November 16, 23, and 30, 1869.

CHARLES HUTTON GREGORY, President, in the Chair.

No. 1,225.—"On the Present State of Knowledge as to the Strength and Resistance of Materials."[1] By JULES GAUDARD, Civil Engineer, Lausanne. (Translated from the French by William Pole, F.R.S., M. Inst. C.E.)

DISCUSSION.

DR. POLE stated, that in the absence of the Author, he, as translator of the Paper, would venture to say a few words upon it.

There were two branches of the question of the strength of materials, very distinct from each other, namely, the experimental branch and the theoretical branch. The former comprised the obtaining of experimental data as to the strength and resistance of materials of various kinds, and tested in various ways; the latter implied the processes by which the data so obtained were reasoned upon and made applicable. Each of these afforded a wide field for investigation; but the Author had confined himself almost entirely to the latter. He doubtless concluded that the experimental part of the investigation, being so purely practical, was likely to be already well understood by engineers in this country, and he accordingly devoted his chief attention to the theoretical investigations, which he had good reason to know had been much more thoroughly and carefully followed out by continental mathematicians.

It would be interesting to trace the various steps by which the theory of the resistance of materials had arrived at its present advanced state; but this would require a volume.[2] Mr. Pole would only refer to a few of the most salient points of the history. The architects of the middle ages appeared, by their often bold designs, to have had some efficient practical views as to the strength of the materials they were using, but there was no record of any of their calculations. The first person who had left any enduring mark in this way was Galileo, who formed the earliest known theory as to transverse strain; he explained why the increase of depth of a beam added proportionately more to its strength than a corresponding increase of breadth; and he had a good idea of the nature

[1] This Paper was printed in extenso in the Minutes of Proceedings for the last Session, vol. xxviii., pp. 536-571. The discussion upon it extended over portions of four evenings, but an abstract of the whole is given consecutively.

[2] This information has been collected in a very valuable "Abrégé Historiqué," by M. Barré de St. Venant, in his work quoted by M. Gaudard.

and effect of the neutral axis, though, by his want of knowledge as to the nature of elasticity, he put it in the wrong place. The true principle appeared to have been first discovered, about 1676, by an Englishman, Robert Hooke, who not only corrected Galileo's error as to the elasticity, but also published the well-known and important law of the proportionality of the extensions and compressions to the forces applied; enunciating it in the often quoted short sentence, "ut tensio sic vis." This discovery formed the basis of all the subsequent reasoning on the subject.

Mariotte, Leibnitz, Bernouilli, Euler, Lagrange, Young, and others, occupied themselves with the problem, and, in 1773, Coulomb wrote a celebrated memoir, in which he made important advances in the theory of the stability of structures in general.

Early in the present century appeared in this country the well-known treatises on the strength of timber by Barlow, and on the strength of cast iron by Tredgold, both works especially adapted for the practical use of the engineer. After these came the researches, practical and theoretical, of Mr. Eaton Hodgkinson, whose careful experiments on the elasticity of iron, and on its most favourable application, were very valuable.

On the Continent, about this time, the subject was greatly advanced by the labours of M. Navier, who, in his lectures to the Ecole des Ponts et Chaussées, from 1819 to 1826, put the whole subject on a new footing, and laid the foundation of the modern theory of elasticity, which had ever since been the great guiding principle in investigations on the resistance of materials.

The system of Navier was made known in England in 1843 by Professor Moseley, in his admirable work on the "Mechanical Principles of Engineering and Architecture." Adopting chiefly this system, as well as the theorems of Coulomb, Poncelet, and others, and adding thereto much original treatment of his own, he placed before the English engineering student a manual of the principles of art, far eclipsing, in a mathematical point of view, anything existing before. Important practical use was made of Professor Moseley's data, in the discussion in this Institution on the Torksey Bridge,[1] and in the calculations for Mr. Robert Stephenson's Britannia Bridge, published in Mr. Edwin Clark's work.

Since then the science had gone on advancing, particularly in reference to the development of the theory of elasticity. At a more recent date, the valuable works of Professor Rankine and of Professors Sir William Thomson and Tait had further illustrated the subject for English readers. But the Paper now before the Institution comprised a more complete exposition of the subject than any heretofore given. It was, as the title expressed, a complete résumé of the

[1] *Vide* Minutes of Proceedings Inst. C.E., Vol. ix, p. 233, *et seq.*

present state of knowledge on the theoretical part of the subject, although in a very compressed form; the recent labours of Messrs. Bresse, Belanger, Barré de St. Venant, Phillips, and other French mathematicians, the Author included, being all described or referred to. The Paper, from its mathematical nature, was not one well calculated for general discussion, but Mr. Pole had reason to believe several competent members of the Institution had paid attention to it, and he hoped to hear some remarks on it from them. He believed, indeed, that any students who were capable of entering into its details would find them both interesting and instructive. The Author had discussed at length the resistance to extension, compression, torsion, flexure, shearing, and sliding of the particles on each other; and had added various remarks on the texture of bodies, the limits of safety, the best forms of structure for certain purposes, continuous beams, the combination of different materials, and the effect of shocks and vibrations from masses in motion.

Mr. Pole would however recall attention to the fact, that M. Gaudard's Paper, valuable as it was, only touched one branch of the subject; the other branch, the practical investigation of the actual numerical data of the strength of materials, could only be derived from accurate and repeated experiments, conducted with careful regard to the theoretical requirements of the various cases, and patiently carried out with materials of different kinds, and tried under different kinds of strain. There was already a great fund of information on this subject, but it required to be collected, sifted, and put into form for application. And further, in consequence of the continual improvements and changes in manufacture, a vast deal more still remained to be done. This sort of investigation was more particularly fitted to the English practical mind, and he hoped it would receive, ere long, attention as complete and interesting as that devoted to the theoretical branch in M. Gaudard's Paper; and any competent member of the Institution who would take up earnestly this branch of the subject, and would do for it what M. Gaudard had done for the theoretical branch, would confer an almost incalculable benefit on the profession.

Mr. Pole's own part in the matter had been very simple; he had only to render an excellently written Paper into corresponding English terms; the translation had been a work of great interest to him, and he was much gratified to know that it had received the full approval of the Author.

Mr. Callcott Reilly desired for one to record his thanks to the Author for this memoir. It ought to be of the greatest use to students of the subject, because it exhibited a most skilfully condensed epitome, or abstract, of the results established by the best and most recent investigators of the theory of the strength of materials, excepting those of some writers whose works had only appeared within the last two or three years. It would serve as a useful and intelligent guide to the voluminous writings of those Authors, extending in the aggregate over thousands of pages. He might mention that the principal work of M. de St. Venant, so often quoted by M. Gaudard, contained original investigations that would fill two bulky volumes; there were others whose writings were nearly as extensive, and he believed that little if any of that great and splendid body of scientific literature could be spared. The modern theory had been established chiefly by French writers, and engineers throughout the world owed a debt of gratitude to them for the important results which they had achieved.

The subject of the theory of the strength of materials was the discussion of three principal problems, which, not being formally stated by M. Gaudard, he would quote briefly from one of the most distinguished French writers. The first of the three general problems was this:—"Having given all the external forces which act upon a body, to find the intensity of the stresses, that is, the intensity of the molecular forces developed at each point of the body, on which depend either the preservation, or the destruction, or the deterioration of the body." The second problem was:—"All the exterior forces being still given, required to compute the change of form, or the deformation, of the body due to the action of those external forces." The third general problem was:—"To find, among all the external forces, those which are not given *à priori*, and which proceed from the connection of the body in question, with other bodies."[1] The inquiries involved in those three problems constituted the subject of the theory now under discussion, and the solutions which had been achieved did infinite honour to their authors, on account of the formidable nature of the difficulties they had overcome.

Another eminent writer, M. Collignon, had recently stated that the "practical object of the theory is to determine the interior stresses developed in the various elements of a given or proposed construction, and to ascertain if those stresses are within the limits known to be safe; also to determine with the greatest possible economy the forms and the dimensions which would insure the sufficient resistance of the work, that is to say, would correspond to local intensities of stress within those prescribed limits."[2] Mr. Cawthorne Unwin had justly observed that "The first requisite for success in design is a thorough knowledge of the distribution of stress. . . . That the constructor may not be a slave to rules and formulæ, he should attain to a clear mental image of the distribution of stress, and should mould his material almost as a sculptor cuts the marble to an ideal form pre-existent

[1] *Vide* "Mécanique Appliquée." Par M. Bresse. Vol. I. Second Edition. Paris, 1866.

[2] *Vide* "Mécanique Appliquée aux Constructions." Par M. Edouard Collignon. Premier Partie. Paris, 1869.

in his mind."[1] Mr. Reilly thought such an accurate mental conception could only be attained by a study of the deformations that the action of any given stress produced, or tended to produce, in the piece, or combination of pieces, under consideration. He believed that in this consisted one element of the admitted superiority of the modern French text books over all the English ones treating of the theory of construction. The French writers first investigated the motions among the particles of the body caused by the action of applied forces, and thence deduced the conditions of stress; the English writers deduced the stress directly from the applied forces. The former method at first sight appeared more complex in mathematical expression; but it was in reality, and ultimately, the more simple, because it included in one harmonious theory all cases, from the most simple one of a straight homogeneous rod extended by a single applied force, to the most complex cases of combined stresses, as in mill-shafting, continuous girders, metallic arches, and heterogeneous structures. On the other hand, the method successfully applied by English writers to the solution of simple cases was incapable of dealing with those more complex problems, and had after all to be abandoned in favour of the theory of deformation, which was then brought forward, disconnected from its application to those simple problems that would otherwise have already thrown light upon its difficulties.

