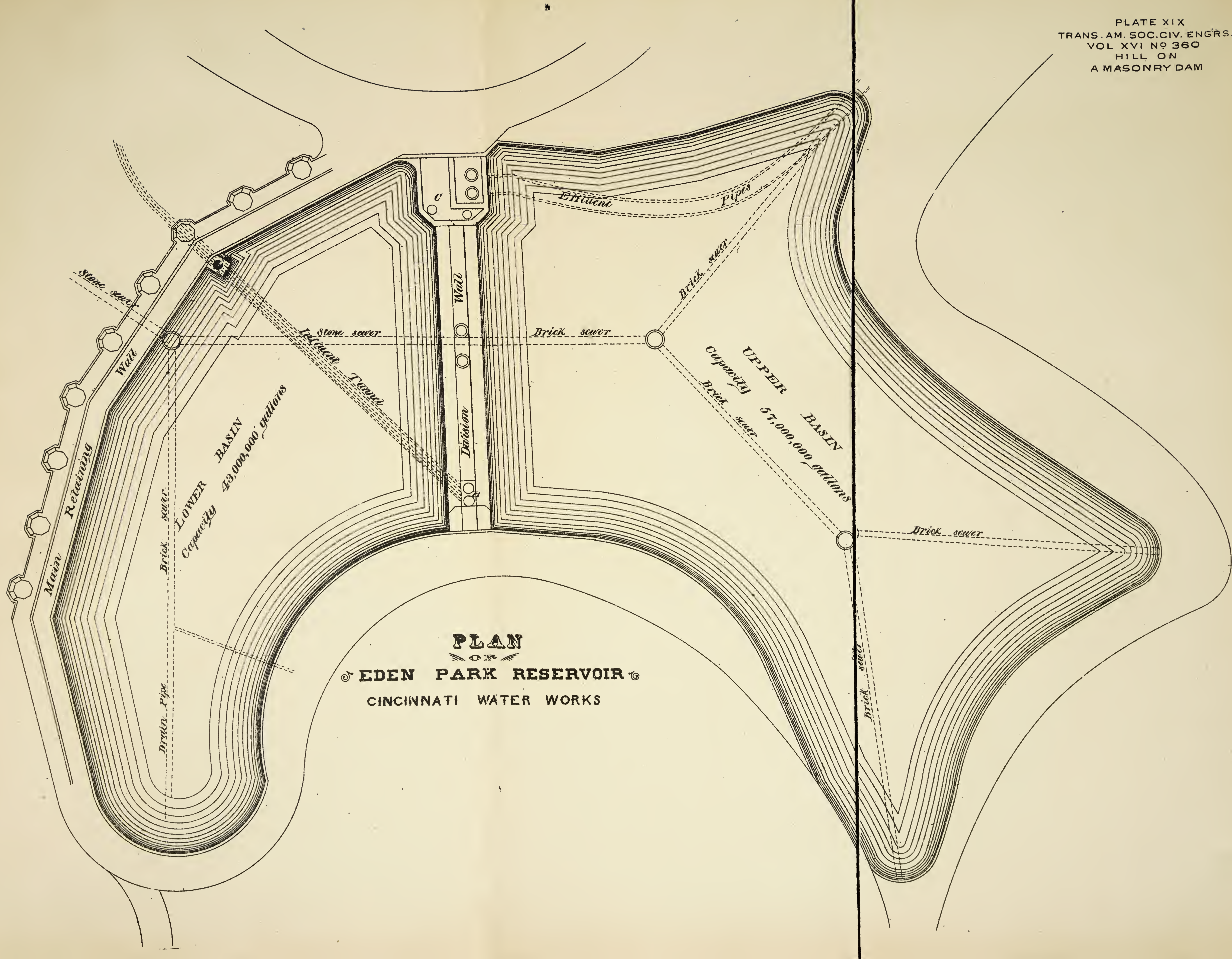


PLATE XX
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XVI Nº 360
HILL ON
A MASONRY DAM

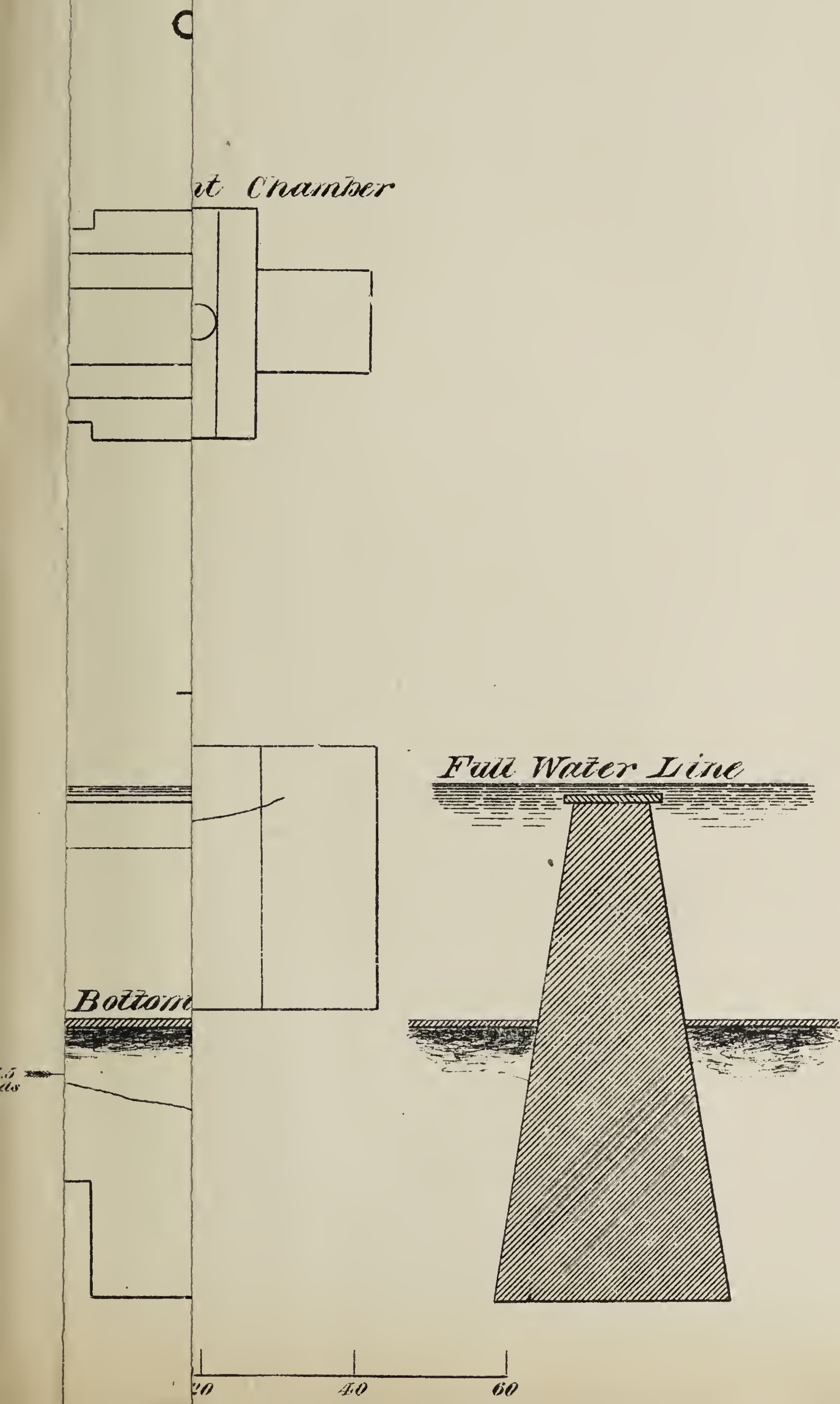
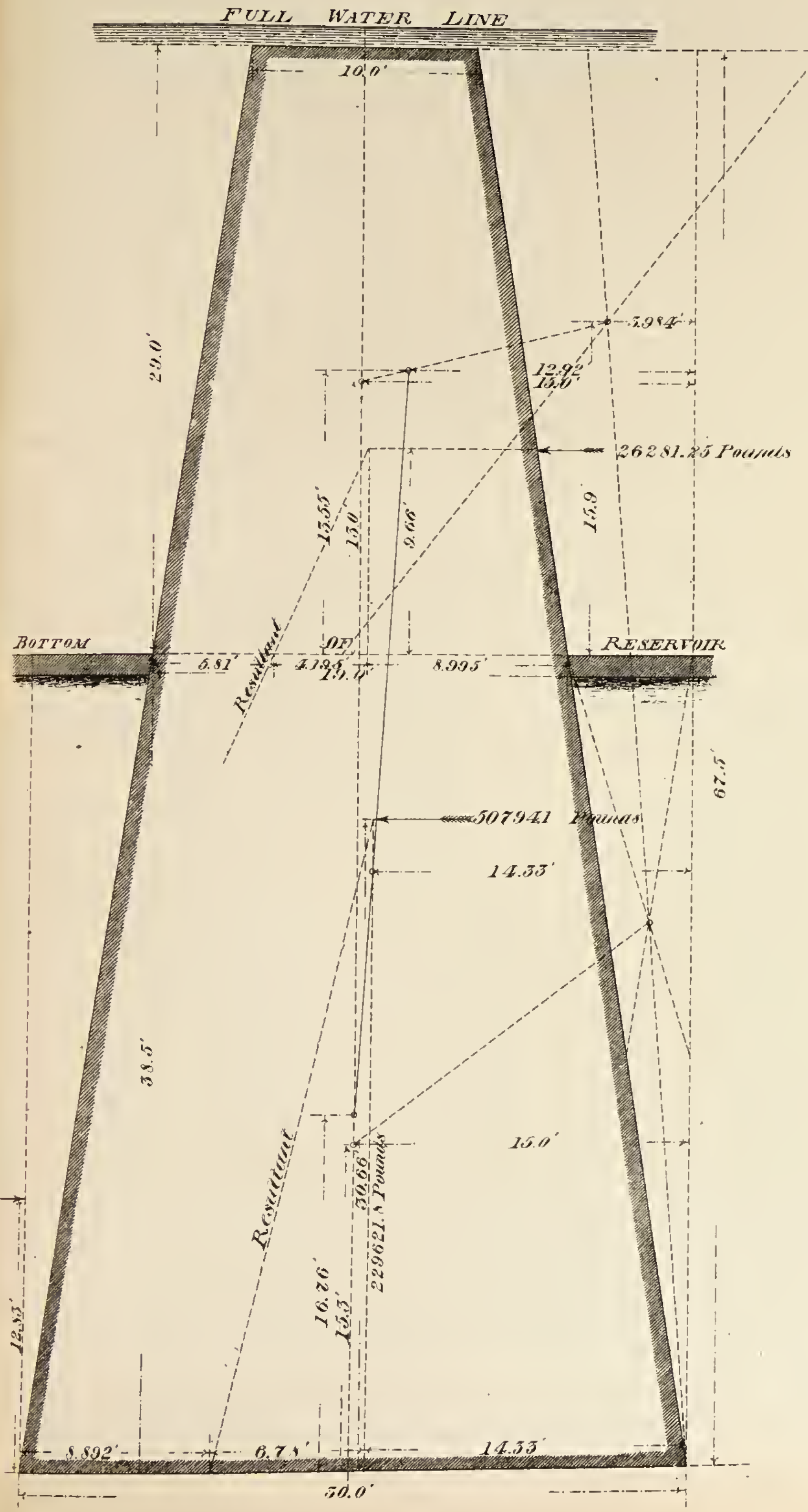


PLATE XX
TRANS. AM. SOC. CIV. ENGRS.
VOL XVI Nº 360
HILL ON
A MASONRY DAM

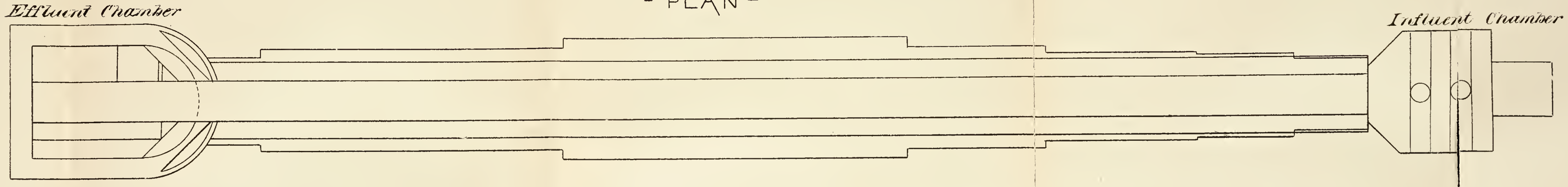
DIVISION WALL - EDEN RESERVOIR

CINCINNATI WATER WORKS

SECTION OF DIVISION WALL



- PLAN -



- ELEVATION -

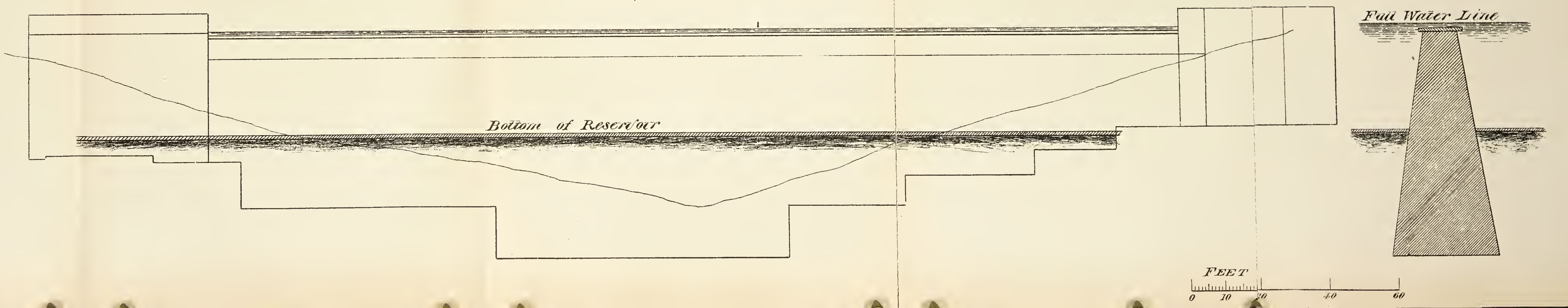


PLATE XXI
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XVI No. 360
HILL ON
A MASONRY DAM

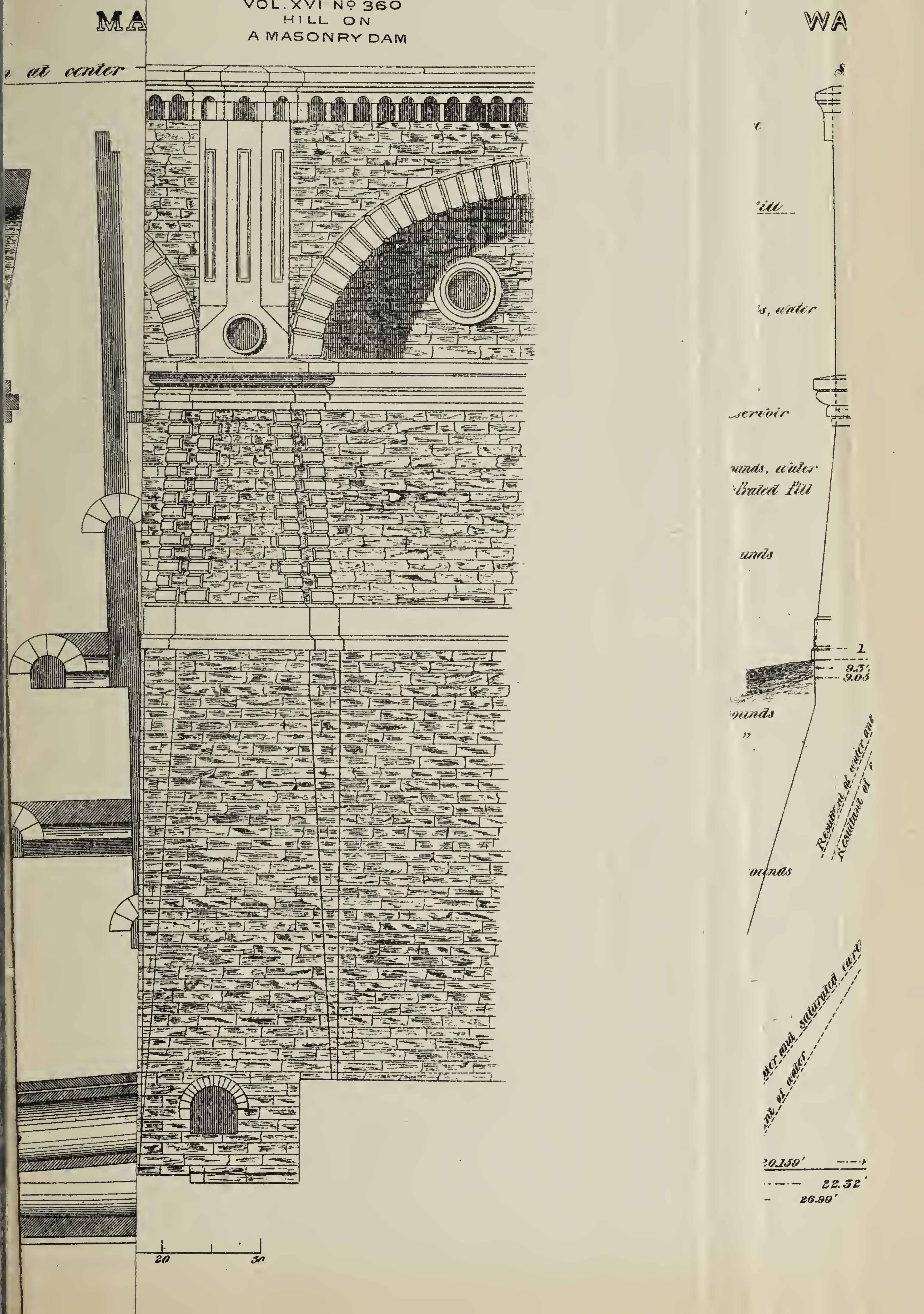
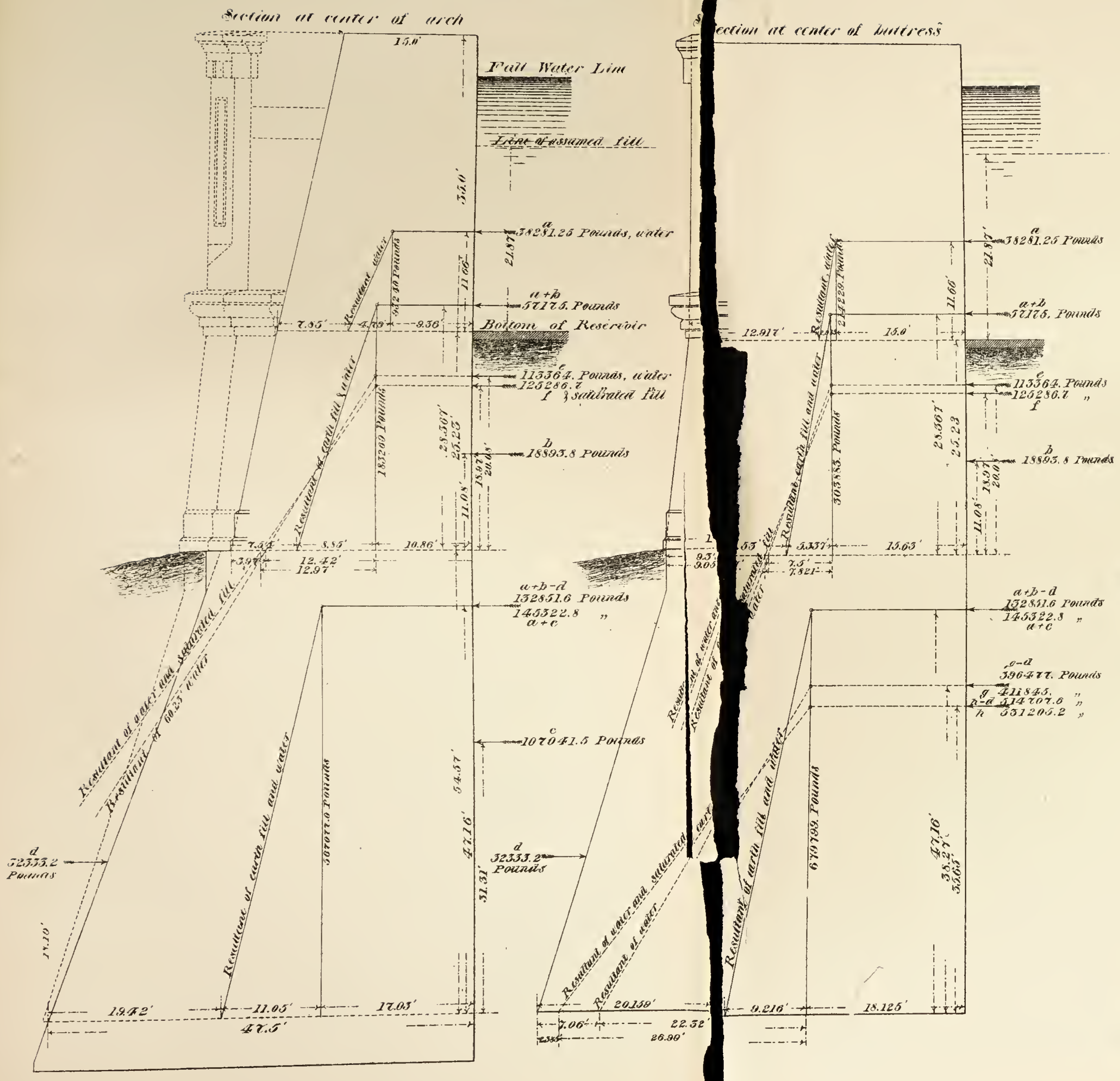


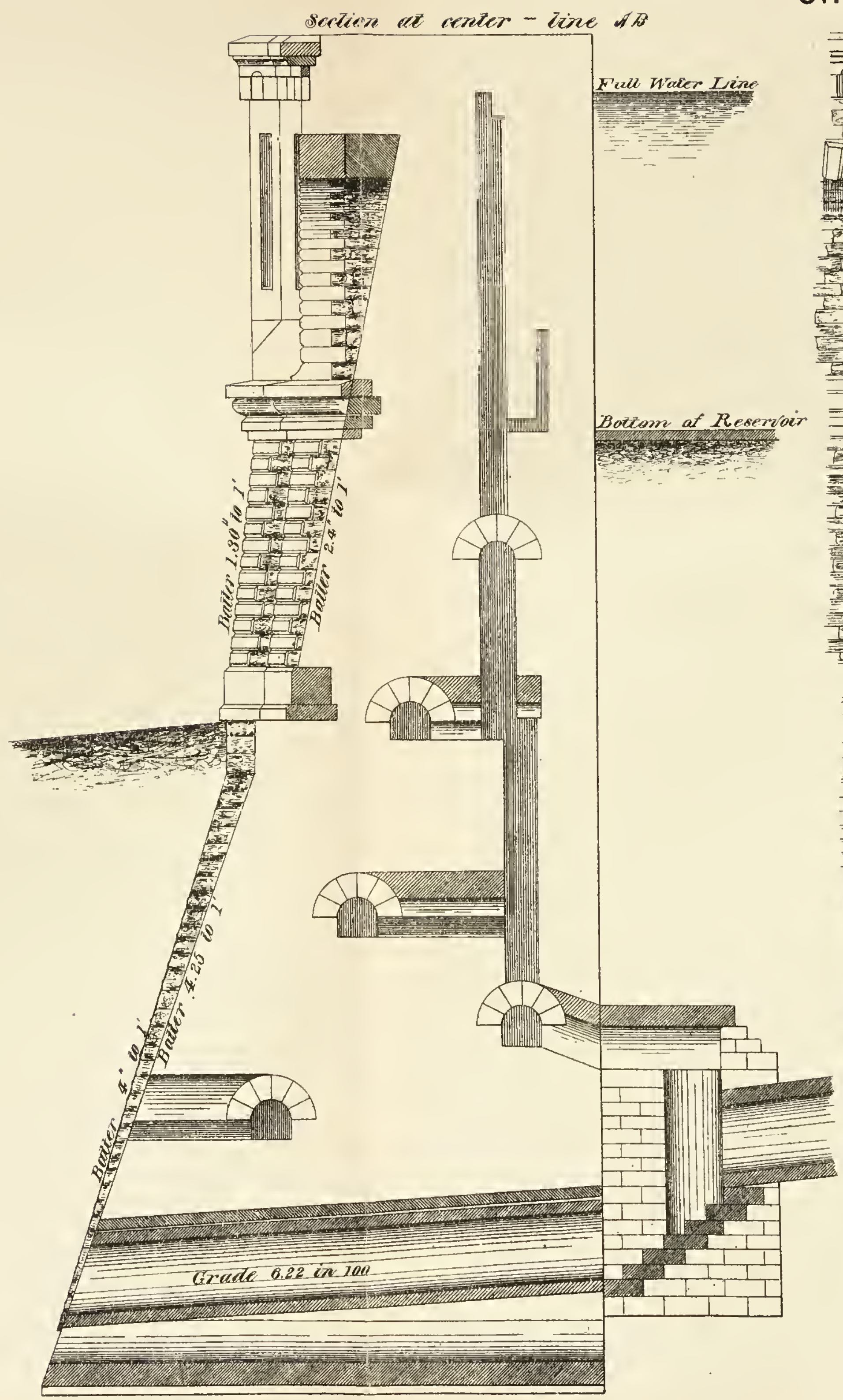
PLATE XXI
TRANS. AM. SOC. CIV. ENGRS.
VOL. XVI Nº 360
HILL ON
A MASONRY DAM

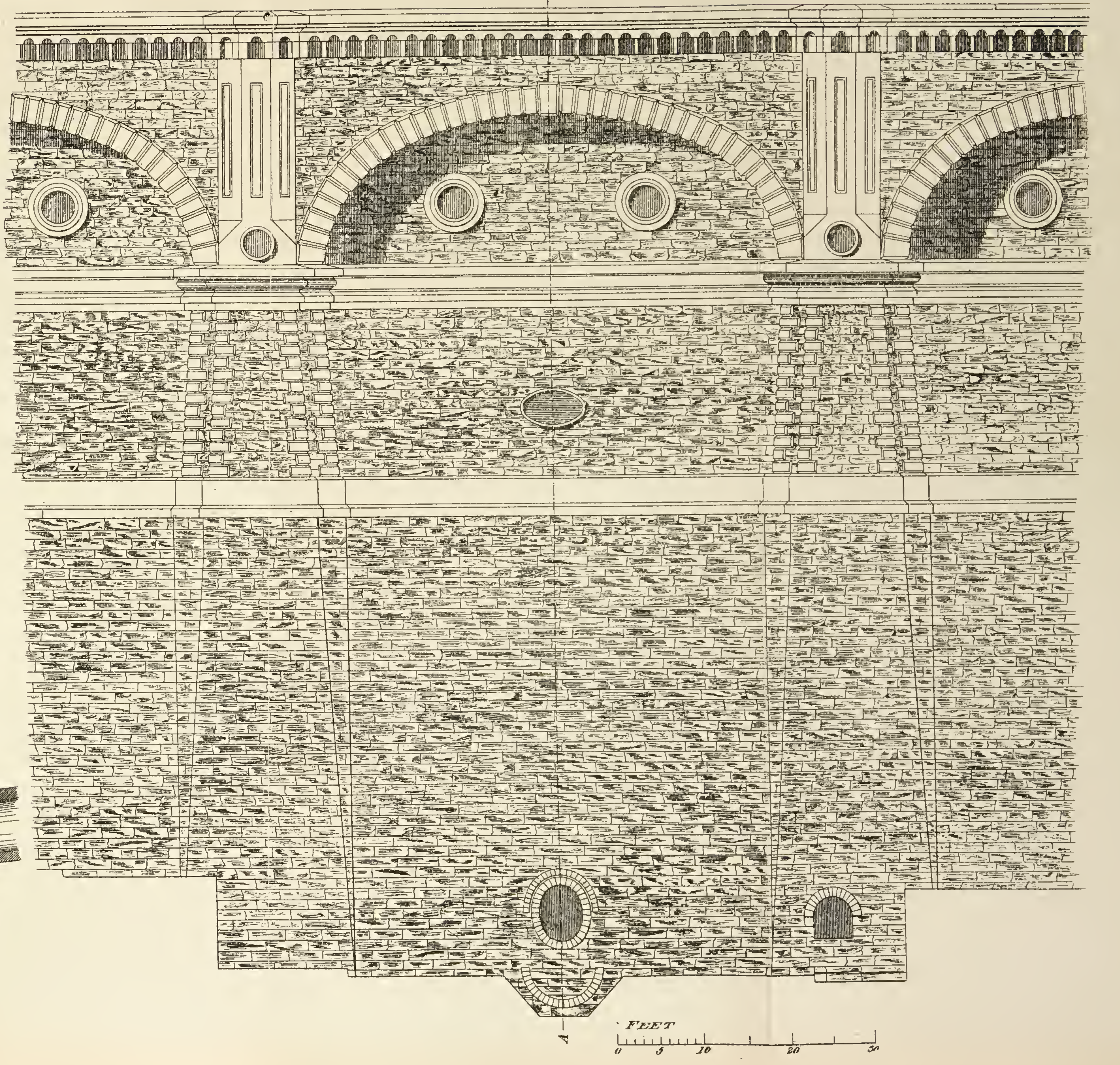
MAIN RETAINING WALL



MAIN RETAINING WALL - EDEN RESERVOIR

CINCINNATI WATER WORKS





1 272.7 cubic feet, and weight on joint 60.23 feet from top = 183 269 pounds.

Entire section of wall at depth from top, 114.8 feet; top width, 15 feet; bottom width, or base width, 47.5 feet; volume per unit of length, 3 587.5 cubic feet; and weight on foundation, 516 600 pounds; to which must be added the weight of the prism of earth below the surface of fill at back of wall, supported by the batter $= \frac{54.57 \times 18.5 \times 100}{2} = 50\ 477$ pounds, and the total weight on foundation is, $W = 567\ 077$ pounds.

In estimating the quantities and weights for the section of wall opposite and including a buttress, it is assumed that the weight of an arch as shown in the elevation of back of wall is borne by the adjacent buttresses and section of wall from which the buttresses project.

Volume of masonry above joint, 35 feet below top of wall, per unit of length, 1 487.7 cubic feet; weight on joint same depth, 214 229 pounds.

Volume of masonry above joint, 60.23 feet below top of wall, per unit of length, 2 110.3 cubic feet; weight on joint at same depth, 303 883.2 pounds.

Volume of masonry above foundation, 114.8 feet below top of wall, per unit of length, 4 441.35 cubic feet; weight on foundation, 639 554.4 pounds; to which must be added the weight of the prism of earth below the surface of fill at back of wall, supported by the batter $= \frac{54.57 \times 14.75 \times 100}{2} = 40\ 245.$ pounds; and the total weight on foundation becomes 679 799 pounds.

The centers of gravity, or rather the horizontal ordinates from face of wall to the centers of gravity of the three horizontal sections, taken for purposes of calculations, are as follows:

For section of wall between buttresses; center of gravity from face of wall for horizontal section above joint, taken 35 feet from top, 9.36 feet.

For horizontal section above joint, taken 60.23 feet from top, 10.86 feet.

For section of wall from foundation, 114.8 feet from top, 17.03 feet.

For wall opposite and including buttresses:

Center of gravity from face of wall for horizontal section, taken 35 feet from top of wall, 15 feet.

Center of gravity from face of wall for horizontal section, taken 60.23 feet from top, 15.63 feet.

Center of gravity of entire section of wall from foundation, 114.8 feet from top, 18.125 feet.

Estimating the stability or strength of the wall after the method proposed by M. Debauve, the pressure upon the joint at back of wall, at depth of 35 feet from the top, has been calculated as follows:

Horizontal distance from center of gravity of section of wall to center of pressure,

$$x_1 = \frac{38\,281.25 \times 35}{93\,240 \times 3} = 4.79 \text{ feet};$$

and center of pressure from edge of joint, of section of wall between buttresses,

$u = 22 - (9.36 + 4.79) = 7.85$ feet; then, the pressure per square foot at extremity of joint or back of wall =

$$Py = 2\left[(2 \times 22) - (3 \times 7.85)\right] \times \frac{93\,240}{22^2} = 7\,879 \text{ pounds},$$

or 54.72 pounds per square inch.

The limiting pressure, according to M. Debauve, is 92.5 pounds per square inch.

In estimating P for the section of masonry above a joint 60.23 feet from top of wall, or at the joint where the outside earthfill terminates, we have to consider the pressures developed by the inside fill above this point, and the depth of water, which may be 35 feet, and the depth of earth fill to the joint, 25.23 feet.

The angle of repose of the earthfill has been taken at 40 degrees, and the angle of the plane of fracture which the earth wedge will make with the face of wall $= \frac{90-40}{2} = 25$ degrees.

It will be convenient to estimate the horizontal component and its point of application first for the earthfill, and then combine the component so found with that of the water in the basin.

Let the specific gravity of the earthfill be taken at 1.60 water, then will a column or depth of earth of $\frac{35}{1.6} = 21.87$ feet produce the same base load as the depth of water.

The actual depth of the earthfill to the joint under consideration is 25.23 feet, and the whole depth, for the purpose of calculating the horizontal pressure, is

$$25.23 + 21.87 = 47.10 \text{ feet}.$$

Let $H =$ depth of wall to joint, $H - h = h_1 = 47.10$, $h_2 = 21.87$ and $h_1 - h_2 = h_3 = 25.23$ feet.

$$\text{Let } P_1 = \frac{h_1^2 \text{ tang. }^2\varphi \times 100}{2} = \frac{47.1^2 \times 0.466^2 \times 100}{2} = 24\,087.05 \text{ pounds},$$

and

$$P_2 = \frac{h_2^2 \text{ tang. }^2\varphi \times 100}{2} = \frac{21.87^2 \times 0.466^2 \times 100}{2} = 5\,193.25 \text{ pounds};$$

then the horizontal pressure upon the wall due to the earthfill and superimposed depth of water will be,

$$P_3 = P_1 - P_2 = 18\,893.8 \text{ pounds}.$$

The point of application of P_1 will be from the surface of the actual and assumed fill, $y_1 = \frac{2}{3} h_1$ and the point of application of the pressure P_2 will be from same surface $y_2 = \frac{2}{3} h_2$ and the point of application of the presssure,

$$P_3 \text{ is } y_3 = \frac{2}{3} \times \frac{h_1{}^2 + (h_1\, h_2) + h_2{}^2}{h^1 + h_2} =$$

$$\frac{2}{3} \times \frac{47.1^2 + (47.1 \times 21.87) + 21.87^2}{47.1 + 21.87} = 36.02 \text{ feet,}$$

and distance from joint to point of application of pressure, 47.1 — 36.02 = 11.08 feet, and from top wall 49.15 feet.

The horizontal thrust of the water is 38 281.25 pounds, and is applied at a depth of $\frac{2}{3} \times 35 = 23.33$ feet from top of wall.

Taking moments around the upper edge of wall coincident with its face, where the horizontal component of the water pressure, lever arm upon which such component acts, and the resistance of wall $= o$; we have, as the combined horizontal pressure, 57 175.05 pounds, and the point above the joint in the masonry (60.23 feet from top) where the components meet, as

$$x_0 = 60.23 - \frac{(38\,281.25 \times 23.33) + (18\,893.8 \times 49.15)}{38\,281.25 + 18\,893.8} = 28.367 \text{ feet.}$$

The horizontal distance from the center of gravity to the center of pressure, for wall between buttresses will be,

$$x_1 = \frac{57\,175.05 \times 28.367}{183\,269} = 8.85 \text{ feet,}$$

and horizontal distance along the joint from center of pressure to edge of joint is,

$$u = 27.25 - (10.86 + 8.85) = 7.54 \text{ feet,}$$

and pressure per square foot at edge of joint,

$$P_y = \frac{2}{3} \times \frac{183\,269}{7.54} = 16\,204 \text{ pounds,}$$

or 112.5 pounds per square inch.

Assuming a water pressure for the same depth of wall, which is a condition that may at some future day subsist, we have:

Horizontal component of head of water, 60.23 feet;

$$P = \frac{60.23^2 \times 62.5}{2} = 113\,364.15 \text{ pounds,}$$

and the horizontal distance from center of gravity of the section of wall to the center of pressure will be,

$$x_1 = \frac{113\,364.15 \times 60.23}{183\,269 \times 3} = 12.42 \text{ feet,}$$

and $u = 27.25 - (10.86 + 12.42) = 3.97$ feet, and pressure per square foot at edge of joint,

$$P_y = \frac{2}{3} \times \frac{183\,269}{3.97} = 30\,775.6 \text{ pounds,}$$

or 213.7 pounds per square inch.

If, however, the water is permitted to leak through the concrete bottom of the basin, it will or may produce a saturated earth, the specific gravity of which would be 1.6 water, and the angle of repose $\varphi = o$, or nearly o; in which event the horizontal component of pressure would be, taking angle (φ) of repose at o,

$$P = \frac{47.1^2 \times 100}{2} - \frac{21.87^2 \times 100}{2} = 87\,005.5 \text{ pounds,}$$

and the vertical distance above the joint where such pressure would be applied, as in the case of the actual and assumed earthfill = 11.08 feet, and from top of dam 49.15 feet; and as before for the earthfill, taking moments around the upper inner edge of wall, to determine the point of application above joint, of the combined moments of water pressure and pressure of saturated earth, we have

$$x_o = 60.23 - \frac{(38\,281.25 \times 23.33) + (87\,005.5 \times 49.15)}{38\,281.25 + 87\,005.5} = 18.97 \text{ feet.}$$

The horizontal distance from the center of gravity to center of pressure will be,

$$x_1 = \frac{125\,286.75 \times 18.97}{183\,269} = 12.97 \text{ feet,}$$

and from edge of joint to center of pressure,

$$u = 27.25 - (10.86 + 12.97) = 3.42 \text{ feet,}$$

and the pressure per square foot at edge of joint,

$$P_y = \frac{2}{3} \times \frac{183\,269}{3.42} = 35\,724.9 \text{ pounds,}$$

or 248.0 pounds per square inch, or 2.68 times the limit of pressure assigned to structures of rubble masonry by MM. Debauve, Krantz, Sazelly and Delocre.

Upon comparison of the weight of the section of wall above the joint and the horizontal component of the pressure, it will be obvious that with a coefficient of friction of less than 0.70, the wall would be on the point of sliding outwards.

It may be urged that the pressures calculated for the 60.23 feet depth of water, and the same depth of water and saturated earth, will never subsist; and the calculations exhibit a weakness of wall upon impossible conditions. But, are these conditions impossible? The experience of less than eight years with this reservoir has shown the bottom to have broken and permitted the passage of water to the earthfill below; and is it possible to make such joints of the concrete with the masonry of the wall that water will not pass below the floor of the basin? Or, if such joints be made, is there an assurance that they will always remain tight, with the concrete resting upon an earthfill subject to indefinite settlement? Excepting this result can be attained, or excepting the reservoir and its adjuncts be subject to better management than has prevailed in the past, the assumed conditions may gradually develop during years of service and the fact of their existence be not known.

