Discussion.

Mr. AM ENDE desired to add a few words of explanation. With regard to the diagrams of very large girder bridges, he wished to lay more stress upon their meaning or import. It was intended chiefly to derive from them certain coefficients relating to the wind structure and the secondary bracing; also to show that the construction of such large bridges was feasible. It was, however, a different question whether those large girder bridges should be recommended for practical application. For himself, he thought that their application must be very limited. If, in practice, very large spans were required, the foundations would generally, not always, afford inclined resistances, and such inclined resistances would be mostly a source of economy and rigidity to the structure; but structures acted upon by inclined supports were not girders—they were arches; and he would go so far as to say, in many cases where engineers now designed and constructed girder bridges, especially with high piers, arch bridges might be applied with greater advantage. He also wished to refer to Fig. 8. The figures indicated theoretically the weights of girders, and not practically the weights of bridges; namely, the coefficient for the web, and the items for the wind bracing and the secondary bracing, were not accounted for in those figures; their weights increased with the rising of the point characteristic of the form of the girder; at the same time the weight of the flanges diminished. The influence of the coefficients for those other items would, consequently, be to lower the range of the minima of weights, and make them approach nearer to the form he had adopted. It was therefore possible that although that form did not represent theoretically the most economical girder, it might practically represent the most economical bridge. He desired also to refer to a formula for girder bridges with parallel flanges and diagonal ties and struts at an angle of 45°, as follows:— Mr. am Ende.

$$Q = \frac{1}{\left(\frac{s}{0\cdot 00213} - \frac{n\,L}{2}\right)D - \frac{D^2}{2} - \frac{L^2}{6}}\Big\{(P+M)\frac{L^2}{6} +$$

$$+ (P+M)\,D^2 + \left(P + \frac{7}{6}M\right)\frac{L\,D}{2}\,n +$$

$$+ W\,(0\cdot 2\,D + 18)\left[\frac{0\cdot 75}{(0\cdot 3f - s)}\frac{L}{B}\left(\frac{9}{48}\frac{L}{D} + \frac{1}{2}\frac{D}{L} + \frac{1}{8}\right) + \frac{1}{2}\frac{L}{D} + \frac{5}{4}\right]D^2 +$$

Mr. am Ende.

$$+\frac{s.\ 0\cdot0073}{0\cdot00213}\mathrm{D}\sqrt{(\mathrm{D}-10)(\mathrm{B}-6)}\Big\}$$

$$n = m + (m-1)\frac{\mathrm{Q}+\mathrm{P}}{\mathrm{M}}\left(1-\sqrt{\frac{\mathrm{M}}{\mathrm{Q}+\mathrm{P}}+1}\right)$$

$$n = \text{from } 1\cdot5 \text{ to } 1\cdot25$$

$$\text{for } \frac{\mathrm{Q}+\mathrm{P}}{\mathrm{M}} = \quad ,, \quad 0 \quad ,, \quad \infty$$

$$\left(\frac{s}{0\cdot00213}-\frac{n\,\mathrm{L}}{2}\right)\mathrm{D}-\frac{\mathrm{D}^2}{2}-\frac{\mathrm{L}^2}{6}=0$$

$$\text{for}\begin{cases} s = 8 \\ n = 1\cdot25 \end{cases} \qquad \mathrm{L} = 3{,}130;\ \mathrm{D} = 1{,}800$$

The construction of this formula was much on the same principle as that for the parabolic girder bridges; and in the same manner formulæ might be constructed for the parabolic fish-girder, for the Schwedler girder, for the Pauli girder, and for other forms, which were theoretically defined, and also for the arch; but inasmuch as the theory of strains in arches was more intricate than the theory of strains in girders, the task of constructing such a generalising formula would be more difficult, although not impossible. He might mention that the subject which he had brought before the Institution had been treated, among others, by Mr. Baker,[1] by Dr. Winkler,[2] of Vienna, and also, he thought, by Mr. Barlow. He believed the two former had treated girder bridges as girders. The items for wind structure and for secondary bracing were expressed, together with the coefficient for the web and the coefficients for the connections, by two or three factors derived from existing bridges. The objection to that method, and the advantage of separating the coefficients as much as possible, had been sufficiently stated in the Paper. He did not mention the matter with any desire to depreciate the works of the authors to whom he had referred, which, in some respects, were more thorough and complete than his own, but in order to show that he was not the first who had treated the subject, and at the same time to justify his bringing it before the Institution in a different form.

Mr. Thomas. Mr. W. H. THOMAS asked the Author why he did not make the girder approximate more closely to a true parabola. As he understood from the Paper, the subsidiary verticals were struts, and

[1] "Long-span Railway Bridges." By B. Baker.

[2] "Vortrage uber Brückenbau." Heft II. Von Dr. E. Winkler.

were interposed with a view to supporting the upper members. Mr. Thomas. It was clear that, if the upper member took the form of an arch, it would be relieving the verticals to a certain extent of their work, besides approximating more truly to the form of a parabola, which it was well known was the true form for such a girder.

Mr. W. Shelford had lately had occasion to visit Germany, Mr. Shelford. for the purpose chiefly of seeing the modern practice of German engineers in the details of the construction of girder bridges. The Author had stated that arch bridges were the best for large spans; and he might mention that, of all the bridges of large span built over the Rhine, the most modern—one built in 1879—was an arch bridge, and he believed it was the result of the experience of German engineers, who, he thought, were the most scientific bridge-builders in Europe. The bridge was on the new route from Berlin to Metz, which embraced the construction of many bridges, not only over the Rhine but over the Moselle—up the valley of the Moselle and the valley of the Lahn. He had noticed that the bridges were made very light, and that the use of plates had been dispensed with as much as possible. Angle-irons had been used largely in the flanges; the system of smaller divisions had been deliberately abandoned, and the larger divisions which the Author recommended, and in many cases the form of girder used in his designs had been adopted. So much was that the case, that where a bridge, as at Nassau, was constructed a few years ago to carry one line of rails, and another had lately been constructed to carry the other line, the old type of bridge, a lattice girder of 200-feet span, had been abandoned, and an entirely different type erected close alongside. He had not observed in Germany that such depths as those recommended by the Author, and shown in Appendix IV. to be the most economical, were adopted; and he should like to ask the Author whether he thought that English and German engineers were in the habit of making their girders too shallow.

Mr. E. A. Cowper would first briefly allude to the subject of Mr. Cowper. the true form for an arch, as every girder had, so to speak, an arch within itself, the top flange connected, as it must be at the ends, by the bottom flange, constituting a flat arch in most cases, and an arch in any case. The arch contained in a long girder was generally too light and too flexible to keep its shape without bracing or plating, to act the part of diagonals, and to distribute the strain of a passing load and prevent distortion of the arch.