The established theory as epitomized by M. Gaudard was incomplete, because, on account of certain physical and mathematical complexities which might some day be simplified or removed, it neglected certain considerations which entered into the mathematical theory of elasticity of solid bodies. But those undeniable defects had not been shown to have any practical importance in the applications made by the engineer; therefore, there was no reasonable doubt that the theory in its present state might be applied with perfect confidence by the engineer who thoroughly understood it, and was cognisant of those deficiencies.

It was frequently alleged, as a reason for withholding full confidence in the theory, that many important discrepancies occurred between the conclusions of the theory, and the facts disclosed by experiments upon the ultimate strength of materials. But that objection in his opinion was of no importance, provided the theory was applied as intended by its Authors. The theory assumed a strict proportionality between the intensities of the forces, and their effects upon the form of the body considered. Now it was admitted that such proportionality was very nearly exact for all the materials of construction, provided the stresses produced by the applied forces did not exceed the limits compatible with the

[1] *Vide* "Wrought Iron Bridges and Roofs," p. 3. 8vo., London, 1869.

durability of the body. Beyond those limits, the proportionality assumed ceased to exist. But that was a circumstance of no importance to the engineer, because he need only concern himself with the condition of stress and the deformation of the materials, in the actual circumstances in which they were placed, when performing the work assigned to them; such work being always intended to be consistent with the preservation and durability of the materials.

It was true that M. de St. Venant, perhaps the ablest investigator of this subject, had attempted the theoretical solution of problems relating to the ultimate strength of materials, and his extraordinarily skilful labours had been attended with some measure of success. But his success in this department was of less interest to the engineer than to the student of molecular physics.

If, after saying so much on behalf of the theory, Mr. Reilly might venture on a little hypercriticism of the foundation of a system built up by so many able minds, he presumed to submit that the ordinary definition of the coefficient or modulus of elasticity, denoted by the letter E, was open to some improvement. The ordinary definitions given of this function E seemed unsatisfactory from the point of view of mathematical exactness, although that defect happened to have no practical importance in the applications of the theory. The ordinary definitions were deduced from the law of proportionality between the applied forces and their effects, and they might be described as follows:—

Professor Moseley, following Dr. Young, defined E as the force which would extend a bar, whose section was unity, to double its original length; or, on the other hand, if applied in compression, would shorten it to half its original length, supposing the material to admit of that change of form. The French writers said that E was the force which would extend or shorten the bar by an amount equal to its original length. If the law of proportionality were true for all forces, however great, and the rate of shortening equal to the rate of extension, then it was obvious, that the shortening produced by the force E, must be not the half length, but the whole length of the bar. And that was what the French writers said, whose definition was therefore more consistent, in that point of view, than Dr. Young's and Professor Moseley's. But the French definition involved an absurdity, or rather a violation of the law of continuity; for if the force E applied to compress a prism, shortened it by its whole length, so that its length then became nothing, what would happen from the application of a force greater than E? It had, therefore, always seemed to him, that the ordinary definition of the French writers ought to be stated with some qualification. He mentioned this notion two years ago to Mr. Heppel, who agreed with it, and with his usual skill and fertility in these matters, immediately set to work to provide a new definition of E, which should be free from those objections. He thought his definition was worthy of being put on record, and he believed it might be advantageously placed alongside the older definitions, although it need not, and indeed was not likely to, supersede them.

Mr. Heppel defined E to be the limit of the product of any minute force multiplied into the ratio of the altered length to the minute difference of length occasioned by it. Thus, let L be the original, and x the altered length of the bar, of section unity, and let p be the force, which produced the elongation x—L; then $d\,x$ was the infinitesimal change of length produced by the action of $d\,p$, and the definition was expressed thus—

$$E = x\frac{d\,p}{d\,x};$$

a consideration[1] of which led to the equation—

$$E = \frac{p}{\log x - \log L}.$$

The ordinary definition gave—

$$E = \frac{p\,L}{x - L} = \frac{p}{\frac{x}{L} - 1}.$$

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[1] Thus,
$$d\,p = E\,\frac{1}{x}\,d\,x;$$
when
$$p = 0,\ x = L,$$
then
$$p = E\int_L^x \frac{1}{x}\,d\,x,$$
$$= E\ \{\ \log x - \log L\ \};$$
$$\therefore E = \frac{p}{\log x - \log L}.$$
If p = E, (say a tension equal to E),
$$\log x - \log L = \frac{E}{E} = 1;$$
or,
$$\log\frac{x}{L} = 1;$$
$$\therefore \frac{x}{L} = \epsilon \text{ and}$$
whence
$$x = \epsilon\,L = 2{\cdot}71828\ldots L.$$
If $p = -$ E (or a compression = to E),
$$\log x - \log L = \frac{-E}{E} = -1;$$
or,
$$\log\frac{x}{L} = -1,$$
whence,
$$\frac{x}{L} = \frac{1}{\epsilon},$$
$$\therefore x = \frac{1}{\epsilon}L = 0{\cdot}36787\ldots L.$$

but where $\frac{x}{L}$ was very nearly 1, as occurred in all practical applications, then[2]

$$\frac{x}{L} - 1 \text{ was very nearly } \log \frac{x}{L}$$

or,

$$\log x - \log L.$$

So that for the purposes of practical application the two values of E might be considered identical. But if Mr. Heppel's value was applied, instead of the ordinary one, to ascertain the relative extension or shortening produced by a force equal to it, the extension was found to be $1 \cdot 71828 \times L$, and the shortening to be $0 \cdot 63212 \times L$; results which satisfied the law of continuity violated by the ordinary definitions.

Mr. Heppel had pointed out to him that the two equations in Article 12 of M. Gaudard's memoir—

$$g' = \frac{d\,z}{d\,x} - y\,\phi,$$

and

$$g'' = \frac{d\,z}{d\,y} + x\,\phi,$$

were unintelligible because not homogeneous; for instance, g'' was made to appear as the sum of an abstract number and a quantity varying with the unit of length. As it stood, supposing the unit of length altered, the right sides of both equations would be changed, whereas the mere change of the unit of length ought not to alter g' and g'', unless there was some restriction placed upon its meaning. Mr. Reilly had since referred to the original work by M. de St. Venant, and it there appeared that the unit of length was a fixed quantity, namely, the perpendicular distance of two contiguous sections of the prism, of which the surface y O x, Fig. 2, Article 12, was one. $y\,\phi$ and $x\phi$ were two components, parallel to the axes, of the projection of the unit of length, after it had been deflected from the perpendicular by the action of the torsion; so that if the unit of length was altered, the three quantities g, g' and g'' varied accordingly, which removed the apparent inconsistency.

Mr. C. W. Siemens observed that the Author of the Paper appeared to base all his calculations, which were very elaborate and valuable in themselves, upon the breaking strain of materials. He thought, for practical information, it would be necessary to follow out a similar investigation, carried only to the limit of elasticity, which the Author had entirely ignored. If the limit of elasticity of all materials was proportionate to the breaking strain,

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[1] If $\frac{x}{L} = 1, \log \frac{x}{L} = 0 = \frac{x}{L} - 1.$

the one investigation would cover the two cases; but materials differed greatly in this respect. The ultimate strength and flexibility of a metal, such as would be conformable to the calculations of the Author, as for instance, lead, was, in its property of yielding to moderate force, very different to iron, and in a still greater measure to steel. Steel would yield, within the limit of elasticity, up to a much higher point than, he believed, any other metal. In devising engineering works it was of the utmost importance to know, not merely when a structure would give way, but when any destructive action would commence.

In dealing with transverse strain, the Author illustrated the case by a figure, signifying the strain on every fibre in the beam. That figure was perfectly correct for breaking strains, where the fibres first permanently elongated by the strain brought the next into greater tension, and so on in succession, till the limit was reached where the outer fibres would actually break. But before such a diagram of resisting forces could be true, permanent deflection must have taken place; and it was of importance to engineers to know what was the distribution of strains before any permanent effect had been produced; and within those limits he maintained the form of the diagram would be more nearly represented by straight lines crossing each other in the neutral axis.

He was ready to admit that the limit of elasticity was not an absolute point; that there was a slight set produced in straining a bar for the first time; and that the ultimate limit would be more correctly represented by a bend in a curve than by a sudden change of direction. But, nevertheless, he maintained that the position of this bend in the curve, denoting elongation and compression in each material, was of great importance, and could not be ignored without arriving at erroneous conclusions.

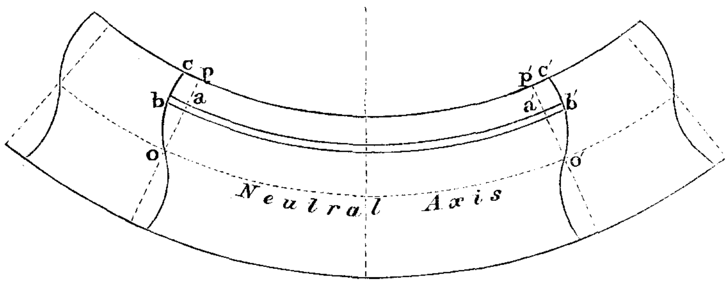
The resisting force of cast steel—a material that would hereafter enter largely into all mechanical construction—was nearly three times greater than that of iron up to the limit of elasticity. He believed that the Railway Inspectors of the Board of Trade would be willing to acknowledge the greater strength of cast steel, if that material could be readily distinguished. No doubt, at first sight, it was difficult to ascertain whether the plates of a bridge were of cast steel or of wrought iron; but this he would suggest might readily be ascertained from the specific gravity of the metal. He had, in his own laboratory, submitted a number of specimens of steel and iron to this test; and he found that in all cases wrought iron was 2 per cent. or 3 per cent. lighter than cast steel of nearly the same chemical composition. Fused wrought iron had a specific gravity of 7·87; but if 2 per cent. of carbon was added to it the specific gravity was reduced to 7·79; common bar iron had a specific gravity of 7·55; and puddled slab 7·53 only.