This structure cost about $1 700 000, of which the main retaining wall alone cost nearly half, and was not built for a generation or several of them, but presumably for all time.

Masonry dams said to have been built upon better foundations, and known to have had a higher factor of safety than this, have yielded after many years of service; and what immunity from the disintegrating forces in structures of a similar character does this work possess?

Estimating the pressure at the outer edge of wall at its base, we have $114.8 - 35 = 79.8 = h_3 =$ depth of earthfill; and taking the angle of the plane of fracture of the earth wedge with the vertical face of wall $= \varphi = 25^\circ$ the horizontal component of the pressure of the fill against the wall will be,

where $h_1 = 79.8 + 21.87 = 101.67$ feet,

and $h_2 = 21.87$ as before for the depth of the assumed additional fill, (the base load per unit of area of which = the base load for a column of water 35 feet high or deep).

$$P_1 = \frac{101.67^2 \times 0.466^2 \times 100}{2} - \frac{21.87^2 \times 0.466^2 \times 100}{2} = 107\,041.53$$

pounds.

$$Y_3 = \frac{2}{3} \times \frac{101.67^2 + (101.67 \times 21.87) + 21.87^2}{101.67 + 21.87} + 13.13 = 83.49 \text{ feet},$$

and the point above foundation where the combined components of the water pressure and the earth pressure meet,

$$x_0 = 114.8 - \frac{(38\,281.25 \times 23.33) + (107\,041.53 \times 83.49)}{38\,281.25 + 107\,041.53} = 47.16 \text{ feet}.$$

But the thrust of the earthfill, and the depth of water above it at the face of the wall, is partially counterbalanced by the thrust in the opposite direction of the fill at the back of wall.

The depth of this fill from base of wall has been given as 54.57 feet, and taking same angle of natural repose as for the fill at face of wall, and assuming the wedge of earth between a vertical cutting, the base at the edge and the back of wall exerts no thrust, we have as the horizontal component of the earth pressure,

$$P = \frac{54.57^2 \times 0.466^2 \times 100}{2} = 32\,333.2 \text{ pounds.}$$

The point of application of this pressure is $\frac{54.57}{3} = 18.19$ feet from base, from which is obtained the unbalanced thrust of the horizontal component of the pressure of the earthfill and water at face of wall,

$$P = 38\,281.25 + 107\,041.53 - \frac{32\,333.2 \times 18.19}{47.16} = 132\,851.6 \text{ pounds.}$$

Horizontal distance from center of gravity to center of pressure,

$$x_1 = \frac{132\,851.6 \times 47.16}{567\,077} = 11.05 \text{ feet,}$$

and from edge of joint to center of pressure,

$$u = 47.5 - (17.03 + 11.05) = 19.42 \text{ feet,}$$

and pressure per square foot at edge of joint.

$$P_y = 2\left[(2 \times 47.5) - (3 \times 19.42)\right] \times \frac{567\,077}{47.5^2} = 18\,468.46 \text{ pounds,}$$

or 128.25 pounds per square inch.

The pressures at the extremity of the several joints respectively 35 feet from top of wall, 60.23 feet from top, and at base of wall 114.8 feet from top, for the wall opposite and including the buttresses and arch, under the same conditions of loading as taken for the section of wall between buttresses, are tabulated below,

PRESSURES AT EXTREMITY OF JOINT.

(*Wall including Buttress.*)

Vertical depth from top of wall to joint	35	60.23	114.8 feet.
Water pressure, pounds per square foot	10 118.7	21 621	64 192.6 pounds.
Water pressure, pounds per square inch	70.27	150.14	445.78 "
Water pressure and pressure of earthfill, pounds per square foot	......	17 491.55	20 803.5
Water pressure and pressure of earthfill, pounds per square inch	......	121.47	144.47
Water pressure and pressure of saturated earthfill, at water side of wall, pounds per square foot	......	22 385.3	190 021.0
Water pressure and pressure of saturated earthfill, at water side of wall, pounds per square inch	......	155.45	1319.6

In the calculation of the strains in the wall for its full height the center resistance of the earth at the back has been taken as the passive force due the pressure of the fill against the batter; but if we assume the wall to be in motion, as it would be under the condition of a saturated fill under the basin, with the basin carrying its usual depth of water, then the resistance to motion would be, not the horizontal component, but the abutting power, or, "the greatest horizontal pressure consistent with the stability of the earth."*

* Rankine's Applied Mechanics, pp. 220–221.

This, according to Rankine, is,

$$R = \frac{w\,x^2}{2} \times \frac{1 + \sin.\,\varphi}{1 - \sin.\,\varphi}$$

Let w = weight of the earth per cubic foot, x = vertical depth or height of earthfill or bank, φ = angle of natural repose of the material in the bank, and, R = the greatest horizontal resistance which the fill is capable of opposing to the motion of the wall; then,

$$R = \frac{100 \times 54.57^2}{2} \times \frac{1 + 0.6428}{1 - 0.6428} = 684\,913.6 \text{ pounds.}$$

The center of resistance of the earthfill is $\frac{54.57}{3} = 18.19$ feet, above base of wall, and the total resistance to the sliding of the wall upon its base, becomes

$$R_1 = 684\,913.6 + (679\,799. \times 0.60) = 1\,092\,793 \text{ pounds.}$$

This fill upon the outside or at the back of the main wall is not permanent; was made upon private property; and, excepting the ownership or control of the land below the retaining wall, is vested by legal process in the city; is subject to removal, and such support as it may now offer will then disappear.

It is not likely that the city will either purchase or lease the ground upon which the fill was made until compelled to do so by stern necessity.

Earlier in this paper, which has been expanded perhaps more than the subject warrants, the writer suggested that the main retaining wall was defective in an engineering point of view; and the resultant pressures at the extremities of different joints under different conditions of loading have been worked out, to support the writer's opinion that the design of the wall is defective.

Although the wall was not constructed to meet the conditions of water pressure for its entire heighth, or of the greater pressure due a saturated earthfill, it should have been so constructed, because such conditions are not only possible, but have partially subsisted for the past five years. I remember reading some years since, in a work on masonry dams, a suggestion that such walls should always be constructed to meet a water pressure for the full height, and I have no knowledge of any other masonry dam which is constructed as a retaining wall for earth pressure for a portion of its height, and as a dam for water pressure for the remainder.

Assuming the wall should have been built upon the principles employed in the construction of masonry dams, and comparing its section with the profiles of Rankine, Graeff and Mongolfier, we find it lacking in base width and subject to resultant pressures greatly in excess of those taken as a safe limit by the older constructors. It will of course be urged that the wall can never be subjected to water pressure for its full height, because the sewer through the base will be large enough to conduct away all water which may filter through the fill under the basin. But in a structure of this character, the destruction of which would be fraught with disaster not alone to the residents below it, but to the Water Department, which depends upon this reservoir as a distributor of more than three-fourths the total consumption of the city and its suburbs, is it good engineering to base the safety of such an important structure upon so many assumptions, *i. e.*, that the sewer will never become clogged or choked and fail to discharge the drainage delivered to it; that water will never accumulate under the concrete floor above the wall, and subject it to pressures beyond those provided for in its construction; and that a saturation of the earthfill under the water basin is not likely to occur?

The volume of masonry per unit of length is now quite 75 per cent. of what would have been required to construct a dam after any of several well known and acceptable profiles, and it seems to the writer if some one of these had been adopted, the present damage to the wall would not have occurred, and the break in the concrete bottom of the basin, and the saturation of the fill below, would have been of little consequence as affecting the stability of the wall. As it is, the future success of the main wall depends entirely upon the ability of the Water Department to render the bottom water-tight, or to construct vertical drains at the face of the wall, which will convey the leakage quickly into the outfall sewer and prevent its accumulation in the earthfill.

But the latter arrangement is a poor expedient, and if adopted may result in a daily loss of several million gallons of water pumped against a total head of 250 feet from the Ohio River into the reservoir.

DISCUSSION.

WILLIAM E. MERRILL, M. Am. Soc. C. E.—In the spring of 1886 I was employed by the Board of Public Works, on the recommendation of Mr. John W. Hill, M. Am. Soc. C. E., Consulting Engineer to the Board, to examine the Eden Park Reservoir in the City of Cincinnati, and report on the best method of repairing it. The main retaining wall was cracked, and the concrete floor had a break in it, besides several long cracks.

Soon afterwards the Board of Public Works was legislated out of office, and their place was taken by the Board of Public Affairs. My appointment was continued under Mr. Arthur Moore, Mr. Hill's successor, and my final report was submitted in August, 1886.

It was my conclusion that the main retaining wall was safe as long as the pressure of the filling underneath the concrete floor remained an earth pressure, and did not become a mud pressure. As the latter condition was not improbable, it seemed necessary to provide against it, either by increasing the cross section of the retaining wall until it became strong enough to sustain the maximum possible pressure of mud or water, or else to devise methods of preventing saturation by and the creation of mud.

As the first plan would have entailed a very heavy outlay on the part of the city, I decided to recommend the second. This plan, which was adopted, consisted in digging a trench along the inner face of the retaining wall down to bed rock; building a drainage culvert with open joints on the floor of the trench; replacing the excavated earth by gravel, broken stone, or other porous material, and finally extending the concrete flooring over the top of the filled-up trench. My theory was that whatever leakage came through the concrete floor into the filling would ultimately find its way into the porous vertical stratum, and thence pass off through the drainage sewers without saturating the fill. As these sewers are easily accessible from outside, any excess of leakage can easily be detected and prevented after emptying the basin. As the water of the Ohio River is usually heavily charged with sediment, the floor of the basin is quickly covered with mud, and all minor cracks in the concrete are soon choked up.

The cracks in the main retaining wall are doubtless due to settlement of the foundations, and there is no other remedy than to fill them up as they occur, which is a comparatively easy matter, as the basin can readily be emptied.

I cannot agree with Mr. Hill that the loss of water through the porous lining next to the main wall will be excessive, as this lining will be compacted by the water pressure above it, and will be covered with the general concrete floor of the basin. It is evident that at first there will be considerable settlement, with probable rupture of the concrete floor; but this condition can easily be detected through the drainage culverts, and the basin can be emptied and the concrete repaired. The same thing might occur several times at increasing intervals, but the cost of repairs would be trifling, and the settlement would ultimately cease. In my judgment this method of treatment is much more rational than to dig down sixty feet and more (for the ravine descends rapidly) and build another wall against the face of the present one.

I would state, in conclusion, that my calculations for the pressure of earth against retaining walls were based on the methods indicated by M. A. Gobin, Chief Engineer of *Ponts et Chaussées,* in an article published in the *Annales des Ponts et Chaussées* in the latter half of 1883. This article demonstrates the unsoundness of Rankine's hypothesis, and as it is in accord with the latest experiments on retaining walls, I took it as my guide. There are several articles on earth pressures in recent numbers of the *Annales* which are worth consulting by members interested in such subjects, but the article of Gobin is the least obscured by excessive mathematics, and is to me the most satisfactory of them all.

JOHN W. HILL, M. Am. Soc. C. E.—The principal objection to Colonel Merrill's method of repair for the damaged east basin of Eden Reservoir, was that it assumed a continuance of the leakage through the concrete floor, which, as a measure of economy (if not safety) in the use of the basin was, if possible, to be avoided.

The water as pumped into the reservoir from the Ohio River carries in suspension the usual amount of clay and silt, which will in time close the small cracks and fissures in the concrete floor; but under a head of 30 feet of water, cracks or open joints between the concrete and the masonry walls would pass the suspended matter quite as freely as water.

At the time of examining the damaged basin, it seemed to me the proper method of repair was to remove the concrete floor over such portions of the earthfill as was saturated with the leakage, and after restoring the loss of fill and rendering the soil as compact and solid as possible, by rolling and hand ramming, to relay the concrete floor; being careful in making the joints between the old and new concrete to avoid leakage, and constructing the joints between the concrete floor and the wall in such a manner that the head of water would press the concrete against rather than force it away from the masonry.

A method of repair which was calculated to prevent the leakage of water through the bottom of the basin, was especially to be desired, since it not only removed the danger of over-pressure on the dam or retaining wall, but saved the Water Department several thousand dollars per annum by avoiding the pumpage of water into the basin to compensate for the leakage.

The formula for abutting power, or resistance to horizontal thrust, of a vertical plane of earth, was taken from Rankine's Applied Mechanics, without any independent investigation of its exact merits; and for information it is regretted that Colonel Merrill did not restate the equation for the active resistance of the earthfill at the back of the retaining wall, from the data given according to the method proposed by M. Gobin.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
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TRANSACTIONS.

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361.

(Vol. XV.—June, 1887.)

STEEL: ITS PROPERTIES; ITS USE IN STRUCTURES AND IN HEAVY GUNS.

By WILLIAM METCALF, M. Am. Soc. C. E.
READ MARCH 2D, 1887.

WITH DISCUSSION.

Much of what is written in this paper has been written and published before, yet it seemed better to indulge in some repetitions in order to try and bring together sufficient data on which to base a reasonable argument. A few definitions may not be out of place, as there seems to be some confusion in the use of terms in recent writings.

The word temper has two distinct meanings among steel-makers. Applied to steel not hardened, the temper is said to be mild, medium or high, according to the amount of carbon the steel contains; thus we recognize and use daily in the crucible-steel business fifteen tempers, each so distinct from the other in the fractured ingots, that there is no uncertainty in their selection and separation.

The mean difference in carbon between any two adjacent tempers is .07 per cent. When speaking of the temper of a piece of tempered steel, the final condition of the steel is referred to; that is to say, it is straw-color, orange, light brown, brown, pigeon-wing or blue, as the case may be. If the piece has not had the temper drawn, it is said to be hardened and not tempered.

To temper a piece of steel is to heat it, harden it by quenching, and to draw the temper to the color or degree of softness required.

The recent United States Navy specifications would read better if they said to be annealed, hardened in oil, and to have the temper all drawn out.

Steel-makers call the last operation drawing black.

Annealing steel is the operation of heating it slowly and uniformly to the necessary degree to soften it, or to relieve internal strains, or to secure uniformity of texture.

Now we have to define steel.

The question, What is steel? was left thoroughly mixed by the discussions which took place in 1876 and 1877. The law, however, says now, that "steel is iron which has been produced by fusion by any process, and is malleable."

I offer a new definition—Iron is a liquid, congealed to a solid at ordinary temperatures.

This definition was first suggested by U. S. Senator John T. Morgan who enforced it with an able and exhaustive argument. It was suggested next, and independently, by Professor John W. Langley, of the University of Michigan, Ann Arbor.

Professor Langley was also the first to observe the varying rate of expansion, due to increase of temperature, between high steel and low steel. He also was first to note the presence of free oxygen dissolved in iron, a discovery which was received with ridicule in this country in 1877, and which has been confirmed by eminent European chemists within the last two years. This liquid, which we will consider now in the form known as steel, congeals at a high temperature; as it congeals it crystallizes in as many forms almost as are to be found in snow-flakes. The sizes and forms of the crystals are affected—from fusion—

First.—Largely by the rate of cooling; slow cooling favors the formation of large crystals, and quick cooling produces small crystals. Chernoff observed further that agitation produced fine crystals, and gave this as the reason why a heavy hammer, thoroughly and quickly applied at the right time, produced fine grain, increased density and greater strength.

Second.—The size and form of the crystals are affected by the foreign substances present.

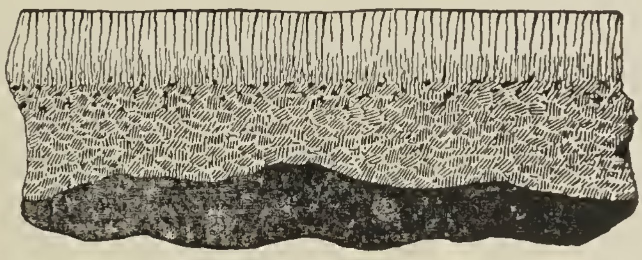
We are not yet familiar enough with the effects of all of the components of steel, to be able to read off a complete analysis by looking at a fractured ingot; but the effects of carbon are so clear, so exact, and so unvarying in well-melted ingots, that experience enables us to select our fifteen tempers with absolute certainty, and a skilled operator can split these fifteen into thirty tempers; so that it is a fact that when ingots are high-tempered enough to be fractured, they can be analyzed for carbon at a glance—much more accurately than it can be done by any known chemical system of color tests.

Third.—The crystals are affected by the walls of the mould in which the liquid is cooled.

This is very marked in chilled iron, and in what the melters call scalded ingots. The effect of the wall can be noticed also in any casting.

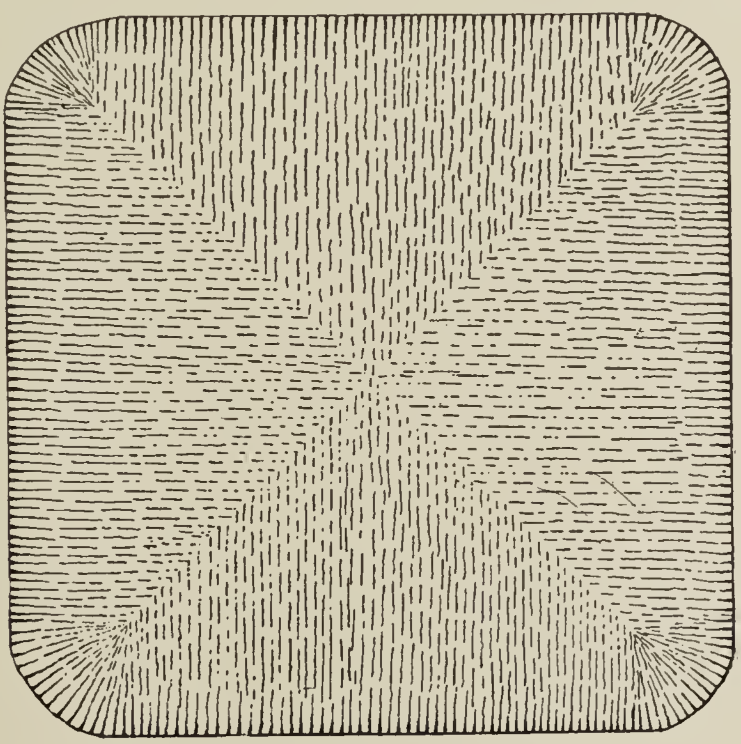
When steel congeals, the foreign substances are driven out to some extent by sudden cooling, just as cold ice is clearer and sounder and stronger than slowly-formed ice.

This can be seen plainly in chilled iron, where the graphitic particles are found just at the edge of the chill, thus:



The normal lines show the crystals of the chill; the heavy dots the particles of graphite that are driven off; and the fine dots the amorphous gray iron.

A scalded ingot looks like chilled iron, having this appearance:



But the crystals are not hard like the crystals of a true chill, and it would seem better to call such a body a polarized ingot.

A polarized ingot is very weak and brittle, so also is chilled iron very brittle.

There is much evidence to show the tendency to the extrusion of foreign elements as molten steel cools, of which the two cases following are given.

An ingot weighing several tons was drilled at the top and bottom, and analyses gave—

	For the top.	For the bottom.
Silicon	.023	.078
Phosphorus	.014	.032
Sulphur	.023	.027
Manganese	.306	.425
Carbon	.725	.775

A large bar of steel made for rolls by Krupp, of Essen, was turned and bored, and the turnings were analyzed, with the following result:

	Outer.	Inner.
Silicon	.130	.195
Phosphorous	.044	.050
Sulphur	.000	.005
Manganese	.448	.425
Copper	.234	.224
Carbon	.852	1.020

The latter case is not so marked, except in the carbon, as in the case of of the ingot, yet these two cases indicate that the elements sink as if by gravity, and leave the surface as it cools.

In the light of the liquid theory, the above cases illustrate a reason for the well-known unequal distribution of the elements in steel.

They also point to the idea that the elements in steel are there as alloys or in solution, and not in chemical combination. It may be true that there is a definite carbide of iron in steel, yet if there is, it is evidently there in solution.

So far as I have been able to observe the facts, during an almost daily experience of more than twenty-eight years, there is no property of steel that is not common to cast-iron; as, for instance, the hardening of steel and the chilling of cast-iron, and softening of either by heat; and I believe that from the mildest steel, containing only traces of carbon, to the highest cast-iron, we have simply one substance, iron, containing various quantities of alloys or substances in solution, and that the properties which we observe vary only in degree, due to the quantity of alloy that is present.

Let us consider now the effect of temperature on steel.

It is well known to all workers in steel and cast-iron, that the whole structure of the ingot or casting varies very decidedly with the temperature at which the metal is poured, and this fact is constantly made use of to produce desired results.

But outside of those who have a large and varied experience with steel, it is not so generally known what a sensitive substance it is, both in volume and structure; and that in every piece of steel that is in existence to-day, there is a sure record of the last temperature to which it was subjected, as well as of the manner in which the steel was worked.

I mean to say that for every variation of heat which is visible to the naked eye, there is a corresponding variation in structure, which is equally visible to the naked eye if the record be opened by fracturing the piece.

Professor Langley's research on the specific gravity of differently heated pieces of steel (*American Chemist*, November, 1876,) shows that there is also a different specific gravity for each difference of structure.

This being the case, there is, of necessity, a permanent internal strain for every variation in specific gravity, because each change in specific gravity means, of course, a corresponding change in volume.

These strains vary from the slightest up to those that produce rupture; the piece cracks.

A piece of .53 carbon steel will vary in specific gravity from 7.844 to 7.818 from the bar finished at ordinary red heat to the bar cooled from a scintillating heat respectively, a difference of .026. A bar of 1.079 carbon under the same conditions will vary in specific gravity from 7.825 to 7.690, a difference of .135.

This shows that for a double quantity of carbon we have five times the difference in specific gravity, due to an equal difference in temperature. This is the "mystery" of the brittleness, and the tendency to crack, in high steel. If engineers who are in the habit of dealing with structural steels are disposed to think that these are both cases of high steel, I will explain that these particular experiments were made on a fine grade of tool steel, and that compared to the ordinary Bessemer and open hearth steels, the .53 carbon tool steel would grade in softness and ductility about the same as .25 carbon Bessemer steel.

Experience teaches us too, that this rule of change in volume holds good all the way through the carbon series. A piece of .10 carbon steel may be heated white hot and plunged into water without breaking it; but if the same piece be quenched at a red heat, and also at a white heat, in different parts, and the parts are then broken, the different grains of the pieces will present a record of temperatures which once seen will never be forgotten.

On the other hand, if a piece of steel of 1.079 carbon be quenched at a bright orange color, it will be a very remarkable piece of steel if it does not fly to pieces. Granting these facts, does it follow that a piece of steel which has been unevenly heated, and so left in a strained condition, is injured irretrievably? No. This question brings us to the subject of annealing, a consideration of which will bring out some of the most useful and important properties of steel. Every piece of steel is at its best in all physical properties when it has been so annealed that it is in the condition which steel-makers call refined, that is to say, when the grain is in the finest condition possible, or when its crystals are the most minute and most uniform in size.

This statement is subject to a slight modification in considering a piece of hardened steel; when steel is hardened properly, the grain is slightly finer than it would have been if it had been allowed to cool slowly, but the difference is very slight, and if the hardened piece be subsequently annealed this difference disappears.

Each temper of steel refines at a different temperature.

A piece of .10 carbon steel will refine probably at a lemon color. I am not sure about this temper.

A piece of .30 carbon steel will refine at a dark lemon or bright orange color. A piece of 1.00 carbon steel will refine at a dark orange color, or the color that is reached just as the last shades of black disappear.

As a rule, the best heat to harden at is the refining heat, and the same heat is a good guide for annealing, although the heat may be raised very slightly in annealing high steel, but it should be done with great care, and it should be lowered considerably in annealing mild steel to avoid over-annealing. It is a remarkable, and probably the most important property of steel, that no matter what the grain may be, no matter how coarse from over-heating, or how irregular from uneven heating, if it be heated uniformly to the refining heat and kept at that heat long enough, the crystals will change in size and will all become small and uniform, so that the fracture will be so even that it will be called fine-grained and amorphous. I do not like that word amorphous, however, because the magnifying glass will reveal a crystalline structure in the most beautifully refined steel. If a piece of chilled cast-iron be kept at a bright red heat for an hour or two, the chill will not only become soft, but the long crystals will disappear altogether, and the whole piece will be ordinary looking gray cast-iron.

If a scalded or polarized ingot be kept at a bright red heat for an hour or two, it also will lose every trace of its needle-like polarized crystals, and will become a uniform fine-grained piece of steel, and it will be as tough as if it had never been scalded and brittle.

If any ingot be annealed properly, it will lose every vestige of its distinctive carbon crystallization; it will become refined and tough.

Unannealed ingots are brittle, easily broken with a sledge, and are distinctively marked; annealed ingots are fine-grained and tough, and must be cut with a set to be broken; and when broken, an effort to grade them by the fractures is the wildest guess-work, in which none but a great expert should indulge. If a well-annealed piece of steel is the best piece of steel in every respect, an over-annealed piece of steel is the very worst piece, and should always go right back into the melting furnace. Over-annealed steel is brittle, harsh, not ductile, will not harden and will not temper, and I know of no way but melting to make it good.

A friend of mine told me he had started a wire-drawing shop. He was trying to draw his own dead soft open-hearth steel; he knew it was of excellent quality, but he could not draw it at all. It was not ductile, it broke, it did everything but go through the dies, and he asked me if I would help him out of his trouble. I took him to the wire-shop and first pointed out the annealing furnace; he said, "Do you anneal at that heat?" I said yes. "What is the temper of your steel?" I told him a hundred carbon. He replied, "I am going right home; I don't want to go any further; there must be some fools up our way."

Over-annealing is caused by too much heat too long-continued. I think time has more influence than temperature, but whatever the cause may be, it changes the entire nature of the steel, and there seems to be no remedy.

The time required for annealing is arrived at by experience, and the time must be longer for a large piece than for a small one. I suppose the rule is, that the whole mass must be brought to the right heat, and must be kept at that heat but a short time; but I have no exact data to give, and it would be wrong to give any guesses that might be misleading. I cannot tell from analysis what change has taken place in over-annealed steel to make it so worthless, but I think it is probable that the injury is caused by the absorption of gases, because it is necessary sometimes to submit excessively hard steel to a longer heat than would be safe in an open fire, and this is accomplished safely by burying the steel in fine sand.

We can consider now the much discussed question as to whether steel and iron crystallize in service after a long duty, and having been subjected to many repetitions of strains, vibrations and shocks. If it be true that the largest crystals and the coarsest and weakest structure are formed when iron and steel are allowed to cool slowly and in a state of rest; and if the finest crystals and the best structure can only be formed by quick cooling and the violent agitation of the hammer or of the rolls; or by careful heating to just the right temperature to cause the formation of fine crystals, it would seem somewhat anomalous to assume that this is all reversed in the cold state, and that cold iron and steel can be shaken up into coarse crystals and a weak condition.

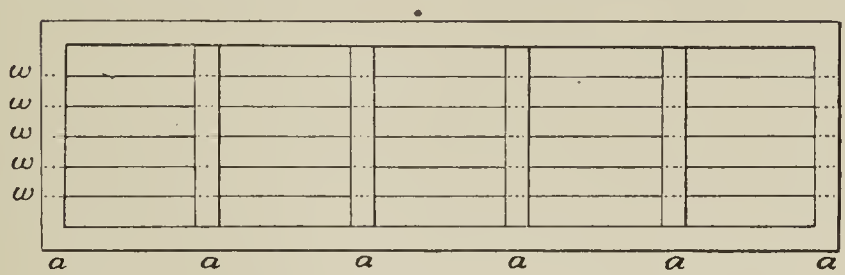
It may be possible that such an anomaly could exist, but it seems more reasonable to suppose that when an axle or a crank-pin breaks and develops in the interior large, fiery and weak crystals, that those crystals were formed there by too much heat, too slow cooling, and too little work when the piece was formed.

It is proper to remark here that the hammering of a round piece between flat dies is a dangerous operation; it is a common thing to find round hammered bars of steel burst in the middle for long distances, of which there is no evidence at the ends or on the surface; therefore, round pieces for structural purposes would be safer if they were hammered in swedges or rolled in grooves.

Piped ingots should be looked after too; it is quite likely that the hollow rail that broke with such disastrous results in New England lately was rolled from a piped billet.

It is but fair to add here that Professor Langley has observed that acids and the fumes of his laboratory will change the very structure of metals.

He had in his laboratory a frame of the following form:



The wires w, of copper, brass and German silver, were run through the cross-bars a, which were of wood, about 2 inches wide. After three years he noticed the wires breaking, and upon examination he found them to be coarsely crystalline, brittle—in fact rotten, and entirely changed in structure.

He found also that the parts that were in the wood, and so protected from the fumes, were soft, ductile, and entirely unaffected. All of the exposed wires were affected similarly, and all of the protected parts were equally unaffected, except the copper wire, which was stiffened, but not materially changed in structure.

We come next to the consideration of some of the physical properties of steel.

Mild steel, such as is used commonly for structural purposes, is more ductile, stronger, and tougher than iron; it is more easily and safely produced in large masses than iron, and when worked properly it can be put into the most difficult shapes and be made to do good duty.

High steel is hard and brittle, and generally of great tensile strength; its use is hazardous, because of its great change of volume for a slight change of temperature; yet it can be made very ductile by careful annealing, as is illustrated in the cold-rolled, cold-hammered, and cold-drawn samples before you, which contain 1.00 carbon, and have been worked cold into their present shapes.

This cold working has not injured the tempering properties of the steel, these being samples of ordinary clock spring, hair spring, and drill-wire steel. Hot working of steel increases its specific gravity, and cold working reduces its specific gravity; therefore cold bending of structural steel is not good practice, unless it be annealed afterwards.

When steel is to be subjected to repeated deflections or alternations of strain, the mild steel is the more enduring, if we can accept the conclusions of Mr. Benjamin Baker in connection with the Forth Bridge work; but when steel is to be subjected to rapid vibrations, as in a pitman or a hammer rod, a higher steel is much better than dead soft steel. In using higher steel, however, it is important to have it well annealed and perfectly smooth.

I have known a sound and good hammer-rod to break in an unusual place from having a fine lathe tool mark left on it, because the scratch formed a check to the free vibrations and allowed a crack to start, which ruptured the rod. This rod was not annealed.

No sharp angles or corners should ever be allowed in structural steels, because there is no fiber in steel; it is crystalline. Admitting the truth of Mr. Baker's conclusions in regard to alternating and torsional strains as he applied them, we cannot apply them to the deflections and torsions of springs.

A great railroad company discovered not long ago that the moduli of elasticity of mild, medium and hard steels, tempered and untempered, were practically the same. Next it was decided that the strains in coiled springs were torsional; then the moduli of elasticity were applied to the formula for torsion, and it was discovered that if the bars were of the proper size it would make no difference how much or how little carbon they contained, nor whether they were tempered or untempered, the springs would be all right.

Finally it was specified that no spring should contain less than .90 carbon, and, of course, they were to be tempered. This may sound absurd, but it only proved the wisdom of the engineers; they were smart enough to test their own formula, and the result was a well designed set of springs and an admirable specification.

Mild steel does not afford good resistance to abrasion, it is too ductile and flows too readily; the flow causes heating and increased friction, and the low tensile strength yields to the friction.

Dr. Dudley's famous paper on rail specifications and the wear of rails, proves this when it is interpreted properly, and subsequent experiments in Europe give, I think, without exception, the result that the hardest rails show the least wear.

Probably the greatest test of resistance to abrasion and flow is found in the wire-drawing die. These dies require to be made of excessively hard steel, as is shown by the following analysis of two unusually good dies.

Wire Dies.

Silicon	.013	.119
Phosphorus	.014	.019
Sulphur	.009	Trace
Manganese	.396	.356
Copper	.....	.019
Carbon	1.975	1.92

It will be observed that these might be called cast-iron, were it not for the fact that they are good malleable cast-steel. Such a die drew the other day, in my presence, a coil of 1.00 carbon wire 1 160 feet long from .105 to .092 inch diameter, and the diameter did not vary in the whole length of the coil .00025 inch.

The effect of the chemical constitution of steel is very marked, and is well defined in high-tool steel, but it is not so well defined in mild steel, nor in Bessemer and open-hearth steel, therefore engineers do well not to meddle with chemistry at present; but it is safe to assume, in all cases, that the nearer the steel comes to being pure iron and carbon the better it is. The last remark has no application to what might be called alloy steel; there are possibilities in the alloys of iron and other metals far beyond our present knowledge, but it is not the intention in this paper to indulge in speculations.

It may be gathered from what has been said, that in general it is better and wiser for engineers to adhere as closely as possible to mild steel for large structures, where the material is used in comparatively large masses.

First, because it is more ductile than high steel, and therefore not so liable to break under sudden stress; and second, because it can be safely worked into shape by less skilled hands than are required in the manipulation of high steel; yet there are cases where it is wise to take advantage of the superior strength of high steel in the largest structures, of which we have notable examples in the staves of the arches of the St. Louis Bridge and in the wire of the cables of the East River Bridge.

On the other hand, there seems to be danger in the enthusiam of some of the admirers of mild steel, whose statements that it will stand any amount of "abuse and punishment," etc., may mislead them and others into the idea that it can be handled without even as much care as is ordinarily bestowed upon wrought-iron.

If the statements made in this paper are accepted as facts, it must be obvious that care is always necessary, especially as regards heat, and particularly uneven heating.

I will give one case out of hundreds that have come under my notice to illustrate this point.

An engineer and bridge-builder brought to us a piece of steel about six inches wide and two inches thick, clean fractured. It was part of an eye-bar which had broken under a load which was, I believe, below the required elastic limit, and the engineer thought it could not be the mild steel it was supposed to be. The fracture was perfectly sound, and the grain was uniform and of proper luster, except on one corner, where in an area of less than one square inch were to be seen the large, bright, lustrous crystals, which registered "white heat."

We tried in various ways to harden the piece, but it was persistently "dead soft." Then we told our friend that that same thing would happen every time that his men heated up a corner of steel into the condition of that fiery corner.

Those weak crystals could have been annealed out of the bar, but it would have been a great deal better not to have put them there at all.

In reference to steam boilers, so far as strains are concerned, it would seem that high steel would be the best, but when we consider the daily alternations of heat and cold to which boilers are exposed—fire on one side, water on the other, mud deposits inside and bags outside—and the general ignorance of physics of the men who handle them, it is obvious that mild, tough steel is the only kind that is safe, and the milder and tougher the better.

Now as to guns.

The recent reading of the paper by Mr. Edward Bates Dorsey, M. Am. Soc. C. E., before the United States Naval Institute, has, for the first time, brought out from the advocates of built-up guns, clear statements of their views and the data upon which they are based.

All who read the admirable and temperate remarks of Lieutenant R. R. Ingersoll, Captain Rogers Birnie, Jr., and Lieutenant Austin M. Knight, must be convinced that these gentlemen have studied their subject thoroughly in regard to the properties necessary to a good gun, and with equal thoroughness, from a European standpoint, they have convinced themselves as to the best way to make the gun.

Messrs. Morgan and Davenport have shown also their exceeding skill in making the material which these gentlemen require.

Captain O. E. Michaelis, M. Am. Soc. C. E., Corps of Ordnance United States Army, however, seems to be inspired by the spirit and courage of Rodman, and knowing that Rodman started out with the right principle, he proposes to take up that principle where Rodman left it and carry it on into the higher domain of steel, where great success and great honor await the man who makes the first gun of the future.

My preceptors, Wade and Rodman, held that the qualities required in a gun were elasticity, springiness and power to resist abrasion, combined with high strength and a power to offer a uniform resistance in every direction to all of the strains to which it might be subjected.

All of the properties were reached in the highest degree possible in the material with which they had to work, and none of their guns ever failed.

If Rodman had lived, the advent of good steel in great masses would at once have been seized upon by him, and before now he would undoubtedly have cast the best and biggest, the safest and the cheapest guns that were ever made.

Rodman was a true engineer, and it was a cardinal principle with him, that any gun had a certain number of foot tons of work to do, whether it were to batter down an earthwork or to sink a ship; and he always claimed that the best gun was the gun that would do this work for the fewest cents per foot ton, including in the cost the making of the gun.

Some more modern writers seem to scorn this question of cost, and at the same time to insinuate that civilians have not a dollar at stake in the gun question anyhow.

Possibly not; yet there may be civilians who have a stupid notion that they can trace the ultimate cost down into the taxpayer's pocket, and the taxpayer may be thick-headed enough to think that he is helping to pay for his own protection.

When an officer berates those misguided people who delay appropriations, by making suggestions as to the best way to use the money, he reminds one of the average minister of the gospel, who when he is criticised, always says: "Now you stop, it is wrong for you to do that, you are interfering with my spiritual work."

I have not a word to say against the views given by the officers who discussed Mr. Dorsey's paper, as to the proper characteristics required in a metal which has to endure the strains to which a gun is subjected. That part of the subject they understand thoroughly.

I cannot imagine how a good gun could be made of dead soft steel. The bore of such a gun would enlarge from the first round; the lands of the rifling would give way under the pressure of the projectile; the vent would wear out rapidly; and altogether I should expect that after a hundred rounds such a gun would be about as symmetrical as an old battered hat.

My objection to proposed methods is: to the building-up system; the notion that "definite shrinkage" is a practical possibility; the idea that rings can be so shrunk together that each shall be strained to exactly its elastic limit, when in fact that elastic limit cannot be known; the enormous cost of unnecessary operations, and the doubtful utility of the operations after they are performed.

Lieutenant Ingersoll says:

"What we want with gun-steel is uniformity; but this should be a development with high rather than with low qualities, and the tendency and march of events indicate that this will be attained by: First, a more intimate chemical knowledge of steel; Second, a less barbarous forging-machine than the hammer; Third, annealing; Fourth, oil-tempering."

As to the "First," when the departments begin to dabble in the chemistry of steel there will be no more guns made; what is needed instead is an intimate knowledge of the physics of steel.

To the "Second," all will agree who know anything of the subject; and we may add, we want a less barbarous forging-machine than the hydraulic press. We want no forging-machine at all, the steel can be made to forge itself by static pressure and by heat.

To the "Third," there can be no objection, as there is no known way of getting improper strains out of high steel except by annealing.

The "Fourth" is of doubtful utility. It is not probable that the benefit derived, if there be any, can compensate for the cost, especially when we reflect that the parts are annealed subsequently. What is the object of the annealing? Mr. Davenport answers that as follows:

"It is generally admitted that the effect of tempering in oil or any other liquid is to *fix*, by rapid cooling, the amorphous conditions existing in the heated mass, thus preventing the formation of a coarsely crystalline structure, and destroying the irregular and more or less crystalline condition existing in every forging of considerable size when it leaves the hammer.

" Besides this, the molecular condition of the mass is far more uniform after treatment than before."