He thought it would be well if more consideration were given to the use of the catenary curve in calculations and designs

Mr. Cowper. for arches, and he believed the architect would benefit by such attention quite as much as the engineer, as it was certain that the catenary curve, or a distorted catenary, according to the loading, was the true theoretical form for an arch. He was well aware of the trouble of calculating the form of catenaries, and particularly distorted catenaries; and he would not propose in every case that that amount of work should be undertaken, particularly when all that was needed, in point of accuracy, could be attained with very little trouble, and what was more, in such a simple way that the designer could see that he was right, without having any misgivings as to the possibility of a slip in some of the long and somewhat abstruse calculations that were needed to obtain the several results by figures. The moving diagram or model, shown by Fig. 1, was a chain composed of pieces of wood jointed together loosely by wood screws. It was hung from a flat bar of wood, and after jarring it a little to enable it to settle into a true catenary curve, the flat bar of wood to which it was hung might be turned 180°, and the jointed pieces of wood would then stand up as a true catenary arch (Fig. 2). Again, if one side was loaded with pieces of lead it would form a distorted catenary curve (Fig. 3); and if turned up in like manner, it would stand up as a distorted catenary arch (Fig. 4). Yet, again, Fig. 5 might represent a catenary arch or an iron roof, say of corrugated iron, which, being very flexible when applied to large spans, would become a distorted catenary with a slight wind on one side; but if formed with two diagonals and flatter curves, as in Fig. 6, would become at once exceedingly rigid and stiff, and resist a side wind, as the extra thrust of the curves would be taken by the diagonals, and then one side could resist a considerable extra pressure given by the other side.

He wished to say a few words on the broad distinction that existed between girders, arches, and suspension bridges. In girders and arches the chief member was in a state of severe compression, and in very large spans it needed great assistance, or side support, to enable it to keep in line, and not fail from bending sideways or upwards. He could easily conceive that a span, far short of 1,500 feet, would require so much in the way of struts and ties for bracing, and for resisting the side pressure of the wind, as to limit the span greatly, from the mere fact of carrying so much dead-weight, which did not add strength directly to the main arch for carrying weight when equally and fully loaded. A very long curved narrow rib, was like a tall and thin column, and was liable to give way by bending, and so a very high arched

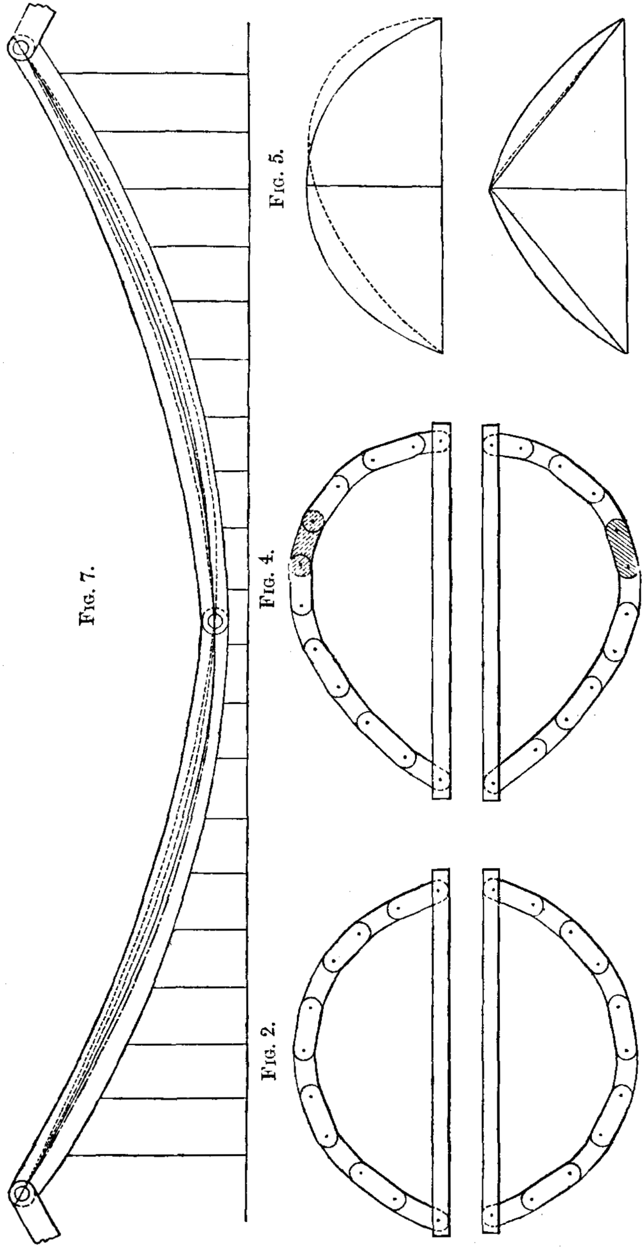


Fig. 7. Fig. 5. Fig. 6. Fig. 4. Fig 3. Fig. 2. Fig. 1.

Mr. Cowper. rib of large span was likewise liable to give way by bending. Now this meant that the metal under such circumstances would carry but a small pressure per square inch in compression; in other words, do very little duty. Besides the diagonals in a girder, the heavy bottom flange had to be taken into account, and had to be carried as part of the weight of the bridge. But whilst the best material would do but little duty in compression, under such circumstances, it would do excellent duty in tension, as the strain of tension helped not only to keep the material to its true line, but to bring it back if it had been moved out of it. He therefore held that metal, say mild steel, put in the form of a catenary curve or suspension bridge, being in tension, was capable of forming a much larger span of bridge than any construction of arched ribs or girders, in which the metal was in the state of compression. He was well aware he should be told that a suspension bridge was affected too much by a passing load, to permit of its being used as a railway bridge; but if he showed how the metal could be disposed, so that there should always be a line of metal to take the line of strain, there would result no more motion, on the passing of a load, than occurred in a girder bridge; that was to say, no more motion than just enough to put one part in strain in place of another part.