The specific gravity of puddled iron of the greatest purity never reached 7·6; while that of mild cast steel, with carbon varying from nil to 4 per cent., always exceeded 7·7, a distinctive difference that could be easily recognised. There was an easy way of determining roughly the specific gravity of metal: chip off the corner of a plate, suspend it from the arm of a balance, and weigh it both in and out of water; divide its weight in the air by the loss of weight in water and the result was the specific gravity of the metal. But if it were said this was too much trouble for engineers or Government Inspectors to undertake, the Board of Trade might appoint inspectors for the purpose. The manufacturer might pay the inspector's expenses, and the latter might stamp upon each plate, which he had seen made and properly annealed, a mark signifying that it was cast steel of a certain quality. He thought there should be no practical difficulty in deciding which material existed in a structure; and with a material such as cast steel, there would be, as he had stated, an available strength three times greater than that of ordinary wrought iron.

Mr. E. W. Young had been led, by the title of the Paper, to hope that rather more information would have been given on the phenomena of materials under strain; at the same time the Author had investigated at greater length than was to be expected the mathematical part of the subject. The effect of alternate extension and compression on metals, to which he knew some engineers objected, had been apparently omitted. He had never heard on what ground the objection rested, or whether experiments had been made to show that iron, or any other material, was injured by being alternately extended and compressed; and he failed to find that information when he looked for it in the Paper. The position of Figs. 3 and 5, in Articles 16 and 17, was puzzling, as the extension was shown on the upper edge of the beam; whereas it was commonly placed in the reverse position. In Fig. 4, where the form of a beam subject to transverse strain was given, he observed it was the bottom part of the beam which was under compression.

In Article 17, the Author, in treating of the sliding action between the fibres of a prismatic solid subjected to unequal flexure, observed that the law of the slidings compelled the transverse sections, which were originally plane, to undulate in curved surfaces. Outlines of these were given in Fig. 5, and were also shown in Fig. 12, but in a reversed position, to accord with the usual ideas of a deflected beam. M. Gaudard did not give a very distinct theory of these curves. He stated that "The longitudinal slidings between the fibres develop, among themselves, tangential reactions or frictions of adherence, energetic in the neighbourhood of the central fibre, but which decrease and vanish in the fibres of the external contour which are supposed free." That these reactions were greatest in the neighbourhood of the central fibre arose no doubt from the fact that the reactions proceeded from the centre of the beam outwards; that was to say, each fibre had not only to

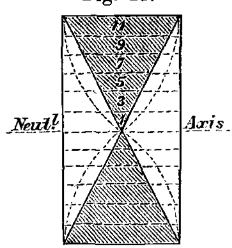
Fig. 12.



bear the proper strain due to its leverage, or distance from the neutral axis, but had to support some of the strain which came upon the fibre external to it, which fibre, but for its adhesion to its inner neighbour, would not be strained at all. The result was that the fibres nearer the neutral axis did more work than might be expected, having regard to their leverage, and consequently the outer fibres did less.

Fig. 13 represented the cross section of a beam. The neutral axis was shown passing through the centre. The upper and lower portions of the beam were each divided into six equal divisions, representing separate layers of fibres. If the beam were subject to 'equal flexure,' the strain upon each layer of fibres would be represented by the shaded portion included between the diagonal lines. The strains would, in fact, be as the areas 1, 3, 5, 7, &c., on the several layers of fibres. The outline of a section, under these circumstances, was, as M. Gaudard remarked, 'strictly plane,' and its sides were bounded by straight lines perpendicular to the neutral axis, such as were shown dotted in Fig. 12; $o\,o'$ being the neutral fibre and $p\,p'$ the external, the intermediate fibres being bounded at their extremities by the converging dotted lines $o\,p$, and $o'\,p'$. Therefore the length of each fibre diminished in arithmetical progression from the centre outwards, and since the strain was directly proportional to the alteration in length of the fibre, the strains increased in arithmetical proportion from the centre to the surface, as shown by the shaded portion in Fig. 13. But supposing the contour of the transverse sections to be a curve, such as M. Gaudard described, and had shown in Figs. 5 and 7, then the lengths of the fibres would diminish in an irregular ratio—a ratio increasing as

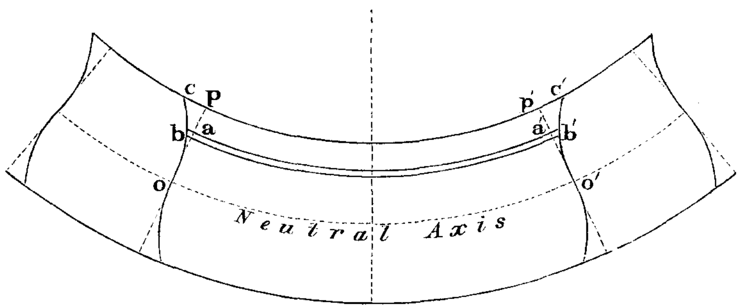
Fig. 13.



the fibres approached the exterior. In other words, the strain on the outermost fibres would be greater than that due to their leverage, instead of less. This result was so anomalous that he thought there must be some mistake in the illustration of the curves given by M. Gaudard.

Fig. 14 was a diagram of a beam similar to Fig. 12. Here the form of curve fulfilled the condition of the theory, that the strains on the fibres in the neighbourhood of the neutral axis were greater

Fig. 14.



in proportion to their leverage than those to which the outer fibres were subject. This curve, he considered, should be tangential to the perpendicular to the neutral plane at the point of intersection, and should make with it a gradually increasing angle as it approached the external surface of the beam.

Fig. 15 represented a thin transverse slice of an undeflected beam

Fig. 15.

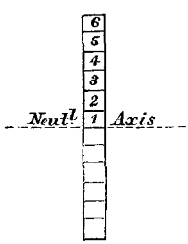
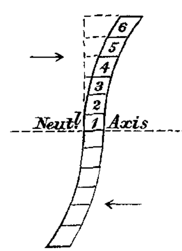


Fig. 16.



seen edgeways, and Fig. 16, what he conceived to be the form which this assumed when the beam was undergoing unequal flexure. The layer of fibre No. 1 became distorted from a rectangular to a rhomboidal shape. Layer No. 2 partook of the obliquity of No. 1, and received also the additional amount of distortion peculiar to itself. Thus the amount of distortion increased in each layer of fibre towards the outside.

Figs. 17 and 18 served to compare the two curves. Suppose the fibres on the right-hand side of the dotted line *o c* to be in compression. Then if *o c* were the outline of the section, it was obvious that the strain on the fibres would increase in uniform proportion from the neutral axis at *o* to the surface at *c*, as represented by the shaded portion in Fig. 13. Let *a*, Fig. 18, be the extremity of a given fibre, then if *o b c* were the true outline of the section, the

Fig. 17.

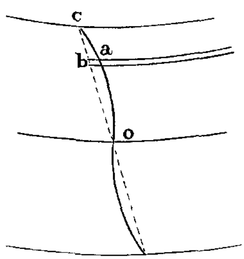
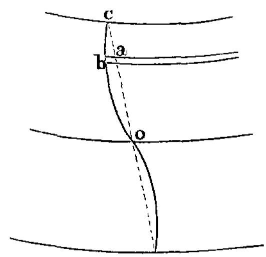


Fig. 18.



extremity *a* should reach as far as *b*; that was, the fibre of which *b* was the extremity would be longer by the amount *a b* than it would have been had the line *o c* been the outline of the section, showing that it would be less strained. Applying the same reasoning to Fig. 17, it would be found that the fibre of which *a* was the extremity, would be shortened by the amount *a b*, and consequently subject to greater strain. Since then the fibres in the neighbourhood of the neutral axis received a greater strain than that due to their leverage, the space included by the dotted curved lines, Fig. 13, more correctly represented the strains on the fibres than the shaded portion included between the diagonals. The nature of these curves would depend upon the form of the curve, Fig. 16. The curve there drawn was a parabola, of which the apex lay in the neutral axis of the beam. Probably the curve would be found to be a parabola; but whether that were so or not, if the curve were known, the form of the dotted curves in Fig. 13 ought to be deducible therefrom. The fact that the fibres, in the neighbourhood of the neutral axis, experienced a greater strain than that due to their leverage, rendered it possible to explain satisfactorily the somewhat unaccountable behaviour of an ordinary rectangular beam under a transverse strain. Assuming the strains on the fibres to be such as were shown by the shaded part in Fig. 13, it would be found that a beam required a much greater load to fracture it, than should, according to theory, put a breaking strain upon the outside fibres. This extra resistance was ascribed to friction between the fibres. Now the absorption of power by friction, where there was motion, was conceivable, but it was difficult to imagine how friction alone could be made to explain the extra resistance of a beam to a load. If there was friction it must react upon the fibres, which would be more strained than was usually supposed. This was, doubtless, the true account of the matter, but it would be well to substitute for friction some such term as elastic adhesion, or tendency to slide. Fig. 16 was intended to give a pictorial representation of this molecular characteristic. It seemed to him not difficult, from experimental data, to calculate the true outline of the profile of sections of beams composed of various substances under unequal flexure.

The statement of the Author, that "the sliding on the section for any point is the same thing as the longitudinal sliding between the fibres," seemed to involve the difficulty that the actual strain, on any particular portion in a beam, formed an angle of 45° with the neutral axis. If there was a certain vertical shearing force, and a certain horizontal sliding force, and those were equal, the resultant must lie at an angle of 45° with the neutral axis. He did not see how that could be, but it was possible, and he mentioned it in case any one was able to offer an opinion upon it.