Thus is the whole forging business effectually damned by the trusted defender of the system, whose unquestioned skill and success in this hazardous business entitle his statements to the fullest credence. Lieutenant Knight quotes the lamented A. L. Holley, M. Am. Soc. C. E.; let us quote him a little farther than the gentleman went. Holley first quotes Chernoff's experiment: " A coarse-grained, sound, cast-steel ingot was cut lengthwise into four parts. One of the quarters was cut, in a lathe, into a test bar; the second was heated to a bright red, forged under a steam-hammer, the forging being stopped while the piece was yet rather hot (probably cherry-red); the third piece was heated up to the point at which the hammering of the preceding piece had been left off, and was allowed to cool slowly. The fracture showed a very fine grain similar to that of the forged piece. These two quarters were also turned into test-bars."

The results of the tensile tests were as follows:

	Breaking load.	Elongation.	Dynamic resistance.
Unforged	34.8	0.023	0.8
Forged	41.5	0.053	1.1
Annealed	38.7	0.166	3.21

Dynamic resistance in tons. Ultimate strength + ½ elongation.

"The obvious conclusion is that it is possible to make a steel in its cast state just as strong as if it had been hammered."

Next he gives a " bored, annealed, and tempered-in-oil" " gun-tube."

	Limit of elasticity, Tons per square inch.	Tension at rupture, Tons per square inch.	Elongation, per cent.
At the back, No. 1	22.0	42.5	11.1
" " 2	22.2	39.6	8.7
In front, " 1	22.5	38.1	15.1
" " 2	22.7	38.5	15.0

The New Navy Specifications Require for Tubes—

	Limit of elasticity, pounds per square inch.	Tensile strength, pounds per square inch.	Elongation, per cent.	Contraction of Area.
A..............	38 000	80 000	22	35
B..............	34 000	72 000	20	20
C..............	33 000	70 000	12	15

The unforged pieces are very close to the requirements. Holley gives other data, and then says of a test of the above tube, which was fired 100 rounds of heavy charges:

"No flaw of any kind was discovered, and the deformation of the chamber was found to be less than half the average in forged-steel tubes."

He then continues:

"It should appear * * * that the American system of cheap ordnance—cheap because it is cast—is to be successfully realized.

"If so, it will follow that the just criticism upon the standard American gun, that it is comparatively worthless because it is cast-iron will be reversed.

"We can hardly conceive of a fact of greater magnitude, from a defensive point of view, than this: that while the United States has at this moment not a single standard type of naval gun, or gun of position, that is comparable in efficiency with the guns of foreign states, it has, by means of the good policy of its Ordnance Department, studied the results of foreign experiments and avoided the enormous cost of original investigations, and that this policy must be now rewarded by the establishment of the cheap cast gun, the metal to be, not crude iron, but steel having three or four times the strength."

He then gives the cost of guns in 1865:

Armstrong 10.5 inch wrought-iron hoop gun....	33.6	cents per pound.
Krupp 15-inch solid steel gun..................	87.5	" "
Blakely 10-inch steel tube, hooped with steel....	78.5	" "
Whitworth 7-inch steel tube, hooped with steel..	64.5	" "
Parrott 10-inch cast-iron, wrought-iron hooped..	17.0	" "
Rodman 10-inch cast-iron......................	9.75	" "
" 15-inch "	13.2	" "

Holley continues:

"The hammering of a large mass of steel—for instance, a forty-ton ingot for a gun or marine shaft—is a very costly and hazardous undertaking. * * *

"Forging, under the heaviest hammers, reaches only the parts in the immediate vicinity of impact; the piece is therefore subjected to a series of internal strains due to the difference in the molecular arrangement of adjacent parts.

"It is thus left subject to internal strains which may cause ruptures when and where least expected."

We might be told just here that the Terre Noire people have not succeeded in producing good cast guns.

We know it. But they never carried out Rodman's principles, and therefore they have never tried what we advocate.

For the information of any who may not know, I will say that the Rodman plan consists in casting a gun on end, breech down, with a hollow core. Water is circulated through the core to cool the interior rapidly, and a strong fire is kept in the pit to keep the exterior of the gun warm, thus forcing the metal to contract all in one direction and on the interior. The operation is so simple, so easy, so sure and so scientific, that it is beyond criticism, and it would seem superfluous to add any further arguments than those given in the early pages of this paper, to make clear the possibilities of this process, properly applied, to steel. There are plants in the country now which only require the addition of some pits and moulds to prepare for the casting of 40-ton guns for trial; and the extension of these plants to the casting of 100-ton or 150-ton guns would cost but a comparative trifle.

The cost of one huge hammer, or one hydraulic plant, would build a half dozen casting plants.

Splendid steel castings up to thirty and forty tons weight can be bought now for less than six cents a pound, while we are told to think of forty cents a pound as the price of rough-bored, rough-turned, annealed, oil-tempered and annealed gun parts. My own opinion is that forty cents a pound is not a high a price for such work.

To this price must be added probably forty cents a pound more for the cost of finishing these much treated parts; this brings us very close to the figure given for the Krupp gun.

Now the people of the country are asked to bid on 1 310 tons of gun parts, and they are invited to build enormous plants to make them, and they are to be allowed thirty months in which to prepare.

1 310 tons at $800 per ton = $1 048 000.

This is a large sum of money, and if the work can be done for half of it, and the other half could be made to build the plant, the contractor would come out with his plant for profit, minus the interest and wear and tear.

Not a bad state of affairs if the plant were worth anything; but as such a plant could have no commercial value, since there would be no commercial use for it, as soon as Congress tired of making appropriations or the departments changed their minds about the style of guns they wanted, the plant would be a scrap heap and the profits would vanish into nothingness.

In conclusion of the gun subject I repeat what I have said before: I believe the built-up gun to be unscientific and unmechanical. To settle the question of the mode of manufacture, before the Government goes to enormous expense to build such guns, and before private parties make the hazardous investments involved in building the necesary plants, is it unwise to suggest the expediency of spending two or three hundred thousand dollars in testing the Rodman plan applied to steel, not merely because it is cheap, but because it is full of promise, and it is pre-eminently American? It served us well in our hour of extremity, and it laid the world at our feet.

It has cost Europe probably a hundred millions of dollars to produce an uncertain gun of greater power, and we may hope that it will cost her another hundred millions to produce something to equal America's gun of the future.

For ready reference, for the use of engineers, the statements made may be summarized as follows:

Iron and all metals are liquids.

Cold steel is congealed iron, containing in solution various ingredients, which give to it certain marked properties.

Heat is the power which gives to steel all of its good and all of its bad conditions.

Steel changes in volume and structure with every degree of heat that is applied to it, and the changes may be read in the fracture as surely as we read the changes in volume in the mercury column.

Slow, quiet cooling from a high temperature causes the formation of large, irregular crystals, and renders the steel weak.

Quick cooling and agitation, form small, uniform crystals and a strong condition.

The application of heat alone will change the form and the size of the crystals.

The change of volume due to a unit of heat increases as the content of carbon increases; therefore high carbon steel must be handled with exceeding care.

The temperature to which it was last subjected, moderated by its subsequent treatment, is always recorded in the structure of steel, and may be read there if the piece be fractured.

Annealing, making soft, ductile and uniform in texture, is the most important of all operations from an engineer's point of view.

Although annealing will relieve strains and change a coarse structure to a fine uniform grain, it must not be supposed for a moment that any amount of annealing will heal a rupture.

I do not believe there is any cure for a rupture, because I do not believe steel can be welded.

Steel being crystalline, has no fiber; therefore there should be no sharp angles, no sharp edges, and no unfilleted corners; the surfaces should be smooth and free from tool marks or indentations caused by sledge blows and the like.

With our present knowledge, the BEST STEEL for structural purposes is that which is most nearly composed of iron and carbon.

Finally, good steel, properly worked, is the most useful of all of man's productions, and it may always be relied upon to do its full work to its utmost limit; but if the laws of its being be violated, it will as certainly respond, causing disappointment and disaster.

DISCUSSION.

Lieutenant J. W. DANENHOWER, U. S. N.—I had not expected to speak this evening, and if I had I could not have come prepared to speak upon the chemical and physical properties of steel, which have been so ably treated by the writer of the paper under discussion. As to guns, I have had, certainly, experience. Mr. Metcalf has spoken ably about a theoretical gun, and I venture to say for myself, and for my brother officers at Annapolis, that we would be very glad indeed to have such a gun, particularly if it were cheap and convenient, and made of cast-steel on the Rodman principle, if that is possible.

There is one question that I would like to ask Mr. Metcalf, and that is: What becomes of the compression of the Rodman gun after the gun is annealed?

Certainly we are very willing to look to engineers of experience in building structures to aid us in this direction, and to look to steel manufacturers of the country to provide the material, and, if practicable, cast it as has been suggested.

You will remember that after the war we had a vast stock of ordnance on hand. Then followed with us about ten years of inaction, although foreign governments were developing armaments and arms. Then, when it was demanded by economy, we took the 11-inch gun, bored it out to thirteen inches, and inserted a cast-iron tube; put a ring on the muzzle to keep it in place, and used that gun very successfully. Our muzzle-loading rifle did better work than the corresponding gun in the English service. Initial velocity was a requirement of that caliber of gun ten years ago. An initial velocity of 1 400 feet per second was thought sufficient then, and, indeed, was deemed high; now we require an initial velocity of 2 100 feet.

It was found in Washington, after 1876, that the hooped guns did useful work. Krupp has made some 17 000 guns, and only 18 have burst.

The navy is limited in the length of its guns, because guns of too great length are not serviceable on shipboard. The 6 and 8-inch guns have met with success. We have one which has been fired 274 times without showing any sign of weakness. Those at Annapolis have been fired as rapidly as possible with a force of only ten men, and those only ignorant, ordinary hands; this shows how readily they can be managed. The 6-inch gun was fired ten times in eleven minutes. The initial velocity was 2 105 feet, while the pressure was only 14½ tons at the breech-chamber. That is what we desire, a good gun with a high initial velocity. We think we have got it now, and we want a good trial of it.

Two or three years ago Congress made an appropriation of $24 000 for casting three guns, one of open-hearth, one of Bessemer, and one of crucible steel. That is not a very large appropriation, but it is a pretty large slice out of the army appropriations to run the Bureau in Washington for ordnance for the navy.

There is little more that I can say, except this, that I am very glad this meeting has taken place, and that such men as those that compose the American Society of Civil Engineers have taken an interest in this subject.

Mr. William Metcalf.—As to Lieutenant Danenhower's question as to what becomes of the compression of the Rodman gun after the gun is annealed: Well, what becomes of the hammering after the gun is annealed? In making a gun, if the first preparation by annealing does not give that condition of strength and fineness necessary, it is perfectly obvious to any one that a simple way to get it is to heat it to the proper point, and bring every part of the steel to the best condition, then cool from the interior of the gun. It is a great deal cheaper and easier to apply heat than a hammer. It seems so simple that I almost wonder the question was asked.

As to being a theorist, it has amused me a great deal to be classed among theorists. I have made in the past years a hundred thousand tons of guns.

To justify the position I have taken, I want to say that my remarks apply to heavy guns; that is what I have talked about. The 6 and 8-inch guns are not heavy guns, they are little things. The difficulties increase as the square of the size, as Mr. Danenhower very justly remarks, in regard to the strain. It is a fact in regard to the hundred-ton gun, that the tensile strength they afford is of an extremely low grade.

Before the discussion goes any further, I would like to say that if we call the gun a steel cast gun when talking of it, instead of a "cast-steel" gun, we would be clearer in our argument. There is an abundance of cast-steel to-day, but no steel cast guns.

I said distinctly, and if I tried to make anything in the paper clear, it was as to the effect of hammering large masses; all of that hammering is worthless. All of the best properties of steel can be obtained by annealing from the ingot without any forging. I have showed that heat affects steel as certainly as it does a column of mercury, but after steel has been heated no one can know the elastic limit without testing for it; to assume that it is a certain amount, because some supposed to be known temperature has been applied to it, is mere guess-work.

Commander H. B. Robeson, U. S. N.—I had not expected to be called upon this evening, and came as a listener without intending to take part in the discussion.

I have been very much interested in Mr. Metcalf's paper, and have learned a great deal in regard to the working of steel, and its application and use in the manufacture of ordnance.

There is one point however which has not been touched upon this evening, and that is in regard to the weight of the proposed steel cast guns. This is an important consideration, especially in the construction of guns which are to be mounted in our modern ships.

In the cruiser or ironclad of the present day, the increased weight of coal, engines, armor, and the mechanical appliances necessary to promote the efficiency of our floating fighting machines, renders it imperative to utilize to the utmost every ton of displacement; and for this reason, if for no other, it is of great importance that no unnecessary weight should be added to the battery.

The increased length given to modern ordnance, in order to obtain the required muzzle velocity without unduly increasing the pressure in the bore, also renders necessary an increase of weight in the different calibers; and the built-up system was employed so that the gun could be made of steel having the requisite elasticity and a tensile strength, giving maximum durability and safety with a minimum weight of metal.

It undoubtedly will be a great advance in gun construction if the Rodman system can be successfully applied to the fabrication of steel cast guns. It would seem proper to say, however, that if this plan of construction is to increase the weight of the gun, it would be better adapted to ordnance mounted in forts and land defenses rather than on board ship.

I would like to ask Mr. Metcalf what would be the difference in the weight of the 6-inch guns, and whether he would propose to make a cast gun of the same weight as one of the built-up guns constructed by the Naval Bureau of Ordnance and now on board the Atlanta.

Lieutenant Danenhower has so thoroughly exhausted the subject that I can add but little more, except to say that it is a matter of great satisfaction to the officers of the navy that this question of gun construction has been taken up for discussion and is exciting so much attention.

Mr. William Metcalf.—As to the relative weights of built-up and cast steel guns, I have made no figures whatever. I do not know the dimensions of the 6 and 8-inch guns, having never seen either the guns nor the drawings of them. In all I have said about guns I have had in mind only large guns, say from one hundred tons upward.

Mr. R. W. Hunt, M. Am. Soc. C. E.—It is with great trepidation that I venture to say anything on the subject, particularly in this presence. I have not had any experience in making guns; but I may say from what I know of steel castings for other purposes, that I have a very strong hope that the theory of the steel cast gun will prove to be right. I think there is hardly an engineer in the country to-day who would trust a hammered steel shaft for large work. Experience has proven time after time that they are unreliable, no matter how much they have been annealed after forging. High cast-steel is giving satisfaction for many purposes. I only hope that Congress, or the Navy Department, or the War Department, being accelerated by Congress giving it some money, will have some cast guns made from it.

An ordnance officer has said to me: "If you steel men believe so strongly in steel cast guns, why don't some of you get together and make some?" We are patriotic, but at the same time we are committed to the principle that the Government should pay for public works; the general public should bear the expense.

Suppose that during the several weeks this matter has been discussed, the Government officials should have said to Mr. Wellman, for instance: "You have an order from this Government to make a gun for us as big as your establishment will permit." He would have executed it directly, while we have been talking about it, and the matter would have been well towards settlement. Unfortunately that has not been the case and if it had we would not have had this delightful evening, nor the opportunity of meeting here in discussion.

I will not detain you longer; I can only say that I sincerely hope that somebody will give us a chance to try this steel cast gun, and that I consider Mr. Metcalf's paper one of the most valuable yet written on the treatment of steel. It is worthy of a place as a text-book in metallurgy.

Mr. George Breed, U. S. N.—Mr. Hunt remarks that no hammered steel in America has yet been found satisfactory.

Mr. Hunt.—I said I thought that engineers were not willing to place their trust in hammered steel shafts because so many of them have been found unsatisfactory, and that the tendency is to rely on steel cast shafts.

Captain O. E. Michaelis, M. Am. Soc. C. E.—The announcement of the reading of this paper induced me to make a long journey to-day for the purpose of hearing it, and the valuable information imparted by the author has fully repaid me for any trouble taken. To speak with authority upon the construction of steel guns in all its phases demands the conjunction of three conditions: the utterances must be founded upon the science of the engineer, upon the practice of the steel manufacturer, and upon the experience of the gun-maker. Among the few men who in their persons unite these three necessary qualities, the essayist of the evening occupies a prominent place. Three years ago I read before the Society a paper upon "The Heavy Gun Question," * in which I advocated the trial of a steel gun cast on the Rodman principle, and I can say little to-night that would not be a repetition of what I then stated. It has been repeatedly argued that if the steel manufacturers have such faith in the practicability of a cast gun, why do they

*Transactions, Vol. XIII, p. 215, July, 1884.

not fabricate one and present it for trial? Many rejoinders can be and have been made. Possibly the simplest way of answering the question is to ask another. Why do not those steel-makers who have faith in the built-up gun fabricate and present for trial, say a 12-inch rifle? They have a great advantage at the start. They know that the system is approved by those in authority; they have the benefit of the researches and experiments, ably and thoroughly made by bright officers of the navy and army; and they have at their command the advice and the sympathy of these officers. Why then under such favoring circumstances do these gun-builders not produce at their expense a heavy rifle? All the preliminary experience is at hand, obtained by hard study and at great expense. Why then do they not undertake the construction at private expense of great guns? The reply usually is that in the direction of built-up guns we are working on a certainty, and that therefore Government is justified in "footing the bill." This certainty, according to the bulk of published opinion, is founded upon the results of European experience; though in justice to a few officers I must state that they claim that their views are founded upon American experience, and that their methods are radical departures from European models. This question of European successful experience with steel built-up guns, upon investigation, is found to depend upon the great success of the Krupp guns. These guns are made of crucible steel, and the substitution of open-hearth steel, if based upon experience alone, is almost as decided a departure from precedent as the proposed hollow steel cast gun founded upon the success of the Rodman cast-iron guns. No comfort can be drawn from the experience of England and France with their built-up guns. As regards England, I need only name the Colossus, the Active, the Collingwood, and the Ajax. In France I am informed that two 42-centimeter DeBange guns failed in the proof at Ruelle. And that the question is not so absolutely settled as the opponents of a Government trial of a steel cast gun would have us believe, is shown by our own experience. Notwithstanding the fine and original work done by our ordnance officers; notwithstanding the ability and the enthusiasm exhibited by our steel-makers, the tube of the 8-inch army gun of English make, the second one sent, the first having been rejected, has proved to have been subjected to injurious internal strains, and no one can foretell the moment of its failure. The science, the ability, the experience that evolved the most powerful cast-iron guns, that to-day has done such advanced work with the shrinkage system, is still at our service, and an opportunity can soon determine whether or no a steel cast gun is a feasible construction. I contend that enough has been said and written to justify the recommendation to Congress of a liberal appropriation for the exhaustive trial of the plan.

Even in England they are beginning to believe "that with good metal a considerable approach might be made by judicious treatment towards the qualities imparted by forging."

Even if it be proved that serviceable guns cannot be cast, the cost of the experiment will not be a waste of public money, for the experience necessarily gained in casting large masses of steel will be for the general good.

Colonel Wm. C. Church—Mr. President: I labor under a very serious disadvantage. I was detained to-night and was not able to listen to the paper. I am sufficiently well acquainted with Mr. Metcalf's views, however, to be quite sure that I indorse what he has said upon this subject.

I think to produce the best results, our ordnance officers should revise what they have learned from a too exclusive study of European examples, and with open minds come into consultation with American manufacturers who unite a practical experience in the handling of steel with a theoretical knowledge of its properties; and especially with one like Mr. Metcalf, who has had in addition a large experience in the manufacture of guns. Let them not overlook the significance of the fact that it is the foreign manufacturers—Krupp, Whitworth, Armstrong and others—who have led in the foreign development of ordnance, and afford our American steel men a like opportunity to test their abilities in the direction of further improvement.

Lieut. W. H. Jaques, U. S. N.—It is always a pleasure to listen to what the author of this paper under discussion has to say, particularly upon the subject of the "use of steel in heavy guns." It matters not whether they agree or disagree with him, you always learn something. Therefore my visits to Pittsburgh are looked forward to with much interest, even if they do not compass a meeting at the Duquesne Club, whose seducing influences, as many of you know, leave only the most delightful memories.

There are many elements of satisfaction to be found in the paper before us, for in the author's statements to the House Commission on Ordnance and War Ships, November 23d, 1885, and to the Senate Committee on Ordnance and War Ships, December 10th, 1884, he intimated that he had withdrawn from any interest or further action in connection with guns or the subjects relating to them. To the former commission he said:

"I am not interested in any open-hearth plant and I am done with the gun business." To the latter committee he stated: "I have no interest whatever in either heavy armor or guns except as an American citizen. I never want to see another gun, so I write freely, wishing to give you all the information that is possible. The concern with which I am connected is entirely out of the line of such work, and, for myself, I have had enough of it, and like to think of the gun business as belonging to the dead past."

That he has again taken up the subject is therefore a matter of deep interest, particularly since we find his views modified to so great an extent as to accept the results obtained by Messrs. Morgan and Davenport, the good work of my fellow ordnance officers, and the "views given by the officers who discussed Mr. Dorsey's paper, as to the proper characteristics required in a metal which has to endure the strains to which a gun is subjected. That part of the subject," he says, "they understand thoroughly."

The author's treatment of the subject also from a theological or spiritual standpoint is a unique departure, and one which I will not attempt to follow.

Passing over the author's very interesting history of and opinions concerning the properties of steel and its use in structures, I desire to call attention to some of his statements made in this paper, and previously, which bear directly upon the supply of heavy ordnance. On page 42 of the Report of the House Commission already referred to, will be found the following, appearing as a part of his testimony:

"Inasmuch as the large steel manufacturers of this country are willing to undertake the manufacture of heavy cast steel guns, provided the Government furnishes sufficient money for the experiment, now in order to do away with all jealousy and prevent talk about rings and favoritism, the following plan is proposed: Let the Government offer three prizes, large enough to enlist the confidence of manufacturers, to be given to those who succeed in making the best 6-inch cast steel guns, this size being within the limits of present capacity of nearly all our steel plants. Three prizes also for 12-inch guns, to be given to the successful competitors for the first prize. This plan would save millions of dollars to the Government and give the best attainable results.

"Let the guns be made according to the method deemed best by the manufacturers. All the guns to be submitted to the same destructive tests, and classed according to endurance, as 1, 2, 3, etc. There should be no effort made to keep our mechanics within the circle of the experiments of the English, German, or French, but leave them free to act as they think best; and in five years' time the results obtained will show a progress in the manufacture of heavy ordnance that would astonish the world."

There are serious objections to this plan, as it would restrict competition to cast guns; and, as Hon. Thomas B. Reed, of Maine, remarked in the consideration of the conference report on the Fortifications Bill, March 3d last, "the iron gun foundry which required no additional expenditure for plant could produce a gun to compete, while the steel manufactories, which would require an expenditure of $2 000 000 for additional plant could not afford to enter into the competition." The cast-steel gun would have greater ballistic power than the cast-iron gun, but the competition would be limited to guns of the former character; and as far as a comprehensive test, or a comparison of cast and built-up guns is concerned, we would be no nearer a solution of the problem than before, and the jealousies would be greater than ever.

If the Government is to offer prizes for the most efficient ordnance, the field should be unrestricted, except by conditions of efficiency. It would not be a difficult task for artillerists to lay down rules which would insure a thorough comparison of weight, economy, energy, penetration, etc. But let this competition be open to the world; to any kind of material; to any system of construction; and to any method of manufacture; the guns to be made at the competitors' expense, the Government paying a handsome reward to the successful one only.

The proposal of the Bethlehem Iron Company, March 22d last, describing the inauguration of a plan for the manufacture of modern steel gun forgings and solid steel armor, so long in advance of any positive inducement or guarantee from the Government that even a single contract will be awarded them, is a powerful and gratifying indorsement of the decision reached so long ago by the Naval Bureau of Ordnance that forged steel built-up guns were the best, and solid steel armor the most efficient.

This action of the Bethlehem Company in making such enormous expenditures is a practical lesson to the advocates of cast-steel ordnance, none of whom appear to have been willing to indorse their opinions by any financial risks.

Americans, as a rule, are not slow to undertake work if it contains a reasonable assurance of financial success. I therefore cannot understand why the Pittsburgh manufacturers, who have urged so firmly the use of cast guns, have not made them at their own expense and risk, if such guns can be manufactured so easily and so wondrously cheap as is claimed, and if such "great success and honor await the man who" will make them.

I heartily indorse the author's reference to the importance of the "question of cost," provided he does not sacrifice ballistic power. We must continue our estimates further than the first cost of the gun. A small increase, for instance, in the weight of a gun will largely augment the cost of application and use. From the beginning of my studies I have kept this carefully in view, and am confident that no recommendation of mine has omitted it.

The Naval Appropriation Bill, approved March 3d, 1887, contains a clause providing that

> "Twenty-four thousand dollars, or so much thereof as may be necessary, may be used, in the discretion of the Secretary of the Navy, for the purchase and completion of three steel cast, rough-bored and turned, six-inch high power rifle cannon of domestic manufacture, one of which shall be of Bessemer steel, one of open-hearth steel, and one of crucible steel; provided that the castings for said cannon shall not be paid for until the cannon shall have been completed and have successfully stood the statutory test."

This will be a partial answer at least to the author's suggestion of the expediency of spending \$200 000 or \$300 000 in testing the Rodman plan applied to steel. The act is not very clear, but it seems to me that the entire responsibility of design, finish and test should be put upon the manufacturer, and that the Department should not be required to accept the responsibility of design, finish and test while the manufacturer is only to make the rough casting. I beg no one will accept this reference as an under-valuation of the skill and experience required to make good castings, for I appreciate the difficulties that stand in the way of obtaining them. Terre Noire's experience alone will prove a weighty example.

The methods practiced in Mr. Metcalf's works in Pittsburgh suggests to me the following question. He says, "we want no forging machine at all; the steel can be made to forge itself by static pressure and by heat." Why then does he not send his products into the market without the enormous amount of forging he puts upon them by his rolls and hammers? Why does he not employ "heat," and what he calls "static pressure," instead of these forging instruments which he indicts as "barbarous?"

It will be seen therefore that the views of the author and those of the opponents of the theory of the equal or superior efficiency of cast-iron or cast-steel guns over built-up forged steel guns are not so wide apart, for Mr. Metcalf advocates that the best gun is the one that will do the greatest amount of work for the least money; that the qualities required in a gun are "elasticity, springiness and power to resist abrasion, combined with high strength and a power to offer a uniform resistance in every direction to all of the strains to which it might be subjected;" that "care is always necessary, especially as regards heat, and particularly uneven heating;" and concludes by saying that "good steel, properly worked, is the most useful of all man's productions, and it may always be relied upon to do its full work to its utmost limit; but if the laws of its being be violated, it will as certainly respond, causing disappointment and disaster;" all of which principles and opinions have been advanced or practiced by the authorities I named on page 81 of the discussion of Mr. Dorsey's paper.*

Whitworth, and Krupp, and Schneider have succeeded because they have heeded the laws that a long experience has provided; because they properly work and treat their steel; because they understand the benefits of annealing and the controlling power of heat; and they build up guns because they have acquired methods of performance which are both scientific and mechanical.

I thank you for your attention and for your courteous invitation to take part in this discussion.

Mr. William Metcalf.—In answer to Mr. Jacques' funny question as to why we use hammers and rolls in our business, I would say that our stock account shows that we have to make about 6 000 different sizes and shapes of bars to meet the wants of our patrons, and we don't

* Proceedings United States Naval Institute.

know how to cast these shapes or to get them economically in any other way than the one we use.

I must say here that I cannot understand how the three-prize proposition became attached to my name in the Ordnance report. I never made such a proposition; it belongs to some other person.

Dr. R. J. Gatling.—The paper of Mr. Metcalf on steel, etc., certainly contains much valuable information. My opinion is, steel for gun construction should be neither too hard nor too soft, and should have the combined qualities of toughness, strength, and elasticity. Such steel should be made from the most carefully selected material, and should not be over-heated; indeed, the greatest possible care should be exercised in all the processes of its manufacture. Gun steel, in my judgment, should not be hammered, for the reason that when large ingots are placed under the hammer, the blows of the hammer will harden and condense the parts of the steel struck, and in a measure render the mass of metal less uniform in texture; and it may be doubtful if the best system of tempering and annealing will bring it back to a homogeneous state. Hard steel, which contains a large percentage of carbon, if used in gun construction should be heated and annealed with the greatest possible care; and it often happens, after undergoing the most careful treatment, it will be left under internal strain and will be liable to crack or break when subjected to sudden shocks or violent and repeated strains. Such being the case, I think a mild and tough steel of good quality, that is uniform in texture and possessing a high limit of elasticity, will be the best material for gun construction. At all events guns made of such mild steel will not be so liable to burst, and consequently will be safer to the men using them than guns made of harder and higher grades of steel. It should be borne in mind that the best qualities of steel of any kind can be made worthless as gun material by over-heating and over-annealing. I concur in the views expressed by Mr. Metcalf, that heavy guns of great power (especially for fort use) can be cast of a good grade of steel on the Rodman principle, that will have great "elasticity, springiness, and power to resist abrasion, combined with high strength and power to offer a uniform resistance in every direction to all the strains to which they might be subjected." Such guns would be able to do good service ashore or afloat, and would be a great deal cheaper than built-up guns made from a high grade of hard steel. Steel cast guns designed for fort use could be, if necessary, made of greater weight than guns designed for naval service. If guns for naval use can be made on the built-up plan, that have more power in proportion to their weight than steel cast guns, then and in that case it would be well for such guns to be made, regardless of their cost; but in these days of economy it would seem to be but the part of wisdom to have cast guns made of good qualities of mild steel for fort use, cast on the Rodman principle, which would be equal in power and effectiveness to any guns in the world. Mr. Metcalf truly says: "If Rodman had lived, the advent of good steel in great masses would at once have been seized upon by him, and before now he would undoubtedly have cast the best and biggest, the safest and cheapest guns that were ever made."

Mr. F. Collingwood, M. Am. Soc. C. E.—The use of steel in large masses has been brought so prominently before the public within the last decade, that every engineer has been obliged to devote a certain time to the study of its qualities and behavior. Its introduction has been tentative, and its advocates have been forced to overcome the conservatism and the traditions of centuries.

The argument that has been most tenaciously held against its use, and which is not yet entirely overcome, is that of lack of uniformity. I remember very well a remark made to me by the late A. L. Holley, M. Am. Soc. C. E., who was an undoubted master in everything relating to steel, that there had "never been so large an amount of steel of the high grade and uniform quality required for the wire of the East River Bridge made under one contract;" but he ventured to say that "if asked for it would be produced." This was but about fifteen years ago.

The present controversy turns upon somewhat the same lines of thought. European engineers have become convinced that they can be more certain of the condition of the steel after its manufacture as to uniformity of composition and of strain, if it be cast in moderate-sized masses and built up into guns of the requisite dimensions by the ingenious devices and methods they have worked out.

On the contrary, Mr. Metcalf and others, reasoning from analogy, say that cast-iron and steel, being the same metal differently alloyed, but behaving essentially the same under the same conditions, the method which was so successful with the one is at least worthy of a trial with the other.

Were this reasoning advanced by a novice we might possibly hesitate; but when it comes from one whose experience with gun-casting is beyond that of perhaps any living man, and who is at the same time an acknowledged expert in the manufacture of steel, its rejection without fair trial can only be set down to the account of prejudice.

Situated as the authorities of the United States are, with the whole subject before them, it is inconceivable that they should be willing to launch the country upon the sea of expense that will be inevitable if a system of built-up guns be adopted without incurring first the very moderate expense that will be necessary for a thorough test of the Rodman system as applied to steel.

Theories in this case should give way to facts. The conditions are too complex to be theorized upon except in the most general way: and the engineering profession can only look on until the main facts have been made clear by experiment. So far as we now know from the experiments on built-up guns, uniformity of production has not by any means been reached, and there have been some lamentable failures. Mr. Metcalf points to the uniform success met with in the production of cast-iron guns, and claims that the manipulations necessary for a similar production in cast-steel are well understood and not difficult.

There is a substantial agreement as to what is required in the ideal gun; let us not follow in the beaten track as copyists, if by a bolder and truly American course we can do better by following up our own traditions. The result is worthy the effort.

Mr. Jaques.—I do not know to what particular guns Mr. Collingwood refers, but as the accident to the English Collingwood gun has been given great prominence, I ask your attention to the fact that this gun was not all steel, but built of an iron jacket and steel tube of low grade, which tube was not hooped to the muzzle. Of the material of this gun the investigating committee reported: "The metal was irregular in its character. The metal had not been subjected to annealing processes, annealing not having been adopted until a date subsequent to the manufacture of this gun."

I ask also your consideration of an extract from my letter to the New York Chamber of Commerce, dated February 2d, 1887, as follows: In regard to the reported failures of modern steel guns, I assert that there never has been an explosion of a modern high-power, forged steel, built-up gun of the material and system of construction recommended by the Gun Foundry Board in 1883, and now being manufactured by the Naval Bureau of Ordnance.

Mr. Joseph M. Knap, M. Am. Soc. C. E.—I agree with the author of the paper under discussion that we have not yet arrived at the solution of the heavy gun problem, and coincide with his views that the best heavy gun of the future will be that known as the steel cast gun.

As regards smaller guns, say from 8-inch caliber and less, such satisfactory results have been obtained both in Europe and in this country with built-up guns made of forged steel, that our Government should be encouraged in their efforts in this direction.

While I would stand second to none in my admiration and appreciation of the late General Rodman, both as a scientist and an eminently practical ordnance officer and inventor, I must say that while he gave us the best heavy gun of his day, and thus placed us a long way in advance of all other nations in the matter of heavy ordnance, that is no reason why we as Americans should decry everything European, and shut our eyes to the fact that during the last twenty years other nations have made long strides in advance of us.

The essayist has given us the bright side of the Rodman cast-iron gun; it had its dark side as well. Much has been said of the treacherous nature of steel, more particularly that of high steel. What metal is not treacherous? Cast-iron certainly is. I well remember, while associated with Mr. Metcalf in the manufacture of guns at Fort Pitt Foundry, our losing two or three fifteen-inch Rodman guns by their breaking in the mould. In each case the result was due to bad manipulation—carelessness in preparation of the small hollow core leading through the sinking head, by which a portion of the water was allowed to trickle down between the mould and the surface of the gun, chilling and weakening that portion to such an extent that it could not withstand the shrinkage strain. The metal and treatment in the cases of these cracked guns were the same as usual. No doubt had a weaker and softer iron been used, the rupture would not have occurred; but General Rodman did not lower his limit of density and tenacity on that account, for he held that the higher the density and tensile strength of the metal consistent with safety as regards shrinkage strain, the stronger and more enduring would be the gun. It should be so, in my opinion, in regard to steel guns. Mr. Dorsey, in his paper on "Steel for Heavy Guns," recently read before the U. S. Naval Institute, advocates the use of "mild" steel with a very low percentage of carbon, and having a tensile strength of 55 600 to 65 000 pounds, his main argument being that such steel is not so liable to break by the application of sudden shocks as steel of a higher density and tenacity.

Why not go back to soft cast-iron guns and done with it, if hardness and tensile strength and elasticity are not requisites in a gun?

One advantage gained by General Rodman's method was the securing of a higher density and increased tenacity of the metal near the bore, where the strain due to impact and wear due to abrasion are the greatest.

The history of the manufacture of the heavy cast-iron guns in this country shows that a constant increase in the density and tensile strength of the metal led to a corresponding improvement in the guns, both as regards their endurance and reliability.

Of course there was a limit, and there is doubtless such a limit in steel. Nothing but practice and careful experimenting will decide where that limit is; but there have been enough both in this country and in Europe to show us that very reliable serviceable guns of medium caliber may be constructed of forged steel having a tensile strength of from 90 000 to 100 000 pounds, and elastic limit of 45 000 to 50 000 pounds. Now, while I would advocate the construction of such guns without delay for our army and navy, it seems to me that Congress should be urged to appropriate a sufficient sum for the construction of a cast-steel gun of large caliber—say 12 inches bore and 50 tons weight, to throw a projectile weighing about one thousand pounds.

The necessary additional cost of plant to that already available in private establishments in the United States would be trifling compared to the outlay which would be required for heavy hammers, etc., for the construction of a built-up gun of the same size; and the cost of manufacture would be far less.

Too much credit cannot be given to the ordnance officers of our army and navy who, in spite of limited appropriations, have brought our steel ordnance up to its present high standard of efficiency; and whether the Rodman of the steel gun era is evolved from the army, the navy or from civil life, let us not fail to recognize the admirable services rendered by the "Gun Foundry and Fortification" Boards, and the "Select Committee on Ordnance and War Ships," nor to appreciate the painstaking, laborious and intelligent work of our friends in the ordnance corps in both branches of the service.

Mr. A. H. Emery.—I did not come here this evening with the intention of making any extended remarks on the subject of ordnance, but I would say that it is a subject to which I have given a great deal of thought and upon which I have spent considerable money. I have also spent several years of my life in that kind of work, which has given me a very considerable knowledge of materials.

In regard to the paper by Mr. Metcalf, I have read it with a great deal of pleasure. I visited the foundry during the construction of the first 15-inch gun that was ever made; it was a great gun at that time. The charge was 30 pounds of pebble powder with a 300-pound shell. This was afterwards increased to 50 pounds. Three 20-inch guns have since been made on the same plan; the 20-inch bearing a charge of 200 pounds of coarse powder, with a 1 000-pound projectile. We require to-day not 300 pounds of metal for a projectile for the 15-inch gun, but 1 600; and not a velocity of 1 400 feet, but at least 2 000 feet at the muzzle of the gun. It is believed by engineers who have given much thought to the subject of cast-iron, and the action which occurs in the interior, that a metal with a tensile strength of 30 000 to 33 000 pounds, no matter how uniform, cannot endure those strains which must come upon the interior of the gun to give this high velocity to such heavy projectiles. I know there are some interested parties who are willing to risk money in the belief that such a velocity can be reached.

Mr. Hunt, of the South Boston Iron Foundry, has cast a 12-inch rifle which weighs about one hundred and eight thousand pounds, which has succeeded in giving a projectile of 800 pounds weight a velocity of 1 750 feet; that is a very great degree of success.

Mr. Wm. J. McAlpine, Past President Am. Soc. C. E.—Was that a cast-iron gun?

Mr. Emery.—Yes, sir; a cast-iron gun. There is no doubt in my mind that Mr. Hunt can repeat that gun a hundred times and it will be just as good as the first one. Mr. Hunt is of the belief that he can make those guns of the same length that we propose to make the steel guns, which will be able to increase the velocity of the same projectile (800 pounds) to 2 000 feet per second. My own opinion is that he will be disappointed if he makes the effort; that the additional strain upon the gun will burst it before it has had many firings. I have no doubt that such a gun would stand firing a number of times with that velocity, but not anything like 200 rounds.

The cast-iron gun, I will here say, has one element of value and strength which many builders of steel guns have not properly considered, and that is the ability of cast-iron to bear high strains of compression without being crushed. It is true cast-iron yields under strains of compressiou with a low pressure past its limit of elasticity, but when this metal has passed its limit of elasticity, the yielding is not so great but that it transmits this pressure to the walls of the gun without flowing.