Fig. 7 showed a paper diagram to illustrate his meaning. It would be seen that the two halves of the suspension bridge looked like two festoons. This was from the fact of their containing all the distorted catenary curves that could be produced by a passing load; so that whether the load was relieved from one side or the other, the bridge remained practically without motion. The various dotted lines within the depth of the curved ribs were the several lines of strain, caused respectively by half the bridge being loaded, and the other half unloaded; by the centre being loaded, and the ends unloaded; and by the ends being loaded, and the centre unloaded. Thus there was always a line of metal to receive a line of strain—the curved ribs would, in fact, be made with heavy flanges at the top and bottom, so that the strains would at all times be taken by both, though often unequally, according to whether the line of strain approached the top or the bottom of the rib. Now such a bridge did not require the whole of the ribs to be plated, as it could well be made open, like a lattice bridge, with straight and diagonal ties or struts. These ties and struts did form a certain amount of load upon the bridge, and were not in use when the bridge was fully loaded; but they were very short, and had little to do compared with the ties and struts in a girder

Mr. Cowper.

or large arched bridge. The extreme depth of the curved ribs, or festoons, in the middle was far less with very large spans than would be at first imagined, as the weight of the bridge, being great, the weight of a moving load did not distort the line of strain much; thus for a 1,500-feet span, assuming the weight of the bridge to be four times the moving load, the extreme depth between the lines of strain was only 23 feet, so that two plated chains or ribs could be braced together with struts and diagonals of moderate length to form one curved rib or festoon, which would practically not be deflected or moved by the passing of a train. The joints in the centre and over the piers were necessary, to allow of the expansion and contraction of the metal by changes of temperature in very large spans, the only effect being that the bridge rose or fell a little in the centre. This arrangement of a pair of curved ribs, to form a rigid suspension bridge or jointed bridge, was submitted to the Institution of Mechanical Engineers[1] in 1847; and for moderate spans he proposed to do without the joint, and have a plain ribbon or rib, as shown in the engraving published at the time. This would do perfectly well if suspended between two rocks, like an arched rib springing between two piers; but with long retaining chains to the anchorage the jointed form was the best. One great advantage of this bridge was, that it could be erected over an abyss or sea-way, without more scaffolding than a few wire ropes with planks on them, to enable the men to joint together, first one chain of links with pins, and afterwards each plate in its proper place; in this way the cost of fixing would be reduced to a minimum, which was a matter of great importance in bridging over large openings.

Sir William Thomson.

Sir WILLIAM THOMSON said the plan of Mr. Cowper appeared to him an admirable one. It gave the firmness of a girder bridge, together with the tensional condition of a suspension bridge, in which every long bar was under tension and no long one under compression. He had for some time known what he thought was the same, or nearly the same invention, as contrived by his brother Professor James Thomson. His brother had found almost exactly the same contrivances as he had devised, brought out by someone else, while he was engaged on the subject; and now it appeared by the date, 1847, given by Mr. Cowper as that of his invention that the priority would belong to Mr. Cowper, in so far at least as the inventions were alike. The method appeared to be one of the greatest value;

[1] *Vide* Proceedings, 1847. "Description of an improved Suspension Bridge, *for carrying a railway, and for other purposes.*"

Sir William Thomson. and, as an amateur in such matters, he had often wondered that it had not come into use. He should like to know whether there was any particular reason against its adoption. It appeared important that firmness should be attained, when possible, without the introduction of supernumerary ties or struts, or tie-struts; and that the stresses in the bars, or forces transmitted longitudinally by them, should be determinate and not rendered indeterminate by changes of temperature. He ventured to make those remarks with a view of eliciting from others a practical consideration of a subject which appeared to be one of great importance.[1]

Mr. Bender. Mr. CHARLES BENDER said if he ventured to remark upon the Paper under discussion, it was because for fourteen years he had been occupied exclusively with the construction of iron bridges, and having been educated in Europe, and having lived about twelve years in the United States, he had had excellent opportunities of making observations. He thought the Author had given the Institution an excellent Paper containing many valuable suggestions. The gist of it consisted of two principles—few panels and great depth. Those two principles had been adopted in the United States for some time. In England for very large spans panels of considerable length had been used, as in the Chepstow bridge; and of late for smaller structures panels of greater length had been generally used in Europe. He had, in 1872, used 19-feet panels for a bridge of 152-feet span, and he had lately constructed a bridge of 320-feet span with panels of 32 feet. The economy obtained by long panels was very great. The depth of one-tenth of the length of span in England was adopted because the first wrought-iron bridges built were plate girders, and for such girders one-tenth of the span was a suitable height for moderate spans. But in making the height of the bridge too great, risk was incurred of reducing the lateral stability considerably; the width had to be greater, and with greater width more extensive masonry was required. Making the panels too long rendered necessary an increase of the material to form the platform. To combine

[1] Sir William Thomson has since obtained from his brother some particulars as to dates, &c., from which the following are offered here for reference.

Professor James Thomson worked out on paper, and in a large model, before February 1861, designs for a rigid suspension bridge, self-adjusting as to effects of temperature. He then met with almost exactly the same in an article in "The Civil Engineer and Architect's Journal" for January 1861, vol. xxiv., p. 6, entitled "On the Construction of a Rigid Suspension Bridge." By C. Köpcke of Geestemünde, Hanover. Later an account is given in the Transactions of the Royal Scottish Society of Arts, vol. ix., p. 185, of substantially the same invention as brought out by Mr. T. Claxton Fidler, M. Inst. C.E.

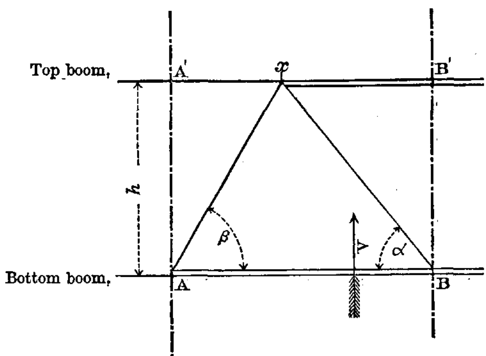
those conditions was a difficult matter, and he had never ventured to construct a formula for that purpose. Though preparing estimates every day he found it necessary to lay down the results of the estimates upon paper in the form of a curve, and he had used such curves since 1872. Most of the American bridge-makers constructed such curves, the specifications changing every day. For very large and unusual structures he admitted that an approximate formula might be used, but in the general run of estimates for bridges the estimator, who was already acquainted to a great extent with the weights necessary, did not require to make a formula. In order to test the formula given by the Author he had taken a 250-feet span, and he found that the weight calculated according to that formula was correct as far as his own details were concerned. Instead of using panels of such great length as the Author had chosen, he found that by adopting thirteen panels instead of eight, and making the depth only one-seventh, he obtained 0·79 instead of 0·89 ton per lineal foot. With booms parallel for one-fifth of the length of the span he obtained 0·66 ton per lineal foot of the bridge, which was less than the Author obtained for even one-fourth; so that for parallel booms he thought that about one-fifth would be the most economical depth. For small spans he had used one-fifth, but for large spans he would not recommend it. For a span of 100 feet it was necessary, if the platform was laid on the lower flanges of the main girders, to have a depth of one-fifth, so as to allow railway carriages to pass through. For a span of 250 feet he would not use a depth so great as the Author had chosen, 41 feet; he would hardly make it more than 35 feet. In Germany great attention had been paid to curved top-booms, and very different forms of girder had been tried. He believed that with parallel booms and vertical posts as much economy could be gained as that obtained by the Author, if not greater. He did not think that the number of panels should be limited to eight. It would lead to a great many additional constructions—sub-constructions which were really bridges by themselves—for bridges, of 1,500-feet span, 200 feet between the panels, which was going a little too far. The coefficients used by the Author in calculating the strength of the members were those generally adopted; but going to original experiments on a large scale, it would be found that the mere deduction of the rivet-holes was not sufficient to obtain the same strength per square inch of area as given by experiments on integer bars. Previous to the construction of the Britannia bridge a great many experiments had been made with riveted tubes; Mr. Bender.