The remarks about continuous beams were worthy of consideration, and he could endorse them to the extent of saying the calculation of a continuous girder of ordinary shape was a very complex matter. It seemed to be of such importance to have the supports level, that he should in future make one of the bearing points of a girder adjustable, which was an easy thing to do. The piers might be made level; but after the girders were placed upon them they might settle, and a slight settlement might throw out the calculation very seriously. One device was to have the girder built in position, but that could not always be done. The Author had given a rule by which to make a continuous girder of equal strength throughout, but it was not intelligible to him. M. Gaudard said, "According to the analogy of certain simple cases, the adoption of the beam of equal resistance would only tend to augment from $\frac{1}{8}$ to $\frac{1}{6}$ the moments upon the supports, and to reduce slightly those at the middle of the spans." That might be true; but there must be some given thickness originally which might be increased or diminished as necessary; and the Author did not say what was to be taken as the normal area of flange, or whether the strain over the piers, or the strain at the centre of the span, was to be taken as the normal strain.

Mr. W. C. Unwin observed, in regard to the remarks in the Paper on the purely empirical character of existing formulæ for the resistance of long columns, that it might be useful to mention that before Professor Hodgkinson's experimental research, there existed rational formulæ for the strength of pillars. Though in England it had been the custom to follow Professor Hodgkinson's formulæ, the German engineers had not abandoned the older formulæ; and so far as he was able to judge, they were right in adhering to them. These formulæ were perfectly rational, depending only on Dr. Young's modulus, and they were more simple than Professor Hodgkinson's. The question was this,—seeing that in practice columns were only loaded to very moderate stresses, for which the elasticity of the material was sensibly perfect, ought they to be proportioned by formulæ true for a perfectly elastic material, or by formulæ which, being derived from experiments on the breaking point, were only true for an imperfectly elastic material?[1] It had been sometimes said that the English were behind the French in knowledge of the theory of elasticity. He thought that on one point English writers were in advance of the theory given in the Paper. M. Gaudard stated that it had been proved by M. de St. Venant that for an elastic isotropic prism, a transverse bulging $\frac{\delta}{4}$ per unit of the linear dimensions corresponded to a longitudinal compression δ per unit of length. This might seem an unimportant matter, but it was in reality of great importance. M. Gaudard used this ratio in determining the transverse from the direct elasticity, and again in very important formulæ for combined stress.

It was a celebrated conclusion of Navier and Poisson, that the ratio of the proportional bulging and contraction was $\frac{1}{4}$, and M. Gaudard was in error in ascribing it to M. de St. Venant. It had, however, been shown that this conclusion was altogether erroneous. Professor Stokes first showed that, in many bodies, there was a great discrepancy from this ratio. Thus, that in clear elastic jellies and india-rubber the ratio must be sensibly $\frac{1}{2}$. Wertheim found the ratio for brass and glass equal to $\frac{1}{3}$; Kirchoff, ·387 for brass and ·294 for iron. M. Gaudard seemed to imply that, if not a satisfactory ratio for ordinary natural bodies, it was still the proper ratio for a perfectly isotropic body or ideal perfect solid. By showing that cork suffered much less transverse change of dimension than metals or glass, Sir W. Thomson had finally disposed of the theory of such an ideal perfect solid.

The errors which would be originated by the assumption that the ratio of lateral to longitudinal change of dimension was constant, might be judged by comparing the values of the transverse elasticity deduced on that assumption ($G = \frac{2}{5} E$) with the actual values of the modulus of transverse elasticity (G).

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[1] Euler's original investigations of these formulæ will be found in the Berlin Memoirs for 1757, and the Petersburg Commentaries for 1778. In the form in which they are now used, they are given in Reuleaux's "Der Constructeur," p. 44. Braunschweig, 1865.

MODULUS OF DIRECT AND TRANSVERSE ELASTICITY.

	E.	G.	$\frac{2}{5}$ E.
Brass wire . .	14,230,000	5,330,000	5,692,000
Copper . . .	17,000,000	6,200,000	6,800,000
Cast iron . . .	17,000,000	2,850,000	6,800,000
Wrought iron. .	29,000,000	9,000,000	11,600,000

The ratio $\frac{G}{E}$ of the modulus of transverse elasticity to the modulus of direct elasticity had different values, varying from 0 to $\frac{1}{2}$.

Sir W. Thomson had expressed the ratio of longitudinal and transverse change of dimension in the following way:—

When a straight wire or column was acted on by a longitudinal force P, it suffered a linear elongation,

$P\left(\frac{1}{3n} + \frac{1}{9k}\right)$ per unit of length in the direction of the applied stress, and a linear contraction; $P\left(\frac{1}{6n} - \frac{1}{9k}\right)$ in all directions perpendicular to the applied stress, n being the rigidity, and k the resistance to dilatation (reciprocal of compressibility).

So that the ratio of linear contraction to elongation per unit of dimension $= \frac{3k - 2n}{2(3k + n)}$.[1]

M. Gaudard had, however, employed the erroneous value of the ratio to determine the amount of stress which a body would bear in tension and compression: he said, "Compression is only dangerous on account of the lateral bulging which it causes," and therefore "a prism may carry four times more by compression than by tension." He did not think that assertion was well founded, even for a material which bulged; but materials had mostly to be dealt with which did not give way in short prisms by bulging, but by shearing, and for them this conclusion would not apply. Mr. Pole had pointed out that this conclusion was not true in practice, and he wanted to show why it was not. M. Gaudard had given part of M. de St. Venant's investigation of the torsion of prisms, but he had not pointed out a general

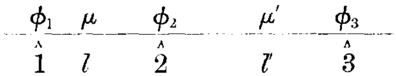
[1] *Vide* "Treatise on Natural Philosophy," by Sir W. Thomson and P. G. Tait. Vol. i. p. 521. Oxford, 1867.

conclusion which M. de St. Venant drew from his analysis, viz., that, contrary to expectation, for all the simple cases he had investigated, in prisms which were twisted, the greatest strain was not at the salient angles, but at those points of the boundary which were nearest to the centre. Sir W. Thomson confirmed this, but added a more general conclusion:—"A solid of any elastic substance, isotropic or aeolotropic, bounded by any surfaces presenting projecting edges or angles, or re-entrant angles or edges, however obtuse, cannot experience any finite stress or strain in the neighbourhood of a projecting angle (trihedral, polyhedral, or conical); in the neighbourhood of an edge, can only experience simple longitudinal stress parallel to the neighbouring part of the edge; and generally experiences infinite stress and strain in the neighbourhood of a re-entrant edge or angle; when influenced by any distribution of force, exclusive of surface tractions infinitely near the angles or edges in question."[1] Sir W. Thomson pointed to the well-known practical conclusion, that a re-entrant angle must be rounded off, and this remark applied to axles and tension bars turned with square corners. Mr. Unwin had noticed, ten years ago, that a bar recessed with square corners and broken by tension, gave way at the corner with much less load than a similar bar with the corners well rounded.

Mr. J. M. HEPPEL wished to express his high appreciation of the merits of the Paper under discussion. He thought it chiefly remarkable for its suggestiveness, directing, as it did, the attention of Engineers to many branches of mechanical science, where careful study and thought might be expected to lead to sounder theory, and, consequently, to improved practice. It was scarcely possible to speak generally on a Paper which treated of so great a variety of subjects, and he should, therefore, confine himself to that part of it which referred to continuous beams, to which he had for some years paid attention.

A somewhat remarkable theorem was the basis of all modern calculations of the conditions of equilibrium of these structures.

In the diagram let 1·2, 2·3 represent any two consecutive spans



of a continuous beam, the lengths of which were l and l', and the loads per unit of length μ and μ'; let the bending moments over the points of support be represented by ϕ_1 ϕ_2 ϕ_3, and let it be supposed that the beam was originally straight and of uniform section

[1] *Vide* "Treatise on Natural Philosophy," by Sir W. Thomson and P. G. Tait. Vol. i. p. 550. Oxford, 1867.

throughout, and that the points of support ranged in a straight line. Then the relation between the bending moments $\phi_1\ \phi_2\ \phi_3$ would always be expressed by the following equation:

$$8\,(l+l')\,\phi_2 + 4\,l\,\phi_1 + 4\,l'\,\phi_3 = \mu\,l^3 + \mu'\,l'^3.$$

This equation, though not altogether unknown in England, had acquired a much greater celebrity on the continent, where it was universally recognised by the name of the "Theorem of the Three Moments."

Mr. Heppel proceeded to point out the mode of using this equation, either in its simple form, or with certain modifications to suit varying conditions, to the determination of the stresses and deflections at all points of a continuous beam. This had all been fully explained by him in a Paper laid before the Institution some years ago.[1] He also noticed the several steps by which the theory of this subject had been brought to its present state of perfection in France and in England; but as these were given, so far as regarded the former country, very concisely and completely in two original letters from M. Bresse, Professor of Applied Mechanics at the "Ecole Impériale des Ponts et Chaussées," which were annexed, it might be sufficient to refer to those letters, except so far as concerned M. Bresse himself, of whom he remarked that he

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[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xix., p. 625. In this Paper, as printed, the following errata occur, which it is desirable to correct. In equation (8), last term of right hand number, for $\mu_{25}\,l^5_{23}$ read $\mu_{23}\,i_2\,l^3$. In equation (10), for $21\,P_1\,.\,l_{12}$ read $24\,P_1\,l_{12}$. In equation (17), for $4\,\phi_2 + \phi_1 + \phi_3 + \frac{3}{8}l\,(W_{12} + W_{23})$, read $4\,\phi_2 + \phi_1 + \phi_3 = \frac{3}{8}\,l\,(W_{12} + W_{25})$. In the calculation of the Britannia tube (p. 642), for $t_2 = -\,0\cdot00409$ read $t_2 = -\,0\cdot00407$.