An experiment was made at the Watertown Arsenal with some cast-iron cylinders. They were bored out and lined with wrought iron, as the interior of the guns had been bored and lined. The wrought-iron had a limit of elasticity at twenty-two thousand to twenty-three thousand pounds to the square inch; the cast-iron had a limit of elasticity at less than fifteen thousand pounds. These cylinders were all 11 inches in diameter outside, and when finished, 3.3 inches inside; one set was bored out to 3.5 inches and lined with .10-inch copper; another set was bored out 5.1 inches to receive a wrought-iron lining .9 inches thick. Now we have $\frac{9}{10}$-inch of wrought-iron lining in one set to offset $\frac{1}{10}$-inch of copper and $\frac{8}{10}$-inch of cast-iron in the other set; that is to say, the 11-inch exterior has a 3.5-inch bore in each case, but we have replaced the interior walls of one set by $\frac{9}{10}$ of an inch of wrought-iron to take the place of $\frac{8}{10}$-inch cast-iron and $\frac{1}{10}$-inch of copper in the other set.

But on being tested under the pressure of bees-wax on the interior, the set which was bored out and lined with wrought-iron burst with much lower pressure than did the set which was lined with $\frac{1}{10}$ inch of copper, because the wrought-iron flowed with much lower pressure than did the cast-iron which it replaced. The pressure in bursting the cast-iron cylinder was something exceeeding 90 000 pounds per square inch. The wrought-iron commenced flowing with less than 30 000; it was flowing very considerably with 40 000, and rapidly with 50 000 to the square inch, and was then aiding the wax to burst the walls around it. The cast-iron did not flow on the interior but slightly by that pressure which burst the cylinder, hence the ability to bear a much greater pressure before bursting.

Now if we take the same considerations and pass to the soft steel gun, we will certainly find that if we undertake to produce large pressures in the bore and hold them within the walls, that the interior walls will flow and try to burst the exterior.

In regard to making a gun of one piece of cast-steel annealed, I would say that some years since I was at Pittsburgh and visited the works in which Mr. Metcalf is interested, and was pleased to see a forging machine taking cold bars of steel and reducing the bars very considerably; it passed through the machines again and again cold, being reduced in diameter by the hammers each time. I was very much impressed by the reducing of this bar of cold steel by the rotating hammers. I have had occasion to use a number of those bars. I must say that they are very fine steel, better, I think, than casting can possibly be.

I think in regard to the low cost of the solid cast gun (I mean by that a steel gun cast in one piece and cooled from the interior) that we shall not find the cost low, but the quality may be. I do not expect to live until we can make a large mass of steel as good as the small masses have been made. As regards the cost, I agree with Mr. Sellers, who says in regard to making cast guns in one piece that if he were to do it he would require a greater price than for the built-up gun. He would consider the difficulty of getting a gun of good quality so great, so many of them would have to be condemned with the balance all uncertain, that he would greatly prefer to forge the gun in pieces and bore and assemble them.

As to what the gun of the future will be, I may say briefly this—built-up gun of steel—and I would say that my experience as an engineer teaches me that the smaller the mass we finish the pieces of steel in, working it by the hammer or hydraulic forging, the smaller the mass the better we shall find its quality; the smaller the mass, the better we can inspect it and know whether it is good.

I want to digress from Mr. Metcalf's paper so far as to answer one point in a paper presented by Mr. Dorsey before the U. S. Naval Institute, in which he stated that no engineer would put high steel to a structural use.

There was one place where I could not put a large mass of metal, this was a pin to go through the bars which had to be broken. I was obliged to keep the diameter of the pin which endures the whole load to 6 inches; and in order that I might get a piece of steel strong enough to stand this without being crushed, that would endure the shocks with safety, I used a piece of steel so hard that the men who dressed it in the lathes stated that they had great difficulty in drilling the centers. That steel has been in place for eight years and appears to be as strong to-day as the day it left the lathes; the pins are, so far as I last heard, entirely uninjured, so that some engineers would trust high steel.

Mr. Emery (in reply to a question).—If there is no initial strain in the metals, then the question will be determined very largely by the length of the barrel. If the section loaded is very short, the solid ring will bear considerably more than the two rings, and if the section loaded is very long there will be no difference whatever.

Mr. Theodore Cooper, M. Am. Soc. C. E.—What do you mean by very long?

Mr. Emery.—I mean to say ten or fifteen diameters. If the whole interior is loaded and the tube is not more than one caliber thick and the length of the tube was ten or more diameters, so that it is loaded through its whole length, there would be no difference between bursting the one with a solid wall or the one where two or more tubes of the same quality were used to make walls of the same thickness.

Mr. Cooper.—Is that based on a test?

Mr. Emery.—It is a matter of fact.

Mr. Cooper.—You have experiments bearing that out?

Mr. Emery.—Yes, sir; it is perfectly immaterial whether the wall of a given thickness is made of 50 or 100 tubes, so long as the quality is the same and there are no initial strains.

Mr. Cooper.—Have you tests showing that?

Mr. Emery.—No; but these are the facts of the case.

Mr. Cooper.—It is not a fact but an opinion.

Mr. Emery.—If the section loaded on the interior has the same amount of compression on the loading material, the amount of tension with the same quality of metal will be the same on the walls with the thin tubes as with the thick tube; and it is immaterial whether that is a pound or 50 000 to the square inch until the limit of elasticity is reached, and then I see little or no difference to be expected until after more distortion than would ruin the green tube.

Mr. Cooper.—I don't agree with you.

Mr. Emery.—When several thicknesses of tubes are put together I don't know any difference between the strength of several and the strength of one of equal thickness and quality; but in the case of thick walls like these, the interior tube must break first, because the strain is greater than on the exterior.

Mr. John Coffin.—In order to make what I shall say intelligible, I will preface my opinions by referring somewhat extendedly to an article written by J. A. Brinell, which is, in my opinion, the leading one of the age upon this subject. It was published in the first number for 1885 of *Jern Kontoret's Annaler*, in *Stahl und Eisen* for November, 1885, and in "Notes on the Construction of Ordnance, No. 37," printed June 22d, 1886. The *Stahl und Eisen* edition is exhausted, but *Jern Kontoret's Annaler* may still be obtained, and is very valuable for its excellent plates, which are the best I have ever seen.

Believing, in this case, an understanding of the subject will be more easily reached if the ordinary course is reversed, I repeat the Summary of Conclusions first, after explaining that by "hardening" carbon is meant that form of carbon found in hardened steel (whatever it may be), and by "cement" carbon that form of carbon found in annealed steel.

Summary of Conclusions.

First.—If steel loses its coarse crystalline structure without mechanical treatment, the change of structure must always be accompanied by the change of "cement" carbon to "hardening" carbon, or *vice versa*, and the cause of the breaking up of a coarse crystalline structure is always the change of carbon.

Second.—A coarse crystalline structure is entirely broken up only when the change of carbon is caused by heating; the coarse crystalline structure of either hardened or unhardened steel being completely broken up as soon as the temperature just reaches the point necessary to cause the change from "cement" to "hardening" carbon.

Third.—Steel which has been heated white hot must, if it is desired to change its carbon to the cement form, be cooled slowly to a temperature below that at which "cement" carbon is changed to "hardening" carbon while heating.

Fourth.—The change from "cement" carbon to "hardening" carbon is very rapid as soon as the temperature has reached the proper point. On the contrary, "hardening" carbon changes to "cement" carbon very gradually.

Fifth.—The change from "hardening" carbon to "cement" carbon is always accompanied by a production of heat, and it seems probable therefore that the change from "cement" carbon to "hardening" carbon is accompanied by an absorption of heat.

Seventh.—Rapid cooling never gives an amorphous or fine crystalline structure in steel which was of a coarse crystalline structure just before quenching; but it prevents a steel which was amorphous, or melted just before quenching from becoming crystalline during the cooling. In other words, rapid cooling fixes the structure which the steel possessed before quenching.

Eighth.—Change of carbon from the "hardening" to the "cement" form requires not only a correct temperature, but a certain amount of time; while, on the contrary, the change from the "cement" to the "hardening" form depends only on the temperature; and quenching seems consequently to prevent any change from "hardening" to "cement" form.

Ninth.—Crystallization also requires time, as well as a certain temperature; and if the time of cooling is shortened by quenching in water or by other means, the formation of crystals is limited or entirely prevented.

I omit conclusion sixth, thinking the experiments cited do not verify it, and am tempted to believe it is a misprint, or that the true meaning is obscured in the translation, as numerous experiments which I have made seem to refute it.

I think conclusion third would be a better deduction from the experiments, and a fuller statement of it if written: "Steel, while at a heat above that at which its carbon was changed to 'hardening' carbon while heating, must, if it is desired to change its carbon to 'cement' form, be cooled to a temperature below that at which 'cement' carbon is changed to 'hardening' carbon."

I have given the foregoing conclusions in full, for I believe them to be correctly drawn, having repeated about eighty of the experiments given, and tried many original ones; and although I have formed opinions differing from his in some instances, I indorse the majority of his statements.

Brinell uses the term "cement" carbon probably in deference to Akerman and Rinman, but until chemists agree more fully upon the question of the chemical relation of carbon to steel, and while theories of solutions, combinations and alloys are at war with each other, I prefer the much more expressive and simple term "non-hardening" carbon, and hereafter I will use this expression to indicate that kind of carbon found to be the greatest part of the carbon in annealed steel.

I append an illustration, Plate XXII, copied from Brinell's article, showing the appearance in the dark, of a flat bar of tool steel while cooling, after being heated at one end to a bright orange.

One might judge, while performing this experiment, that during the time of cooling (or at some time in its transition from the hot to the cold state) it was slightly reheated, since portions that a moment before were colder than other portions, become hotter even when more exposed to cooling influences. Opinions based on this fact alone are not however conclusive, for the measure of the heat by the eye being only a comparative one, the fact that the relative heat of different portions of the bar changes, does not prove that the colder portions becomes any hotter, for this result might have been produced by the hotter portion growing very much colder. But the order in which these changes occur, leads us to believe that the steel is actually self-heating.

The first and most noticeable change of appearance is the formation and subsequent widening (in one direction only) of a bright band located between the hot and cold portions of the bar. Accepting the theory of the reheating of this band being caused by the change of carbon, we would infer that the first appearance of the band indicates the point of lowest temperature at which the carbon was in the hardening form before cooling; and as the point of lowest temperature of the hardening carbon portion of the bar is that point lying next to the non-hardening carbon portion, it follows that the bright band is the boundary line between the different forms of carbon.

I have tried many hardening experiments to prove this fact. Referring to Fig. 5, you will see a dark spot in the center of the bar, which, if this theory is correct, would indicate that in this place the carbon has not been changed to the non-hardening form, and if the piece be cooled in water the spot will be hard. Such we find to be the fact, anomalous as it may seem.

Another point in the observation of the cooling bar tends to strengthen conclusion third; the bright band does not appear until some time has elapsed and the bar has cooled somewhat.

To prove beyond a reasonable doubt that there really was a self-heating, and a consequent expansion, I tried a contraction experiment, and found that a bar of .90 per cent. carbon, four feet long, at one time during its transition from a hot to a cold state expanded $\frac{1}{32}$ of an inch.

I append Plate XXIII, a somewhat abridged copy of Brinell's chart, which furnishes us with graphic illustrations of the experiments for determining changes in the structure of steel. The horizontal lines indicate different degrees of heat, the lower lines representing cold or black heat, the upper ones white heat. *W* is the most suitable hardening temperature, or the refining temperature. *V* is the temperature at which the principal change from hardening carbon to non-hardening carbon commences, and is between red and low red in the dark. The vertical lines show the way in which the experiment was performed, the light lines meaning slow heating or cooling, and the heavy lines sudden cooling in water. In all cases the slow cooling was done by imbedding the steel in dry coal dust, if possible; otherwise it was allowed to cool in the open air. Small circles indicate the starting point, arrow heads the end of the experiment.

The eighty-two experiments were made with steel from the same Bessemer ingot, the chemical composition of which was as follows:

Carbon	.502
Manganese	.48
Silicon	.13
Sulphur	.Trace.
Phosphorus	.026

The letter underneath refers to the type of fracture, a composite fracture being indicated by two letters, and the following are the descriptions thereof.

A.—Coarse-pointed crystalline. The large crystalline surfaces shining. Color of fracture tending to blue.

B.—Pointed crystalline. Surface of crystals bright. Color of fracture tending to blue.

C.—Fine-pointed crystalline. Crystals small and bright. Color bluish.

D.—Coarse granular. The large crystal surfaces of a silvery color. Color of fracture white.

E.—Fine granular crystalline. The small crystal surfaces silver-white.

F.—Light amorphous. No plain crystallization can be seen by the naked eye.

G.—Flaky crystalline. Surfaces of crystals bright. Color of fracture somewhat bluish.

H.—Dark amorphous. Fracture soft and dull with no crystallization.

I.—Large coarse crystalline. Crystal surfaces looking like unpolished silver.

The treatment of the steel before the experiments was as follows:

Group I.—Finished forged at a red heat and cooled slowly. Fracture *C.*

Group II.—Heated white hot and then cooled slowly in dry coal dust. Fracture *A.*

Group III.—Heated white hot and then quenched in water. Fracture *D*.

Group IV.—Heated to temperature *W* and then quenched in water. Fracture *F*.

Group V.—The same as IV. Fracture *F*.
Group VI.—The same as I. Fracture *C*.
Group VII.—The same as I. Fracture *C*.
Group VIII.—The same as I. Fracture *C*.
Group IX.—The same as I. Fracture *C*.
Group X.—Melted.

I have placed charts before you and explained them thus exhaustively, because I wish to refer to some of the experiments in my subsequent remarks; and the article from which they are copied has never, to my knowledge, been published in English, except as an ordnance note, which had a limited circulation. The *Iron Age* published the conclusions some time ago.

Annealing Steel.

Annealing steel may be properly treated under three heads:

First.—Changing its carbon.

Second.—Removing its internal strains.

Third.—Changing its structure, or refining it.

If a piece of steel is hardened and then slowly heated, its carbon begins to change to the non-hardening form at a very low temperature. Not having studied the changes which occur when it is heated below a bright straw color, I am not prepared to say there are any, though I am inclined to think some take place.

In my examination of Brinell's acid test for hardening carbon, I made the following experiment with twelve pieces of tool steel cut from the same bar, each piece being one inch square and a quarter of an inch in thickness. I first numbered them on one side, and hardened them uniformly, then polished the unmarked side and annealed them (drew the temper) as follows:

No. 1, I left hard; No. 2, I made a very faint straw color; No. 3, a darker straw color, and so on up to No. 10, which I made an ash-gray color. No. 11 showed a very little red in a dark room, and No. 10 was a full red, or as soft as I could make it. I then repolished the surfaces, and placed on each a drop of nitric acid with specific gravity 1.23, leaving it for 45 seconds before washing in running water. I then arranged them in order by examining the colors, and, on turning them over I found I had followed the numerical order. There was a good deal of difficulty in distinguishing the dead hard one from the light straw color, and also with regard to the three which had been made hottest. No. 1 was a brownish-black, and No. 12 a bluish-gray. The reason of these different shades of color I do not know, but that the variation is caused by the fact of the carbon in each case being in a different form, seems very likely, since a piece of hardened steel of low carbon gives the same blackish color, even though it may be softer than No. 10 of the above series.

Although the comparison of the different kinds of carbon by this test seems to end practically at a blue heat, beyond this point drillings may be readily made, and direct chemical tests be tried.

The elastic limit of a tensile specimen is a good comparative test where the elastic limit of the same steel, having the same structure, is known when fully annealed.

The carbon is not all changed to non-hardening carbon; or, in other words, the temper is not all drawn out until a low but distinct red is reached. In all steel ranging from 20 per cent. up, my experiments have tended to prove this statement true, and though I had never experimented on boiler-plate stock, I intend to do so as soon as I am prepared for it.

Mr. Metcalf says: "The recent United States Navy specifications would read better if they said to be annealed, hardened in oil, and have all the temper drawn out."

Perhaps they might read better, but they would call for a quality of steel never before used in guns, as it is not customary anywhere in the world to draw the temper all out for this purpose.

We have been making gun steel under the following specifications: Elastic limit, 55 000 pounds; elongation 12 per cent., in a 6-inch test piece; and a piece with the temper all drawn out gave elastic limit 44 000 pounds; ultimate, 86 150 pounds; elongation, 25 per cent.

The fact is our gun steel is tempered steel, and I think, from numerous experiments, the temper is not all drawn out until a temperature is reached corresponding to *V* on Brinell's chart. I can see no reason for a connection between the temperature at which carbon begins to change to non-hardening carbon while cooling, and the one at which the change ends while heating, but they seem to be one and the same. The influence of time on the change to non-hardening carbon appears to be limited after the lapse of about two hours, since at a regular low heat all the carbon that can possibly change at that temperature seems to change at that time, and if it is desired to make the steel softer, the heat must be increased. If in drawing the temper of a gun hoop we fail to get it soft enough, repeated heating to the same temperature seems to have no effect on it, it being necessary to raise the temperature in order to draw the temper more.

Internal strains are caused by unequal cooling after casting; by hammering at a cool heat; by tempering; or by unequal cooling of any kind. Whatever may have caused them, however, they may always be removed by annealing the steel properly, and I will endeavor to show that those caused by tempering, though alike in their effect, can be removed at a lower heat.

I have discovered that while the carbon is changing from hardening to non-hardening, or *vice versa*, the steel is very weak and easily bent, and I will illustrate the fact by the following beautiful experiments: I took a bar of steel whose length and section were sufficient to sustain its weight when it was supported at the ends in a furnace at a temperature above *W*. This bar was heated to a temperature greater than *W* while lying on the level furnace bottom, then raised and supports placed under the ends. After half an hour in this position it had not deflected an appreciable amount, but on being taken from the furnace and allowed to cool while similarly supported, as soon as temperature *V* was reached its downward motion was so rapid as to be readily seen by the eye.

Being then straightened, its behavior was observed while heating on the supports, and it was found to pass temperature *V* without any deflection, but at temperature *W* it bent down as it had before done at temperature *V* while cooling; in both cases it bent while the carbon was changing form. To discover if the same weakness occurred while the carbon was changing to the non-hardening form during the drawing of the temper of hardened pieces, I performed the following experiment. In Plate XXIV, *A* and *B* were pieces of half inch square steel, five inches long, cut from the same bar, and filed as shown, leaving bearing places at *c*, *d* and *e* a trifle higher than the rest of the bar. Care was taken to have the bars of exactly the same section, and they were then uniformly hardened, and the temper drawn to a bright blue on the one marked *A*. The bearing places *c*, *d* and *e* on both bars were then ground and lapped until they fitted on a surface plate, the center spot *d* on both being left an appreciable degree higher, so that when pushed on the end they would turn on the middle, although light could not be seen under the ends. They were then polished on the sides and clamped together at the ends, as shown, with a very thin strip of metal separating them in the middle. The strain was not severe, for after one clamp was put on the other ends could be easily held together with the fingers. They were then heated slowly and uniformly to a very light straw color and allowed to cool slowly. On *A* there was no change of carbon during this heating, for it had previously been heated to a blue. I found it still revolved on the center, and was in exactly the same condition, as far as I could determine, as before. On *B* there was a change of carbon during the heating, as I found it was bent to such an extent that it revolved on the ends and light could be seen under the middle, but only a very small portion of the tension put on by the clamps was removed.

I have formed the conclusion that if there is a change of carbon during annealing, internal strains are removed at a lower heat; but from the detail record of physical tests, verified by experiment on rings put under known tension and heated, I have concluded that all internal strains were removed below *V*. When steel is heated to remove its coarse crystalline structure, it is necessary to fully understand the effect of heating to achieve the best results. You will see by referring to

Group II, Brinell's experiment, where a steel was previously heated high and cooled slowly, there was no change in reheating experiments 1, 2 and 3 both giving fracture *A*, but as soon as temperature *W* was reached, in experiment 4, we have a different fracture. The same result is observed in experiments 7, 8, 9 and 10. Comparing experiments 4 and 10, in which the same temperature was attained, we find that 10 gives the amorphous fracture *F*, while 4 gives the pointed crystalline fracture *C*. It appears evident that all these crystals were formed while cooling slowly from *W*, and if you will refer to Group V, you will see that no crystallization took place below the temperature *V*; so it is fair to presume that in experiment 4, Group II, all the crystallization took place between the temperature *W* and *V*. I see no reason why crystallization should take place only when the carbon in steel is in the hardening form, but such seems to be the case.

Another fact not shown by these experiments, but observed by myself in many cases, is that the slower steel is cooled from temperature *W* to temperature *V*, the coarser its crystalline structure will be. The steel bars on which these experiments were tried were about three-eighths of an inch thick; therefore as the time in passing between given temperatures must necessarily be short compared with the time required for a large forging to pass through the same range, the thought may suggest itself to some that if, for example in experiment 3, Group II, the hightest temperature shown had been maintained for several hours, the fracture would have been different. I have not found this to be true. Hundreds of examples present themselves to my mind in which such a temperature had been maintained for hours and even for days, without affecting the fracture. We broke the other day a large 5-foot steel pinion which had been running two years. This pinion, before putting in, had been annealed by building a small furnace around it and firing with coal. The annealing certainly benefited it very much, by taking out the internal strains, else it would not have given such good service, but the fracture of the broken tooth looked exactly like the fracture of an unannealed steel-casting. A steel roll was annealed in a pit by means of a wood fire being made around it, and after the desired heat was reached the pit was sealed up. I noticed that the temperature was not quite uniform, the top being a little cooler than the bottom. Now the effect of heating in this pit with a large mass of glowing charcoal and unburnt wood is to maintain the temperature for a long time, yet in dressing out the wobblers it was found that while the side that was down in the pit was fine-pointed crystalline, the top side gave the characteristic fracture of an unannealed casting. Since the above pieces were annealed, we have, I think, made some progress in the art of annealing steel castings. Our mode of operating now is to heat uniformly to the temperature *W*, and if the very best results are desired, we open the sides of the furnace and cool as rapidly as is possible by drafts of air until temperature *V* is reached, when we seal up the furnace and cool slowly. The temperature W does not vary much for different grades of ste It seems to be modified more by the varying amounts of manganese and silicon than of carbon.

I am not prepared to state at present exactly what these effects are, but will say broadly that the temperature W is practically the same for all grades of steel we use, commencing at .25 per cent. carbon, Bessemer and open-hearth, up to 1.50 per cent. carbon, tool steel. You have before you two samples of steel, the one labeled A is 1.50 per cent. carbon and .14 per cent. manganese. The one labeled B is .50 per cent. carbon and .67 per cent manganese.

You will see by examination of the fractures they are refined about alike. They were heated to the same temperature as near as possible without putting them in a lead bath. They were heated side by side in a large charcoal fire with very little blast. When the temperature W was reached, they were quenched in water. Neither piece is very good steel; the soft one, you will observe has a piping, the hard one happens to be a poor piece of tool steel. I cannot give the reason.

While trying these experiments I thought I had a piece of .30 per cent. carbon, but afterwards found it to be .50.

Sample C is before you, to show the effect of refining three times a piece of steel which had been very much overheated. Perhaps twice would have been enough had I known it was .50 per cent. carbon.

It is shown by these experiments that the act of changing carbon to hardening form causes a breaking up of crystals. We also see that there are important phenomena manifested when carbon changes to non-hardening form, and among these is the tendency to break up crystals, as shown by the interchange of crystal particles at the faces causing dull crystal faces, and it seems fair to suppose that the only reason why the crystals are not entirely broken up as in the other case, is because the temperature is not high enough. My experiments in this direction are not complete enough to warrant me in offering this, except as a mere conjecture; but if it be true, and I am of the belief that subsequent experiments will establish its truth, it follows that if steel is to be in any way refined by the change of carbon to non-hardening form, the higher the temperature at which this change takes place the better the refinement. I will illustrate. Suppose we have two pieces of steel under treatment. The first piece is heated up to W, and becomes for the moment amorphous. It is then quenched suddenly in water, and presents a fine bright surface, nearly amorphous as seen by the naked eye. It is then heated nearly up to V, or at a temperature high enough for all its carbon to pass into the non-hardening form; this change of carbon causes a partial destruction of the small crystals, so that under a very strong glass it may appear amorphous, and the color of the fracture is now much darker than before, showing that there are fewer reflecting faces. The second piece under consideration is heated in the same way to W, cooled rapidly to V and then cooled slowly.

We have the steel then in exactly the same conditions as regards its carbon, and these conditions were achieved at a different temperature; that is, the carbon of the first piece is changed at the lower limit of its temperature of change, and that of the second piece at its higher. The sample I submit to you (marked *G*) was treated by the second method. Under an achromatic triplet of high power I can detect no crystal forms; and though I recognize the term amorphous as applied to steel only a comparative one, this sample, I think, is fully entitled to the distinction. Steel treated in this way is very tough indeed. This piece had sharp *V* grooves planed across, as shown, before treatment, yet it was so tough that it bent through an angle of at least forty-five degrees from its original position before breaking. It was from the same bar of .50 per cent. carbon steel as the other fracture tests.

In comparison with this fracture and the others obtained by sudden cooling alone from temperature *W*, I present the specimen *D* obtained from the same bar of steel, by heating to the temperature *W* and slowly cooling in hot ashes. The character and strength of this form of crystallization is enough of itself to furnish matter for a long discussion, but I will not enter into it now, except by calling your attention to samples *E* and *F*, *E* being from a properly annealed casting, and *F* from an unannealed one.

The conclusions we have reached bearing on the subject under discussion may be summarized as follows:

First.—Steel must in any case be heated to temperature *W* to break up its coarse crystalline structure, a lower temperature continued for a long time having little or no effect upon it.

Second.—The more rapidly steel is cooled from temperature *W* to temperature *V*, the less crystallization will take place, and the stronger the resulting structure will be.

Third.—The slower steel is cooled from temperature *V* the more carbon will pass into the non-hardening form, and the greater interchange of molecules will occur between the crystal faces, giving them more cohesion, and, other things being equal, making the steel more tough and ductile.

Fourth.—Internal strains are removed at a lower temperature in steel which has its carbon in the hardening form before heating.

Fifth.—All, or very nearly all, internal strains are removed in any case by heating to temperature *V*.

Considering the question, Can we make a Rodman gun of steel such as we now use for steel castings? It is necessary to consider the definition of a Rodman gun. If a Rodman gun is a cast gun, having its initial tension developed by unequal cooling from the molten state in the mould, I answer positively, no, for steel castings have coarse crystals, strong in themselves, but weak in their union; and cooled rapidly from one direction, prismatic crystals are formed with their axes normal to the cooling surfaces (see sample), having very little cohesion between their side faces. These crystals cannot be broken up without reheating to temperature W, and before temperature W is reached the initial tension is lost.

If a Rodman gun is a cast gun, in which the initial tension is attained by some heating and cooling method other than shrinkage of separate hoops, the question broadens itself out into new fields and becomes one of elastic limit, or material accuracy of manipulation and certainty of results. If anything is ever accomplished in this direction, the imperative steps of the process will be casting with the first cooling portions down; cooling evenly to prevent internal strains severe enough to cause automatic rupture, or the starting of such; reheating to temperature W, and cooling suddenly enough to temperature V to prevent a weak crystal formation; and finally, attaining the desired initial tension by heating and unequally cooling below temperature V. The difficulties in the way are great. A very large piece of varying section; uncertainty of maintaining desired temperatures throughout; a limited knowledge of the effect of these temperatures; and the great cost of all experimental work in this direction, lead me to believe that the day is very far distant for Pittsburgh's dream to be realized.

I am a patriotic believer in American talent and enterprise, and if solid steel gun construction is ever successfully accomplished, I would like to have the honors here, but in our struggle to again gain the front let us not like the crawfish go backward.

Commander C. F. Goodrich, U. S. N.—In common with other officers who have been more or less identified with the new guns, I confess to a confidence in them which I believe to be justified by the firings at the proving ground. Nevertheless the naval service is not, I trust, bigoted, and if Mr. Metcalf or any other person can give it weapons as efficient, as light, and as safe as those now coming into use, with the added merit of greater cheapness, I am sure they will be welcomed.

It is hardly worth while to record my doubts of the entire success of the proposed scheme. Such a procedure would be equivalent to prejudging a case at law already on trial in the courts.

The last naval appropriation provides for the building of three cast-steel 6-inch cannon; and their behavior in practice will settle the question more fully than is possible by any amount of discussion, however intelligent and honest.

In conclusion, let me say that the interest manifested by the engineering community in this subject of such vital importance is full of promise to us naval officers.

Lieut.-Commander F. M. Barber, U. S. N.—The well-known reputation of Mr. Metcalf entitles his paper, so far as it relates to heavy guns, to more consideration than that of any other metallurgist in the United States, because he is the only practical expert who takes up squarely the position that modern high-powered guns of any size can be made of steel, cast in one piece on the Rodman principle. I do not value the favorable opinions expressed by non-practical men, such as army and navy officers, in a matter of this kind, because we do not have the shop experience, and outside of the mill the only method of obtaining information is by reading, inquiry and study, and the more one searches the more he becomes convinced that there is no information whatever on the subject of hollow-casting steel guns.

I have had access to the information on file both in the War and Navy Departments (and I know of no other source of equal value), and there is nothing that I can find which affords proof that any one has ever had success in casting whole unforged steel guns, either solid or hollow, except in field pieces. All the information is of a negative character and the opinions of writers are qualified. Mr. Wellman, of the Otis Works, for example, says in the Naval Institute discussion on steel for heavy guns.

"One thing has not to my knowledge been proved as yet, and that is that the high ductility that is asked for in hard gun steel is at all necessary. If this should prove not to be the case, then I see no reason why as good, if not a better gun, cannot be made by some modification of the Rodman process applied to a steel cast gun."

Mr. L. S. Burt, of Steelton, in a letter to Captain Michaelis, in 1884, thinks that they can be made, but states that the risks incurred in handling steel is much greater than that of handling iron, as it chills so quickly; and wants to know, if in the event of failure, would the loss fall on the contractors.

Mr. A. Purcel, of Terre Noire, the most famous steel-casting establishment in the world), has, so far as known, never tried the Rodman principle on guns, but even with regard to large castings of any kind he says in his paper read before the Iron and Steel Institute at Vienna, in 1883 :

"But the final solution of this problem is still a long way off. The production of castings of any form and of any dimensions in steel of a well-determined chemical composition, combining the resistance and rigidity of steel with the smooth surface and homogenity of iron castings, is a very complicated problem and one which presents material difficulties of more than one kind."

At the time he was reading this paper, the most important work at Terre Noire was some hydraulic cylinders intended to stand a pressure of 10 000 pounds which were to be supplied to a Paris engineering firm. The physical characteristics were high and the test specimens gave excellent results. In the *Iron Age* of February 10, 1887, is recorded the fate of these cylinders; one of them burst at 1 000 pounds and the others were condemned. Four years ago Terre Noire had perfect success in casting small gun hoops. Large amounts of money have been expended there by the company and practical encouragement has been given by the French Government, but we cannot hear that they have yet advanced from hoops to jackets, still less from jackets to tubes, to say nothing of complete guns. It is rumored that they are now in financial trouble. Still, as Mr. Metcalf says, they have never tried the Rodman principle. One wonders why they have not, as it has been an open secret for more than twenty-five years.

At Bofor's, in Sweden, they have succeeded in making cast-steel gun tubes up to 4.5 inches in diameter of bore (they use the Terre Noire process for getting solid castings), but the gun tubes are hooped with forged steel. No one since the date of Holley's book (1865) has produced better results than this, and yet the solid steel casting is only the starting point for a successful forged gun. It has not been found possible up to this time to make a modern high-powered forged steel gun except by commencing with perfectly flawless steel castings, either solid or hollow—for Whitworth patented the latter over twenty years ago—so small in proportion to the mechanical means employed, that each can be worked by mechanical means as well as by tempering and annealing, and each separate part submitted to inspection before the gun is put together. This is why the gun is necessarily built up. If it is unmechanical, as Mr. Metcalf says, there are some eleven obstinate jurors who disagree with him. If "definite shrinkage" is not a practical possibility, it is so near it that we get guns that are stronger than by any other process of fabrication yet practically established.

Mr. Metcalf thoroughly understands steel; he states in his paper more difficulties with it than I ever knew before. He points to the extrusion of foreign elements and the sinking by gravity when the metal cools; the variation of structure corresponding to the variation of temperature; the variation of specific gravity; the change in volume; the permanent internal strains arising from the two; the variation of the carbon; the cooling; the hardening; the annealing; the over-annealing; the non-fibrous, but crystalline character; the avoidance of angles and the marked effect of the chemical constitution. He states all these things with great clearness, and also specifies with equal lucidity what is sound doctrine as to the requirements of the metal of which a high-powered gun should be made, and then concludes by saying that he can take this very peculiar metal and cast a gun of any dimensions from it on the Rodman principle.

Like the gentleman whose opinion of his adversary was so high that he would not disagree with him even when he asserted two and two made five, I certainly am not prepared to dispute Mr. Metcalf's ability to do what he says he can; but his own enunciation of the peculiarities of the metal would seem to show why it is so difficult for other practical men to find financial success in making large steel castings where high requirements are demanded in the metal.

There is an apparent confusion regarding this matter of making cast-steel guns by the Rodman method. Mr. Metcalf describes the well-known Rodman plan of chilling the interior of the bore by a circulation of water, and he says he would apply this to steel, but omits to say anything at this time of any after-treatment. Captain Michaelis (who Mr. Metcalf says "seems to be inspired by the spirit and courage of Rodman") states in his paper, read before your Society June 10th, 1884, that he proposes to carry out "a purely American idea—casting from open-hearth steel a Rodman gun annealed from the interior." Holley, who is the favorite authority on this subject, is quoted by Captain Michaelis as saying, "The casting of a piece which has the desired shape and requires no reheating beyond a slow annealing, is so great a progress that it must be obvious to all practical men, especially when it is considered that the product possesses in every part of its homogeneous mass all the physical qualities of forged steel." Holley wrote this at a time when he and every other intelligent man was profoundly impressed with the Terre Noire discovery, and he naturally prophesied a future for it which the lack of uniformity of its product has failed to verify. It should be noted that the remarks I have quoted from Mr. Purcel of Terre Noire, were made two years after Mr. Holley's death.

Now it is, I believe, the prevailing opinion that the Rodman method was designed to introduce initial strains in the casting, and so successful was it that, according to Mr. A. H. Emery (in his testimony before the Conference Committee on the Fortifications Bill, February 10th, 1887), a 15-inch Rodman gun, cast at Fort Pitt about 1863, and fired over one hundred rounds, showed seventeen years afterward strong evidence of its remarkable structure. The gun was condemned on account of scoring in the vent, and was cut up at the South Boston Iron-works some ten years ago. The first cut was taken in front of the trunnions, where the gun was 44 inches in diameter; the tool had gone into it but two inches when the gun cracked entirely through for three-fifths of the circumference with a considerable report, showing that it had been under a heavy strain all these years. Simpern's "Gunnery," published in 1861, says that two 8-inch guns were cast to test the endurance under fire. One was cast solid, the other hollow. The former endured but 73 rounds, the latter fired 1 500 rounds without bursting.

Now are Messrs. Rodman, Metcalf, Michaelis and Holley moving on the same line of thought, or are they not? Is the Rodman plan of casting alone a form of annealing? We ordinarily understand annealing to be a process intended to relieve a casting from internal strains, and if the gun is annealed from the interior after it is cooled, what becomes of the peculiar initial strains which constitute the Rodman principle; and if we continue this annealing until we reach what Holley admires, "that the product possesses in every part of its homogeneous mass all the physical qualities of forged steel" (which simply means uniformity), are we not still further away from the Rodman principle? If gun steel will cast on the Rodman principle, why is it necessary to anneal it afterward at all? I believe the popular superstition is that it chills and shrinks and swells to a greater degree and more irregularly than cast-iron, and does all kinds of things that it ought not to do. Whether this is because the carbon is chemically combined in the steel and mechanically in the cast-iron, according to the molecular theory, or whether the cement carbon turns to hardening carbon, or *vice versa*, whenever steel changes its crystalline structure without mechanical treatment, as asserted in *Stahl und Eisen*, November, 1885, I am unable to say. It is a case in which variety of theoretical information appears more likely to lead to confusion of ideas than to anything else. Still Mr. Metcalf has said elsewhere that if the first cooling were insufficient, subsequent heatings and coolings could be resorted to cheaply and effectively. It is an assertion from an able man, and we are now back to our first position. I am unable to dispute it.

Whatever may be the correctness of Mr. Metcalf's theory, private capital has up to this time been too timid to support it, and the result of the bidding on the navy gun contracts of March 22d, shows that capital can be found in ample quantities to support other ideas as to what the gun of the present is, whatever may be the gun of the future.

I sincerely hope, in connection with this matter, that the clause of the Navy bill appropriating $20 000 for successful cast-steel guns of 6-inch bore will cause Mr. Metcalf's theory to be as thoroughly tried as it can be with such small guns, and I am sure that there is no one in the navy but has the best wishes for his success. I think however that he is rather unkind in that portion of his paper pertaining to "modern writers scorning cost, and insinuating that civilians have not a dollar at stake in the gun question anyhow." This and the succeeding paragraph are calculated to give the impression that officers in authority are arrayed in violent opposition to his theory, and are therefore not only ignorant, but impracticable. The fact is, that so far as the officers in the Departments are concerned (and all other officers, so far as I am aware), there is no hostile feeling whatever. Up to the present time Mr. Metcalf and his followers have deliberately kept his theory in the same category with the theories of the hundreds of regularly recognized disciples of Colonel Sellers (Captain Michaelis asks for an annual appropriation of two millions in his paper), while the wrought-steel gun men have not done so. "The old flag and an appropriation" is no longer recognized by the committees in Congress as a sufficient reason for the expenditure of public money to demonstrate any one's theory. There is a vast difference between appropriating money to pay for a specified quality of goods delivered (the cost of production being at the expense of responsible manufacturers who will guarantee their work) and appropriating money to demonstrate a theory of manufacture and then appropriating more money to pay for the goods themselves if the theory should prove correct, and still more money to compensate the inventor, the Government to lose the original outlay if the theory is not capable of demonstration. There is no question but the Government should pay handsomely for every idea and invention that it adopts, even if it pays the expense of investigation itself (though there are many officers who disagree with me on this latter point); but in this particular case what are we going to do for guns meantime, and what are the wrought-steel men going to say? Under the present condition of circumstances, in view of the enormous expenditure in prospect, the most serious question that Congress has had to solve is not how many kinds of guns shall we appropriate for, but which kind, and it is the disciples of Colonel Sellers who have succeeded in defeating the Fortifications Bill for two successive years by obscuring this point. They would defeat it next year also if Secretary Whitney had not fortunately solved the problem meantime.