Mr. Bender. also made during the sittings of the Commissioners appointed to inquire into the application of iron to railway structures in 1849. Experiments by Mr. Brunel with a girder 60 feet long, and others by Morin in France, all pointed to the fact that a great deal more, namely, up to 30 and 40 per cent. was lost besides the section of rivet-holes. Calculating, from the treatise of Mr. Edwin Clark on the Britannia bridge, the strains per square inch of net area under which the different riveted constructions broke, it would be found that the gain was only 35,000 lbs. per square inch on the average in strength of material, and the experiments of Brunel and Morin realised not over 30,000 lbs. per square inch of net area. Last year he had obtained, at the Exhibition of Dusseldorf, some facts with regard to riveted steel girders, of which, so far as he had observed, no notice had been taken in England. Eighteen girders, three of each kind of material, were tested to rupture by the Dutch Government. There were five different classes of steel, and one class of iron. The first class of steel had nearly a strength of 54 tons per square inch, and the others $42\frac{1}{2}$, 38, 41, 30·8 tons, the iron having a strength of 24·8 tons. The iron beam broke under 22·8 tons, or $92\frac{1}{2}$ per cent. of the net area was utilised. The holes were all drilled, and they matched excellently. There was a loss in the iron specimens of from 12 to 8 per cent. of the calculated strength; but how the strength was calculated he could not say, because all the data were not given. The steel gave only 64 per cent. of the strength of the net area, showing a loss of 36 per cent. of the calculated strength; and some of the results were actually less than the strength of the iron girder. There was steel, for instance, which stood only 15·2 tons per square inch. Steel No. 5, which had a strength of 30·8 tons per square inch, was not stronger than the iron girder; and steel, of 54 tons per square inch, realised only 81 per cent.

As he was not aware of any tensile experiments with riveted angles—angles riveted into plates through one leg only, a construction used in bridges every day—he had got Mr. Samuel Shreve, of the Edgemoor Iron Works, near Philadelphia, to make some experiments which gave the following results:—The angle-iron by itself, on the average of two experiments, stood a strain of 50,600 lbs. per square inch. But if riveted with seven $\frac{3}{4}$-inch rivets in one row at each end to two plates, and then tested in the same machine, it broke under 31,700 lbs. per square inch, and the rupture took place at the first rivet joining the angle-iron to the plate. Another angle-iron, of similar material, had the second leg bent up at the ends and clamped into the machine; it broke under 41,200 lbs. per square inch, near the clamps. The loss of strength, above the loss by rivet holes, amounted to 37 per cent., and in the last experiment to 18 per cent. of the strength of the iron. It was well known that if a hole was made in a bar, the bar would stand a great deal less impact than if it were intact, and had originally only the same net area as after the hole was made. The same thing was true of riveted structures. A riveted bar, if acted upon by impact, must break sooner than if the bar at the point of junction were sufficiently enlarged. Eye-bars had been made in the United States of steel, and he might be permitted to refer to some experiments in the construction of a bridge over the Missouri river at Plattsmouth. The eye-bars were of steel, which in small sections stood a strain of 80,000 lbs. per square inch. As was also the case with iron eye-bars of great dimensions, they did not stand exactly the same strain as the material itself, if rolled into small sections. The bars of 6 inches $\times$ $1\frac{1}{4}$ inch and 6 inches $\times$ $1\frac{5}{8}$ inch stood strains from 66,000 to 73,000 lbs. per square inch. The eye-bars tested for the bridges of the Cincinnati Southern railroad gave, on an average, exactly the strength of the iron itself, or 50,400 lbs. per square inch. He could recommend them as being perfectly safe if properly designed and manufactured. With regard to the connection by pins, a great deal depended upon the system chosen for the web-members. If a girder were chosen in which the strains reversed from tension to pressure, a hammering action might take place, if the bridge were very light. In bridges with vertical posts and tensile diagonals that was not the case. He had seen bolts taken from such a bridge built in Chili by Mr. W. W. Evans, M. Inst. C.E., who, in order to examine the result produced on the pins, had some taken out, and though the play in the holes was $\frac{1}{8}$ inch, and the surfaces were rough, there was hardly any perceptible change in the pins, although they had been in use twenty years. The Author had used a rule, which was given in books generally, that 45° represented, under all circumstances, the most economical angle for the diagonals of a bridge. He believed there was a mistake in the calculation of that quantity. In the United States, and also in Europe, vertical posts were generally used, and the Warren girders had been discontinued in the United States. How was it that in America, where so much depended upon the commercial value of a particular design, the competition being so very close, engineers continually used vertical posts and diagonal tension bars, contrary to what was inculcated in books? He thought Fig. 8 would sufficiently explain the matter. Let A B

Mr. Bender. A′ B′ be a panel of a girder with parallel booms, let B x represent a diagonal tension bar, and A x an inclined post, forming with the horizontal the angles α and β respectively. Whatever these angles might be the strains at the imaginary section A A′, and those at the section B B′, did not depend on the web-bracing, but only on the moments of flexure and on the height h between the booms. All changes, however, which occurred between A A′ and B B′ depended on the web-bracing. The tie B x caused a pressure in B′ x, also the inclined post A x added a certain pressure in B′ x, and an equally great tension was caused by A x in the bottom boom-piece A B. Now by multiplying the tension in B x with B x, the pressure in B′ x with B′ x, and so on, and adding the four products together, and finally dividing this sum by A B, the theoretical value of the bracing was obtained. To ascertain its

FIG. 8.



practical value each product was to be multiplied with a certain factor, which factors were changeable with the detailed design, and those taken from an executed example were as follows :—

for	B x	B′ x	A x	A B
factors	a	b	c	d
	1·07	1·50	1·85	1·15

The shearing force being designated by V, the formula for the value of the web-bracing would be found:

$$\text{Value} = \frac{V}{\cot\alpha + \cot\beta} \cdot \left(\frac{a}{\sin^2\alpha} + \frac{b}{\tan^2\alpha} + \frac{b+d}{\tan\alpha \cdot \tan\beta} + \frac{c}{\sin^2\beta} + \frac{d}{\tan^2\beta}\right).$$

For different angles the following results were obtained :— Mr. Bender.