Equations (9) and (10) as above corrected, and (13), (14) and (15), though correct, may perhaps be more conveniently replaced by the following, which are cleared of the quantity P_1 :—

For (9).

$$y = \frac{1}{24\,E\,I}\left[\left(12 - 4\frac{x}{l_{12}} - 8\frac{l_{12}}{x}\right)x^2\phi_1 + \left(4\frac{x}{l_{12}} - 4\frac{l_{12}}{x}\right)x^2\,\phi_2 - \left(\frac{l_{12}}{x} - \left(\frac{l_{12}}{x}\right)^3 - 1\right)x^2\,\mu_{12}\right]$$

For (10). $y = \dfrac{l_{12}^{\,2}}{384\,E\,I}\left(5\,l_{12}^{\,2}\,\mu_{12} - 24\,(\phi_1 - \phi_2)\right)$

" (13). $x = \dfrac{l_{12}}{2} + \dfrac{\phi_1 - \phi_2}{l_{12}\,\mu_{12}}$

" (14). $\phi = \left(1 - \dfrac{x}{l_{12}}\right)\phi_1 + \dfrac{x}{l_{12}}\,\phi_2 - \dfrac{x\,(l_{12} - x)}{2}\,\mu_{12}$

" (15). $x = a \pm \sqrt{a^2 - 2\,\dfrac{\phi_1}{\mu_{12}}}$, where a is the value of x found from (13).

considered M. Bresse's name should stand first amongst those who had contributed to perfect this method, after its original discoverer, M. Clapeyron. M. Bresse, though as he subsequently found slightly anticipated in this particular part by M. Bertot, cleared M. Clapeyron's equations of some encumbrances which made them troublesome to apply to calculation, and thus obtained independently, the general equation above referred to as the "Theorem of the Three Moments." He did, however, much more than this: the theorem in that form was only correctly applicable under the special circumstances of original straightness both in the beam and in the line of supports, and of uniformity of section throughout. M. Bresse extended the theorem to take in the variations of level of the supports, the beam being supposed originally straight, or the precisely equivalent hypothesis more usually adopted amongst English engineers of a departure from straightness in the original form of the beam, the line of supports being straight. He also extended the equation to the case of a load distributed in any given manner, replacing the simple algebraical terms $\mu\, l^3$ and $\mu'\, l'^3$, which applied to uniform distribution, by definite integrals involving the element of load as a function of the distance from one of the points of support. As regarded this latter generalization, Mr. Heppel had not been able to find that any great practical use had been made of it, even by its Author. He believed indeed that in most cases it would be comparatively unimportant. On the other hand, the former one was of the utmost importance, and practically most essential to be taken account of. Through neglect of this condition, which in England was usually known as imperfect continuity, MM. Molinos and Pronnier, in repeating the calculations of the stresses of the Britannia Bridge, had necessarily arrived at results differing widely from the truth. This was the more to be regretted, as they appeared to have led M. Clapeyron, in a Memoir presented to the French Academy in 1857,[1] to make some strictures on that work, which would probably have been much modified, or altogether omitted, if the real state of the case had been known to him.

Having brought the general theory of the subject to this point, M. Bresse applied himself to its development into rules and tables conveniently applicable to practice. He investigated the nature of the curves, which were the envelopes of the ordinates expressing the maximum bending moments at all points of the beam, and calculated tables for a great variety of cases comprising all those of most frequent occurrence. These rules and tables were adopted by an Imperial Commission, of which M. Bresse was a member, and had a character, in reference to such structures in France,

[1] *Vide* "Comptes Rendus Hebdomadaires des Séances," tome xlv., p. 1076.

equivalent to that of the regulations of the Board of Trade in England.

The only point in which M. Bresse seemed to have fallen short of the degree of generality which had been obtained by some others, was in not taking into account the variation of section from one span to another. This, however, was a condition the neglect of which entailed much less serious consequences than that of the one just referred to; and it would be seen that M. Bresse pointed out in his first letter that this generalization was, in fact, so direct and simple a consequence from what he had given, that it might almost be said to be included in it, though the necessity for expressly presenting it had not occurred to him. This was, indeed, easy to be conceived, as it would be found that in France the spans of continuous girders were usually so regulated as to require very little variation in the mean section between one span and another.

A few words, perhaps, ought to be said on a work on this subject, the second edition of which was published by M. Belanger in 1862; where, starting from first principles, he demonstrated the theorem of the three moments in such a form as to provide both for variation of section from span to span, and for imperfect continuity, bringing it, in fact, to precisely the same degree of generality as would be found to have been given to it by Mr. Heppel in the Paper already referred to.

In England little or no attention had been paid to this subject till 1843, when Professor Moseley published his work on the "Mechanical Principles of Engineering and Architecture." In the Fifth Part of that work, which treated of the strength of materials, several important cases of continuous beams were discussed; and, though the method of M. Navier, which was the only one then known, was not expounded in its most general shape, enough was done to suggest the mode of applying it to most cases where it could be used with advantage.

About 1852 the attention of Mr. Pole became directed to this subject; first in reference to the bridge over the Trent at Torksey, consisting of two continuous spans of 130 feet each. In investigating this case, Mr. Pole had to take into account the variation of load on the two spans, according as the rolling load covered both or only one; and, although the general method of M. Navier provided for this circumstance, Mr. Pole no doubt had to devise for himself the method he employed, as his previous knowledge of the subject seemed to have been derived from Professor Moseley's work. About the same time, however, he had to deal with a case of much greater magnitude and intricacy—that of the Britannia Bridge. In this case it was absolutely necessary to take into account both the variation of section from the small span to the large one, and the imperfect continuity over the centre pier. Both these conditions were introduced with perfect accuracy into the equations employed, and to Mr. Pole must the credit be considered due of having, for the first time, arrived at these important generalizations of the theory as previously understood.

The sequence of events now obliged him to refer to some things which had been done by himself. In 1858, being then in charge of the works of the Madras Railway in India, he had occasion to investigate the conditions of equilibrium of a bridge of five continuous spans over the River Palar. Having only at his disposal the works of Professor Moseley and Mr. Edwin Clark, the latter containing Mr. Pole's researches, he found himself unable to extend the method of Navier, which was there employed, to the case in question, and was obliged, exactly as M. Clapeyron had been on a similar occasion nine years before in France, to cast about for some more manageable method.

It occurred to him, as it seemed to have occurred to M. Clapeyron, that if only the bending moments over the points of support at the extremities of any span were known, all the conditions of this span would be known just as if it were an independent beam. Following this clue, he was somewhat more fortunate than M. Clapeyron had been in eliminating other unknown quantities from the fundamental equations; and ultimately arrived at that which he had already explained as the "Theorem of the Three Moments." That was sufficient for his immediate purpose, as the beam in question was originally straight throughout, and of uniform section, and the bearings ranged in a straight line, conditions to which this theorem in its most simple form was strictly applicable; but being under the impression that he was using an entirely new method, and having besides to satisfy others as well as himself of its correctness, he wished to check its results by comparison with some well-known case. The best possible one was the Britannia Bridge, which was at hand for reference; but in order to deal with this, it became necessary to generalize the fundamental equation, so as to take in the variation of section, and the imperfect continuity. Having the advantage of Mr. Pole's treatment of these conditions, this presented no great difficulty, and resulted in an equation (No. 8 of the Paper before mentioned) by the aid of which all Mr. Pole's results were reproduced, and the accuracy of the process thus verified. This equation, as had been mentioned, was, allowing for difference of notation employed, precisely identical with the general equation given by M. Belanger in the work before referred to.

He believed that he had now stated the precise condition of the present knowledge of this important subject in France and in England; and he would, in conclusion, briefly advert to one acknowledged defect with which it was encumbered, and which it would be desirable, if possible, to remove. This was the necessity of supposing the section, or rather its moment of inertia, constant for each span. The conclusions as to the various stresses arrived at by this hypothesis ceased to be absolutely true when any variation of section was introduced; and as this, from obvious motives of economy, was always done, at least in the case of large structures, some uncertainty must always exist as to the amount of error which might thus have been committed. Mr. Heppel had been induced, by the observations of the Author of the Paper, to redirect his attention to this matter, and was not without hope of bringing this circumstance within the province of exact calculation, as well as that of variation of load. He had reason to believe that the result of a rigorous investigation of the effect of these conditions would be to confirm Mr. Pole's view, that the error committed by taking their average value for each span, and considering this as constant, would be too small to be of any practical importance. If so, it would, as it seemed to him, be very satisfactory to have removed a doubt which must in the meantime attach to all the results obtained, and to have converted what might be called a sagacious conjecture into a demonstrated fact.

The following were the literal translations of the letters Mr. Heppel had received from M. Bresse :—

"To Mr. Heppel, Engineer.

"*Chateau la Valliere* (*Indre et Loire*),
"*October* 17, 1869.

"Sir,

"I hasten to reply to your letter of Oct. 12 received in the country; but I beg you will excuse me if I fail to do so with all the precision desirable, as I have no documents to refer to, and am obliged to trust entirely to memory.

"The first researches of M. Clapeyron on continuous beams date from 1849, and have reference to the Pont d'Asnières over the Seine. This engineer had devised a method in which he employed as auxiliary unknown quantities, the bending moments, and the inclinations of the neutral axis over the piers. As it did not occur to him to eliminate the second category of these quantities, he did not arrive at once at the Theorem of the Three Moments. He first discovered this in 1857; and I, for my part, found it out at about the same time.