I congratulate Mr. Metcalf on his happy association of officers with so respectable a portion of the community as the clergy. It is a compliment we do not often receive, but it does not appear that his covert sneer at religion adds much to the force of his judgment. I am sorry to say, however, on behalf of Mr. Metcalf, that our labors, either official or spiritual, are no longer to have the benefit of the advice of one who has been so long our guide, counsellor and friend, since he says in the closing paragraph of his testimony before the Senate Committee on Ordnance and War Ships, December 10th, 1884: "I never want to see another gun, so I write freely, wishing to give you all the information that is possible. The concern with which I am connected is entirely out of the line of such work and for myself I have had enough of it, and like to think of the gun business as belonging to the dead past."

There is only one point in Mr. Metcalf's paper which I particularly wish to criticise, and that is the statement that no Rodman gun ever failed. The statement is probably carelessly made, and may be intended only as a generality; but it is one that has been going round the country very generally during the past few months, and it should be corrected. In the report of the Joint Committee on Ordnance, Senate, page 215, Fiftieth Congress, third Session, there is a list of 249 cast-iron guns which have burst in the United States, mostly during the war. Of these 141 burst under fire, 10 burst spontaneously, and 98 cracked, fissured or ruptured before proof.

Those which failed under fire were as follows:

Parrott rifles	86
Rodman rifles	6
Dahlgren rifles	29
Rodman smooth bore	18
Dahlgren smooth bore	2

Of the 10 which burst spontaneously, 5 were Rodman and 5 were Dahlgren, and a circumstantial account is given of each case.

In conclusion, a few remarks about the late Mr. Holley's writings may not be out of place in order to illustrate the absurdity of quoting him as the advocate of Mr. Metcalf's theories. Holley's "Ordnance and Armor" was published in 1865. On page 417 he says:

"The soundness of steel castings, especially those produced by Naylor, Vickers & Co. (of England) and by the Bochum Company in Prussia, induced Captain Blakely to construct parts of some of his guns, such as outer jackets, of hollow ingots not forged but only annealed, and there is a growing impression in England that the heaviest ordnance will be cast solid from steel."

That is one for Mr. Metcalf. On page 46 he says:

"Messrs. Naylor, Vickers & Co. are perhaps more skilled than any other steel-makers, except the Bochum Company in Prussia, in the art of casting large masses of all shapes, such as tubes, bells, wheels, etc., sound and uniform throughout. It is considered however that the increase of strength by hammering will always warrant the expense of hammering in gun-work."

That is one for the wrought-steel men.

In his "General Conclusions on the Requirements of Guns" however, page 287, which it is fair to conclude are his own opinions, he says:

"On the whole a steel tube so tempered (probably by hardening in oil) as to have the greatest elongation within its elastic limits, and forced into (or otherwise compressed within) a heavy cast-iron jacket of good shape like the United States 15-inch hollow cast navy gun with trunnions and cascabel cast on for cheapness—the slight initial compression of the steel being sufficient to compensate for its want of safe elongation—would appear to be the best system of fabricating strong, cheap and trustworthy cannon of large caliber."

Now who is to find consolation at this day in that opinion?

I respectfully submit the following explanation as reasonable. When Mr. Holley was connected with the "Stevens' Battery," he was for a long time in England with very little to occupy his active, intelligent mind. He at this time collected the information so well expressed in his exceedingly valuable book. He himself was surprised at the popularity of the book, because he had only gone into the subject as an independent, unbiased, unprejudiced thinker, whose real business was entirely different from gun-making. It was undoubtedly this fairness of treatment which gave the book a value which the production of a hobby-horseman could never have equaled. But the book once finished, he lost all active interest in the subject (which the hobby-horseman would not have done), and his business, which was connected with the manufacture of Bessemer steel, fully occupying his mind, he never brought out a modern edition of his grand work. He was urged to do this not long before he died (in the spring of 1881), but he said that the gun and armor business had gone away beyond where it was in his day, and he had not kept up with the subject. It seems to me it is as fair to suppose that he would, if he were alive, be in line with the majority of other careful thinkers, as that he would be an advocate of cast-steel guns on the Rodman principle.

I question the value of the essayist's argument that the Rodman plan of casting should be applied to steel because it is "pre-eminently American." Why should Rodman be glorified at the expense of other American inventors? The success of his invention is undoubted; but it must not be forgotten that its value has been very much disputed even with cast-iron men, and long after the war was over Government made cast-iron guns at Fort Pitt, both solid and hollow for comparison. And in any event how can it be considered "pre-eminently American" when nearly all the other gun inventions now adopted in Europe are American in their origin also? The built-up gun itself is as much American as European. The best system of breech closure, now known as "the French" is undisputedly American. An American gas check is more used than any other in Europe. Slow burning powder is American. All the best machine guns and small arms are American, and so are the cartridges. Round forged projectiles, expansion bands, and rear fuzes are American, and I have been told on good authority that an old farmer from Ohio originated, over thirty years ago, the Whitworth system of rifling which has always given the greatest ranges to cannon.

All these inventions found a home abroad, and were developed there simply because the soil of Europe appears to be better adapted to the growth of bayonets than it is to plow-shares, while ours fortunately is not; but all the same the inventions are American and their readoption in whole or in part in the land of their birth can hardly be deemed unpatriotic.

Mr. William Metcalf.—If Mr. Barber will read carefully the discussion on Mr. Dorsey's paper, he will find, written by some officer, a sneer at the opinions of those "who have not a dollar at stake."

I distinctly disavow any "covert sneer at religion," as my faith in Christianity is incomparably deeper than my faith in any guns. I did crack a joke at a well-known peculiarity of the members of the highest profession, but it was at the men and not at their principles.

Lieut. R. R. Ingersoll, U. S. N.—I have read with very great interest Mr. Metcalf's paper on "steel," and have been much instructed by it. It is not my purpose to attempt a discussion of any part of the paper which does not relate to the finished gun. The best plan to obtain certain physical characteristics in gun steel belongs to the domain of manufacturers of steel, and while the various processes possess peculiar interest to the student of ordnance matters, yet there are none so competent to judge of the methods by which good gun steel is produced as those engaged in that work.

The condition in which the finished gun is delivered to the artillerist is however a legitimate topic for a naval officer to discuss. I regret that the lecturer gave but few facts concerning the condition and strength of the gun as it leaves the hands of the gun-founder.

No one will question the value of the Rodman process for producing a stronger cast-iron gun than can be obtained by any other method of construction with that metal. When applied to steel the natural conclusion would be that the process of hollow casting, if successfully accomplished, would give a stronger steel gun than by any other system of construction when steel of the same quality is used. Up to this point, the completion of the casting, there will be but little difference of opinion, but the steel cast gun is subjected to the process of annealing and it is in regard to the effect of this process on the elastic strength of the finished gun that I venture to ask the lecturer to give us a little more light.

All who have studied the subject of the strength of guns will agree that the principle of "initial tensions" must be utilized to the greatest possible extent in any system of gun construction. That stronger guns of the same weight and caliber can be made by its aid than by any other process which does not use it in some form.

This important principle is best illustrated in the Rodman process of casting, if it could be carried out to the theoretical limit, which is to produce an initial compression of the bore equal to the elastic limit of the metal for compression, the outside layers of the gun being in a state of initial tension. Now the question arises: What effect has the process of annealing on the initial tensions of a hollow cast steel gun? The popular idea is that the annealing of steel is resorted to primarily to get rid of the tensions which may exist throughout the mass, and if that is the effect of annealing, what becomes of the increased strength of the gun which is obtained by putting the bore in initial compression due to external tensions. It is an absolute necessity for the steel cast gun to retain the initial tensions produced by hollow castings, because if the gun by annealing is allowed to return to a normal state, a homogeneous tube of excellent steel, it can be shown without a doubt that it cannot compete in elastic strength with a steel built-up gun of the same weight and caliber in which equally good steel is used. Take as an illustration a 6-inch gun. At the powder chamber the internal and external radii are 3.5 inches and 10.25 inches respectively. If the steel has an elastic limit of 45 000 pounds per square inch, the safe powder pressure which will not deform the bore is found by Clavarino's formulas to be 13 tons. But we already use a powder pressure of 15 tons per square inch, and hope soon to be able to use 20 tons when wire-wound or ribbon-wound guns can be successfully built. The built-up steel gun of the same dimensions and weight, built of equal quality of steel shows a safe powder pressure of 20 tons per square inch. So the built-up gun is the superior as to strength over the steel cast homogeneous gun in the ratio of 20 to 13. If on the other hand the initial tensions can be retained to the extent of compressing the bore to its elastic limit, then the steel cast gun could withstand a safe pressure of not less than 25 tons per square

PLATE XXII
TRANS. AM. SOC. CIV. ENG'RS.
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COFFIN
ON STEEL.

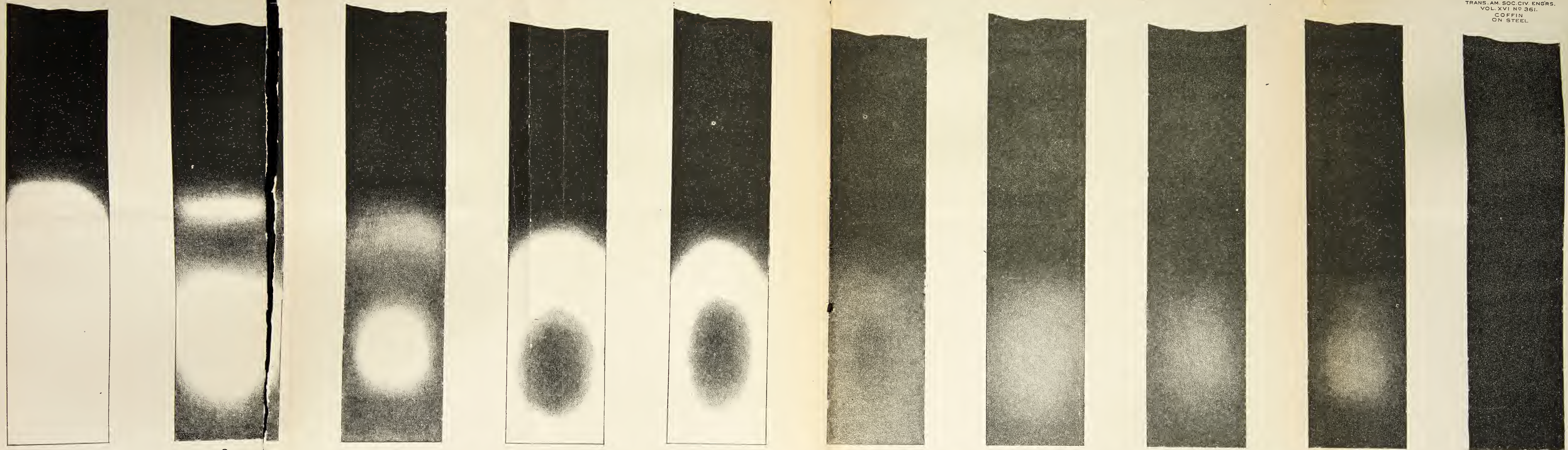

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PLATE XXIII
TRANS. AM SOC. CIV. ENGRS
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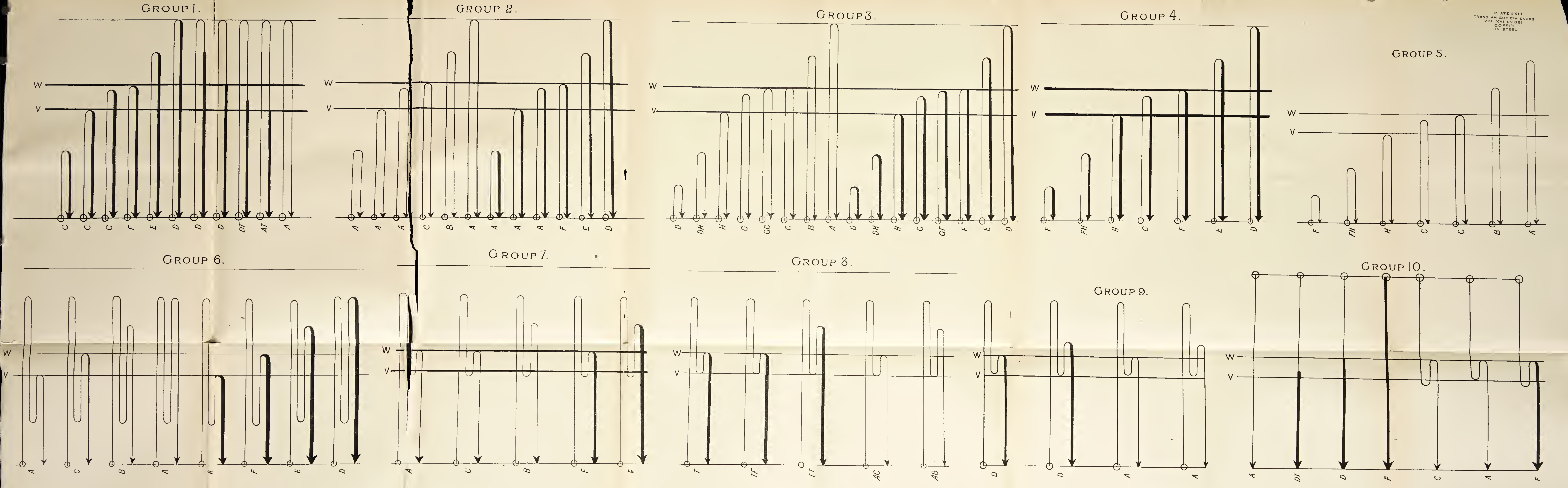

PLATE XXIV
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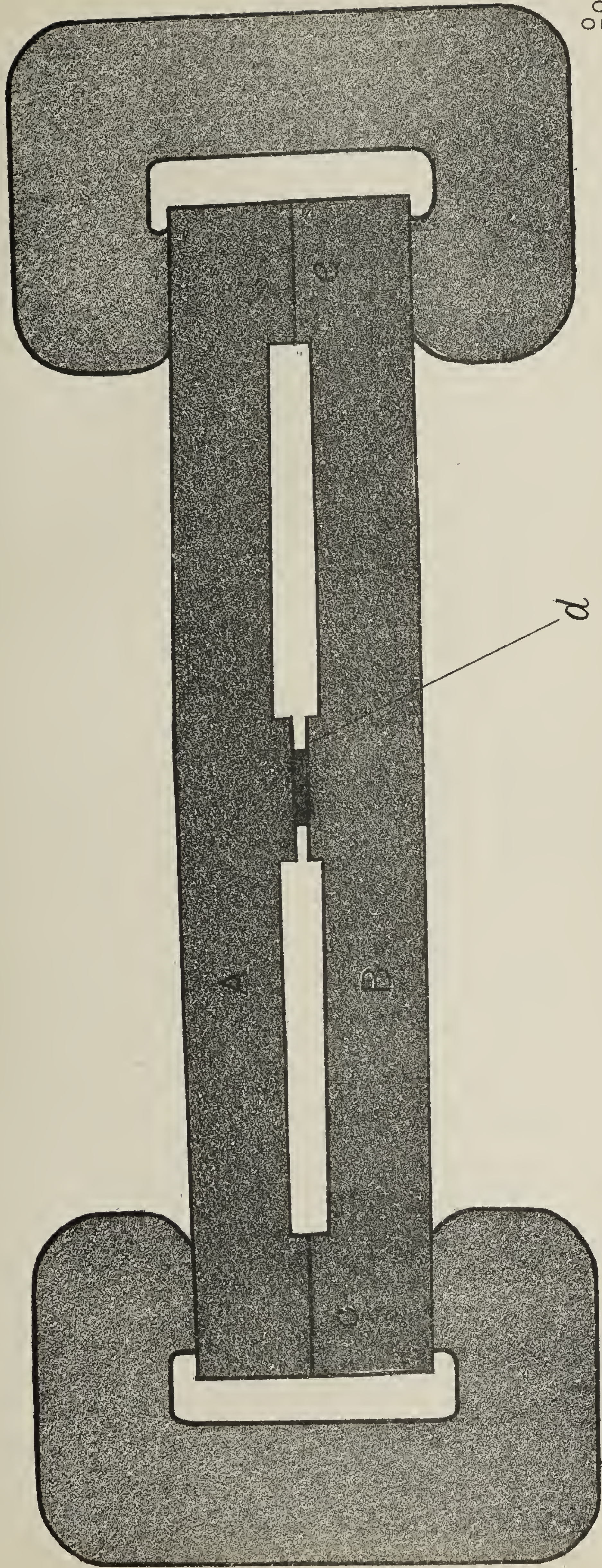

inch. It would seem then that the annealing should not be allowed to disturb the initial tensions produced by casting.

It would perhaps aid in convincing skeptical minds if the lecturer would indicate how the elastic strength of the steel cast gun would be computed. The strength of guns should be a matter of computation just as surely and accurately as in the case of any other mechanical structure.

Mr. Metcalf is a little severe on those officers who view with some concern the efforts which have been made from time to time to change the plan of gun construction which has thus far been followed, but I venture to think that if for years past he had been obliged to witness, in common with naval officers, the smile of derision and contempt with which our miserable show of force is greeted abroad whenever our ships enter the ports of civilized powers, he would perhaps appreciate the feelings of those who, having seen the work of gun construction so happily begun, cannot help but show uneasiness at any influence being brought to bear to delay that work. I do not believe that officers of the navy, as a body, are unwilling that the steel cast gun should be tried; they are not willing, however, that all work should cease until it is tried. It may or may not prove a success; but, if successful, does any one doubt its adoption?

I beg to thank the Society for the honor of being allowed to discuss this important subject. It is a matter for congratulation to naval officers that the support of this able and scientific body of men is thrown in favor of steel guns in some form, because it has been a fight to get guns built of steel.

Mr. William Metcalf.—In my own mind I thought always of casting the gun hollow by the Rodman method, but the difficulties of managing the core would be so great, that I believe Mr. S. T. Wellman's plan would be wiser; *i. e.*, to cast the block solid, bore out the center, and then proceed with the treatment. This is no modification of the principle, but it simplifies greatly the whole operation, and removes from my mind the only doubt I ever had about the feasibility of the plan.

What I have tried to say and what I adhere to, is this: I believe a large mass of steel can be treated by heat more surely, more cheaply, and better, than by any known mode of forging; and that by cooling on the Rodman principle a gun can be made to have with absolute certainty, the tension in exactly the direction and to the degree that is most desirable.

In answer to Mr. Ingersoll's question about computations, I beg to be excused. Our officers have proven themselves masters of the art of calculating, while I have to admit that during twenty-nine years of shop-work I have had little time to think of mathematics, and that is the reason why I have confined myself in this paper rigidly to physics.

Lieut. AUSTIN M. KNIGHT, U. S. N.—As I understand Mr. Metcalf, he has no dispute with ordnance officers as to the qualities which a gun should possess. The high tensile and elastic strength and the power to resist erosion which they demand, he demands with equal emphasis. But he believes that these qualities can be obtained with greater certainty and at a less cost in a cast than in a built-up gun. In this most officers of the army and navy differ from him; but their difference is, I believe, a matter of honest conviction, and not, as he implies, of narrowness and exclusiveness.

The feeling of the navy, at least towards such civilians as Mr. Metcalf and Mr. Dorsey, is, so far as I have been able to sound it, one of extreme cordiality. Mr. Metcalf questions this I suppose, because he has been for some years preaching the virtues of a cast gun without securing a test of his system. It would seem that the responsibility for this neglect lies rather with Congress than with the army and navy. So far as the navy is concerned, the money that has been appropriated for guns within the last five years has been appropriated for a certain definite purpose—the production of a specified number of guns of a specified caliber.

The officers upon whom has devolved the carrying out of this purpose have had before them then the problem: Given a sum of money, how shall that sum be invested to produce a certain number of guns of given weight and power, and of the greatest attainable degree of reliability ? They have had no lack of systems of construction from which to choose.

Mr. Norman Wiard and some others have told them that the gun for their purpose should be made on Mr. Wiard's principle, and of cast-iron. Ten such guns were tested in 1873. Of these four burst, and one was complely ruined at the first fire.

Mr. Haskell has advocated a multicharge gun, also of cast-iron. One such gun has been tested, it burst at the fifty-third round.

Mr. Metcalf, supported by Captain Michaelis—a high authority in all matters of ordnance and military engineering—has urged the adoption of a steel gun cast on the Rodman principle. No such gun has ever yet been made.

Most officers of the army and navy have favored a built-up gun. Some fifty thousand such guns are in service abroad. In fourteen years about ten of them have burst. Others have failed, but not in the way to endanger life.

Having before them the above systems and their records, or absence of records, the navy might have decided to test them all; supposing that the Acts of Congress involved could have been so construed as to admit of such an expenditure of the money appropriated. But now comes in the question: What constitutes a test ? Mr. Metcalf and his supporters ask that a few guns on their system be made and tried. But from their own point of view this could prove but little as to the merits of the system. Fifteen built-up guns have already been made and severely tested in this country without a single failure, and still Mr. Metcalf insists that these guns are unreliable. If the success of fifteen built-up guns proves nothing, would the success of three cast-steel guns prove more? If not, where is the test to stop? When will it be admitted that gun manufacture in the United States (under whatever system) has passed beyond the experimental stage, and when may the army and navy proceed with the armament of our ships and forts without being continually called upon to stop and wait until it can be learned whether some other system is not better than the one they are using?

Mr. Metcalf is right in holding that not the success of fifteen, nor the success of fifty guns, can prove the absolute reliability of a system. Nor should the failure of fifty guns in fifty thousand be held to prove its absolute unreliability. So long as gunpowder remains the mysterious agent that it is at present when confined and burnt in a gun; so long as the effects of variations of temperature and moisture accompanying its manufacture and its explosion are as little known as at present; so long as the nature of the vibrations which it communicates to the gun remains uninvestigated, and the tension which it exerts upon the walls of the gun remains but imperfectly determined; so long as we are ignorant of the changes effected in steel by variations of climate, by repeated vibrations, or by long periods of rest from all vibrations, so long will there be some danger of failure with any system of construction.

The tests then of the cast-steel and other systems proposed, could not, even from Mr. Metcalf's point of view, have stopped with any moderate number of guns. They must have been carried so far as to involve vast expense and long delay in the beginning of a work which Congress and the country, as well as the army and navy, felt to have been already too long delayed.

The alternative was to adopt that system which had the weight of evidence in its favor, and, having adopted it, to push forward in the work of national defense along the road thus entered upon without stopping at every stage to wonder if some other road would not have been better. This was the alternative accepted.

The built-up gun was adopted, but with many changes from European models. European nations had learned much in twenty years of experience with such guns, and we were able to reap the advantage of their experience. The latest designs of all governments and manufacturers were studied, and an effort made to take the good and reject the bad features of all.

As the enemies of the new navy never weary of asserting that the models of the English government were blindly followed, it may be worth while to remark in passing that the design adopted differed rather more widely from the English system than from any other in existence in 1881, the year when the present Naval Bureau of Ordnance design was made.

At that time, although an English Ordnance Committee had experimented with breech-loading guns, no such gun had been adopted for service. The English guns of that period were muzzle-loading, and were built up of steel and wrought-iron. It was nearly two years after the American naval design of a breech-loading all steel gun was definitely adopted that the English Government accepted these two most important features.

All the same, certain of the followers of Mr. Metcalf and Captain Michaelis, not those gentlemen themselves, will continue to the end of the chapter to assert that Woolwich designs have been blindly followed, and that we have therefore to anticipate only a repetition of English experience.

I have said that most of the officers of the army and navy differ from Mr. Metcalf's theories. Their grounds of difference I have not attempted to explain, because they have been already stated. But though, in common with other officers, I dissent from Mr. Metcalf's views, I am not opposed to a test of his system. I should be glad to see it tested. But the test should be thorough; it should be applied to many guns, and to guns of various calibers. It will require large sums of money, which should be specially appropriated for the work, and much time, which in the present state of our national defenses we cannot afford to lose.

I protest against the demand that at this time we should stop work on guns which we know to be as good as any others in the world, to experiment for a few years with guns which Mr. Metcalf hopes may be better than any others. When the country shall have been placed in a state of tolerable defense with guns that we know about, then it may be good policy to devote our money and our time to experiments upon promising theories. If it shall then appear that we have expended more money than was necessary, we shall be only in the position of a man who has insured his property, and whose house has not, after all, burned down.

Mr. Metcalf would reject, as only less barbarous than a hammer, the hydraulic forging press; yet he attaches great value to the forging effect of statical pressure in the molten metal itself. To me there seems little difference in the effect between this pressure and that of a hydraulic press, except that the latter would be felt equally throughout the mass of the metal, while the former is very great at the bottom but decreases rapidly toward the top. With regard to the question of cost, Mr. Metcalf gives no figures that can apply to his gun. True, he tells us that splendid castings may be had for six cents a pound, but he surely does not mean that a gun can be cast and finished on his plan for any such price as that. I would remark also that what is a "splendid" casting for ordinary commercial purposes might be a very poor casting for a modern high-power rifled gun. What is needed here is not a splendid casting, but a perfect one, if any.

The position of ordnance officers with regard to the question of expense, is simply that the cost of a system of gun construction must always be held as secondary to its efficiency. There is only one good gun, and that is the best gun. A system therefore which appeals for their support primarily because it is cheap, stands at a disadvantage as compared with one whose first claim is proved efficiency.

Just at this time, moreover, officers hold that in the question of our national defense, time should also be considered before cost; and in this I believe they have with them a large majority of the taxpayers of the country.

In conclusion I desire to say that it seems to me a matter for earnest congratulation on the part of the army and navy that the civil engineers of the country should have come to take the active interest in military affairs of which this discussion is an evidence. From the interest and co-operation of a profession so closely allied to their own, officers may expect to derive assistance and instruction of the greatest value. If the instruction comes in the form of criticism as honest and as friendly as that of Mr. Dorsey and Mr. Metcalf, it will not fail I think of frank and full consideration.

I beg to express my thanks to the Society for the privilege of taking part in this important discussion.

Mr. William Metcalf.—Mr. Knight says I charge officers with "narrowness and exclusiveness." I certainly had no such intention.

My whole object has been to call attention to the possibilities of the casting system for the purpose of producing, economically, very large guns, say 100 or 150-ton guns, and to urge that Congress should direct that the plan be tried. I fail to see how this would interfere necessarily with the work of the navy and army as at present conducted.

Their guns are good, undoubtedly; let them go on making them as fast as they can, and their operations need not be retarded one day by other experiments. They allow now thirty months for preparation for what they want. The Otis Iron and Steel Company, of Cleveland, can make an 80-ton casting with their existing plant, and if they were authorized to cast a gun, or guns, they could be made, finished and tested inside of one year.

Is it not true that if the Ordnance Departments would recommend such a trial, even faintly, Congress would grant the necessary means ?

Mr. William Sellers, M. Am. Soc. C. E.—In this discussion I propose to confine my remarks to the question of guns, for the reason that I find no other point in Mr. Metcalf's very able paper upon which to hinge a discussion; and even as to this, I am not prepared to say that the American gun of the future will not be a solid mass of cast steel, but only that I doubt it; because, although this may prove to be the cheapest way to make great guns in the future, it does not appear to be so now, and with the improvements in progress it is less likely to be so hereafter.

If we could assume that every great casting for a gun would prove perfect, one very important element of cost would be eliminated; but if the present high standard of quality is to be maintained, it may be safely affirmed that a notable quantity of such castings will be condemned, and then what shall we do with them ? We cannot permit such vast masses of material to cumber the ground, and it will cost far more than the material is worth to cut it into pieces small enough to remelt; and yet this is all that we can do, and the cost of doing it must be added to the cost of the good castings.

The casting of an ingot heavy enough to make the tube of a sixteen-inch gun, boring it and forging it to the proper length, would be accompanied with far less risk than to cast the same weight in one piece of that length, with a core throughout cooled on the Rodman principle; and I believe that the cost need not be greater, while the quality of the material would be far more uniform in the forged piece, not solely because it was forged, but because the quality of the short ingot would be more uniform than that of the long casting.

That the built-up gun is unmechanical, or that the quality of the material in such guus cannot be had in as perfect condition as in the solid casting, I do not believe. That the material is not in its best condition in built-up guns as now constructed is, I believe, true; but the principles upon which the several parts of such a gun should be manufactured have long since been published to the world by Rodman, and the application is simply a question of time. That this principle has not been applied to the built-up gun demonstrates that man is rather imitative than original in his habits. So long as it is a gun that is to be made of cast-iron, the Rodman principle is applied just as Rodman applied it; but when it is only a part of a gun, and that part not of cast-iron, it would seem that the same principle had no place, and yet that principle is far more easy of application in the built-up gun than in that to which its discoverer applied it.

That principle is, prevent radiation from the exterior and cool from the interior.

The comparatively short ingot from which the tube is made can be cooled from the interior far more easily than the long cast gun, and in every step thereafter, in the construction of the built-up gun, the same principle can be applied with greater facility than in its first application to the casting. The oil tempering, or rather the tempering, for oil is not necessarily the medium, may be done on the same principle, and when the several parts are finished they may be assembled in observance of the rule that the exterior must be the last portion to cool; and whenever this is done, the shot from the American gun will attain a higher velocity, while the endurance of the gun itself will be superior to any heretofore constructed or to any that will hereafter be made in one solid piece.

Mr. Charles A. Marshall, M. Am. Soc. C. E.—In discussing this paper I shall mainly devote the limited time at my disposal to criticising the argument and conclusions of the author as regards heavy guns.

The subject of ordnance is a broad field for discussion and equally as broad for investigation. The newspapers frequently have a fling at ordnance bureaus and the like, and many men with theories have succeeded in getting them aired in print, all tending to discredit the scientific and professional standing of the officers who are responsible for getting the best return in guns and material for the people's money devoted to that purpose. The particular proposed kind of gun advanced by Mr. Metcalf has probably more and more intelligent civilian backing than any other. It certainly deserves careful consideration; but are we to suppose that it has not been studied by ordnance experts? I am not informed on that point, but refer to it in this way because I have tried to look at the subject so as to form an impartial judgment based on facts, as should those on whom rests the responsibility of spending money for none but good guns, and of doing effective work with them when the emergency for their use shall arise. Responsible persons cannot afford to make blunders; it is not only fair but logical to give them the benefit of the doubt till they are proved wrong; and in this instance I believe and hope to show that the advantages are greatly in favor of the hooped as against the cast guns.

The conditions to be met by a gun to-day are different from those required in the day of Rodman cast-iron guns, referred to by the author as being "to batter down an earth-work or to sink a ship." The earthwork of to-day has its guns of position protected by heavy armor of steel or of chilled iron; the ship has heavier and more impenetrable armor. These facts imperatively demand that the piercing power of our weapons be increased; that is to say, the foot tons of work that the gun has to do in order to be effective must be given in larger doses to the separate projectiles. To reach this result by larger bores, and heavier projectiles thrown with diminished powder pressure as compared with the best guns made to-day, I presume all will admit would be a step backward.

Rifled guns with elongated projectiles capable of using high powder pressures are acording to all our lights the guns we want. Since it was pretty definitely ascertained by Whitworth that there is a certain best ratio of length of projectile to diameter, the conclusion is irresistible that improvements in guns to-day should be in the direction of adapting them to higher powder pressures. Increasing the length of bore with the use of slow burning powder is, of course, another way to gain velocity and increase the energy of a shot, but this has been done; the disadvantage of unwieldiness presently puts a stop to improvement in this direction. Now I submit that the author has not even given a glimmer of hope that the cast-steel gun will bear higher powder pressures than the built-up gun. His proposition is mainly to give as good a gun at less cost. The question is whether this is possible.

It is not claimed that cast metal can be so treated as to have a higher limit of elasticity than steel that has been forged, only that it can be brought to equality. For the argument I shall take this for granted (though I do not believe it, never having seen an example to prove it, and having seen plenty proving the contrary; and I am surprised that one so conversant with his subject as Mr. Metcalf, should ignore in his argument the beneficial effects of work on steel), and I shall take for granted at present also that the reliability of the two metals will be equal, but wish distinctly to note the reservation that the conditions of casting and of treatment by heat must be equal or equally favorable.

As to conditions of casting, the built-up gun is made of moderate sized ingots which cool quickly; thus the tendency to separate out the carbon and other metalloids is minimized. The only limit to the rapidity of cooling is that it shall not cause cracks; this is entirely controlable. Piping of ingots as a source of injury to the steel is eliminated by boring out the central portion of all ingots whether piped or not. This also improves our material if segregation of metalloids has taken place, since the center is the most injuriously affected by that defect. Against these important advantages as affecting integrity and uniformity of material, we have the sole advantage in favor of hollow castings that the metal will be prevented from cracking by being cooled from the center out. It is assumed that the top part of the casting will be thrown away in each case. Disadvantages of casting the gun as a whole on the Rodman system are:

First.—Enormously greater weight and size of casting; largely increasing risk of making. Cast-iron is easier to handle than steel, yet the South Boston Foundry lost three castings before they succeeded with one.

Second.—Very much slower cooling, which is the condition favoring ununiformity by segregation. The Rodman process doubtless seems to some minds a quick process of cooling, which is so, considering the size of the castings. Yet General Rodman states (Senate Rep. Com., 266, XL Congress, 3d Session, p. 80) that the time of cooling a 15-inch hollow-cast gun was about six days, and that the outside temperature did not fall below 500 or 600 degrees temperature for about two days.

Third.—As another consequence of the slow cooling, large, weak crystals.

Fourth.—As another consequence of the slow cooling, a very low elastic limit to the outer metal certainly, and for the inner metal a higher value, but for the same kind of steel not anything to approach the treated gun tube quoted by Mr. Metcalf from Holley. The elastic limit to the outer metal would be not over 29 000 pounds per square inch for metal having same carbon contents, as gun-hoop steel, which, it may be borne in mind, is the hardest steel used in the guns to day—such metal as Cambria Iron-works are producing in hoops with an elastic limit of 55 000 pounds minimum by specification.

The following three tests from the same ingot exhibit this:

Specimens .564 inch.; diameter, 6 inches gauged length.

Test—How taken.	Elastic limit.	Ultimate strength.	Elongation.	Reduction of area.
	Lbs.per sq. in.	Lbs.per sq. in.	Per cent.	Per cent.
From casting, natural	29 000	82 400	4.9	6.6
From 3 inch square, cut out of ingot and annealed; cooled in open air	46 000	100 560	12.1	15.0
From forged and treated gun-hoop	58 000	107 240	13.1	23.1

The following examples of elastic limits from various castings, all having in the neighborhood of 0.50 per cent. carbon, go to show what we may expect in large castings annealed. The above example of piece 3 inches square, it is seen received a higher elastic limit than can be obtained by annealing the larger masses.

	Elastic Limit.
48-inch roll casting, annealed, two tests	34 000 36 000
Blooming-mill pinion, annealed, two tests	34 000 33 000
Blooming-mill pinion, annealed, four tests, first two at top, second two at bottom as it lay in pit	36 000 34 000 38 000 39 000

I will now refer to the army 8-inch hooped steel rifle recently built and successfully tested. The data for this gun are given in Report of Chief of Ordnance U. S. A., 1885.

Refer to section at powder chamber. Dimensions are inches; pressures, pounds per square inch.

Internal diameter, 9.5.

Thickness of tube, 2.25; of jacket, 4.0; of first hoop, 2.15; of outer (second) hoop, 2.6.

External diameter, 31.5.

Limiting value of metal in tension at interior of outer hoop, where maximum, 50 000.

Maximum tension under fire at interior of outer hoop not given, but if the internal pressure were 55.500, then the maximum tension would be equal to elastic limit = 50 000.

Maximum tension, system at rest (.001163 × E) = 34 900.

Initial compression of internal fiber of tube (.0016 × E) = 48,000, approximately equal to elastic limit.

Powder pressure, since used on trial = 36 000.

Next I shall inquire just what must be the condition of initial strain in a homogeneous cylinder to enable the condition of the above gun to be fulfilled, viz.: no part to be stressed beyond the elastic limit by an interior pressure of 55 000 pounds per square inch. At the outset we are confronted with an utterly unknown factor—the law of variation or distribution of the stresses caused by any given mode or rate of cooling. It is, however, near enough for present purposes to assume that the stress varies regularly from a certain compression inside to an equal tension outside, since, as we shall see, what might be gained in one place would have to be practically lost in another. It would be of interest and value if some advocate of cast-steel guns could by scientific experiments, which need not cost more than a few hundred dollars, demonstrate the law upon comparatively short cylinders. Of course the difficulty of so treating longer cylinders as to arrive at definite results would remain, but such experimental data would help to determine the question of ultimate possibility of success which is yet unproven.

To proceed with this inquiry. The elastic limit of the interior metal must be as great in the cast gun as in the hooped one, since here will be found the severest stresses, both radial and tangential, 50 000 is this value.

Assuming that the gun is not treated after casting, we must take 29 000 as elastic limit of external metal as already given. By subsequent treatment, as to be later referred to, it seems just possible that an external elastic limit of 40 000 might be reached, hence I have included this case in the table and diagrams.*

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* The formulas are the following (Longridge, "Treatise on Application of Wire to the Construction of Ordnance," pp, 35, 36), external pressure being taken = O.

$$t_i = f_0 \frac{m^2 + 1}{m^2 - 1} \quad . \quad t_e = f_0 \frac{2}{m^2 - 1}$$

$$t_y = \frac{t_i\,(R^2 - y^2)\,p^2 + t_e\,(y^2 - p^2)\,R^2}{y^2\,(R^2 - p^2}$$

in which

f_0 = internal pressure

t_i = " tension

t_e = external "

t_y = tension at y

R = external radius

p = internal "

$m = \frac{R}{p}$.

Exactness was not aimed at in the calculations.

TABLE No. 1.

$m = \dfrac{\text{External diameter}}{\text{Internal diameter}}$. All pressures in pounds per square inch.

Assumed elastic limits.	m	Permissible initial tension.	Internal tension under 55 000 pressure.	External tension under 55 000 pressure.
	3	15 250	53 500	29 000
Internal = 50 000	$3\frac{1}{2}$	15 880 to 18 120	47 760 to 50 000	26 760 to 29 000
External = 29 000	4	12 330 to 21 670	40 660 to 50 000	19 660 to 29 000
	5	9 580 to 24 420	35 220 to 50 000	14 160 to 29 000
	3	18 750 to 26 250	42 500 to 50 000	32 500 to 40 000
Internal = 50 000	$3\frac{1}{2}$	15 880 to 29 120	36 760 to 50 000	26 760 to 40 000
External = 40 000	4	12 330 to 32 670	29 660 to 50 000	19 660 to 40 000
	5	9 580 to 35 420	24 160 to 50 000	14 160 to 40 000

The smaller limit of permissible tension in table is obtained by subtracting 50 000 from the internal tension in an initially unstrained cylinder that would be caused by an internal pressure of 55 000 pounds per square inch. The greater limit is equal to the external elastic limit less the external tension due to same pressure. In the diagrams, Plates XXV and XXVI, tension is measured up from $O\ Y$, compression down; P is the internal radius and $O\ Y$ the external.