$\alpha =$	$\beta =$	Value.		System.
		Theoretical.	With Coefficients.	
45°	45°	400	557	All diagonals under 45°.
45	90	400	549	Vertical posts.
60	60	364	490	Warren girder.
60	90	462	654	

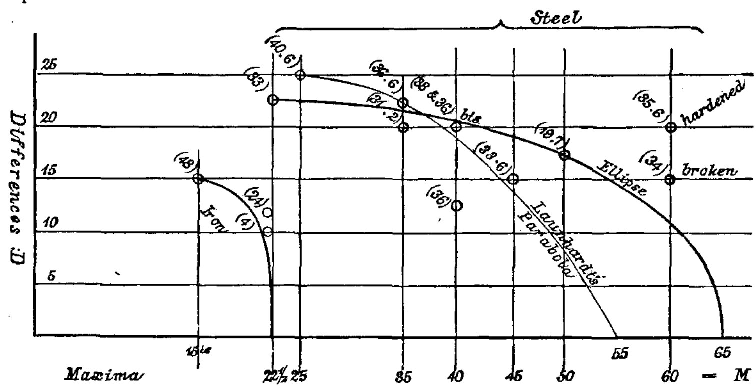
These figures justified the use of vertical posts and tension bars under 45°, or the use of Warren girders, in preference to a system with all diagonals under 45°. The Author had made use of the formula of Professor Weyrauch, who had presented to the Institution a Paper on that formula.[1] As the matter was rather complicated, he might be permitted to say a few words in explanation of its origin. In 1858 Mr. Wöhler, the manager of the locomotive department of a Prussian railway, began some experiments with a view of ascertaining the lifetime of axles and springs, and for that purpose he constructed certain instruments to measure the deflections of the axles in use in railway carriages and engines —carriages with two axles and with three axles—and also to ascertain the deflections of railroad springs. In doing that he made experiments in the shop to discover how many revolutions an axle so used could stand, or how many deflections a railroad spring could sustain, and he continued the experiments from 1858 to 1870, when he was called away on other service. The inquiry was continued for a short time by Professor Spangenberg, but not in a manner to do justice to the subject. The publications issued by Mr. Wöhler were in "Erbkam's Zeitschrift für Bauwesen" for 1858, 1860, 1863, 1866, and 1870. In 1866 he published certain facts which appeared to Mr. Bender so interesting that he had put them on paper to ascertain whether there was a law in the results; but he found that the experiments stopped just where they became interesting in reference to the discovery of the law. The final facts were not sufficient to lay a curve through the points gained. The most interesting experiments were these. Taking a bar of iron or steel, it was bent to a maximum tension

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. lxiii., p. 275.

Mr. Bender. and then relieved of a portion of the strain, and then bent down again—the same being repeated many million times to ascertain how much stress the material could endure. Some of the experiments were made with Krupp steel for the purpose of ascertaining the life of springs. He began, for instance, with, say, steel at 50 tons, and then released the strain entirely and put it on again, and he found that, after a few revolutions, it would break. He then tried releasing the strain, not so much as before, but only perhaps to the extent of 20 tons; then putting it on and relieving it again; and he found that there was a certain limit for each maximum strain. With a maximum strain, as shown on the

Tons per sq. inch.

Fig. 9.



Tons per sq. inch.

Results of some of Wöhler's Experiments.

Variations or Differences: D between maxima M and minima strains. The figures in parentheses are repetitions of experiments divided by 1,000,000.

Equations:

$$\text{Ellipse: } D^2 = 22\tfrac{1}{2}^2\left(1 - \frac{(m - 22\tfrac{1}{2})^2}{42\tfrac{1}{2}^2}\right)$$

$$\text{Parabola: } D = \frac{55\,m - m^2}{55 - 25}.$$

diagrams, of 60 tons, he found that between 40 and 50 tons minimum strain could be allowed; for 50 tons, 33; for 45, 25; for 40 tons, 20; for 35, between 12 and 16 tons minimum strain; and so forth. He could strain the material to 22 tons, and relieve the strain entirely and continue the operation thirty-three million times without rupture. Some of the bars did not break and some of them did, even with a strain of 33,000 lbs. per square inch. One bar broke after fifty-seven million revolutions, and another, of the same material and under 1,200 lbs. lower strains, broke after three and a half million revolutions. The results were shown in Fig. 9. It would be seen that the points were scattered all

Mr. Bender.

over the paper like shots on a target; so that it would require a great deal of imagination to lay a scientific line through them. One thing was very remarkable in these experiments. If they were conducted to the verge of rupture of the material, it was still possible to carry a remarkable reduction to the ultimate strain. He had used, for instance, iron made at the Phœnix Iron Works, which stood a strain of about 22 tons per square inch; he strained it to 22 tons less 800 lbs.; he then relieved the strain by 12 and by 10 tons, and again put on 12 or 10 tons; then took it off and put it on again, and he found it would break, in the first instance after two and a half million repetitions, in the second it did not break after four million revolutions. It was certain, therefore, that very near the ultimate strength a considerable variation was still possible. For iron it was nearly 10 tons, and for steel about 15 tons. For iron a strain of 15 tons could be imposed, then released entirely, and put on again, and so on *ad infinitum*, without causing rupture, and at the limit there might still be a variation of 10 tons. If there was any law, the curve would rise abruptly at the point of ultimate strength and then bend over. The subject, interesting as it was, did not receive attention in Germany for a long time. In 1873 Professor Launhardt, of Hanover, brought out a certain formula, and from that moment great interest was manifested in the question and formulæ poured forth from all quarters. He had himself constructed three new formulæ, and he was prepared at short notice to furnish five or ten more. What was the formula of Professor Launhardt? He assumed the point 25 tons to 0. He further assumed that under a strain of 55 tons per square inch steel broke, which was not true according to the experiments; and all that he did was to lay a parabola through these two points. Another formula might be made by laying an ellipse through the results, or a hyperbolic spiral, or a logarithmic line, and all would be equally good.

Mr. Matheson.