"We had, however, both been anticipated by M. Bertot, a French civil engineer, who published the same theorem in June, 1856 (I believe), in the bulletin of the Society of Civil Engineers of Paris. Still, in my opinion M. Clapeyron is the true Author of the discovery, as he furnished the parent idea in introducing into the calculation the bending moments over the piers. M. Bertot, who was acquainted with the labours of M. Clapeyron, had only to make a partial elimination of certain quantities between equations furnished by the latter.

"My ignorance of the English language (which I much regret) has prevented me from becoming acquainted with your Memoir on this subject published in December, 1859. Otherwise I should certainly have noticed it in my work.

"As you justly observe, the equation quoted in your letter has in a certain point of view more generality than the corresponding fundamental equation given at the commencement of my third volume, as you suppose the section variable from one span to another. At the same time, if you examine my demonstration you will see that it adapts itself almost without alteration to this hypothesis, which I admit did not suggest itself to my mind. It is only necessary to modify very slightly the end of the calculation in the elimination of the quantity which I denote by θ_2. Instead of making the elimination between the equations—

$$\varepsilon\,(y_1 - y_2 - \theta_2\, a) = X_1 \frac{a^2}{6} + X_2 \frac{a^2}{3} + \frac{1}{2}\int_0^a (a - x)^2 \;\frac{d\,\phi\,(x)}{d\,x}\; d\,x\,;$$

$$\varepsilon\,(y_3 - y_2 - \theta_2\, a') = X_3 \frac{a'^2}{6} + X_2 \frac{a'^2}{3} + \frac{1}{2}\int_0^{a'} (a' - x')^2 \;\frac{d\,\omega\,(x')}{d\,x'}\; d\,x',$$

we should put ε' for ε in the second, and thus find—

$$6\left(\frac{y_1}{a} - y_2\left(\frac{1}{a} + \frac{1}{a'}\right) + \frac{y_3}{a'}\right) = X_1 \frac{a}{\varepsilon} + 2\,X_2\left(\frac{a}{\varepsilon} + \frac{a'}{\varepsilon'}\right) + X_3 \frac{a'}{\varepsilon'},$$
$$+ \frac{3}{a\,\varepsilon}\int_0^a (a - x)^2 \frac{d\,\phi\,(x)}{d\,(x)}\,d\,x + \frac{3}{a'\,\varepsilon'}\int_0^{a'} (a' - x')^2.\frac{d\,\omega(x')}{d\,x'}\,d\,x'.$$

This equation would then be a further generalization of yours.

"To complete as far as possible my answer to your request for information, I may tell you that the researches of M. Clapeyron have never received more than a very incomplete publicity. Working as Engineer to a Company, he made reports or drew up notes which were communicated to various persons without being actually published. I have only seen as coming from him an autograph Memoir presented to the Academy of Sciences of Paris in September, 1857.

"I believe that his first method is reproduced in the work of MM. Molinos and Pronnier on Metallic Bridges.

"I am, Sir,

"Your obedient servant,

"Bresse."

"To Mr. Heppel, Engineer.

"*Paris, November* 23, 1869.

"Sir,

"I thank you for the pamphlet which you have been good enough to send me. I shall endeavour to study and profit by it; but my ignorance of English will make this rather difficult.

"As to my letter of Oct. 17, I see no objection to its publication if completed. I wrote to you (as I said) from the country, without any information at command; and, besides this, the details I entered upon as to the question of priority between MM. Clapeyron and Bertot made me forget some labours of importance.

"The following shows in chronological order what has to my knowledge been done in France on the question of continuous beams, subsequent to Navier and other persons who have followed in his steps:—

"1849 to 1855. M. Clapeyron treats the question and conceives the happy idea of taking as auxiliary unknown quantities not the reactions of the points of support (as Navier had done) but the bending moments over the piers. He also introduces the inclinations of the axis of the beam at the same points. The labours of M. Clapeyron receive hardly any publicity, and are scarcely known except by the engineers of the 'Compagnie des Chemins de Fer du Midi (en France.)'

"1855. M. Henri Bertot, Civil Engineer, who was acquainted with M. Clapeyron's method, conceives the idea of eliminating the inclinations over the piers. He arrives thus at the 'Theorem of the Three Moments' (See 'Comptes Rendus de la Société des Ingénieurs Civils de Paris,' volume de 1855, pages 278 et suivantes).

"1857. M. Clapeyron and I, each for himself, rediscover the Theorem of the Three Moments, without being aware of the work of M. Bertot (See 'Annales des Ponts et Chaussées,' 1860, 2me semestre, pages 405 and 406). At the end of this same year M. Clapeyron presents to the Institute a lithographed Memoir (little known) on continuous beams.

"1860. Two French 'Ingénieurs des Ponts et Chaussées' attached to the 'Compagnie des Chemins de Fer Russes,' MM. Edouard Collignon and Piarron de Mondesir, each publish a Memoir, the first at St. Petersburg, the second at Paris. These Memoirs have many points in common, but M. Piarron de Mondesir, though not the first to publish, originates the most. He has been the first to give general formulæ for finding the reactions of the supports as well as the bending moments and shearing forces over these points. He has also studied the distribution of moving load, which occasions the greatest stresses: and has demonstrated several theorems on this subject.

"1861. I produce nearly completely the matter of the third volume of my course.

"1862. I communicate this work to the Academy of Sciences (See the 'Comptes Rendus').[1]

"1865. I print it.

"1866. The 'Annales des Ponts et Chaussées' contain two Memoirs on this subject, one by M. Renaudot (volume du 1re semestre, page 311), the other by M. Albaret (volume du 2me semestre, page 53). These authors have worked without knowledge, or with a very cursory knowledge, of my researches. Their publications coming after mine lost, as they themselves have acknowledged, the greater part of their interest. Still, some original things are to be found there, as particularly in the Memoir of M. Renaudot the idea of simplifications to be introduced in the envelope curve of the moments: in that of M. Albaret the influence of the hypothesis of a constant section, an hypothesis not generally in conformity with the reality.

"In this very rapid enumeration I have simply mentioned my own labours without entering into any detail regarding them more than I did in my former letter. It is unbecoming to speak of oneself, and one is not a competent judge in the case. I believe, however, that on the theory in question I have effected some advances which are not without importance. I hope, therefore, that you will speak of them. As to what mention they merit, I leave it to your appreciation, being convinced by anticipation of its courtesy and justice.

"I have the honour to remain, &c., &c.,

"BRESSE."

Mr. BARLOW drew attention to the theoretical ratios, or relations, which existed between the tensile, the compressive and the transverse resistances of materials, as described in the Paper. The question he wished to raise was how far those relations, stated as the

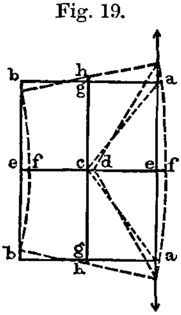
[1] *Vide* "Comptes Rendus," 1862, tome liv., p. 912, et tome lv., p. 388.

result of the theory now propounded, agreed with those which practical engineers found to subsist. He would first consider the relations between the tensile and the compressive elements. It was stated in the Paper that the relation in a body which yielded by bulging at the side was as 1 to 4. Now wrought iron was a substance with which all were familiar, and that body yielded under pressure by bulging; yet it was well known that, in using it within the limits of elastic resistance, the ratio of the tensile to the compressive resistance was, in fact, 1 to 1, and not 1 to 4, as this theory indicated. Mr. Pole, in making the translation of the Paper, appended a note to the effect that this statement required considerable modification in practice. The multiple 4 seemed a large modification; and he drew attention to it more particularly, because the newly-adopted material, steel, was on the eve of being introduced into structures, and it was important not to begin with a wrong idea of what that body could bear in compressive resistance as compared with tensile resistance. Still further he was induced to call attention to this point, because Sir W. Fairbairn, in a Paper in the "Journal of Science" for January, 1869, gave a series of experiments, from which he deduced a ratio of 2 to 1. In those experiments the tensile strength had been taken as that which broke the steel, and as a test of the compressive resistance, small columns 1 inch in length, and $\frac{3}{4}$-inch in diameter, were employed, upon which a weight of 100 tons had been placed; the effect of which was to reduce the columns to two-thirds their original length. But the weight might have been increased by another 100 tons, and the only effect then would have been that the length would have been reduced, and the sides swelled still more; therefore there was no comparison between the resistance of compression, so arrived at, and the resistance of tension. Mr. Barlow, in conjunction with Capt. Galton and Mr. G. Berkley, had lately been conducting a series of experiments on steel, and there was no doubt that the resistance of steel in compression bore the same proportion to the resistance of tension within the limits of elasticity as wrought iron. If, therefore, it were required to make a steel girder, the ratio of its flanges should not be 2 to 1, as Sir W. Fairbairn suggested, or 4 to 1 as might be inferred from the Paper, but 1 to 1 as in a wrought-iron girder.

The next point he would direct attention to was the transverse strain, as ascertained from the tensile resistance. The theory set forth in the Paper was that of Leibnitz, and might be termed the fibre theory. By this a beam was supposed to be composed of a mass of detached fibres, which were described as being unconnected, and exercising no mutual lateral action upon each other. The Author of the Paper said, "This voluntary error is excused, not only for the sake of simplification, but also because it appears to be of a kind which may be neglected in the presence of certain accidental irregularities." But he believed that it was a serious error to omit the lateral action of the fibres; for, if a bar of cast iron was broken by transverse force, it would be found to bear more than double the strain indicated by theory. There was an anomaly in this, which was attempted to be explained at one time by supposing that, in a beam under transverse strain, the neutral axis rose to the concave side. Another mode of accounting for it was by supposing that, when the strain was at right angles to the length, the planes at right angles to the length of the beam became curved instead of straight.