The important thing to observe is the narrow range of permissible initial tension. In the case of diameter equal to the army hooped gun ($m = 3\frac{1}{3}$) the range is only 2 240 pounds per square inch, with external elastic limit = 29 000, and 13,240 if that limit be 40 000. Plainly the gun cast without subsequent treatment is condemned to increase in diameter and weight. Increase diameter to $m = 5$, giving outside diameter = 47.5 inches, or as great as a 12-inch rifle if built up, and the foundryman still must hit within 14 840 ÷ 2 = 7 420 pounds per square inch of a certain definite stress. But it is not worth while to pursue the subject of casting without subsequent treatment any further. My own opinion is, considering the difficulty of making ordinary castings, even of iron, but especially of steel, always sound, that the cooling from center and outside will have done its full duty if it produces castings without cracks; I should even then expect frequent cases of hidden cavities. That is to say, if we can keep within a range of double the elastic limit we shall do very well. Note that it is external surface we have to deal with, since the internal surface is hard and unyielding practically at the time the initial strains are induced. Expansion of iron or steel under heat is about .0012 of length for 180 degrees Fahr. Figures would indicate that working within a difference of temperatures of 290 degrees Fahr. saves cracking the casting whose external limit of elasticity is 29 000 to per square inch.

Now to consider the possibility of treating a hollow cast gun so as to equal the built gun in efficiency. The range of initial tension when $m = 3\frac{1}{3}$ has already been referred to; it is 13 240 pounds per square inch = 66 degrees Fahr. of temperature. Aiming at the middle of range, I would inquire: Has anybody the means of registering temperatures at an orange or high red heat so closely as 33 degrees Fahr. with any certainty? If so, and if means could be provided for testing the effect each time, since different melts of steel vary in the temperature at which they are affected similarly, there would be possibilities ahead in this line.

Recurring to the tests of castings given on page 345, 39 000 pounds per square inch is the highest elastic limit. Specimen was taken from the neck of pinion, hence repesents a smaller casting than the guns we are considering. I would call attention to the vital fact, not mentioned in Mr. Metcalf's paper, that size very greatly affects the results of treatment by heat. One cause of this is however mentioned in the paper, viz., that slow cooling favors the formation of large crystals, which are a source of weakness and more especially of brittleness and untrustworthiness. There is another cause, which is that slow cooling permits a change in the condition of carbon; this takes effect principally at a high temperature or, according to Brinell (*Eisen und Stahl*, November, 1885; also "Notes on the Construction of Ordnance, No. 37," Ordnance Department, U. S. A.), almost altogether between certain two temperatures which for the same steel are quite well defined. The change is from "hardening" or "combined" carbon to "non-hardening" or "graphitic," as it has been called, though it is pretty well ascertained that the dark looking matter which led to the name is not graphite, but a compound of carbon and iron. There is also a third cause, in the absence of any considerable compressive effect due to unequal contraction at the proper temperature for forging. How much this amounts to is problematical; Mr. Metcalf seems to rely upon such an effect to forge his castings, and doubtless it would benefit the steel if the casting could be cooled rapidly enough. But to cool a casting from the core out is not to cool it rapidly. To cool forgings rapidly they are quenched in oil. It is only necessary to glance at the relative proportion of cooling surface to mass in order to appreciate this. A gun-hoop 4 inches thick immersed in oil has one-half a square inch cooling surface to the cubic inch of metal, about. A cylinder 32 inches diameter, with 8½-inch core, has .035 square inch to the cubic inch inside, of which the effect is available to its full extent, and .133 square inch outside, of which the effect is to be retarded in the process as proposed.

As to the effect of forging or "work" upon steel, I will state a law which may throw light on the subject under discussion. The effect of work upon the physical characteristics of mild steel is greater than of high carbon steel, and conversely the effect of treatment by heat upon the characteristics of high carbon steel is greater than of mild steel. Upon the medium steel of which guns are and should be made, whether by hooping or as the author proposes, both work and heat have potent effect.

Applications of the foregoing considerations to the hollow cast and subsequently treated gun, leads me to think that the conditions of Table No. 1, elastic limit 50 000 pounds per square inch inside and 40 000 pounds per square inch outside, are not possible without so great an increase of carbon as to render the steel entirely too "mercurial" for safe treatment or safe use. The moment it becomes necessary to lower the elastic limit inside, our margin of safety begins to disappear, or else powder pressure must be decreased.

It may be possible to attain elastic limit equal to that named in the new navy specifications for tubes at the expense of higher carbon, but the difficulties in treatment still remain to be overcome; indeed, the range of permissible initial tension becomes less.

It may be profitable to enumerate in the most general way some of the advantages which the system of building up guns has over that of casting them whole. They are:

With metal of equal strength, greater ductility. With metal of equal ductility, greater strength. Metal of more reliability, because (1) the blow-holes, if any, have been closed up or got rid of; (2) thorough tests can be made.

Character of metal is known throughout. Character and amount of strains are known throughout and are controllable, so that two guns of same pattern are stressed the same.

To test this last statement, take the smallest diameter where definite shrinkage is required in the army 8-inch gun. It is 14 inches. Measurements of diameter to .0001 inch are quite feasible. Suppose we could measure no closer than .001, or with a probable error of .0005, then the error of stress due to this error of measurement would be $= E \div 28\,000 =$ say 1 100 pounds per square inch. I doubt if it is feasible to so thoroughly remove internal strains from a cast gun as to leave no part with an initial strain of that much, to say nothing of the possibility of putting on definite tensions within any such limit of error.

I do not wish to disparage the genius of Rodman, but the statement of the author in speaking of Wade and Rodman, that "none of their guns ever failed," will not bear investigation. In the Report of Senate Committee on Ordnance, 1869, is a record of the bursting of twenty-four Rodman guns of various sizes, nearly all at low numbers of fires, many at very low number; also of six such guns which burst or cracked spontaneously within the lathe or in the pit; also of many having been disabled short of actual bursting; also of many that were condemned for defects discovered after reception and mounting. The Rodman gun was an intelligent attempt to make a high-power gun out of weak material; we have much superior guns to-day, though very few of them. If the hollow cast (or solid cast) steel gun has a field, it is as a low power gun for commerce destroying or hasty inland defenses. In times of peace the policy of this nation being defensive, we do not want to invest money in commerce destroyers which would be useless in defending our ports, nor do we want to arm an expensive, swift cruiser, even though unarmored, with inferior guns, albeit the crusier would have to run away from an armored vessel. For coast defenses no guns but the best, because if ever used it will be against the best that the enemy has or can get.

Mr. William Metcalf.—In speaking of the effect of slow cooling producing large crystals and weak structure, Mr. Marshall ignores the fact that slow reheating to the right temperature will insure the formation of uniformly minute crystals and great strength, and that, having secured this condition, cooling from the interior, whether rapid or not,

PLATE XXV
TRANS. AM. SOC. CIV. ENGRS.
VOL. XVI No. 361.
MARSHALL
ON STEEL

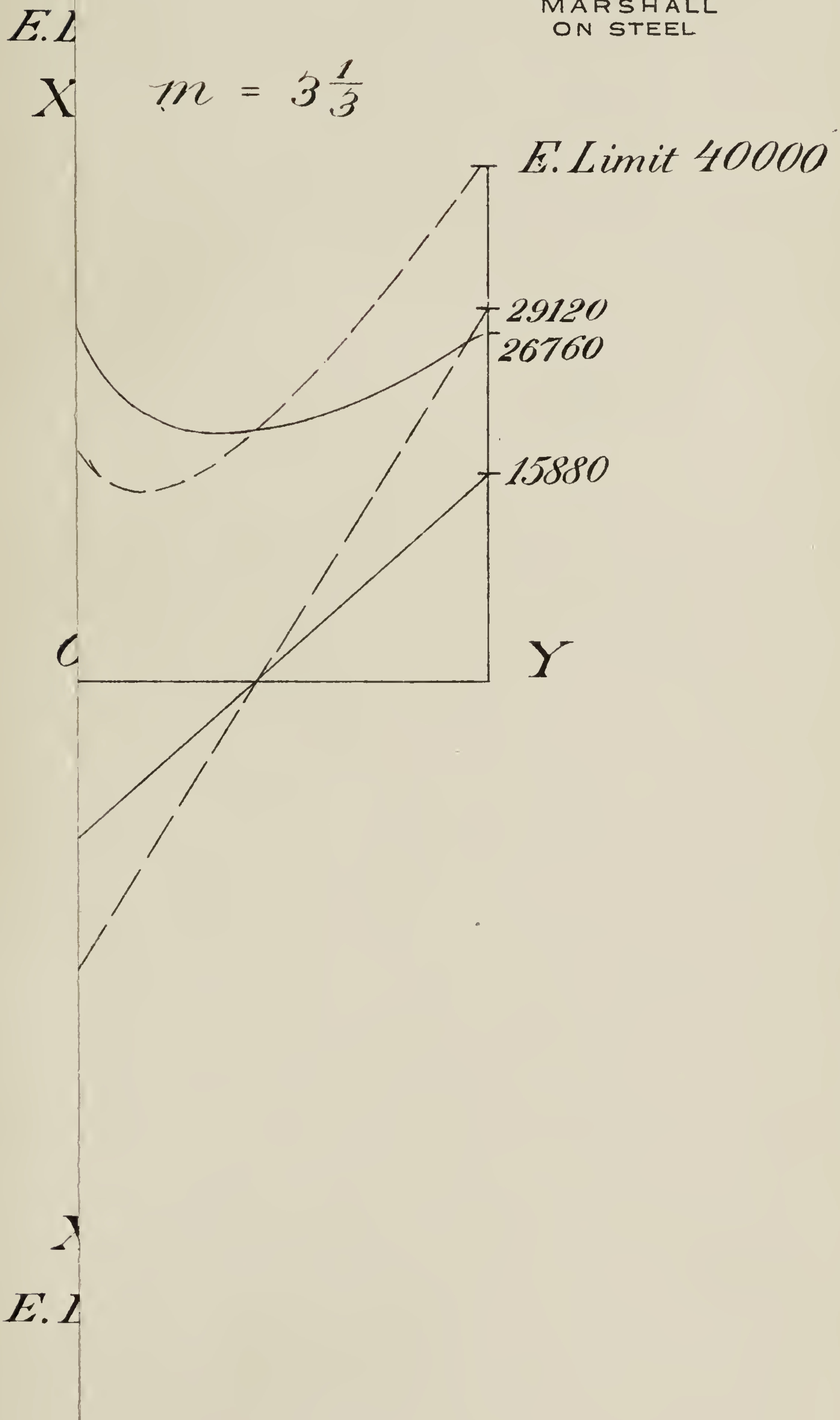

PLATE XXV
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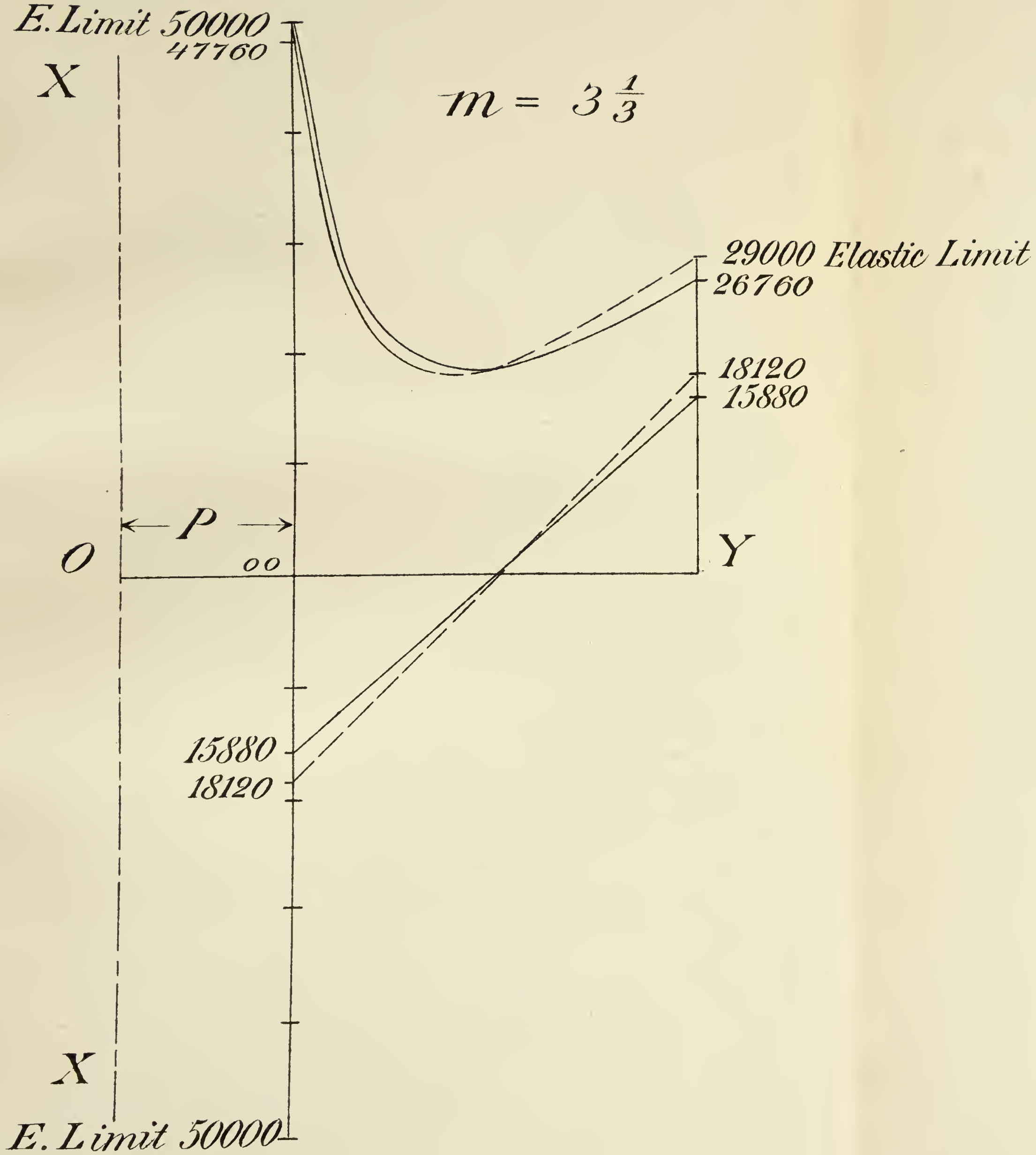


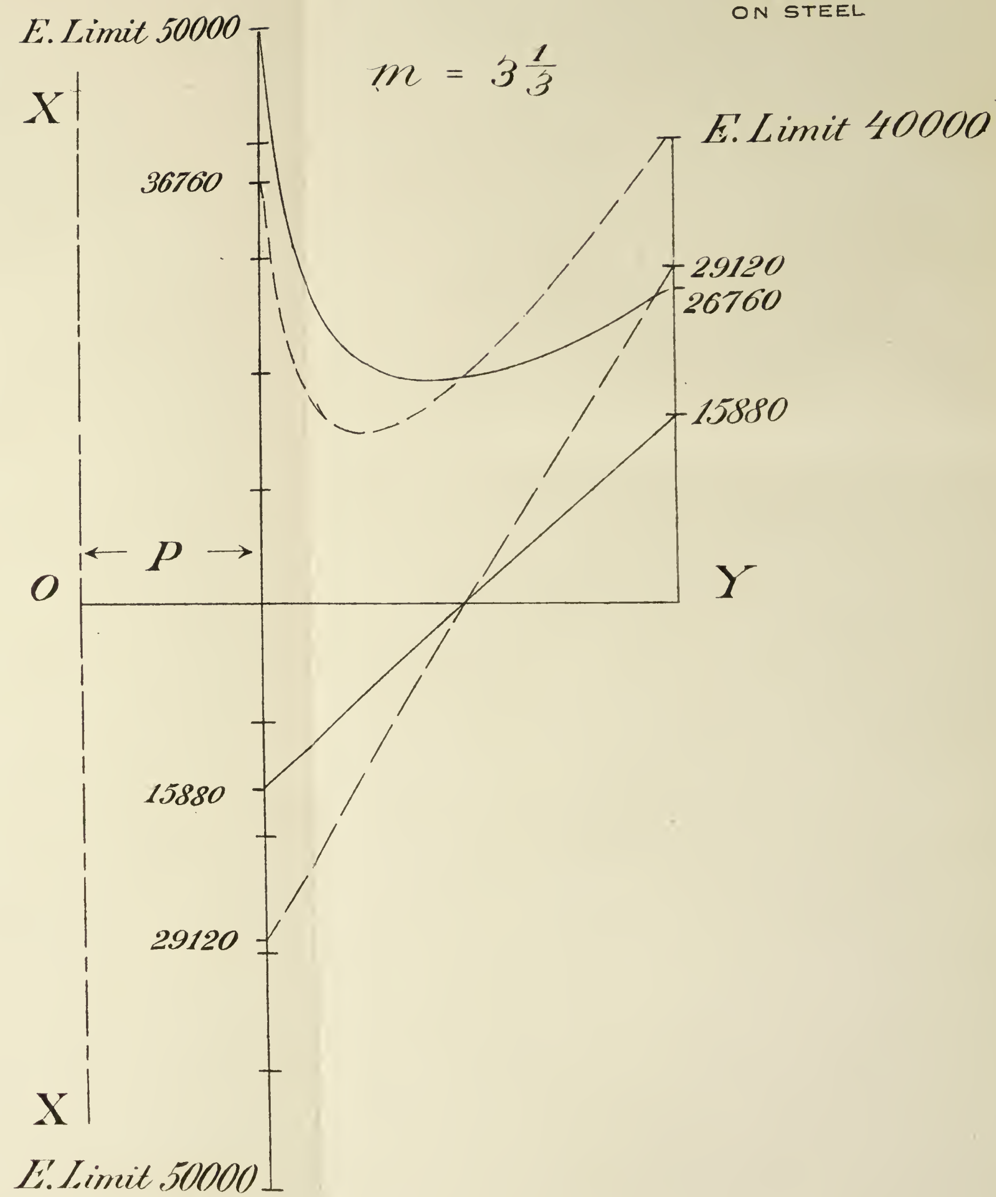
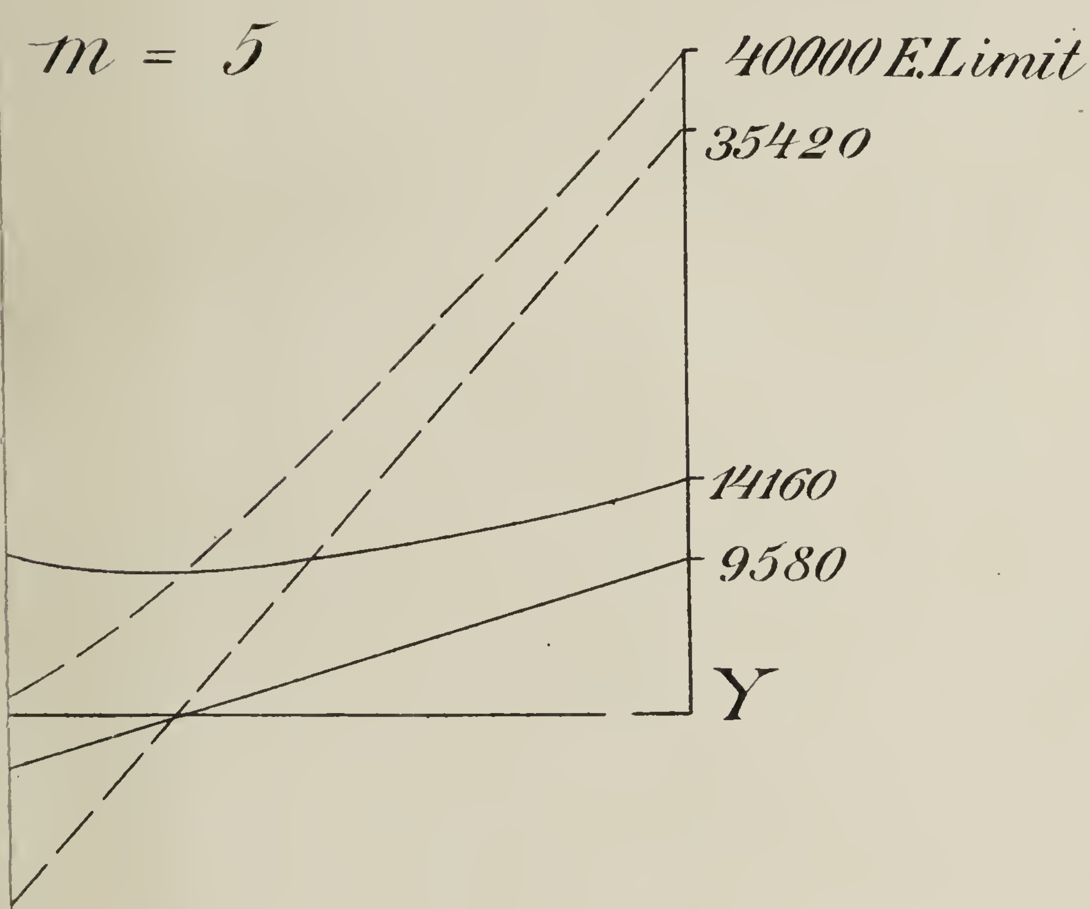
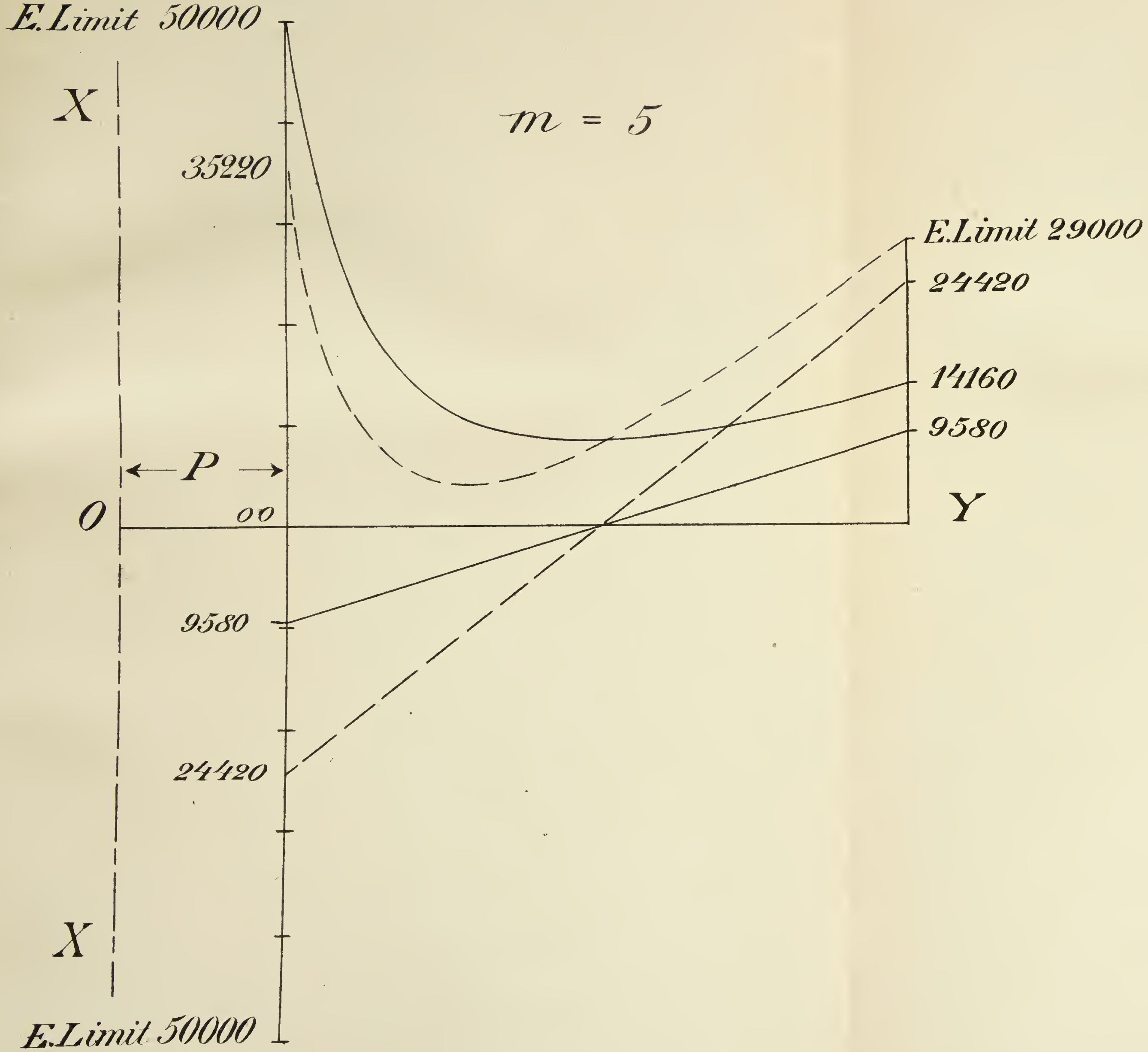


PLATE XXVI
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XVI No. 361.
MARSHALL
ON STEEL





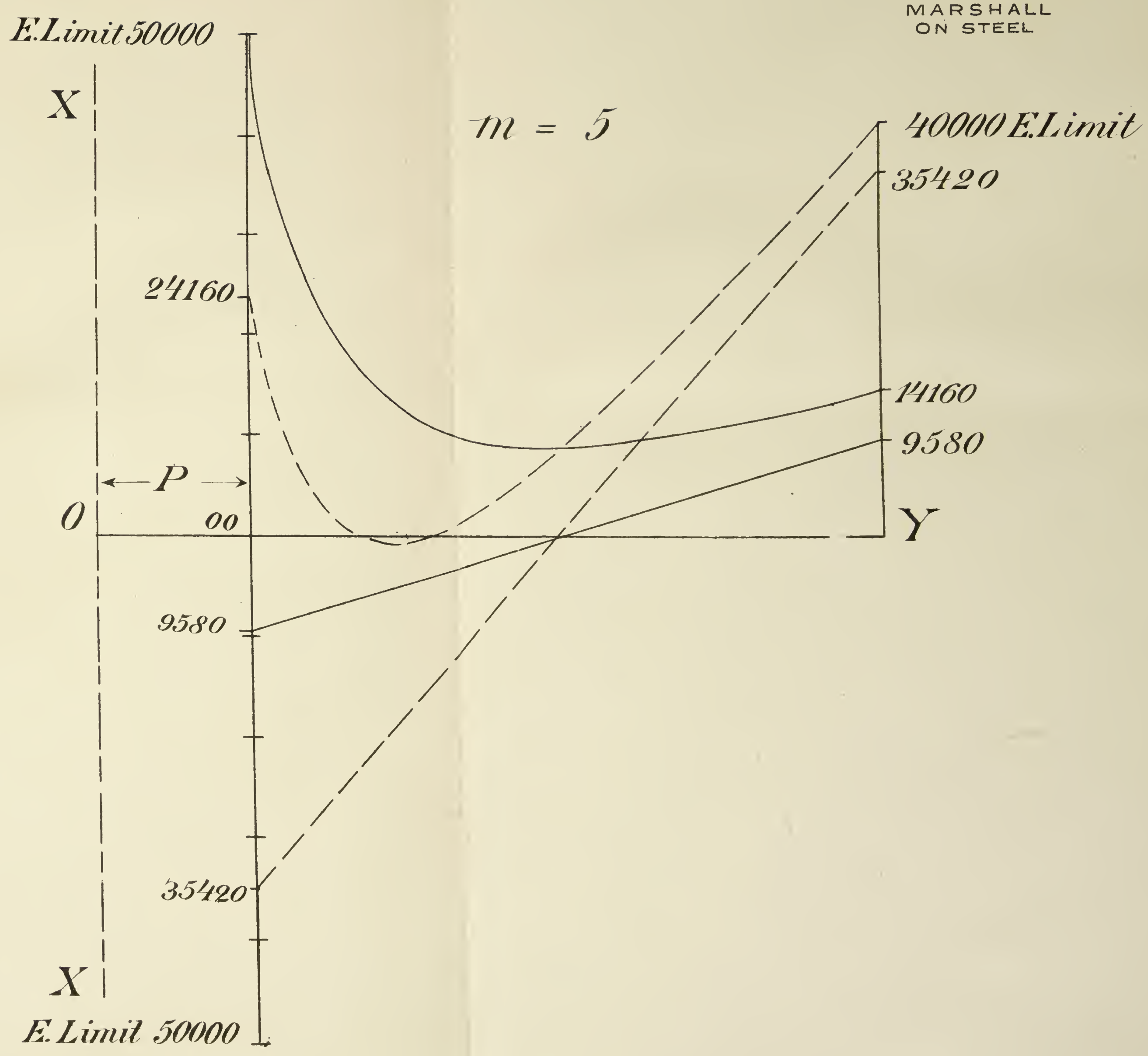


PLATE XXVI
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XVI Nº 361.
MARSHALL
ON STEEL

will secure initial tensions in the direction desired. Heat is the true motive power with which to handle large masses of steel, and not steam.

Mr. Marshall says, "size very greatly affects the results of treatment by heat." This is an admitted fact so far as original heating is concerned; that is to say in cooling from fusion, but that has nothing to do with subsequent annealing, and I never suggested the use of castings without very careful treatment after casting.

Mr. Marshall speaks of "hardening" or "combined" carbon, and of a compound of iron and carbon. I decline to discuss this matter, because I have no faith in any opinion yet put forth as to the condition of carbon in steel. The whole question is subject to the Scotch verdict, "not proven."

As to the relative effect of work upon mild steel and high steel, I will accept Mr. Marshall's dictum in the absence of data, although it is contrary to my judgment, and to what might be called shop experience, which has not been verified by exact experiments.

I am still at a loss to understand what exact measurement has to do with definite shrinkage in the face of Mr. Marshall's admission that he cannot measure the temperature of orange color, and he might have said of any heat above the temperature of boiling water.

If a steel rod of $\frac{1}{2}$-inch diameter be heated to a heat that is uncomfortable to the hand, say 120 to 150 degrees, it will enlarge its diameter .004 inch. Apply this to a gun ring $2\frac{1}{2}$ inches thick and you would have a change .02 inch in thickness. A summer sun will do this and more for any gun ring; then what becomes of definite shrinkage and elastic limit?

The failures of Rodman guns mentioned were reported two years after I abandoned the gun business, and I had never heard of them. Of course I do not dispute the record, but I regret that Mr. Marshall did not give it to us in detail. The guns that "burst in the pit" have nothing to do with the question, unless the makers of built-up guns will report every defective ingot as a failure of a gun.

Mr. Henry M. Howe.—We must anticipate that gun steel, owing largely to its high melting point and the consequent enormous contraction which it undergoes after solidification has set in, as well as to the escape of gases during its plastic state and the consequent formation of blow-holes, will prove far more difficult to treat on the Rodman principle than cast-iron; and the difficulties which present themselves may indeed prove insuperable. Still, now that this principle has been successfully applied to cast-iron, and that the prevention of blow-holes is so well understood, the remaining difficulties hardly appear as formidable as those which Rodman conquered must have seemed before he attacked them. Mr. Metcalf's belief that they can be overcome is extremely encouraging; his opinion carries great weight, not alone because of his mental equipment and general knowledge of the subject, but also because of his special experience in mastering the similar difficulties which arose in applying the principle to cast-iron.

The advantages which would flow from the successful application of this principle to gun-steel would not be economy alone (though this is an important consideration, in spite of the silly sneers at cheap material which we hear; as if, forsooth, where quality is identical the dearer substance should be sought because it is dearer), but more especially because it would enable us to arm our frontier in a very much shorter time than would be possible had we to rely solely on built-up guns. A living dog is better than an unborn lion; how much more is a living lion! Even were a Rodman steel gun slightly inferior to a built-up gun, it would sink more ships than a not-yet-built-up gun.

In view of the advantages of economy and of rapidity of production, I believe that the chances of producing by the Rodman plan a steel gun equal, if not, indeed, superior to the best built-up gun, are sufficiently great to warrant the outlay required to settle the question by actual large scale tests.

The attempt to stop discussion by arraying great names like those of Whitworth, Krupp, Schneider and Fritz in favor of the built-up gun is unfortunate and futile. To-day we know how much expert knowledge is comparative, not absolute, and how much our most strongly entrenched opinions are liable to be dislodged. No; it is not the weight of names, but of evidence and argument, that settles the living questions of to-day.

Some experiments which I have just completed tend to throw some doubt on our data as to the strength of built-up guns. I understand that the strength of the members of a gun is ordinarily ascertained by examining test pieces cut from them, and by deducing the strength of the member from that of its test pieces. Now my experiments indicate that in this case the rule of three does not hold, and that the whole is by no means equal to the sum of its parts. In order to verify certain theoretical speculations concerning the effects of the stress due to suddenly cooling, I hardened several ¾-inch round bars of machinery steel of 0.39 per cent. carbon by quenching them suddenly in cold water from a white heat, under identical conditions. I found that the tensile strength of small test pieces cut from the interior and exterior of these hardened bars was exceedingly high, rising in one case to 248 000 pounds per square inch, while that of the hardened bars as a whole was less than half of this, or 118 000 pounds. This completely verified my deductions, which I shall shortly make public.

From this it is clear that test pieces cut from different portions of a gun-hoop may give misleading information as to the strength of the hoop as a whole. It may be demonstrated but it should not be assumed that annealing, as usually practiced, removes the conditions which create this difference between the strength of the mass and that of its test pieces.

Alfred E. Hunt, M. Am. Soc. C. E.—The different grains in the structure due to the different temperatures and treatment of even the softest or 0.10 per cent. carbon steel that Mr. Metcalf refers to, are also noticeably shown in the tensile and bending results, as indicated by the following record of tests made by the writer.

A plate of soft open-hearth fire-box steel of $\frac{5}{16}$-inch thickness, and having carbon 0.12, manganese 0.36, silicon 0.008, and sulphur 0.017 per cent., was cut into strips $1\frac{3}{4}$ inches wide and long enough to get not only a test strength, but afterwards a bending test from an end allowed to project above the testing machine grips and not subject to strain, and prepared on the planer with straight edges that had all the effects of the shears cut off from them. These strips were subjected to both tensile and bending tests, enough of them in a normal state as the plate came from the rolls to establish the fact of the homogenity of the metal. Others of the strips were tested after being heated to various temperatures as follows: Some to a dark orange heat, some to a medium orange, bright orange, lemon, light lemon, low white heat, and scintillating white heat, at which the slag ran off from the metal. Some of the strips, after being thoroughly and uniformly heated to the temperature described above, were carefully annealed by allowing to cool down very slowly and uniformly in hot sand, and others allowed to cool off at the temperature of the air of the shop on a bed of blacksmith coals; and others still were, at the heats mentioned, plunged into water at 60 degrees Fahr., others into brine at 60 degrees Fahr., and others into oil at 60 degrees Fahr. A part of the quenched specimens in water were again heated to the various temperatures described above and then annealed by allowing them to cool down very slowly in a bed of sand which was at first nearly as hot as the strips, the object of these last tests being to indicate to what temperature the internal strains and hardened condition known to be in the steel due to the quenching would be best taken out in the subsequent annealing.

The results of the tensile and bending tests of these various specimens, after being treated as above described, are given in the following Table A.

My experience, not only from these tests given, but from many others obtained in the general way of my business, is that 0.10 per cent. carbon steel refines or anneals so that its grains are the most minute and most uniform in size at a temperature of about 1 000 degrees Fahr., or at a temperature just below that at which the scale raises on the metal; it appears as a medium orange color considerably below the lemon color. The length of time at which this heat has to be maintained varies according to the size of the piece to be annealed; it should surely be continued until the metal is thoroughly and uniformly heated through and through. Mr. Metcalf's rule that the heat best to harden at is the one at which it is the most thoroughly annealed, seems to be a good one; the tests given in the above table indicate this.

The crystalline character of iron and steel certainly undergoes a change due to temperature. Experiments to prove this have lately been made by Mr. Joseph Ramsey, Jr., Chief Engineer of the Cincinnati, Hamilton and Dayton Railroad, who will bring out the matter in an article to be read before the Engineers' Society of Western Pennsylvania.

Mr. Metcalf says that the engineer in preparing his specification for structural steel does well not to meddle with the chemistry of the metal at present. From his qualification of "at present," and his acknowledgment that the purer the steel is to the two elements, iron and carbon, the better it is, the inference can be fairly drawn that his reason for this is that our knowledge is too limited as to the effects of the various chemical constituents, and our power of producing the same physical results, even with the same chemical composition, is too limited to specify, in addition to the physical requirements, a prescribed chemical constitution as well. In this I would agree, except as to phosphorus, which is a well known and acknowledged element of weakness to structural as well as other steel. While I would cheerfully concede the fact that well authenticated sample specimens of steel have been produced with phosphorus of over 0.20 per cent. which have shown, with very low carbon, all the ductility requirements of the best structural material, and that upon the results have been based many bright and golden hopes, especially by the advocates of the Clapp-Griffith steel process, yet actual experience has clearly shown that these have been simply isolated cases, and they have only caused the expenditure of much money in experimental work with results so far of only signal failure to produce a uniformly ductile steel high in phosphorus. The fact that such results have been obtained, indicates so far only possibilities in the future metallurgy of steel. For my own part I am clearly of the opinion that Mr. Metcalf's statement should be reversed, and that at present it is extremely important to stipulate the maximum contents of phosphorus. This is needful, because in the ordinary structural steel as made to-day, the presence of this element gives rise not only to cold shortness or brittleness, but also to irregularity in the metal. This is needful also because the cost of production may be materially lessened by even so small an increase in the steel as one or two one-hundredths of one per cent. of phosphorus above, say, 0.06 or 0.08 of one per cent., this being a well-approved maximum limit for many grades of structural steel. Just in the same way that iron ore is valued and sold by "the unit," or by the percentage of contained metallic iron, so the stock from which steel is made, is valued and sold according to its freedom from "units" of hundredths of a percentage of contained phosphorus. The amount of sulphur, and the impurities tending to produce red shortness, can very properly be left to the manufacturer, as his own interest in the matter of producing sound steel free from surface defects will regulate the content of these elements. The percentage of carbon and manganese ought not, except within reasonably wide limits, to be stipulated as varying stock, with even the same chemical composition, requires varying proportions of carbon, the chief hardener, and of manganese, the antidote and purifier, to produce the same physical results.

I agree with Mr. Metcalf most cordially in his advocacy of the Rodman principle for the manufacture of steel guns, and would give below some results of tests made from specimens cut both lengthwise and transversely from forgings made for two of the first 6-inch built-up breech loading rifles for the U. S. Navy Department at two different steel-works where the writer had charge of the work. The results given in Table B show what Mr. Metcalf has so ably pointed out, that the temperature at which steel was last subjected, moderated by its subsequent treatment, is always recorded in the structure of the steel, and that the annealing is one of the most important of all metallurgical operations to which the steel is subjected from the stand-point of the engineer.