Mr. Ewing Matheson observed that the Author had given some diagrams of very large and very deep bridges. It was interesting to note, in considering the history of bridges during the last thirty years, that Mr. Brunel had constructed several deep bridges, one, for instance, at Chepstow, and another at Saltash. As a rule, however, the bridges at that date were shallow. Mr. Fairbairn, in his original book, spoke of depths of 1 in 20 and 1 in 15 as not inappropriate. Gradually the proportion had come to 1 in 12, 1 in 10, and even 1 in 8. In America many years ago engineers went to the extreme point that theory would

Mr. Matheson. prescribe, and gave for short spans very deep girders; but, although they always took credit for superiority over English bridges, they appeared gradually to have become restrained by the prudential motives which influenced English engineers, and were approximating to the English practice. While English engineers were adopting 1 in 8 and 1 in 10, American engineers were coming from 1 in 6 and 1 in 7 to the English standard. He thought the Author had shown that, while for small spans deep girders were not desirable, for spans so large that overhead bracing would clear a locomotive chimney, it was economical to have a deep girder. With regard to the Pacuare and Matina bridges, designed by the Author, Mr. Matheson had been concerned in their construction, and was, to some extent, responsible for the adoption of the design. They were erected in Central America, on a railway promoted by Americans and in close contiguity to American bridges. They were much criticised; and he was glad to say, for the credit of English engineers, that they had been successful, and had compared well with American bridges. The Matina viaduct was one of the lightest he had known, and at the same time it had proved one of the safest under the heavy running of railway trains. Reference had been made to the Weyrauch system, and attention had been drawn to a fact which was significant to English builders, that while, during the last ten years many of them had been taught, that the limit of elasticity in iron rather than the breaking strain was the point to be considered, in Germany that idea had been abandoned, and the breaking strain was now considered as the important factor in calculating the sectional area in iron structures; and he believed that view was followed in the bridges which the Author had designed, and which had been actually erected.

Mr. Reichenbach. Mr. OSCAR REICHENBACH asked the Author his reason for thinking that in proportioning the struts of the web, it would be sufficient to use, of the coefficients derived from the Weyrauch and Gordon formulæ, the larger of the two only. It seemed to him that if the use of the Weyrauch formula was justified, it must be on the principle that the strength of iron when subjected to a varying strain was gradually diminished, and that by subjecting it to a less strain in proportion to its variation, deterioration was not prevented, its rate only being made more uniform; or else that the limit of elasticity of the iron was altered in proportion to the variation of strain. In either case it would seem to him to be necessary to construct the parts in compression with a coefficient $(m - 1 + v)$, where v denoted the coefficient derived from the

Weyrauch formula, and not with m or v only. The economy to be obtained by increasing the depth of girders was more marked in swing than in fixed bridges. That point had received more attention in America than in England. The relatively deepest girder that had come under his observation was the swing opening of the international bridge over the Niagara at Buffalo, which had a span from the centre of the round pier to the centres of the supporting wedges of 179 feet, and a depth of 37 feet, or a depth of $\frac{1}{4\cdot 84}$ of the span. The relatively deepest swing-bridge girder with which he was acquainted in England had a span of $5\frac{1}{3}$ its depth. Mr. Reichenbach.

Mr. L. F. VERNON-HARCOURT considered the Table in Appendix IV. a valuable addition to the Paper, as it indicated clearly the conclusions to which the Author's elaborate formula led. He would ask the Author whether the Table applied to steel or iron? He gathered that it applied to steel bridges, but no statement was made on this point. It appeared from the Table that, theoretically, the larger the span the deeper should be the girders in proportion to the span; but certainly that was not the practice either in English or foreign bridges. It also seemed that the greatest economy was attained, at any rate in the form of girder under consideration, by adopting a depth of one-fourth, or in the largest bridges of one-third of the span. The Table, moreover, was interesting as showing the results to which the formula led on approaching what the Author called the "limiting spans;" but its practical value ceased at an earlier stage as it would require a very bold engineer to propose to erect a girder bridge having even a span of 1,500 feet, a depth of 500 feet, and a weight of 16,250 tons, whilst the limiting spans were considerably larger. In fact long-span bridges were only resorted to where intermediate piers would be very difficult or costly to erect; and though engineers might be glad of showing their skill in spanning wide openings, motives of economy would dictate the erection of piers in preference to large spans long before the limiting spans were approached. With regard to the proper widths for girder bridges, the Author, in the early part of the Paper, had given a formula expressing the width as a function of the length. He should have thought that where the Author took a variety of depths, as in the Table, the width should be expressed as a function of the depth as well as of the length, because the wind-pressure would not be so great on a bridge of reduced depth, as the weight of metal would be more concentrated into the flanges, and the web being reduced a smaller surface would be exposed. The Author had only given one form of bridge, and it was a Mr. Vernon-Harcourt.

Mr. Vernon-Harcourt.

question whether it was the most economical that could be obtained. He had compiled a Table,[1] in a form to compare with the Author's Table, from some data in a book by Mr. Baker, giving calculations of weights for various forms of bridges, and it would appear from it that a continuous girder was by far the most economical, and the bow-string and lattice girders were the least economical. He had taken out the weights of one or two

Span.	Lattice Girder.	Bow-string.	Straight Link and Boom.	Canti-lever Lattice.	Canti-lever Lattice. Varying Depth.	Con-tinuous Girder. Varying Depth.	Arched Ribs Braced Span-drels.	Suspen-sion and Stiffen-ing Girder.	Sus-pended Girder.	Straight Link Suspen-sion.
Iron				Weight per Lineal Foot						
Feet.	Tons.	Tons.	Tons.	Tons.	Tons.	Tons.	Tons.	Tons.	Tons.	Tons.
500	4·8	4·0	5·1	2·9	2·3	2·3	3·0	2·5	2·4	2·0
750	..	12·0	..	6·4	4·6	4·2	5·7	5·0	4·8	4·6
1,000	..	..	..	16·7	9·5	6·4	13·4	10·5	10·2	21·8
1,250	..	..	..	..	24·9	9·1	53·0	36·0	35·1	..
1,500	..	..	..	..	..	13·3	..	..	..	..
1,750	..	..	..	..	..	20·0	..	..	..	..
2,000	..	..	..	..	..	35·7	..	..	..	..
Steel.										
500	2·6	2·2	2·0	2·1	1·6	1·6	1·8	1·5	1·4	1·1
750	8·0	4·4	4·7	3·9	2·7	2·6	3·1	2·7	2·4	2·1
1,000	..	9·2	65·2	7·6	4·9	4·2	5·1	4·5	4·0	3·9
1,250	..	34·1	..	16·5	8·8	5·8	9·1	8·1	6·8	11·2
1,500	..	..	..	64·4	15·7	7·6	16·6	14·4	11·7	100·0
1,750	..	..	..	..	46·5	9·9	46·0	38·1	33·1	..
2,000	..	..	..	..	..	13·2	..	..	..	..
2,250	..	..	..	..	..	17·7	..	..	..	..
2,500	..	..	..	..	..	25·3	..	..	..	..
2,750	..	..	..	..	..	38·0	..	..	..	..
3,000	..	..	..	..	..	81·2	..	..	..	..