In 1855 he conducted a large series of experiments,[1] upon beams of sufficient size, for determining practically where the neutral axis was. It was then found that the neutral axis remained constant at the centre of gravity of the section, whether of wrought iron or of cast iron, both of which were tried; and further, that the extensions and compressions proceeded in direct arithmetical ratio from the neutral axis till they came to the surface of the beam. Both suggestions set up in explanation of the difference between practice and theory, therefore, fell to the ground. He attributed the difference to the omission in the theory of the lateral action of the fibres, because this supposed condition of the fibres being unconnected was one which did not really exist. One of the difficulties of the investigation lay in the fact that, when a beam was subjected to transverse stress, three conditions came into operation at once, viz., tension, compression, and flexure, neither of which could subsist without the others. It was a desirable thing to separate these effects. He therefore had a series of plates made so that he could apply tensile strain along the edge without any compression on the other side. From this experiment two effects became visible: one was that the mean length of the plate was extended from $g\ g$ to $h\ h$ (Fig. 19). That was tensile work: next the surface $e\ e$ was moved forward to $f\ f$, and the plate became a curved plate. The effect of the tension on the one edge of the plate caused that side to be so much more extended than the centre, that it produced compression on the opposite side of the plate. The amount of tension on one side was double the amount of compression on the other side. Thus the force applied was called on to perform two different descriptions of work; the one was moving the section forward, and the other extending the plate, and whatever force was required for the

Fig. 19.



[1] *Vide* Phil. Trans., 1855, p. 225.

one, was so much taken off what it could do with the other. In these experiments the forces applied along the edges of the plates did not confine their action to the fibres to which they were applied, as would have been the case if the fibres had been disconnected; but the lateral action of the fibres, or molecules, caused the whole plate to act as one elastic mass, the tendency being to draw the centre of gravity of the section into the line of the strain. Similar effects arose in a solid beam.

Fig. 20.

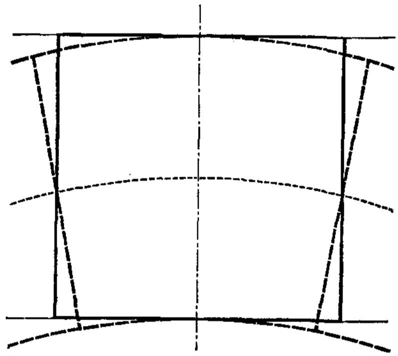


Fig. 20, represented the centre section of a solid rectangular beam supported at the centre, and loaded at the ends. When the load came upon the beam, it would be deflected as shown. The effect, in fact, was this:—first, there was extension in the upper part; secondly, of compression in the lower part; and, thirdly, the moving down of the two ends, and deflection. If the centre part

Fig. 21.

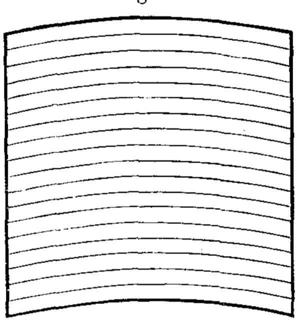
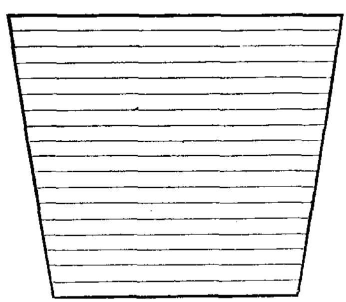


Fig. 22.



of the beam were composed of unconnected fibres, they might be bent down as in Fig. 21, leaving the ends *parallel*;—or the angles of the ends might be varied, that was to say, extension and compression might take place, leaving the *fibres straight*, as in Fig. 22; and it was, therefore, clear, that the two operations were distinct, and the forces required to produce them were separate forces. In the solid beam, the fibres were united into one mass. Lateral action necessarily ensued, and the extension, compression, and flexure could no longer be separated. But the circumstances of these actions occurring simultaneously did not alter the conditions of separate forces being required for each. The theory of Leibnitz only took into account the forces necessary to produce tension and compression, and it omitted those required to produce deflexion or curvature.

He had shown in his Papers to the Royal Society, in 1855 and in 1857, that if the element of lateral resistance of the fibres were introduced into the calculation, all the anomalies in the present theory, as between the tensile and the transverse resistances, vanished. He had experimented upon beams having a great variety of section; square, rectangular, round, and open beams; square beams broken on their angles, also H-formed beams broken on the edges, and with the flat sides downwards; and in every case the tensile resistance could be derived from the transverse resistance, and *vice versâ*. Therefore, with all deference to the theorists of the present day, he could not but express his conviction that the lateral action of the fibres, or molecules, was the missing link in the theory of solid beams.

His father, who, as was well known, had studied and written on the subject generally, took great interest in these experiments. He was slow to convince, but at last became thoroughly persuaded on this question; and in a postscript to the Paper presented by Mr. W. H. Barlow to the Royal Society, in 1857, showed the application of the same principle to beams of unsymmetrical section, similar to those which had been experimented upon by Mr. Eaton Hodgkinson.

Lastly, in Section 16, on the subject of flexure, the Author said, "When the bending moment remains constant for a certain length of the solid, this length bends in the arc of a circle. This case of circular or equal flexure is the only one in which the transverse sections remain strictly plane after deformation: these sections remain normal to the arched fibres, and neither slidings nor lateral pressures are developed between the fibres." On this point Mr. Barlow joined issue. Whether the curve was a circle, or other curve, the plate was bent. If one fibre was more strained than the one adjacent, there must be lateral action. The introduction of this element into the formula just doubled the strength; and this was exactly the extent to which the detached fibre theory was found wanting. Looking, then, at the wide discrepancy between the existing theory of Leibnitz and actual practice, he hoped that theorists would reconsider the whole question, and say what would be the result if the known cohesion and consequent lateral action between the fibres were included instead of being omitted.

Mr. C. Brooke desired to make some remarks, in elucidation of a doubt which had been expressed, in reference to the direction of curvature of the lines of double curvature which represented the displacements of the transverse plane sections of a beam when under the several strains and stresses incidental to flexure; and which intersected the neutral axis at the point of inflexion: that was to say, supposing pairs of these curves to be drawn intersecting each other and their tangents at the point of inflexion, as represented in Figs. 6 and 7, it was said to be doubtful whether the curves widened out from the two straight lines, as they proceeded upwards and downwards, or whether they turned inwards: this did not appear to him a point that need remain in doubt, because a simple reference to analysis would determine it. If that curve was expressed in the form of the equation between the rectangular co-ordinates of molecular displacement, then at the point of contrary flexure, where the curve intersected the neutral axis, the second differential coefficient $\frac{d^2y}{dx^2}=o$; and this coefficient must necessarily change its sign, on the opposite sides of this point: and, accordingly, as it changed from positive to negative, or from negative to positive, there could be no difficulty in determining which direction the curvature took.

Some of the introductory remarks in the Paper expressed what seemed to him an illogical jumble of mechanical ideas. Nothing was more strictly defined than the respective provinces of statics and dynamics. Statics dealt with pressures only, and the effect of those pressures might be estimated in units of pressure; whereas dynamics dealt with the effects of forces producing energy, and the results were measured in dynamic units, or units of work. But the Author said, "External forces are of two kinds: one kind comprises elements directly given, such, for example, as weights." Now weights were not forces, but statical pressures. He proceeded to say, "The other kind consists of reactions, functions of given forces;" therefore "reactions" were "forces." But "reaction" was generally spoken of as that which resisted pressure, and the Author remarked, "In certain cases these reactions may easily be found by the science of statics alone (as, for example, in the case of a beam placed on two supports)." But these "reactions" were not capable of producing motion, and therefore were not "forces;" hence the term "reaction" was obviously ambiguous, and had better be disused. The author continued, "In other cases they will depend on the changes of form of the solid; it is this, for example, which causes the difficulty of calculation in arches." But this was purely a statical question: the stability of an arch depended upon the continuous line of pressure falling within the surfaces of contact.

The final quotation from the Paper he would make was this,— "Or, lastly, it might happen that the body in question may not be in a state of equilibrium, but that its particles may oscillate under variable dynamic influences or forces of inertia." Inertia was not properly a force; it was a mere negation—the negation of energy. A body remained at rest, not in consequence of any force, but because there was a negation of any force which would cause it to move; therefore inertia could not be called a force. Vis inertiæ was an old expression, but it involved mistaken ideas, and had better be avoided. *In all questions relating to mechanics nothing* tended more to the elucidation of the subject, than the separating these two different classes of conditions, viz., those belonging to statics, and those belonging to dynamics. Not only in this Memoir but in other works a confusion of ideas was met with, which resulted from the want of a clearly-defined distinction between these two separate branches of mechanical science.

Mr. R. MALLET said he would offer a few remarks upon the subject of the Paper generally, for to go through each head of so comprehensive a question as that of the strength of materials, might occupy a whole session. In the *résumé* of the history of the successive steps of knowledge of this subject, Mr. Pole had, no doubt accidentally, forgotten to record the name of M. Lamé as one of those to whom the science of one branch of this subject, viz., that of elasticity, was largely indebted. M. Lamé's well-known treatise must be familiar to many of the Members.[1] Reference had been made in the discussion to the extreme desirability of obtaining mean constants for all those most important data, in reference to solids when applied to various uses in construction, such as the mean constants for tensile and compressive strain in cast iron, wrought iron, and steel; and undoubtedly there was nothing that would better reward labour if bestowed upon it, than thus discussing into means those multitudinous results of experiments which were scattered through numberless books and pamphlets, and were often of the most discordant character. A beginning of this labour had been made by M. G. H. Love, a French civil engineer, for some time a pupil at the Ecole Centrale des Arts et Métiers, and Engineer since of some important railways in France. In an able volume[2] he discussed some of the discordant results found in English as well as in foreign books on these subjects, and endeavoured to obtain mean constants from such as admitted of it. These were still very wide of the actual facts

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[1] *Vide* "Leçons sur la théorie mathématique de l'élasticité des corps solides." 2e édition. 8vo. Paris, 1866.