It is also worthy of note that for shafting and for many of the purposes where large forgings are now required, that both the strength and ductility of steel can be obtained by piling and welding steel fagots after the same general methods by which iron forgings are made. To accomplish this, it will be needful to cast the steel in large 16 by 18-inch ingots of approximate composition, carbon 0.16 per cent. and manganese 1 per cent. The high manganese materially hardens the steel and gives it also a welding property, and in this steel the sulphur and phosphorus should be extremely low. These large ingots should be rolled in heavy trains into bars of 6 to 8 inches width by 1 inch thickness, and to a length a few feet greater than that to which the forging is to be completed. The bars may then be bound together and brought up, a part of them at a time, to a good welding heat. They should then be hammered until perfectly welded in the same way as with iron, and then drawn out by forging to the required finished shapes in swage-shaped dies, and afterwards given a careful annealing. Steel of this composition, high in manganese, will weld in skillful hands just as readily and securely as wrought-iron. In this way steel forgings can be made with the crystals of the metal fine and uniform in grain, this texture having been secured by the large reduction of area, by the rolls, from the ingots to the bars. This pile can thus be worked under the hammer much more effectually than the large and unwieldy original ingot from which otherwise the forging must necessarily be drawn out. It is extremely difficult to heat uniformly a single ingot as large as would be needed for a heavy forging if hammered from the ingot direct. It is very doubtful if, under ordinary conditions of working, blows of the heaviest hammers can be made to penetrate effectually such large ingots. On the contrary, the benefit due to the reduction in rolling of the ingots to the small bars is felt throughout the entire mass of the forging when thus built up. In forgings, especially those subjected to torsional strains, if each weld be a good one, it is the judgment of the writer that, in respect to strength, they are by no means a disadvantage in such a crystalline structure as steel.

TABLE A.

Description of Treatment of Specimen.	Tensile strength. Pounds per square inch.	Elastic limit. Pounds per square inch.	Percentage of elongation in eight inches.	Percentage of reduction of area.	Character of fracture.	Bending Test.
						Degrees.
Normal specimen, as plate was rolled and cooled	56 100	32 210	29.20	58.80	Silky	180 on itself.
" " "	55 320	32 190	28.70	59.60	"	180 "
" " "	56 920	32 640	29.50	62.40	"	180 "
Heated to a dark orange	58 400	32 440	29.90	60.80	Silky	180 on itself.
" medium "	62 100	32 600	30.40	61.40	"	180 "
" bright "	60 140	31 450	29.10	59.40	"	180 "
" lemon	57 190	30 100	27.60	54.10	"	180 "
" light lemon	54 400	37 990	24.00	46.30	Amorphous	180 " cracks.
" low white	56 100	36 400	21.10	49.30	"	180 " "
" scintillating white	43 980	37 190	2.10	20.20	Fiery, large crystals....	90 breaks.
Annealed in sand.						
Heated to a dark orange	58 190	31 850	29.20	59.10	Silky	180 on itself.
" medium "	59 150	32 790	29.90	62.30	"	180 "
" bright "	60 010	32 950	30.50	61.10	"	180 "
" lemon	57 110	31 430	29.20	59.20	"	180 "
" light lemon	55 330	30 260	28.50	57.50	"	180 "
" low white	50 100	30 050	27.20	55.50	"	180 "
" scintillating white	40 150	38 580	2.10	4.50	Fiery, large crystals ...	60 breaks.
Allowed to cool at temperature of air on bed of coal.						
Heated to a dark orange	72 590	44 430	21.10	50.50	Silky	180 on itself.
" medium "	73 980	45 580	20.20	47.20	Amorphous	180 "
" bright "	59 110	44 760	21.80	46.10	"	140 breaks.
" lemon	69 840	44 590	21.10	46 50	"	130 "
" light	80 330	50 210	16.20	34.60	Fine crystalline........	110 "
" low white	82 110	51 210	10.10	35.30	" "	100 "
" scintillating white	41 160	38 210	2.40	3,30	Fiery, large crystals....	50 "
Plunged in water at 60 degrees.						

Heated to a dark orange	82 460	55 560	16.10	40.30	Fine crystalline	180 on itself, slight cracks.
" medium "	85 790	58 460	12.90	38.30	" "	180 " O. K.
" bright "	89 310	60 490	15.30	39.20	" "	180 " cracks.
" lemon	86 460	61 150	14.50	38.70	" "	130 breaks.
" light lemon	89 840	63 580	13.90	32.10	" "	120 "
" low white	82 060	59 470	15.80	28.70	Crystalline	90 "
" scintillating white	42 490	37 560	1.80	1.90	Fiery, large crystals	50 "
Plunged in brine at 60 degrees.						
Heated to a dark orange	64 110	35 470	24.80	53.50	Silky	180 on itself.
" medium "	67 230	37 590	25.60	50.10	"	180 "
" bright "	66 490	36 470	22.30	50.10	Fine crystalline	180 " cracks.
" lemon	69 460	39 510	18.10	47.70	" "	160 breaks.
" light lemon	76 830	40 460	15.60	41.50	" "	150 "
" low white	78 850	46 310	14.30	31.20	Crystalline	130 "
" scintillating white	39 760	38 670	0.75	0.75	Fiery, large crystalline.	45 "
Plunged in oil at 60 degrees.						
Heated to a dark orange	59 160	31 450	29.75	59.50	Silky	180 on itself.
" medium "	62 490	32 650	31.20	62.40	"	180 "
" bright "	61 859	32 700	30.50	60.40	"	180 "
" lemon	60 190	32 120	26.70	57.20	"	180 "
" light lemon	61 370	35 670	24.80	54.30	"	180 "
" low white	59 210	30 490	20.70	48.10	Crystalline	180 cracks.
" scintillating white	37 860	36 870	1.00	1.80	Fiery, large crystals	45 breaks.
Annealed in sand after first being heated to a lemon color and quenched in water.						

TABLE B.

Description of Test.	Tensile strength. Pounds per square inch.	Elastic limit. Pounds per square inch.	Per cent. elongation in 2 inches.	Per cent. reduction.	Character of fracture.	Bending test around a 1-inch pin.
Gun No. 1.						Degrees.
Transverse test, cut from a disk from jacket	81 100	50 110	11.80	......	Coarse crystalline	110 broke.
Same, annealed	84 210	50 990	24.70	......	Fine "	180 "
Same, tempered in oil	127 480	88 860	16.10	......	" "	160 "
Transverse test, cut from a disk from the plug	71 840	42 470	12.70	......	Coarse crystalline	140 broke.
Same, annealed	74 310	47 760	28.60	......	Fine "	180 "
Same, oil tempered	90 270	59 470	17.10	......	" "	150 "
Transverse test, cut from a disk from the tube	64 440	34 460	16.30	......	Coarse crystalline	130 broke.
Same, annealed	68 800	37 710	26.30	......	Fine "	180 "
Same, oil tempered	84 350	54 970	19.10	......	" "	180 "
Gun No. 2.						
Plug made from an 18-in. square ingot of.. { Carbon .26, Manganese .35 }						
Tube " 29-in. diam. " .. { Carbon .25, Manganese .51 }						
Jacket " 47 " " .. { Carbon .33, Manganese .66 }						
Plug, 9 in. diam. Tube, 10¼ in. diam. Jacket, 18¼ in. diam.						
Disk at breech-end of gun tube, transverse test	67 450	38 100	16.60	21.1	Coarse crystalline	130 broke.
Same, annealed	69 580	39 110	28.30	58.0	Silky	180 "
Same, tempered in oil	88 350	51 150	24.10	47.6	Fine granular	180 "
Disk at muzzle-end of gun tube, transverse test	66 210	35 240	15.30	19.1	Coarse crystalline	140 broke.
Same, annealed	68 880	39 880	29.99	59.4	Silky	180 "
Specimen cut longitudinally from butt-end of gun tube	70 110	40 440	17.40	21.8	Coarse crystalline	120 broke.
Same, annealed	71 000	40 790	31.80	60.3	Silky	180 "
Same, oil tempered	99 210	70 690	20.10	41.4	Fine granular	180 "
Specimen cut longitudinally from muzzle-end of gun tube	69 350	38 840	14.20	18.8	Coarse crystalline	120 broke.
Same, annealed	70 240	40 410	32.70	53.9	Silky	180 "
Same, oil tempered	94 990	60 210	20.70	23.3	Fine granular	180 "
Disk cut from jacket, transverse test	82 210	50 140	12.10	15.5	Very coarse crystalline	100 broke.
Same, annealed	86 660	52 230	26.00	44.3	Fine granular	180 "
Same, oil tempered	115 990	69 390	18.30	24.8	" "	180 2-in. pin.
Disk cut from plug, transverse test	72 880	40 360	13.40	17.6	Coarse crystalline	140 broke.
Same, annealed	77 490	48 390	29.90	41.3	Fine granular	180 "
Same, oil tempered	99 990	61 140	19.90	26.6	" "	180 "

Mr. William Metcalf.—Mr. Hunt, in advocating the conversion of steel into muck bar to get the grain fine, and then piling, heating and welding the same, ignores the fact, so clearly shown in his own paper, that this welding heat will reproduce "fiery, large crystals."

I hope no engineer will adopt Mr. Hunt's suggestion, for the simple reasons that steel cannot be welded safely by any known process, unless it be the new electrical method confirming Mr. Howe's experiment.

A bar of steel about six inches diameter and forty feet long, broke at a strain of 36 000 pounds per square inch. The manufacturer bored a sample out of the fractured end, and it showed over 100 000 pounds tensile strength. Subsequent bars for the same work were made to meet all requirements, presumably by annealing the bars after they were finished.

I did not say nor intimate that a steel cast gun could be made for 9 cents a pound; I said good castings can be bought for less than 6 cents a pound. It is probable that steel cast guns would cost 20 to 30 cents a pound finished weight.

Senator John T. Morgan (by letter).—I had only one reason for desiring you not to mention my name in connection with the theory of the fluid mixture of iron and steel. It was that I dislike to advance theories that are aside from studies or topics that belong to my line of professional effort. I think the fact important, and very clear, that iron is a fluid. Its importance relates chiefly to the question of the best methods of purifying it and making it tough and hard. The change in the molecular structure of iron by lapse of time simply, without any discernible extraneous force, is due to its fluid character, an automatic faculty of rearranging its crystals. We know that many disappointing and even tragic results have grown out of this change in the structure of iron and steel which has escaped the most accurate observation.

If the fluid theory should enable us to anticipate these changes even, and to provide against their serious consequences, I will be very proud of having thought about a matter of such importance, however imperfectly.

As the subject is now up for examination amongst the thinkers who are deeply studious on such matters, I should feel honored to have it said of me that my mind had been drawn to the conclusion that iron is a fluid, by those simple processes of reasoning that even the faith of men who have no pretension to being scientists.

Professor John W. Langley.—In response to the invitation to join in the discussion of Mr. Metcalf's paper on "Steel," I will try to touch briefly on one or two points connected with the chemical and physical side of the subject.

Is Steel a Fluid?—Engineers have become familiar of late years with the conception that certain metals do behave, under stress, like very vicid fluids. Under mechanical tests the evidence is very strong that they are such. Is there any proof that chemically they behave like fluids rather than solids? I believe that so far as iron and steel are concerned the answer can be given in the affirmative.

First.—In mathematics we may regard the straight line and the circle as being each "particular cases" of the ellipse; and as between these extremes we may have ellipses of all possible ratios between the major and minor axes, so by analogy I consider we may have all possible grades of fluidity between a typical liquid, like water, and a typical solid, such as a single crystal of the diamond or quartz, or alum. These latter bodies do not permit of any displacement of adjacent parts (beyond their limit of elasticity) without rupture; they do not yield gradually to a continuous stress; and they will not, when overstrained, take a "new set." Steel will, however, do all of these things under a suitable pressure.

Second.—All liquids have the property of "surface tension," and so also do most of the industrial metals show the phenomenon of the "skin," or abnormal state of the superficial layers of molecules. If this "skin" is carefully dissolved away by dilute acids, a piece of metal, such as a coiled spring, will change its shape visibly during the action of the acid, but true solids in single homogeneous crystals do not thus change shape by twisting or bending when they are gradually attacked by a solvent.

Third.—The removal of the state of surface tension, or its modification, as by putting oil or alcohol into water, causes currents and molecular movements throughout the entire mass of the fluid till a new equilibrium is established.

Now it appears to me probable that the profound change in the texture and tenacity of the wires in my laboratory, referred to by Mr. Metcalf in his paper, was due to the change on the surface caused by an exceedingly thin film of corrosion due to an atmosphere slightly impregnated with acid vapors. Under the static stress thus set up by altering the "surface tension," the vibrations of the building, extending over three years, were sufficient to rearrange all the molecules of the wire into new forms, and an extreme brittleness was the result. Many cases of the so-called crystalline alteration of car-axles, etc., I think may be traced fully as much to external corrosion as to vibrations. May not both be necessary? Experiments in this line are now in progress.

Fourth.—Chemists, by researches on the heat equivalents of chemical actions, have been led, somewhat generally, within the last few years to consider that there is no philosophical difference between combination and solution; that, broadly considered, solution is a particular case of combination. I have shown, Proceedings American Association for the Advancement of Science, 1883, that even such important decompositions as sulphuric acid from sulphates of lime, copper and iron, could be produced by the simple solution of these salts in pure water.

Is it not, therefore, to say the least, a good working hypothesis to regard the carbon, phosphorus, etc., in steel as being present in solution and not as definite carbides. And just as a salt is partly extracted or crystallized from a hot solution on cooling, so we know that graphite is extracted from white iron by slow cooling. If a definite carbide of iron exists in pig metal we ought to obtain in gray pig crystals of the carbide, but instead we obtain, practically, plumbago.

In conclusion, let me say that a working hypothesis is valuable only as it accords with practice. Is it not probable that many of the well-known peculiarities of iron and steel can be explained and industrially dealt with, with some measure of success, in proportion as we treat the problem as essentially the problem of a viscid fluid.

I take pleasure in saying that during a period of fifteen years in which I have had professional knowledge of Mr. Metcalf's work and views, it has seemed to me that his mechanical and industrial results were not in opposition to any chemical data within my possession.

Professor John W. Langley (at a subsequent meeting). The following reference I was unable to obtain when writing on the discussion of Mr. Metcalf's paper.

As evidence that the carbon in cast-iron and steel is held in a different manner to what it is in manganese and ferro-manganese, see *Comptes Rendues*, page 964, Vol. LXXX, April 12, 1875, for a paper by Messrs. Troost and Hautefeuille on the "Heat of Combination of Carbides of Iron and Manganese."

In this paper the authors show that the heat of combustion of the iron carbides is much greater than that of the manganese carbides and they conclude: "Carburized irons are formed by absorption of heat, starting from their elements. This fact classes these irons in the category of explosives or of solutions."

The manganese carbides they say are analogous to definite chemical salts, being formed with marked evolution of heat, starting from their elements, hence are not solutions.

Mr. M. J. Becker, M. Am. Soc. C. E.—After Mr. Metcalf's emphatic indorsement of the use of cast-steel for heavy ordnance, the answer to the question which I propose to ask him may perhaps be considered a foregone conclusion. Still, if there is any one question in the discussion of which one may expect to be startled by unexpected revelations, it is this very subject involving the physical properties of steel. In the construction of modern bridges, the general tendency has been of late to abolish entirely, or to reduce to the most unimportant details, the use of cast-iron. There have been abundantly good reasons for this. Aside from the general uncertainty of the product, the inferior facilities for casting by the older methods, have made it very difficult, if not impossible, to produce columns of uniform thickness and density;

and when such imperfectly cast members were used in lighter structures, requiring but small sectional areas, any little flaw or inequality would very seriously impair the strength of a small sized piece; and although there have been comparatively but few disastrous results from this cause, the existing danger has been fully realized and has led to the abandonment of the use of cast-iron in general bridge construction. In larger structures, with members of greater sectional area, this danger from inherent imperfections would, of course, not exist to the same extent as in lighter structures with smaller members; but when the confidence in the virtue of the metal was once shaken, the general distrust applied to all shapes and sizes alike. I have noticed, however, quite recently, that some of the leading bridge-builders in this country are returning to the use of cast metal in large structures, and employ soft, malleable cast-steel for such parts as the large bolsters or shoes for the footings of the end columns of bridges of 500 feet span, which require not only metal of great thickness of bearing surfaces, but large sizes generally, with many interior strengthening ribs and webs for the support of pins as large as seven inches diameter. These shoes must be cast whole, and the metal must be of a quality which admits reaming and turning of the large pin-holes, and the planing of the bottom plates for the movement of the friction rollers.

I would ask Mr. Metcalf whether he would recommend the use of cast-steel for such purposes, and if so, what quality of steel he would prefer, considering, of course, not only the static loads to be carried by the castings, but also the constant shocks and vibrations of the moving traffic to which they must be subjected.

Mr. William Metcalf.—Yes, use dead soft steel annealed carefully; never without annealing. Do not mind the blow-holes, they indicate mild steel; but be very careful to avoid sharp angles and complications of unnecessary ribs. Always study simplicity of form, and fillet thoroughly every corner.

In many ways I find in our works that the good, reliable, staying qualities of good steel castings form the chiefest comfort of my shop life. We don't make steel castings, but we use them.

Professor William H. Burr, M. Am. Soc. C. E.—Although not bearing directly on Mr. Metcalf's interesting and instructive paper, it may not be out of place to note a point brought out by late specimen tests of structural (mild) steel.

It is and has been common practice to put into long span bridge columns steel with an ultimate tensile resistance varying from 65 000 to 80 000 pounds per square inch, and to subject the columns to working stresses proportionally increased (or at least approximately so) over those for wrought-iron columns similarly placed. It is both interesting and important to observe that the most recent compressive tests on specimens of mild steel justify these increased compressive working stresses for short steel columns, *i. e.*, for columns under perhaps 175 radii of gyration or 65 diameters in length if with pin ends, or still longer under other end conditions. Below these limits (where are found, of course, all bridge columns) the limit of elasticity governs the strength of the column, while above them the coefficient of elasticity is the basis of column resistance. There is no well defined line between these two fields of resistance, so to speak, as they shade gradually into each other, but the distinction is important, for it holds in full value for the columns used in bridge construction.

Specimen tests show that 65 000-pound tensile steel possesses a compressive elastic limit of about 40 000 to 42 000 pounds per square inch, while 80 000-pound tensile steel possesses an elastic limit of not far from 45 000 to 48 000 pounds per square inch in compression.

When these results are compared with a 26 000 or 28 000-pound elastic limit for wrought-iron, it is reasonable to infer that the working stress in even 65 000-pound (tensile) steel columns may be safely increased from 33 to 50 per cent. over that used for wrought-iron under similar circumstances.

It is also reasonable to infer that tests of full-sized columns, properly designed and built, will verify these conclusions, although published results of such tests are still lacking.

Regarding the cast-steel gun question, a layman should undoubtedly speak with great caution; he may, however, properly regard the matter as more experimental in character than Mr. Metcalf seems to. If a cast-steel gun can be produced for nine cents per pound, equal to the performance of the same duty, pound for pound, and equally reliable in its finished state to that which costs seventy-five to eighty cents per pound, there is little doubt which should be built. But there are serious doubts in the minds of many people whether Mr. Metcalf's estimate may not be a little low. In fact it could be more readily accepted as final if cast-steel guns of the heaviest caliber had already been fabricated at that price. The difficulties attending such a production have not yet been met, and the difficulties are seldom over-estimated under circumstances similar to these. If indeed such excellent results can be so confidently anticipated, it would seem to be a most natural thing for cast-steel manufacturers to produce at least a few small guns, and subject them to the proof of the severe firing tests which built-up guns are constantly sustaining.

Even with the increased price per pound at which a few only of such guns would be produced, the cost of the operation, in view of the profitable business sure to follow, would be a small consideration.

The experience of engineers with ordinary steel castings does not seem to me to justify confident expectations regarding the reliability of a gun produced by casting from steel with internal cooling. I am of course aware that the metal put into guns would not be precisely the same as that used for ordinary castings, but it is a very serious question in my mind whether such a mass of steel with no forging either in the liquid or solid state (for cooling is not forging) would be even approximately homogeneous.

I do not assert it would not be, but in the absence of such a gratifying result as a large homogeneous steel casting free from gas-fissures or bubbles, I do not believe that positive predictions on that side of the question can be at once accepted.

In reasoning from cast-iron guns to cast-steel, it also seems to me that difficulties are not altogether avoided. In the first place, it is not an exaggeration to say that a gun of given caliber, in order to meet the requirements of modern ordnance, must do double the duty of the same gun under similar circumstances twenty-five years ago. Hence any fault in the material due to any part of the process of fabrication will result in far greater relative and absolute damage to the gun. If therefore the process of casting and cooling should prove to be more delicate and difficult to control with steel than with iron, as would probably be the case, the resulting product would be of most doubtful value.

It seems to one not an expert in ordnance material, that the first evidence of the fact that the requisite steel, with proper degree of homogenity, high elastic limit and toughness, can be fabricated without forging either in the liquid or solid, or semi-solid state, is yet to be produced.

Hammer-forging may be damaging to the material, but hydraulic forging is not, nor is forging in the liquid state, such as is obtained under great pressure by the Whitworth process. It certainly is a fact that the best and most highly effective modern ordnance has been manufactured under processes involving those operations, with subsequent hardening and annealing. If the process of building up guns places the right kind of metal in such position and condition as to best resist the high intensities caused by firing, it certainly cannot be called unscientific. But if better results can be obtained at less cost by casting and internal cooling, by all means let us have them. Thus far, however, we have them on paper only, and that step is not a long one.

Mr. A. Gottlieb, M. Am. Soc. C. E.—The subject of Mr. Metcalf's paper is of such character and general interest that it inspires participation in its discussion.

I am sorry to be prevented from attending the meeting and taking part verbally in the discussion, being therefore compelled to confine my remarks to a few of the points touched by Mr. Metcalf.

That gentleman, through his long connection with the manufacture of steel, and by the great attention and close observation he has devoted to the subject of steel, in its manufacture, treatment, and the various uses of it, is an expert on this subject whose views and recommendations are bound to command due consideration.

Nevertheless there may be some points connected with this matter which may be of importance enough to be dwelt upon more than Mr. Metcalf has done, for one reason or another, or which may have come more frequently to the notice of others. To begin with, I cannot agree to the definition of Mr. Metcalf, or of United States Senator John T. Morgan, that iron or steel is a liquid, no matter how exhaustive and able the argument in favor of that theory may be. We have to consider the material as it is when ready for use in its ultimate shape, and the qualities it then possesses. For the same reason we make a distinction between water, steam and ice; although the substance is the same, the forms and qualities are different.

The qualities, at least the physical ones, of molten iron or steel are about the same; there is not much difference between them, except perhaps their density or specific gravity at the same temperature. How widely different are they in the concrete form.

It may perhaps be called putting the cart before the horse when I state right here that, in my opinion, steel is, and always will be, a treacherous material. Without much argument, and without citing my own experience, Mr. Metcalf's paper itself is proof enough for this assertion.

Any material that through a few degrees of heat more or less; through unequal heating or cooling in the same piece; through a blow or scratch on its surface, may fail to perform the duty in critical moments that we reasonably may expect from it, is dangerous, no matter how good it may be if perfect. The condition of perfection is dependent from so many factors, that it is next to impossible to have them all under our control. I agree with Mr. Metcalf and others that mild steel will stand more abuse than hard steel, and perhaps more than iron. But the term mild steel is only a relative one. What Mr. Metcalf may term mild steel from his stand-point, using steel of 1.00 and 1.2 carbon, other engineers using steel for structural purposes may call high steel. Mild steel capable of resisting hard treatment in pulling, punching, shearing, bending, etc., cannot have more than 0.10 to 0.18 carbon; steel of 0.20 to 0.35 carbon of open-hearth or Bessemer process belong already to the high grades of steel for structural purposes. These latter are still mild steel compared with tool steel, but have already all the unpleasant qualities that render their behavior in heating and hammering as uncertain as the highest grades. I will only remark that while the lower grades of steel will stand more abuse than iron under certain conditions, the good qualities can be easier destroyed, by improper heating, than in iron, and generally the physical qualifications are not much better than those of good iron. The cost of production of mild steel may under certain circumstances be less than that for good iron, but there is no large gain in strength over iron by using it.

The higher the grade, or the more carbon the steel contains, up to a certain limit of course for every specific purpose, the more advantage there is to use steel, provided the dangers inherent to such steel can be eliminated.

Equal heating and proper annealing are amongst the foremost points recommended by Mr. Metcalf and others, and by right so. But how difficult is it to have these two operations performed exact in each case.

Mr. Metcalf's example of the piece of steel of only 0.10 carbon (really soft wrought-iron) heated to different degrees and cooled in water, illustrates sufficiently the dangers of unequal heating. In higher carbon steel the difference of heat, to bring about such result, will be considerably smaller than here stated. To equalize such strains produced through unequal heating, it has been generally recommended and adopted to anneal such pieces after their manufacture. In my experience I found that mild steel of 0.10 to 0.18 carbon requires annealing to equalize such strains, for instance, in eye-bars for bridges, when one part of the bar has been heated in the fire and hammered, while the balance was left cold. I found, however, that every annealing diminishes the original amount of carbon in the bars and reduces the ultimate strength in tensions from 2 to 5 per cent. of the original strength before annealing.

Uniformity of the material is the first requirement, no matter what the other qualities may be. To obtain this, equal heating of all parts and annealing is recommended and applied. But there is another just as important an operation, which is usually neglected, and which Mr. Metcalf seems to touch only slightly. Next to the equal heating is the equal cooling.

While rapid cooling produces small crystals, and consequently stronger steel than slow cooling, it may be advisable to dip the hot steel of small size into water or oil. For steel of large masses, like guns or armor plates, I consider this kind of dipping or cooling not only useless, but dangerous. The outside parts of these masses must necessarily be cooled and changed in their structure before the inner parts are reached, which condition in itself must produce unequal strains in the great mass. For such large masses, in my opinion, the proper cooling is that which starts in the center of the mass, keeping the outside from too rapid cooling by external heat, until gradually the whole has cooled uniformly, Even at the risk of producing larger crystals and smaller ultimate strength, I would prefer such a process that would produce more uniform material. In this connection I would mention that, from my observation, hot rolled steel bars placed upon the cooling bed in the mills, one end nearer to the cold outside air than the other, or one side near the cold floor, show entirely different fractures. This would indicate that it would improve the uniformity of steel rails and plates, by cooling them under cover, instead of exposing them to drafts and cold outside air.

This rapid local cooling of certain parts of the same piece causes also the small surface cracks, which may become so dangerous, as mentioned in Mr. Metcalf's paper.

In conclusion I would like to state that, from my observation and experience, I believe myself to be justified in saying, without in the least wishing to deter any of the merits of the ideas and results obtained by General Rodman or Captain Michaelis, and hesitating to take issue with such a recognized authority as our late Mr. A. L. Holley was, that the pressing of steel by hydraulic pressure, and for large masses, preferably the hammering, is not so cruel a process as Mr. Metcalf represents, provided the large masses are properly heated, the hammering or pressures large and strong enough to finish the operation while the steel is not cooled beyond a proper limit; and when the operation is finished the proper means are on-hand to have the piece either annealed or cooled in the right manner. I believe under such circumstances the steel is improved by hammering or pressing.

That the product so obtained is costlier than castings would be, is true beyond question, and for this reason it would be well if the United States Government would institute experiments to demonstrate the comparative merits of the various processes as Mr. Metcalf suggests.

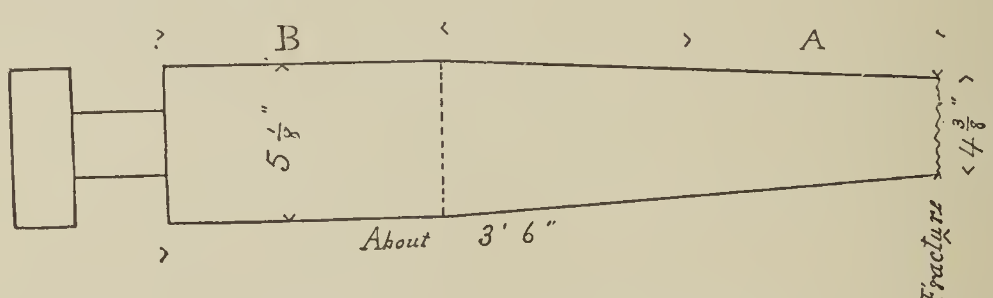
Mr. Percival Roberts Jr., M. Am. Soc. C. E.—The perusal of Mr. Metcalf's paper cannot but impress the reader as the utterances of one who is a thorough master of the subject of which he treats. Leaving the question of gun steel, which really is, considering its tonnage, but a very small item to the annual total consumption of steel, we come to Mr. Metcalf's remarks in reference to the manipulation of steel during manufacture for structural purposes, and I believe no truer words were ever spoken than when he says: "Heat is the power which gives to steel all of its good and all of its bad conditions."

I do not understand by this that steel bad originally will be made good by heat, but that good steel can very readily be made bad by the same. If the above theory is accepted as true, do we in our present practice sufficiently test steel in the finished shape in which it is to be used in our structures, or do we not rather give too great an importance to test of a form under conditions entirely foreign to those existing in the finished product? I refer to the testing of ¾-inch round rolled from either large or small sample ingots. There seems to me to be no more reason for testing a sample ¾-inch round and then accepting the heat of steel than there would be in testing the pig iron before it went into the furnace or cupola. The engineer and the manufacturer each have a certain territory, the bounds of which I think they should not overstep. To the former belongs the right of making all specifications as to quality, either physical or chemical, to which he may wish his steel to conform. He may also, if he sees fit, decide by what commercial method his steel shall be made.

To the manufacturer, on the other hand, should be left the manipulation of the material, free from all interference whatever, until it is delivered in its final form, and not until then is it the duty of the engineer to test or inspect. I think it possible in the future that arrangements can be made and systems perfected to make as full and complete tests of material in its finished form as will satisfy the most critical; not necessarily in a machine for tension, etc., but probably a certain number of each and the balance by bending or otherwise, and each shape or section thus accepted or condemned on its own merits, for which good steel may be stronger than good iron. Bad steel is infinitely worse and more dangerous than inferior iron, and the material is one which must be handled during all processes with intelligent care.

Let us suppose, however, that due attention has been given to tests; are we certain that the testing machine and other various specimen tests will give us the proper and necessary information as to the suitability of steel for structural purposes?

A short time since in filling an order for steel axles to conform to the Pennsylvania Railroad tests and requirements, a lot of Bessemer blooms were used, procured from a reliable maker and furnished under his guarantee to meet the above specification, viz.: The finished axles being $4\frac{3}{8}$ inches diameter at the center were required to stand five blows of a drop weighing 1 640 pounds falling 25 feet, the axle to be placed upon bearings three feet apart and reversed after each blow. Of ten axles tested only one met the requirements, the balance failing at from two to four blows. Three of these axles were then taken and from them test pieces were cut as below:



The test piece *A* was taken immediately adjoining point of fracture, and *B* as far removed from same as possible. The results were as follows:

	E. L.	B. S.	Stretch per cent. 8 inches.	Reduction per cent.	
Axle No. 1 *A*....	36 940	62 100	18.7	48	
" 1 *B*....	36 580	64 020	20.2	27	
" 2 *A*....	32 680	58 500	27.0	50	All per square inch.
" 2 *B*....	32 400	59 150	26.5	50	
" 3 *A*....	36 300	63 060	22.0	47	
" 3 *B*....	36 360	61 820	21.5	45	

Axle No. 1 stood the required drop test unbroken.
" 2 failed at the second blow.
" 3 failed at the fourth blow.

Again, in another instance, four axles made from the same lot of open-hearth steel containing .20 carbon and charged in the same heat at the forge, gave under the drop the following results:

Axle No. 1 broke at	48 blows.	}	1 640 pounds falling 25 feet.
" 2 "	2 "		
" 3 "	6 "		
" 4 "	18 "		

And yet with such discrepancy in the results of these four axles, the deflections measured after each fall of the drop are almost identical. It may be said that the results given above are too few to draw conclusions from, or that the discrepancy in results is caused by bad working or inferior steel. The only answer to which is that they are no more subject to such dangers than any other commercial products that may be furnished, and in fact the writer believes these instances are less so than usual, as they were watched with great care. To me they are a few among a great many illustrations that testing machine samples do not in all cases convey accurate information in regard to the proper quality of material for structural purposes, and that tension tests will not give us reliable data as to resistance of material against shock or impact which is so important a factor in the life of material in service. They also illustrate the folly of accepting finished material upon tests of the same in a preparatory state, which test should be for the guidance of the manufacturer only and not for the engineer. I believe we do give sufficient importance to tests of material within the elastic limit, but place too much dependence upon results obtained from ultimate destruction. The above remarks are offered more as suggestions than as proof of any theory, and I must ask the indulgence of the Society for the hurried manner in which they are thrown together.

Mr. Samuel Tobias Wagner, M. Am. Soc. C. E.—While the paper before us is wonderfully rich in statements regarding the high carbon steel, on which such data is very scarce, it is as to certain properties of low carbon structural steels that I wish to make a few remarks. A very simple thing, but one too often overlooked, is the method of preparation of small test pieces both in iron and steel, but especially the latter, and it is a point that inspecting engineers can well afford to investigate.

The point referred to is one specially pointed out by Mr. Metcalf, and is that surfaces should be free from tool marks and should be smooth. The final preparation of test pieces should always be done on a planer, and by a first-class mechanic, and the surfaces and edges examined with care if a fair test is expected. While in the testing machine, the appearance of a crack in a steel specimen is a sure sign of destruction, as we have no fibers as in iron to arrest the tearing.

Regarding the care necessary in heating steel, I would call attention to one paragraph in the Report of the Naval Advisory Board on the

"Mild Steel Used in the Construction of the Cruisers Atalanta, Dolphin Boston and Chicago," prepared by Assistant Naval Constructor R. Gatewood, U. S. N. (1886).

"In making the quenching tests* at Phœnixville the pieces were heated generally in a smith's forge with blast, in a hollow fire, either roofed with wet coke, or with a board laid over the top and covered with pressed coke. Sometimes they were heated in a small furnace burning soft coal. On one occasion about sixty pieces were placed, five or six at a time, on the top of a close anthracite fire under a small vertical shop boiler, being thus slowly heated to exactly the desired temperature and perfectly uniform. All of these pieces failed to bend as required, some of them cracking all across with bright crystalline fracture at the first blow of the hammer, and with no reduction at the fracture, while second pieces from the same bars heated as usual passed the test well. It is regretted that the cause of this behavior could not be exactly defined. It plainly consists in the surface absorption of some brittle making element from the fuel, most probably of sulphur, though it has been proposed that the atmosphere surrounding the pieces being highly carbonaceous (the fire was dull and the pieces were twenty to twenty-five minutes heating), the trouble might arise from a surface cementation or absorption of carbon itself. At all events this extreme case illustrates the well-known necessity of using pure fuel in heating and especially in making expensive flangings, and points to a possible solution of some of the inexplicable fractures of such plates, while it conclusively shows that the circumstances of making this apparently simple hardening test need some attention."

It would perhaps be advisable to add as to the quality of this steel that the average of 133 accepted heats gave

Ultimate tensile strength, 64,020 pounds per square inch.

Average ductility, 25.52 per cent. in 8 inches.

Carbon, .1633.

Manganese, .443.

Phosphorus, .0875.

The theory that iron and steel crystallize under the action of repeated blows and strains, I think is very well replied to by Mr. Metcalf; and while the theory has many staunch and able supporters, I do not feel justified by the facts that have come under my personal notice in thinking that such a crystallizing action takes place, but rather that the result is caused by some inherent defect in the metal, or that some abnormal stress under unwarranted circumstances has produced it.

The fracture of a full sized member of a structure under suspicious circumstances, revealing a fracture partly good, and a spot or one side containing large, bright crystals, I do not consider should always be charged against the material, nor do I believe that any but an expert

* Quenching Test. "A test piece shall be cut from each angle, plate or beam, and after heating to a cherry red, plunged in water at a temperature of 80 degrees Fahr. Thus prepared, it must be possible to bend the pieces under a press or hammer, so that they shall be doubled around a curve of which the diameter is not more than one-and-one-half times the thickness of the plates tested, without presenting any trace of cracking."

can safely say whether it has been over-heated, or whether it is due to a sudden break under high strain.

A full sized iron eye-bar, 5 x 1½ inches, was tested, and upon reaching the ultimate strength gradually began tearing through a slight flaw on the under side as it lay in the machine. After two-thirds of the section had torn, the bar broke suddenly across the remaining third, revealing a section one-third of which was completely crystalline and the remainder beautifully fibrous. The bar was nicked with a cold chisel three inches from the fracture and broken under a hydraulic press, revealing a fracture entirely fibrous. One not an expert, would, upon seeing the original fracture, have been justified in pronouncing the bar over-heated on one side.

Mr. D. J. Whittemore, Past President Am. Soc. C. E.—My own experience is that as a rule steel suffers about ten per cent. in strength value after it leaves the mill and has gone through the necessary shop manipulation to transform it into bars for bridge purposes. While it appears homogeneous before being wrought, this characteristic is, in a measure, lost on leaving the shop, and now the question is: How can this be prevented? I know that in the manufacture of eye-bars it is claimed that iron suffers the same relative deterioration, and I also know that nearly every manufacturer will claim that this is not so by their processes of manufacture.

I thank you heartily for giving this paper to our Society, and trust that those of our Members who feel competent to treat this subject with some degree of intelligence will give it that attention that is so meritoriously deserves.

In general, it may with truth be said that many of our engineers are as ignorant of this subject as we were when Scott Russell drew marked attention to and warning of its peculiarities thirty years or more ago.

Mr. Joseph M. Wilson, M. Am. Soc. C. E.—Mr. Metcalf gives us much information of value in reference to steel, and in a manner that makes it exceedingly interesting. Surely, with all the knowledge in reference to the manufacture and management of steel which this article intimates, and with the ability of the manufacturer to control the quality produced, which he evidently possesses, the engineer ought to be able to obtain in this material what he wants for his particular purposes. Also the knowledge of the effects produced by conditions of heating, etc., should warn him to exercise that care so necessary in the manipulation of this material, as he shapes it to the forms he desires.

I quite agree with Mr. Metcalf in his remarks on the crystallization theory. I have had wrought-iron tested that had been some twenty-five or thirty years under heavy service, where I knew it had been considerably overstrained, and yet the results were so excellent as to leave no reasonable doubt but that it was still in as good condition as when first put in use. I have also known brass-wire chains, by which hanging baskets of flowers were suspended, and which, when new, were amply strong for their purposes, to become so rotten from the action of the air as to break under their load, the whole character of the wire having been changed undoubtedly by a chemical action of the air, not from crystallization under service.

I do not profess to be an authority on the manufacture of guns, but Mr. Metcalf's remarks strike me as full of common sense and quite *apropos* to the occasion.

I would like to ask Mr. Metcalf if he knows of any way of testing for over-annealing in eye-bars without injury to the bar?