bridges that had been erected, and compared them with the Author's typical bridge. The St. Louis bridge was 520-feet span; it was an arch bridge; its rise was $\frac{1}{9}$ of the span, and the weight of the whole bridge was 1,872 tons. Calculating by the Author's formula, a bridge of the same rise and the same span would weigh 1,440 tons. But it should be borne in mind that the St. Louis bridge had to carry two lines of railway and a roadway; he therefore concluded that that bridge was more economical than it would have been if it had been erected according to the Author's plan. The Ohio bridge was 515-feet span, with a rise of $\frac{1}{10}$ of the

[1] Table compiled from calculated weights given in "Long-span Railway Bridges," by B. Baker. The intermediate values for 750 feet, 1,250 feet, &c., have been approximately interpolated from the nearest values given.

span, and its weight was 1,176 tons. It only carried one line of railway. Doubling the figures, to allow for a double line of way, the weight would be 2,352 tons, or considerably more than the Author's formula would give, namely, 1,611 tons. The Kuilenburg bridge was also considerably heavier than according to the Author's formula, namely, 2,234 tons as compared with 1,247 tons. The suspension bridge at Niagara, if it had been put up as designed, would have had a weight of 800 tons. It was for a single line of rails, and it was 821-feet span. Multiplying the weight by two, it would be 1,600 tons for a double line, and the Author's formula would give 2,900 tons as the most economical girder bridge attainable for that span. It appeared from these figures that the Author's type of bridge would be much more economical than the Ohio and Kuilenburg bridges; but that the arched and suspension forms, as illustrated by the St. Louis and Niagara bridges, possessed a decided advantage over it in respect of weight. Mr. Vernon-Harcourt.

Dr. C. W. Siemens said, though the subject was one rather of construction of bridges, than of the durability or mode of treatment of materials, he thought a few observations might not be inappropriate with reference to the remark of Mr. Bender, to the effect that steel gave way with a strain of 60 per cent. of the calculated strain. Steel was essentially a different material from iron. The difference was like that between parchment and woven fabric. In iron there were fibres which acted separately, so to speak; whereas in steel, although there was a greater total strength, the material must not be strained to an undue degree at any one point. To render his meaning clear, he might be permitted to describe an experiment which he had occasion to make some time ago, with the view of constructing a link for a large bridge. The first idea was to rivet two bars together in the way usually adopted. In trying two steel bars riveted together in the way represented, both bars being tapered breadthways to a point, and the rivets forming a diamond shape, he found that the breaking strain across the joint was, as Mr. Bender had described, about 60 per cent. only of the strain which the net area ought to give. But what was the result of the experiment? The steel did not break across the full section of the united bars, or across the section of the greatest number of holes, but in bringing on the strain to the bar, the first rivet and the last rivet would receive nearly the whole strain. The material itself, being highly elastic, acted as one body in the middle, and the elastic strain was thrown to the end rivet on either side. The end rivet generally gave way Dr. Siemens.

Dr. Siemens. with shearing. The two rivet-holes that followed had sufficient resistance to commence what might be called a tearing action through the solid bars, and the total resistance was certainly much below what it should have been. Another result which came before him in connection with Lloyd's surveys, was this: A test-bar was riveted to a strong pair of tongs; and, to the surprise of the gentleman who made the test, the bar, instead of giving way at the point of least section, broke at the foremost bolts attaching the bar to the tongs, again showing that the two rivets receiving the strain in the first instance, set up a tearing action, which made the line of breakage about three times the length of the line of least section. In all steel structures that ought to be borne in mind. Bars should never be joined by simple riveting; they should be dovetailed together in such a manner that no tearing action could be set up. He believed, when engineers had mastered the art of constructing with steel, it would be found to be a thoroughly reliable material.

Mr. am Ende. Mr. MAX AM ENDE, in reply, said he could not refer to matters that did not concern the Paper. He knew that other systems were sometimes preferable to girder bridges, but he had only treated of the latter. He would only add that the calculation of the weight of arched bridges or suspension bridges, from the formula he had given for parabolic girder bridges, was out of the question. No doubt the proportion of the depth to the span of girder bridges must increase with the span. For very large spans it was necessary to increase the proportions to make it possible to construct the bridges. Plate 10, Fig. 8, showed certain curves which contained all the considerations with regard to the lateral stability, as well as the construction of the webs; and as the designs and calculations had been made conscientiously, the curves might be relied upon. It would be seen that the line of proportion of depth to span of 1 in 6 intersected the curve of weight of the 250-feet bridge not far from its most economical point; while it intersected the curve of the 2,000-feet bridge at a point much higher than the most economical point, rendering the construction practically impossible. With regard to the length of panels, he had found that, in bridges of about 750 feet, the division as shown was as nearly the most economical as possible. If another set of diagonals were introduced, a good deal more bracing—especially secondary bracing, would be put into the web than was now comparatively in the booms; but in the largest bridge he admitted that there might be an advantage in a subdivision of the panels, but he was not sure whether he should put in another set of diagonals rather

Mr. am Ende.

than carry the intermediate verticals down. Introducing a second set of triangulation was a dangerous thing to do in bridges with polygonal top flanges, because, unless the two systems were connected at some point, the flanges might get a transverse strain; and that mistake was sometimes made. The two systems must be connected at one point. Even then, if one system was loaded, the strain from that load had to pass in a round-about way through that connecting member into the other system. On the other hand, by connecting the two systems of triangulation at every point, material would naturally be wasted. Many attempts at perfection in the construction of webs, as well as of girders generally, had been made. All the types thus produced had their good points, but none of them was perfect. It would be seen from the diagrams of theoretical and practical weights (Plate 10, Figs. 5 and 8) that there was a large range within which it was possible to construct girders economically; and there was, therefore, room for practical and æsthetic considerations.

With reference to the calculation of strength of long columns, he was inclined to abandon the present method, which was based upon the ultimate resistance according to the well-known Rankine or Gordon formula, and to rely on the safety against bending, going back to the old Navier formula. The commencement of bending of long columns took place, according to that formula, under a force smaller than the ultimate resistance, as established by experiments, and consequently there could be no objection to adopting that formula. But as the formula, applied to short columns, gave forces even in excess of the breaking strain, another calculation would have to be made for such cases, and that would be done by the Weyrauch formula; or, if that was not adopted, by the ordinary factor for the breaking strain. The result would be that columns exceeding certain lengths would have to be calculated according to one formula, and columns within those lengths according to the other. The method was based upon this view, that bending could be absolutely prevented; or that, if the force did not go beyond a certain amount, bending did not take place, and that consequently the strain was equally distributed over the whole area. He could not then enter into the question what method should be adopted to bring the two formulæ into operation. One point would be to ascertain with what coefficient of safety it was required to prevent bending, and whether it was requisite to adopt the same coefficient of safety, say 3 or 4, as for other parts. The objections to the new method of calculating the strength of bridge parts based upon the experiments made by

Mr. am Ende. Wöhler and Spangenberg with repeated strains, were twofold. On the one hand it was said that the experiments were not sufficiently numerous to justify the belief in certain results which had been proclaimed; and on the other it was said that these results, if recorded in a diagram, did not lie in a continuous curve, and that therefore they did not distinctly represent the law of strength of the materials.

With regard to the first point, and speaking of iron only, about forty experiments had been made to ascertain the strength of iron under strains alternating with no strain, and about fifty experiments under strains alternating with strains of the same intensity in an opposite direction. In about ten or twelve cases only a practically infinite number of efforts was reached, and thus the ultimate working strain of the two kinds was ascertained only by ten or twelve experiments in a direct manner; but on examination of the Tables it was found that the numbers of efforts under which fracture took place diminished gradually with an increase of intensity (only with few exceptions); so that these experiments tended to prove the faculty of the ten or twelve cases to represent the true strength of the material. The similar and more numerous experiments with steel had given similar results, although with different strains, and they also tended to prove, although indirectly, that the above-mentioned ten or twelve results could be relied upon. It might even be asserted that the strength of iron under the above-named circumstances was determined with almost the same accuracy as the ordinary breaking strength of the same material under a single effort was determined. The experiments gave the following relative figures:—

For alternating strains of equal amount in opposite direction;

$$i.e.\ \text{for}\ \frac{\text{minimum strain}}{\text{maximum strain}} = -1 \quad . \ . \ . \ . \ . \ . \ . \ . \quad 160$$

For strains alternating with no strain;

$$i.e.\ \text{for}\ \frac{\text{minimum strain}}{\text{maximum strain}} = 0 \quad . \ . \ . \ . \ . \ . \ . \quad 320$$

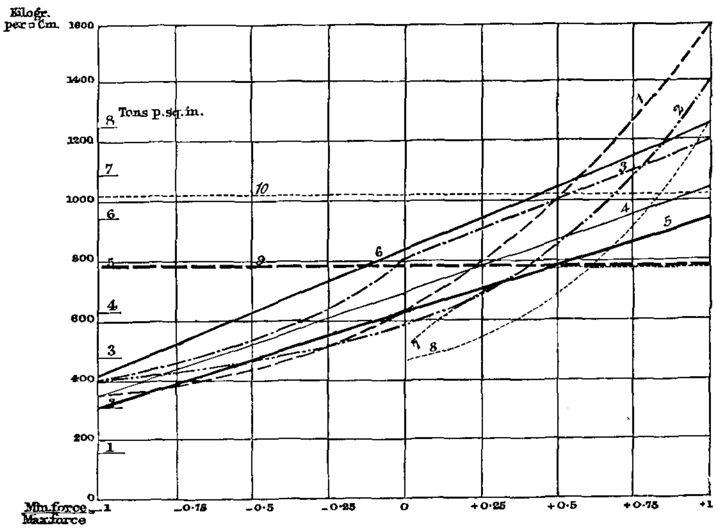
For a single effort;

$$i.e.\ \text{for}\ \frac{\text{minimum strain}}{\text{maximum strain}} = +1 \quad . \ . \ . \ . \ . \ . \ . \quad 480$$

The first two figures, being derived from experiments which required time and an elaborate apparatus, were liable to correction, and it was possible that this correction might amount to 10 or 15 per cent. The third figure, on the other hand, could be ascertained in comparatively little time by an ordinary testing machine, and it might be considered less liable to correction when the tests were repeated. But as the experiments by Wöhler and Spangenberg had revealed differences of 200 and 300 per cent. in the strength of iron, it seemed advisable, not only to regard the results with interest, but also to act upon them, and not wait for the future correction of comparatively minute differences. Mr. am Ende.

The second objection to the proposed new method of calculating the strength of bridge-parts was made on account of the difference of opinion of various authorities as to the way of filling up the gaps between the three above-named figures. In this respect the experiments were scanty, and they were more so for iron than for steel. The existing results permitted the assumption of various curves expressing the law of increase of strength between the three figures. In a paper on this subject by Dr. Winkler[1] was a diagram showing the proposals of various authors by means of curves. This diagram was reproduced in Fig. 10, to which he

FIG. 10.



Line 1. Gerber. 2. Winkler. 3. Launhardt. 4. Weyrauch. 5. Am Ende. 6. Am Ende for steel. 7. Baker. 8. Board of Consulting Engineers, New York, 1877. 9. Board of Trade. 10. Ditto for steel.

[1] "Wahl der zülassigen Inanspruchnahme," in "Zeitschrift des österr. Ingenieur-und Architekten- Vereines," vol. xxix., p. 45 *et seq.* Wien, 1877.

Mr. am Ende. had added two lines expressing the rule proposed in the Paper; also two lines expressing an American rule of the year 1877, and two lines for the English Board of Trade rule.

The six curves based upon the new method lay close together at the left of the diagram, and diverged towards the right. This divergence was chiefly due to a difference in the assumption of the factor of safety, and it would be much less marked if the factor of safety were the same for all curves. For example, the Weyrauch curve and the curves as defined in the Paper, which were straight lines, together with the right hand part of the Launhardt curve, would coincide for an equal factor of safety, and in the same way the other two curves would be brought nearer together. Reduced in this way to the same standard of comparison, these curves would show clearly that it did not much signify which curve was adopted, when compared with one of the old rules, for example, that of the Board of Trade. The straight line connecting the three points defined by the above-named three figures had the advantage of simplicity, and for this reason especially the Weyrauch formula was preferred. As long as the Board of Trade rule prevailed, and greater strains than 5 tons per square inch were not permitted, that part of the line which lay above the horizontal line of the Board of Trade must be omitted, and the horizontal line be put in its place. This was done in calculating the strength of the Matina and Pacuare bridges, for which the Board of Trade rule was stipulated by the Costa Rica Government.

Correspondence.

Mr. Bow. Mr. R. H. Bow was much impressed with the value of the Paper. He had long been an advocate for the adoption of great depth of structure in girder bridges, and this feature of great depth was the most prominent one in the designs of the Author. He might here note some of the steps he had himself taken in this question. In 1855[1] he advised the extension of the depth of parallel girders to $\frac{1}{8}$ of the span—a large increase upon the practice at that time. In 1862[2] he pointed out the possibility of attaining greater economy by a still further increase of depth; and in 1872 he designed a road and viaduct from Leith Walk to Jeffrey Street, Edinburgh, with depths of $\frac{1}{5}$ and $\frac{1}{6}$ of the spans, but

[1] *Vide* "The Civil Engineer and Architect's Journal," vol. xviii., p. 236.
[2] *Ibid.*, vol. xxv., pp. 280, 310, and *passim*.