Mr. William Metcalf.—I do not know of any way to test an eye-bar for over-annealing without injury to the bar, unless it would be by discovering a greatly reduced elastic limit. I have no data on the point, but from the general worthlessness of badly over-annealed steel, I would "guess" that its elastic limit would be affected seriously.

Mr. L. L. Buck, M. Am. Soc. C. E.—Steel possesses so many desirable qualities, such as its great strength, its susceptibility of receiving a fine, true finish, its resistance to corrosion, on the one hand, while, on the other hand, it is so difficult to ascertain what serious defects may lie hidden within any piece from which satisfactory test pieces have been cut, that a paper on the subject by a gentleman who had been so long engaged in the manufacture as has Mr. Metcalf must always possess a peculiar value.

That part of the paper relating to comparative tests of steel in the cast state with that which has been subjected to the process of rolling or hammering, suggests a direction in which investigation might be pursued to advantage in selecting steel for structural purposes.

Most of our tests have been made upon specimens prepared from each blow by rolling or hammering, and upon the results of such tests we base an acceptance or rejection of the whole blow. Consequently we sometimes find, out of a number of bars or plates rolled from an accepted "blow," one that exhibits characteristics that, appearing in the test piece, would have caused the proper rejection of the whole blow. If then, in addition to prepared test pieces, we were to have a cast test piece made in each case, we should not only learn whether such defect as appeared in the tests were due to the blow itself or to subsequent manipulation, but we would soon have data of great value as to the actual strength of steel castings.

In all the discussions upon the subject of heavy guns that have come under my notice, the effect of the inertia of the mass of the gun in resisting the almost instantaneous impact of the discharge has been either overlooked or neglected.

If we suppose a very thick and perfectly homogeneous hollow cylinder of any metal, and with no initial stress in any part of it, and that a heavy bursting stress be applied to it gradually, there will result a greater increment of the inner than the outer diameter, due not alone to the fact that a large cylinder having a sectional area equal to that of a smaller one must be thinner, but also that there is a radial outward compression of the material, as well as circumferential tension. On this account the tensive stress at the surface of the bore is rendered still greater than would otherwise be the case. If now, instead of applying the pressure slowly, it should be produced by an explosion of gunpowder, the same radial compression takes place, but very much greater at that portion next to the inner surface, while it is less toward the exterior, and, consequently, the impact of the discharge is resisted by the inertia of but a small portion of the surrounding mass. In such a case very little of the stress produced would ever reach the exterior, as before it could do so the inner surface would crack, whereby the area pressed upon would be increased with comparatively slight increase of volume of gas, and the cylinder would merely tear apart.

If we can produce a cylinder having a strong initial circumferential tension in the outer surface, and gradually decreasing to the interior till it produces a compression within, and that greatest at the surface of the bore, we shall not only have the strength of the section, but the inertia of the mass as well, to resist the impact.

As to the forging of large masses of steel (or iron), there appears to be no way of accomplishing it which may not be termed "barbarous." Whether rolled, hammered, or compressed, such working must fail to extend to the interior, unless the piece is hollow, and the work and cooling proceed inside as well as without. Otherwise there will always be a portion of the interior that will not only not receive a proper amount of work, but, being the last to cool, will remain with a strong initial tension. This will tend to render whatever defects it originally presented—as, for instance, piping—still more serious. At the same time that the interior surface is subjected to tension, there must necessarily be a compression of the outer circumference. Such a condition is manifestly the worst that could exist where the cylinder is a gun, and one which experimenters have endeavored to reverse, but, as the paper states, with only partial success, if we except the case of the Rodman cast-iron gun.

A process may be found in the future whereby a steel gun of large size can be forged on the inside while the outside is made to be the last to cool, and thus produce the proper conditions. Even if the separate bands of a gun could be made in this way, the built gun would be a successful but costly one. But as at present forged, probably but a small portion of total strength of the section of a solid forged gun is developed by an explosion. The same condition exists in each of the thick bands of the built gun, though to a less degree.

The practicability of making a steel cast gun successfully must depend upon whether a steel cast gun of such massiveness as is required for a large gun can be produced sound and strong enough to give it the requisite strength per square inch of section. If so, there appears to be no insurmountable obstacle to produce a successful gun in the manner proposed by Mr. Metcalf, as, without doubt, the other requisite conditions could be acquired as well with the steel castings as with iron castings. The unit strength of the metal could be considerably less in the cast than in the forged gun, and yet make a stronger gun.

Of course any slight cavity in the inner surface of the steel cast gun would be inadmissible. But if on boring the gun a slight cavity should appear, the gun could be bored larger, and have a forged steel brush turned and forced into it. Indeed it is possible that the steel cast gun might be made a success, if a breech-loader, by casting it on the Rodman principle, then bore it with a very slight taper (slightly largest at the breech); then make a forged cast-steel tube of a pretty high grade of steel, and as thin as might be found practicable, turn it to exactly the same taper as the bored cavity, as well as of the proper size; warm the casting, and let the tube be compressed by the cooling. The tube would then receive a high finish, and present a durable, strong surface to resist the action of the powder and projectile.

Even supposing but one casting out of several should prove perfect enough to sanction its use, we should still have an economical gun.

If such a casting should have a large cavity, or a number of them, which did not appear in any part of the surface, they would be detected by weighing, or by the final tests at the "proving grounds."

Gustav Lindenthal, M. Am. Soc. C. E.—The subject of Mr. Metcalf's admirable paper on steel and its properties, suggests so many thoughts and investigations, that to discuss even a small part of them would make a lengthy paper. I wish only to make a few remarks, therefore, in relation to structural steel.

The statement that iron and steel are liquids, seems to be more in the way of a paradox than a definition. And even the paradox would be obscure without Professor Langley's elucidation, which is meant to apply to other metals also. This theory has a truly mathematical aspect in starting out with a general law, the application of which to metals is at once fascinating and promising of great results in the understanding of their nature; but, as in mathematics, a certain fixed nomenclature of terms and definitions will be necessary before the inductive process of reasoning can be carried very far. At present there is some confusion as to the meaning of vibration, impact, oscillation, concussion, crystallization, etc., and exact reasoning is thereby much impaired. As a consequence, some notions, amounting almost to axioms, have been ingrained into our minds in school and in practice of which we have to rid ourselves first before we can look at things without bias; as, for instance, that rails in a track and car axles will change their texture in consequence of vibration and become crystalline (as it is called), from which long ago has been argued by eminent authorities that iron and steel bridges in course of time must fail from like cause.

But recently again the cold of winter has been accused of changing iron and steel from a fibrous or ductile condition to one of coarse crystalline texture, and an array of tests was furnished in evidence. And as another instance, Mr. Metcalf had at one time furnished facts and his experience with steam-hammer rods and connection bars of locomotives, from which was deduced the theory that hard steel is much better than soft steel for resisting vibrations, which Mr. Metcalf assumes differs from alternate strains only in degree.

It seems to me that the true explanation of failures in above and similar instances is from other causes.

We know of nothing so injurious to the physical structure of iron and steel as the sudden reversion of strains, and next to it sudden variation of same kind of strains. The metal requires a certain time to absorb the strain and to accommodate itself to it. If this time is not given, then a gradual weakening and ultimate failure is the result.

We know from many experiments that the more ductile the metal is the better can it resist sudden variations of strains.

Ductility expresses but the greater capacity of the metal to flow under the application of force; or the more liquid it is (to make use of the new theory) the better it will resist fracture.

In point of liquidity we have soft wrought-iron first, then the low steels, and least liquid are the high steels. Just in this order they fail under tests, namely, wrought-iron gives the greatest number of reversions, and high steel the least for the same ratio of strains to ultimate limit.

It is natural for a steel-man like Mr. Metcalf to say that "mild steel, which is commonly used for structural purposes, is more ductile, stronger, and tougher than iron;" he should have added, in the testing machine. Mr. Benjamin Baker in his paper read before the American Society of Mechanical Engineers in 1886* presented tabulated results of tests with spindles of wrought-iron, soft and hard steel, subjected to reversion of strains. These tests are important and instructive, but in a somewhat different way than the one assumed by Mr. Baker, for he is committed to steel, and his paper is presented "to bear testimony to the admirable behavior of a very good friend of his, namely, mild steel." Nevertheless his very tests show that wrought-iron gave the best results, but he does not make as much of them as those with soft steel.

Thus his table (series No. 1) shows for soft steel from 68 400 to 155-295 reversions; hard steel, from 5 700 to 7 500; best bar iron, 389 050 to

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* Some Notes on the Working Stress of Iron and Steel: Benjamin Baker. Trans. Am. Soc. Mech. Engrs. Vol. VIII. 1887. Page 157.

421 470 reversions for practically the same ratio of strain to ultimate limit (factor a of this table), namely, 1.84 to 1.90.

In my paper on the Monongahela Bridge,* I took occasion to state the experience had in the old Suspension Bridge with short suspension rods subject to alternate bending strains, and I mentioned the fact that good soft wrought-iron rods would last four to six times as long as good selected soft steel, which agrees very well with the tests made by Mr. Baker.

His reference to a greater number of mysterious fractures in a few tons of wrought-iron, presumably of indifferent quality and for temporary purposes, than in 24 000 tons of mild steel, is surprising, but too general for serious consideration. Ordinary English wrought-iron must be of poor quality indeed if a good quality costs double and triple the price of Mr. Baker's steel. It is not so in this country, where excellent bridge-iron is still cheaper than the lowest-priced structural steel.

Rails in the track and car-axles are subject to rapid reversions of strains, which ultimately destroy the cohesiveness of the metal and cause failure, but not necessarily so. For if the rails and axles are large enough, so that the reversions of strains are kept far within the elastic limit of the material, they will last very much longer. The term crystallization in reference to such fractures is used with too much laxity; granulation would be a better term. There are no crystals in the fracture, and no crystals can form out of rigid and cold iron or steel. The fine-grained texture simply changes to a coarse-grained texture, or the fine grains ball up to coarse grains by reason of the inability of the iron to flow fast enough under sudden strains or under a reversion of strains.

Vibration has nothing to do with it. Vibration is the result of impact or sudden strain. It is a wave-like, exceedingly rapid motion of the metal far within the elastic limit, and tending towards rest, as in a piano-string. Impact can cause fracture, but vibration does not.

Oscillation is something different. It is the result of intermittent strains of same kind produced by variations in the action of loads or forces. Oscillation is cumulative, and tends towards fracture without a reversion of strain. A bridge under the regular step of soldiers oscillates, and if sufficiently long continued would break the bridge down. Impact, intermittent strains and strains from steady load can happen in a structure all at the same time. Their different effects then become superimposed.

The breaking of steam-hammer rods and connecting rods of locomotives is not due to vibration (or even oscillation) as alleged, but to the sudden reversions of strain. Mr. Metcalf's version has been cited frequently, but I believe it to be erroneous.

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* Rebuilding of the Monongahela Bridge, at Pittsburgh, Pa. G. Lindenthal, M. Am. Soc. C. E. Trans. Am. Soc. C. E., Vol. XII, p. 353. September, 1883.

His experience was that wrought-iron and mild steel hammer rods would frequently break. By accident a high steel rod was taken at one time to replace a broken one of mild steel, and it lasted much longer than any before. Mr. Metcalf ascribed this to the greater rigidity of high steel in resisting vibration. The fact is, however, that the high steel rod was simply so much stronger. The strains which the rod had to resist were, we will say for illustration, 10 000 pounds per square inch in soft steel, with an ultimate strength of 50 000 to 70 000 pounds. Taking now a material however of 100 000 pounds ultimate resistance, and keeping the diameter of the rod unchanged, must result in greater durability. Had the high steel rod been reduced in size to correspond with the same ratio of strains as in mild steel, the high steel would have been broken sooner than any of the others.

The same reasoning applies to connection rods of locomotives. Of three such rods, one of wrought-iron, one of mild steel, and one of high steel, and all of good quality, the rod of high steel must necessarily last longest. But make these rods of sections, so that each would have the same ratio of unit strain to ultimate strain, then the high steel would break first, the mild steel next, and the wrought iron last.

This would be in agreement with the views of liquidity of iron and steel as stated by Professor Langley, and this is confirmed by Mr. Benjamin Baker's experiments and by my own experience with iron from old bridges.

The lesson to bridge engineers from this is, that in steel bridges the ratio of unit strain to ultimate limit; or, better yet, to elastic limit, should be greater than in wrought-iron bridges for the same margin of safety. But this rule should be used with discretion. It should be kept in view that for a quiescent load the conditions of durability are the most favorable, and that they are the same in all three metals. That the durability (or safety) is least in members exposed to reversion of strains or to suddenly applied strains, and differing in the three metals; and that between those two limits of durability or safety in each metal we should graduate the unit strains in accordance with the changeability of the total strains which the member has to resist.

The bulkiest bridge is the best. As wrought-iron gives a more bulky bridge than one of steel, it is the best for all ordinary spans. Practicing engineers have, from their experience, been led to this view for some time. In longer spans other questions arise, which may modify somewhat the former conclusions. The relation of dead to live load is growing more favorable as the spans become longer, and also the conditions for certain members with cumulative strains become more favorable, so that high steel may be used for such members without loss of safety.

But again. Of three bridges, one of wrought-iron, one of mild steel, and one of high steel, and for same length of span, all of the same bulk, the high steel bridge will be the strongest and most durable.

On the other hand, where the type of construction is such as to produce large secondary strains, as in riveted structures, a high steel should not be used, because it is wanting in sufficient ductility, or liquidity, to resist them without injury. Then there are the textural difference in wrought-iron and steel to be considered in the choice for structural material.

Steel is crystalline in texture; wrought-iron is fibrous. I am aware of the theory which assumes that wrought-iron is also crystalline, only that the crystals are drawn out in the rolls or under the hammer between fine layers of slag. This theory is more elegant than true, and useful only to dialecticians defending the universal crystal theory. We know that wrought-iron owes its great virtues to its fibrous texture. Good wrought-iron varies but little in ultimate limit and elastic limit; eight per cent. for the first and five per cent. for the latter will cover the range of differences. Wrought-iron is not only ductile and tough, but it is also hardy. Scratches, nicks, dents, rough handling, do not hurt it much. During shop manipulation and in the blacksmith's fire it is not apt to get hurt much if of good quality.

But steel, soft or hard, is a very much more sensitive material, and the greatest care has to be observed in its manipulation. To make sure that it did not get hurt in the shop it afterwards requires doctoring in an annealing furnace, and often this doctoring makes it worse, as Mr. Metcalf himself states in his paper.

The variations in ultimate and elastic limits are greater in steel than in wrought-iron. For the same quality they vary so much as sixteen per cent. for ultimate and ten per cent. for elastic limit, or double as much as in wrought-iron.

If the conditions to be observed in the working with wrought-iron are half a dozen, then for soft steel they are at least twice as many, and for hard steel three times as many. If after all this there results a certain economy in the use of good steel under safe conditions in a bridge, it should be used, otherwise it should not be used. Steel for all bridges, for all spans, and steel as the metal of the near or distant future for everything, seems to me a bad rule. And in this connection I should like to ask of steel manufacturers the question: What kind of steel can that be which is sometimes bought for bridges at prices nearly as low as for ordinary wrought-iron in the present state of the art?

To make good bridge steel costs more than a corresponding good wrought-iron in this country. And if a doctored cast and rolled product resembling steel stands certain meager tests in the testing machine, it is not a sufficient warrant for putting it into a bridge, with dimensions so attenuated, as if it were of most excellent quality. The time will come when "true as steel" will not be true of such bridges.

On the alleged influence of frost on iron and steel, the tests of Mr. Joseph Ramsey, Jr., Chief Engineer Cincinnati, Hamilton and Dayton

R. R., have been mentioned as giving some new light on the question. I doubt somewhat the value of the tests as given in his paper before the Engineers' Society of Western Pennsylvania.* A specimen taken from a freezing mixture to a testing machine will not give accurate results. The surface warms quicker than the interior of the specimen, which therefore has inert strains; the skin expands faster than the interior of the specimen. In this condition a blow from a drop weight strikes it and a short fracture is the result. From thousands of tests, made in the cold of winter, the uniform lesson was that no difference in strength or texture existed in the iron in the extreme temperature of our climate. If the woodman warms his axe in the winter before using it, are we not rather to assume as a reason that by striking the cold material into the hard frozen crust of a tree, heat is generated which will expand the thin edge much faster than the bulky metal back of it, and therefore will crack it off ?

The greater frequency of broken rails in the winter can be accounted for in a similar manner.

First.—There is the well-known explanation of the unyieldingness of frozen ballast, exposing the rail to anvil-like blows.

Second.—There is the fact that the under side of the rail in contact with the frozen ground is cold and contracted, while the top and side on a sunny winter day are exposed to the heating rays of the sun.

Uneven expansion and inert strains are set up; the rail, if it could, would like to curve up in the middle. Now comes the train and delivers blow upon blow on a rail weakened by inert strains; and fractures at the weakest spots, such as are experienced, are the consequence. Most of the so-called mysteries could perhaps be explained in a similar way.

Mr. Metcalf states that "a great railroad company discovered that the moduli of elasticity of mild, medium and hard steels, tempered and untempered, were practically the same."

This is important, if true, and it would have been well to mention more of the facts in the case and to give the authority of the experimenter. For if the facts are as stated, it is against the new theory of liquidity of iron and steel in which Mr. Metcalf otherwise firmly believes. He further says it was proven by the fact that coiled springs of any kind of steel, if bars were made of the proper size according to a formula based on same moduli of elasticity, were all right. Now this may be true and still prove nothing of the kind.

I doubt the correctness of the conclusions as to the same modulus of elasticity in all kinds of steel, and in the absence of detailed facts I should prefer sticking to the old view of proven great variations in the moduli. It is possible that Mr. Metcalf mistook some other relation, of which the modulus of elasticity was a function, for the latter alone.

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* The Effect of Temperature upon Structural Iron and Steel: Joseph Ramsey, Proc. Engrs. Soc. Western Penna. Oct. 18, 1887.

The use of steel for heavy guns is a topic which received more consideration perhaps than any other in the discussion of Mr. Metcalf's paper. In view of the strains the gun has to resist, the metal should have a high elastic limit and great ductility combined. This we find only in the very best of tool steels and in small specimens. Mr. Metcalf contends that the necessary physical conditions in gun steel can be produced through the agency of heat and cooling, as by casting on the Rodman principle. But whether cast in this way, or forged by the Krupp method or the Whitworth process, what confidence can we have in the use of steel in large masses, when tests will show that it will break in bulk with sometimes as low as 39 000 pounds per square inch, when the same steel in small specimens in the testing machine will show 100 000 pounds per square inch?

Wrought-iron does not show such great range of uncertainty, but so far has not otherwise been adapted for the production of large cannon.

Would not the so-called Mitis process of casting wrought-iron offer a solution of these difficulties? Supposing a tube of fine hard steel (to resist the abrasion from the shot or projectile) as a core, around which were cast by the Mitis process the body of the gun of wrought-iron; it would melt the outside of the hard steel tube, become thoroughly welded to it and form one whole piece. The ferro-aluminium used in the Mitis process as an addition to wrought-iron is claimed to also prevent the crystallization of the cooling metal, which in addition has greater strength than wrought-iron alone. It seems to me in that direction there is yet a field for great developments.

Mr. William Metcalf.—I made no mistake about the modulus of elasticity; the data can be found in Mr. Cloud's paper on springs, read before the American Society of Mechanical Engineers at its Pittsburg meeting.*

If Mr. Lindenthal had read more carefully, he would have observed that I did not say springs of iron, mild, medium and hard steel, tempered and untempered, would be equally good, but that the formula said so; but the railroad referred to adopted unusually high steel, tempered, in spite of the formula.

Mr. Theodore Cooper, M. Am. Soc. C. E.—The valuable and instructive paper just read is one of especial interest to us. We all realize that steel is to be the structural material of the future; and with a material covering such a wide range of properties, it is absolutely necessary that we should know its characteristics and capabilities. In order to take full advantage of these properties, and thus obtain the best results, we must be able to select which particular grade of this material is best adapted to our purposes. To do so we must know not solely the "test-

* Helical Springs: John W. Cloud. Trans. Am. Soc. Mech. Eng., Vol. V, 1884, p. 173.

ing machine" characteristics of the material, but in what manner each particular grade of the material will be affected by the processes of manufacture through which it must pass before it forms our final structures.

Papers like the one before us, from men who are daily watching and studying its characteristics; noting defective results and searching for the reasons thereof; not proving their conditions by simple laboratory tests, but by the successful attempt of a manufacturing establishment of no mean character, demand our careful study and consideration.

Considering the increased sensitiveness of the higher grades of steel to the action of heat and work, through one or both of which all our structural material must pass during the processes of manufacture, it is our duty to give this fact due weight before selecting the higher grades solely from the increased tensile strength expected.

While I believe we should give preference to the soft or mild steels for such structures as boilers, ships and bridges, I should adopt high steel for guns.

Where we must have the highest resisting power with a minimum weight of material, we must use high steel, and are justified in the additional care and expense by the necessities of the case. In guns we want a high elastic limit and have need of only a moderate ductility. I believe a steel-cast gun can be made which can compete successfully with any form of built-up guns; but I fully realize that there is a costly experimental stage before success is reached. Having faith in our ability to improve upon the best European practice, I would like to see our manufacturers given a fair chance. They should not be expected to enter upon the expense and study without a proper reward in expectation.

Let the Government say to the manufacturers of this country: "If you will give us a certain number of guns capable of standing certain requirements, fully equal to the best guns now made, when compared with due regard to cost and weight, we will order so many guns from you."

Our manufacturers will then have a proper incentive to prepare for such work. The Government can thus get the advantage of the fullest and freest competition without detriment to the results.

I believe Congress has appropriated $24 000 for making three cast guns, but certainly this was not intended in good faith, for who could afford to make the preliminary expenditures and investigations and test the guns for such a sum alone, without any positive promise to keep the plant in reasonable operation upon the guns fulfilling all the requirements?

I do not believe the metal in a built-up gun can be put in that perfect condition which theory requires in order to get the expected result.

In my opinion there is something more to be taken into the account than solely the hoop tension. I cannot believe that a series of concentric rings will resist the radical forces of expansion as well as the same amount of solid metal.

On expanding a circular ring of metal, the metal must flow on certain geometric curves similar to the flow lines produced by punching a piece of steel. These lines, as shown by me in a paper read before the Society,* will be approximately at 45 degrees to the lines of the applied force, which in this case will be radical.

These flow lines measure the resisting power of the material, and being continuous in the solid gun, would lead me to expect a greater resisting power than with the same material in successive rings. This is more a matter of faith than proof, I acknowledge.

Mr. Emery—If I take two bars, 50 diameters in length, or a single bar 50 diameters in length, the single bar having exactly the same quality as the two bars, and break them purely with tension, which will break the easiest, the one or the two?

Mr. Cooper—The one.

Mr. Emery—My own belief is that as regards the two and the one the strength is identical.

Mr. Cooper—No, because in the smaller bar the metal flows more uniformly than it will in the large bar, and hence the average strength of all the fibers will be greater.

Mr. Emery—They should be of the same quality.

William Metcalf, M. Am. Soc. C. E.—I close the discussion with a feeling of disappointment that the subject of steel has been so largely overshadowed by the gun question.

In the latter I have no interest beyond that of any citizen, and if I had known that our Ordnance Departments had advanced so far, and had decided upon their plans so fully, it is more than probable that I would not have said a word on the subject publicly. But when Ordnance Commissions and Special Committees of Congress were traveling over the country seeking information, and were scattering their circulars of questions broadcast, was it not natural to assume that some one wanted to know something ?

When, too, these same commissions and committees buried their work and their information in Government volumes not in general circulation, and allowed the daily press, uncontradicted, to assert for them over and over again that neither steel nor guns could be made in America at all comparable to what could be bought in Europe, was it not natural for a full-blooded Yankee to rise in wrath and attack them ?

Mr. Dorsey then, first, and myself, later, have done a good work in drawing the officers out of their shells. They have proven themselves

*Observations on the Stresses developed in Metallic Bars by Applied Forces. By Theodore Cooper, M. Am. Soc. C. E. Transactions, Vol. VII, p. 174, July 1878.

to be hard-working, painstaking, able gentlemen, genial, courteous, and well fitted to maintain their position.

They have shown us that they have designed and made the best moderate sized steel guns in the world, and they have expressed their satisfaction with American steel-makers.

That is enough; all hail to them! From this time out I shall shout: Give our Ordnance Corps all the money they need, and I shall stick quietly to my opinion that the best way to make a BIG gun is to cast it and treat it by heat alone.

Returning to the subject of steel, Mr. Coffin's contribution is the work clearly of a master mind, and the study of it has given me much pleasure and information.

He first informs us that guns are not annealed after they are hardened in oil, but that they are tempered.

That is just what I supposed, and my criticism of the specifications stands good. It cannot be necessary to remind Mr. Coffin that the words "annealed" and "tempered" are technical and convey exact meanings; he certainly would never order a piece of steel to be annealed when he wanted it tempered.

Mr. Coffin's discussion of Brinnell's conclusions and of his own admirable experiments is almost too technical for a general reader, yet it seems to call for some answer.

Assuming them to be correct, we have the fact that carbon exists in two forms in steel, the "cement" and the "hardening" form; that at a fixed temperature for all grades of steel the change occurs; below that temperature it is "cement," above that temperature it is "hardening."

This theory seems to involve too the other, that in hardened steel we have a definite carbide of iron, and in non-hardened steel the carbide does not exist, although I believe that Mr. Coffin does not make this statement himself.

Mr. Coffin shows farther, by his acid tests, that there are ten forms or conditions of carbon in steel; truly this is a remarkable element, and engineers may well stand aghast at the prospect.

I will not say that there are not cement, hardening, and ten other forms of carbon in steel, for I do not know the contrary. I will believe in these twelve forms when I see them separated and made palpable.

I will not say that there is no definite carbide of iron, but if there be, then by the law of combinations we ought to be able to convert any piece of iron into complete carbide containing something like seven or eight per cent. of carbon; but we cannot put anything like that amount of carbon into iron unless we force it in by the use of manganese.

If there is a carbide of iron, why is it only a little bit in a great matrix of iron?

If a little of the iron will form carbide, why will it not all unite with carbon?

I will ask engineers to set against these remarkable theories Professor Langley's theory, in which I am a firm believer.

Carbon is dissolved in iron; a saturated solution seems to be something under three per cent. of carbon when there is no other element like manganese present to increase the power of solution.

It is simple enough to be at least credible.

Next I offer the theory that hardening is caused by tension; that tempering and annealing are reductions of the tension by heat.

If hardening is a mere change of condition of carbon, why should a piece ever crack when hardened? Let any person take a round bar of steel of any carbon above .60 and overheat it just a little too hot, and with perfect uniformity of temperature, and quench it, and it will split up the middle. A bar of any section will do the same, but the round section splits the easiest. It is no trouble to split a round bar; it is more difficult not to do it. Heat relieves the tension and softens the steel; a given temperature relieves a certain amount of tension and no more; therefore, as Mr. Coffin says truly, if the first annealing does not give you softness enough, no number of repeated heatings to the same temperature will give any greater softness.

It is a happy fact. Given, say a car-spring, it has a certain elasticity, temper, given to it by quenching. It may be frozen to 40 degrees below zero, and heated to 120 or 130 degrees in the sun, thousands upon thousands of times, yet it retains its tension and carries its load for many years. Consider too the permanency and exactness of the springs of watches and clocks. Of course this fact does not militate against the "cement" and "hardening carbon" theory, nor do I understand Mr. Coffin to accept this theory without reserve.

But, under the dual carbon theory, why does a piece of hardened steel lose any of its hardness until the change to "cement" occurs? We all know that hardened steel does grow softer for every single degree of heat to which it is subjected; we can understand this if we accept the tension theory; but it does not seem so plain with the other theory. Professor Langley proved conclusively that a hardening effect was produced when he boiled steel in water, 212 degrees, and quenched it in water at 60 degrees, and he gives us the measure of the hardening effect; surely these temperatures are away inside of the "cement" carbon limit which Mr. Coffin states to be between dark-red and red, in the dark.

Experience teaches that in tool steel at least, we know of no temperature above that of the atmosphere at the time, from which, if steel be suddenly quenched it will not show that it is hardened to an amount due to the temperature applied.

We cannot recogr [illegible] any [illegible] temperature where hardening begins. On the contrary, it is [illegible] funct[illegible] f the temperature and has some value for every degree of tempera[illegible] Also there is a regular increase of softness for every degree of heat that is added, until steel becomes liquid.

Mr. Coffin's explanation of Mr. Brinnell's chart is interesting, and would be equally instructive to engineers if only their eyes were trained to the study of fractures, so that they could see mentally the structures meant by "fine crystalline," "flaky crystalline," "coarse crystalline."

The results given, as far as they can be explained in words, agree with our shop experience, and they indicate clearly what has long been an axiom with us, *i. e.*, that a fracture of steel always indicates the highest temperature to which the steel was last subjected, no matter how it may have been cooled, provided it had not been hammered or rolled, or otherwise worked mechanically.

This fact is of inestimable value to a steel-maker, and it would be of great value to engineers if they would study it.

I would not know how to try to describe the fractures intelligibly without samples, but the mode of learning is simple. Go to the scrap heap; select a lot of pieces (if of known carbon so much the better) and heat them to various temperatures and in varying times, and cool them in different ways. Then break the pieces and study the fractures, singly and in groups.

This costs nothing and is a good amusement for leisure hours. I have known men to become complete enthusiasts over old scrap heaps. There is plenty of scrap about every establishment. One of the most interesting to observe is the fact that all steel, from "dead soft" to the highest carbon, will go through similar changes of structure.

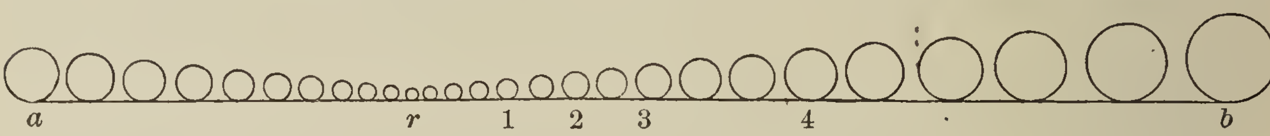
The fractures are not alike for the various carbons, but they are so similar that they will soon convince any one that they all obey the same law.

It is not to be supposed when we say that steel always records its highest temperature, no matter how it may be cooled, that a bar quenched, and another cooled in air, and a third cooled in a bed of warm ashes, from the same temperature, will have the same fractures; they will be markedly different. But a thousand bars, or a million bars, of the same composition, heated to the same degree, and cooled in the same way, will have identical fractures; so that the lesson learned once is learned for all time.

Mr. Coffin says all steels refine at the same temperature. This is so entirely contrary to our experience, that I cannot believe Mr. Coffin can have examined the matter for himself. We recognize a distinctly different temperature for refining for at least fifteen different tempers of steel, and we act upon this knowledge every day. The range is not great, but it is palpable. In steels of gre[illegible] difference of carbon the range is considerable; for instance, the [illegible] at which .30 carbon steel will refine will almost certainly [illegible] a piece of 1.50 carbon steel every time it is applied.

Perhaps it will be better to explain this refining fully.

Take any ordinary bar of steel and break it and let the size of the grain be represented by a circle, the first one in the row below; then heat the bar to different degrees and quench it from each; in this way for every change of temperature that is visible to the eye there will be a change in the size of the grain or in the texture that is equally visible to the eye. If the heats range from the lowest red, or possibly below red, to the highest attainable, say scintillating, and the corners melting, our row of circles representing coarseness of texture will be like this



b being more or less greater than *a*, owing to the condition in which the workman left the bar, fine or coarse.

b will always scratch glass if the steel is high, and it will always be about as brittle as glass.

There are some interesting facts connected with that row of circles.

Perhaps the most interesting, because it is the most surprising, is the fact that the specific gravities decrease regularly from *a* to *b*. It would seem as if they ought to increase from *a* to *r* and then to decrease from *r* to *b*, but they don't; they decrease from *a* to *b* and much more rapidly and to a much greater amount in high carbon than in low carbon steels. This is the "mystery" of cracking and brittleness in high steel—the change of volume is greater and the tension is greater.

How or why *r* is of greater volume than its neighbors to the left, which have a coarser texture than *r*, is an unaccountable mystery to me.

At one time I seized upon the "cement" carbon and "hardening" carbon, and the "critical" temperature theories for an explanation, but they will not do, first, because all of the circles from *a* to *r* are harder than *a*, and each is harder than its neighbor to the left; therefore *r* cannot be the critical temperature where carbon changes form, because the circles below *r* are hardened.

Second.—Because *r* has no uniform position in the row, it will shift towards *a* in high carbon steels and towards *b* in low carbon steels.

Another interesting fact about the circles is that if you take a piece made up of circles *b* and heat it to the temperature of circles 4 and quench it, the fracture will be circles 4; if you heat it to 3 you will get circles 3; 2 will give circles 2; and *r* will give circles *r*, and so on.

This is called "restoring" steel; and it requires no nostrums but fire and water. Now to apply the row of circles to engineers' uses.

First.—Experience teaches that *r* is the best condition to get any steel into. But as *r* moves away to the right in low steels (structural steels) it is obvious that mild steels will bear a high temperature without "raising the grain," therefore there is not much danger of injuring mild steel by moderately high heat if ordinary care be used.

But some care must be had, for any steel can be ruined.

Second.—The "restoring" property makes it possible to anneal even circle *b* back to circle *r*. This shows the value and importance of good annealing. But I do not advise any one to make a practice of getting *b* circles in steel except experimentally; the operation is one of disintegration and it injures steel permanently.

Third—And possibly the most important of all, is that the difference in specific gravity (the difference in volume as represented by specific gravity between *a* and *b*) is in low steels much less than in high steels; as for instance, for 40-carbon the difference between *a* and *b* is only one-fifth of the difference shown in 120-carbon steel.

This is the reason that mild steel is not easily ruptured by a high heat; it is also the reason that a mild steel may be rehardened many more times than a high steel without breaking. It shows too, if the tension theory be correct, that it is difficult to set up a dangerous strain in mild steel, so that engineers need have little fear about using it.

Steel that has been heated once to *b* may be restored to *r*, and if it is high steel it will be hard and will hold a fairly good edge; but steel that has been heated to only an orange color and is left soaking in the fire for hours until it is thoroughly over-annealed, will not harden properly. If it hardens at all it will not temper true, and it will be crumbly or soft and will not hold an edge.

I have known "dead soft" steel to be over-annealed so that no amount of subsequent annealing and coaxing would make it anything but rotten and worthless. I believe this comes more from soaking heat than quick over-heating. I thought at one time that it was caused by a permanent change in the condition of the carbon in the steel; that it became like an exceedingly fine cast-iron; but this will not hold, because cast-iron will harden and temper if it does not contain too much silicon; neither does it accord with either the "cement and hardening" carbon theory, or the tension theory. Therefore, as I said before, I suspect that the change is effected by the absorption of some gas.

To anneal properly, then, heat to the lowest heat that will give the degree of softness required and allow a short time for the particles to arrange themselves; then cool as slowly as possible, the slower the cooling the greater the softness that is retained. Heat produces the softness, slow cooling retains it.

I have stated that all steel will harden if it contains any carbon at all, and also that even cast-iron will harden and anneal; this statement includes the so-called "self-hardening" steels.

It is easy to anneal the hardest self hardening steel so that it may be drilled, filed, and cut, like any other steel, and then to harden it. The best quenching medium for "self-hardened" steel is air; water or oil will crack it almost every time.

One more point. Mr. A. M. Howe stated recently, on the authority of Bessemer and Chernoff, that the size of grain depended on the rate of cooling. That slow cooling always produced coarse crystals, and quick cooling produced fine crystals. Their experiments bear them out as far as they went.

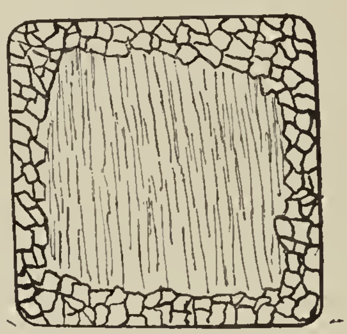
It is hazardous to differ from such eminent authorities; but I do differ from them, because experience shows the general statement to be misleading. It requires a mass of steel to show their experiments; and then it shows another thing, it shows that the difference is due to the tension caused by quick cooling, and that it will not hold in small sections.

For instance, if a piece of steel an eighth of an inch thick, or even a quarter of an inch thick, be heated to its temperature *r*, it matters little how it is cooled, the grain will be fine, and the difference between a quenched piece and a slowly cooled piece will be hardly appreciable.

If a ¼-inch piece be heated to *a b* circle condition, and one end be quenched and the other end be allowed to cool slowly, the quenched end will have rather the larger grain.

It is by this that we detect the character of the temperer's work ; of course a temperer never overheats steel, but then the fracture cannot lie.

If a piece of a bar of steel one and a half inches square be heated to circles *b*, and one end be quenched and the other end cooled slowly, we have a different state of affairs. The quenched end will look like this:



The big crystals around the edge from ⅛ to ¼-inch deep will be nearly as big as peas, some of them, some smaller, and the inner part will be what I suppose Mr. Brinnell calls "flaky crystallines," a term that expresses it well.

Some people might call it fine grain, but it isn't.

The big crystals may be picked off with the finger-nail almost, and their corners will scratch glass.

They are represented by circles *b*, and they are the crystals due to that temperature, unaffected by the rate of cooling ; it is obvious that the flaky interior is an effect due to the binding of the outside rim, and not to the sudden cooling. The outside edge represents the sudden cooling.

In the ¼-inch b r mentioned, it is all outside edge crystals, because the sudden c ling strikes through. The slowly-cooled end of the

$1\frac{1}{2}$-inch bar will have what is called a "coarse amorphous" grain; it will be coarser than the center of the quenched end, but not near as coarse as the outside coat of crystals, which alone are due to sudden cooling.

Professor Langley's specific gravity experiments have let in a flood of light on the physics of steel, and if they have not told us why steel hardens, and what carbon has to do with it, and how many forms carbon is capable of assuming; they have told us why steel cracks, and why high steel is brittle; and they have eliminated the words "mystery" and "treacherous" from our steel vocabulary.

Steel is not treacherous. "True as steel" is as sound a metaphor to-day as it ever was. My friend Mr. Gottlieb need not be afraid of steel if he will get the right kind of steel and then use it right.

Perhaps I ought to say something about the chemistry of steel in answer to some remarks in the discussion, but it would be useless. I do not know the proper relation between the chemistry and physics of steel, and I know of but one person who does know it, and that great "standard" bearer is so entirely alone in his glory that I would not dare to approach him.

Let those who are interested study Mr. A. M. Howe's valuable and interesting work that is being published now in the *Engineering and Mining Journal.* Mr. Howe discusses some two thousand tests by the light of their chemistry, and he finds the fog so thick that he lays down his pen in weariness. If so bright an investigator as Mr. Howe cannot elucidate the subject, it were useless for me to try.