[2] "Des diverses Résistances et autres propriétés de la fonte du fer et de l'acier." 8vo. Paris, 1859.

of the subject even in the commonest cases; authors, for example, were found affirming the tensile resistance of cast iron at from 14 tons per square inch down to 2 tons. Without in the least disparaging the value of mathematics when properly applied, Mr. Mallet could not suppress his belief that on the subject of the Resistance of Materials there was much more to be done in advancing practical knowledge, by means of well-devised and well-conducted experiments carefully, logically, and rigidly interpreted, than by the application of hypotheses and mathematical reasonings, many of which simply concealed real ignorance; and while the Institution was indebted to Mr. Pole who had made available, in an English form, this Memoir of M. Gaudard's, he thought the choice of this particular Memoir was in some degree an unhappy one. The Author had, like most mathematicians, assumed hypotheses rather for the facilities they gave to the play of formulæ, than having rigid regard to whether they were true in nature. Mr. Barlow had very well put one case of this kind during the discussion. In short, any mathematical treatment of the resistance of materials which started with any hypothesis other than, or short of, this—that every particle of a material solid attracted every other particle in all directions,—and did not follow that out mathematically, must always fail to lead to sound results, or to such as could be safe in practice. And it must be confessed that a large proportion of the experimental data published by experimenters both in England and, though in a less degree, abroad, were calculated to mislead, owing either to errors in experiment or to erroneous interpretation. This often arose from the slovenly way in which the experiments had been devised or had been made, and the equally slovenly way in which they had been interpreted. In making so grave an assertion, he felt called upon to give one or two illustrations in support of what he had said, though, in doing so, he felt himself in the unpleasant office of a fault-finder. If the countless experiments that were given in the books on the very commonplace subject of the resistance to direct tension of a bar were examined, it would be found that regard was not usually paid to the form of section of the bar. Whether that were round, or square, or merely a long flat strip or ribbon of metal, provided the area of transverse section was the same, the tension per unit of section was supposed to be the same under equal loads. Now this was far from being true. M. Tresca, with others, had experimentally proved what had been known *à priori* to all who admitted that in nature there was no break of continuity, that gases passed without break into liquids, and liquids into solids. Now when a cylindrical bar of iron was elongated by tension, its particles flowed with different velocities, just as those of water did when flowing through a pipe; the central particles were pulled out more readily than the circumferential ones. It was a well-ascertained fact, that if a cylindrical wire was elongated, so that its diameter was diminished, the volume of the unit length of the wire was increased by the pulling out; in other words, the specific gravity of the wire was diminished.

Fig. 24.

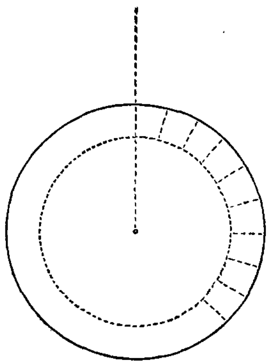


Fig. 25.

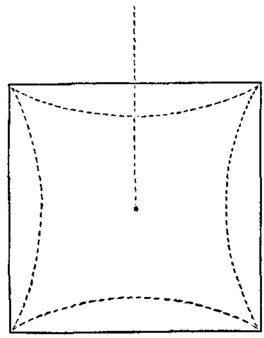
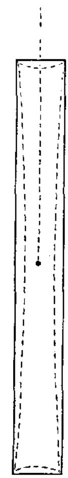


Fig. 26.



The explanation appeared to be that as the outer layers of the cylindrical bar or wire (Fig. 24), being a continuous arch, did not yield inwards towards the axis of the bar to the same extent as the layers nearer the axis, so the particles tended to pass further asunder in direction orthogonal to the axis than before tension was applied, and so the volume was increased, or specific gravity diminished; but the extent of this effect, which modified also the tensional resistance, varied with bars of different shaped sections ; thus a square bar tended to become of the section shown in Fig. 25, and a flat ribbon of that in Fig. 26. The result and the defect of many of such experiments on tension was, therefore, that they were not strictly comparable. If experiments on tension were to be made so as to be comparable, not only must all other points be similar, but the section of all the bars submitted to tension must be the same, or at least similar. That was an example of one class of experiments to which he had applied the epithet slovenly; viz., experiments in which one or more important conditions were neglected. There was another class of experiments, which in the end often served to puzzle rather than enlighten, namely, where the experiments themselves, perhaps, were well made, but the results were illogically, or loosely, or far too generally interpreted. In giving an illustration of this class, and bringing forward in connection with it the honoured name of Sir William Fairbairn, his personal friend for thirty years, he was certain the Institution would acquit him of any disrespect for one who had laboured so successfully for the advancement of professional knowledge. Dr. Fairbairn's experimental researches as to the resistance to collapse of the tubes of boilers when exposed to external pressure, originally appeared in the Philosophical Transactions for 1858.[1] The results as announced, and copied without contravention from book to book, were that the resistance of cylindrical tubes was inversely as their length, inversely as their diameter, and inversely as the 2·19 power, or practically as the square of the thickness. Now, as thus generally stated, neither of those propositions was true. A cylindrical tube, if language meant anything, meant a *perfect* and uniform hollow body, whose solidity was that generated by the motion of an annulus parallel to itself along the central axis, and, of course, it was a tube with free and open ends: that being so, if a unit of length of such a tube was taken, the resistance to external pressure would simply be directly as the cross section. In a plane parallel to the axis, the perfect cylinder had no tendency to deformation, and any dimension except thickness was immaterial. The resistance had nothing to do with the length; whether the unit were 1 foot long or 1,000 miles, the resistance per unit was the same, and simply dependent upon thickness. The experiments of Sir W. Fairbairn, however, were made by submitting to external pressure a tube of a certain length, *encastré* at both ends, and the tube was not a perfect cylinder; its resistance to deformation was, therefore, partly due to the rigid transverse *encastrement* of the ends. The longer such a tube was between such extraneous supports, the easier it would be deformed in consequence of its original form not being a perfect cylinder; and the larger the diameter, and the more the tube originally departed from a true cylinder, the more easily would it be deformed and collapse. But these were not the conclusions deduced from those experiments: the conclusions drawn were wrong, because more general than the premises warranted. If it had been stated in the case of an actual internal boiler-tube or flue, as in that of a Cornish boiler, that the longer it was between the ends *encastré* to those of the boiler-shell, the weaker the flue would be; and the larger its diameter, if it had any, the least, initial departure from a true cylinder, the weaker it would be,—then those statements would be true. That seemed to him to belong to the class of experiments, in themselves good and valuable, but misleading, because the conclusions or results were given in an illogical form which everybody did not see through; and those who did not, were likely to be led into grave errors in practice. Then there was a third class of experiments very

[1] *Vide* Phil. Trans., vol. cxlviii., p. 389.

prevalent, in which part of a question only was experimentally asked of Nature, and yet her answer treated as though the whole had been asked; and here again he was obliged reluctantly to take an illustration from experiments of their much-respected Member whose name he had referred to—namely, those in which he had given the results of experiments on transverse pressure alternately applied upon girders and relieved at intervals. The experiments themselves though few were valuable; but they were called experiments on "repeated vibrations of girders." Now alternate loading and unloading was not vibration. Vibration was something which generated a wave in the solid acted on. If he moved his hand slowly backwards and forwards in the air, he merely caused the air to pass right and left in following the motion of his hand; but if he continued to increase the rate of that motion up to a certain velocity that the air did not follow by as quickly flowing into a new place, then a wave or vibration began to be produced—below that velocity there was none. So, if a girder was pressed up and down by alternations of loading and unloading, that was not vibration, although there might be elastic oscillations both after each loading and after each unloading. But in this case the law of such oscillations was well known; what was wanted to be known was, the effect of the repeated loading and unloading, *i. e.*, of repeated deflections to a given extent, clear of all perturbations of intermediate oscillations. Now, to arrive at any such result of a general character, not only must attention be paid to the "work done" upon the girder in a given time, but to the relation of the factors M and V, of which the work $\frac{M V^2}{2}$ was made up. There must be not only this as well as the dimensions, &c., of the girder, constant, but the relations must be varied between M the load, and V the velocity of its application, and also the number of such applications in a unit of time.

Suppose, for example, the value of V was increased up to that which equalled the "pulse period," or velocity of sound in the material of the girder: still reducing M so as to have the work constant, the effect would be that the very first time the load, or even one of much less value, was applied, it would break the girder; for there would not be time for the particles of the iron to act, and the girder would be in the same condition as if struck with a cannon shot. Therefore, the thing to be determined was this—given the statical load less than the breaking-load, what was the velocity of its application such that, after a certain number of alternations, the cohesion of the material of the girder should become injured or destroyed? or—given the velocity and ratio of the applied load to the statical breaking-load, how many alternations would produce the same effect, deflection being, of course, observed in all cases? If that was not ascertained, the result was of no real value; accordingly the result enunciated of the experiments he had last referred to was isolated and fragmentary.

He could not refrain from making one remark with reference to this matter of vibration. It was one of those things continually talked about in a way so loose, that it reminded him of the manner in which people frequently talked about the value of an invention,—"There is a patent for it, it must be good;" or the way in which a certain class of people summarily accounted for anything they did not understand, and yet would not boldly admit their ignorance:—"It is an electrical phenomenon." So it was continually said that "vibration" had done this and that to iron, &c., but without any exactitude of facts or proof that vibration had been concerned at all. He would venture to affirm this general proposition: that vibration in elastic solids really produced no effect, however long continued, unless there was an extraneous stress constantly acting to near the elastic limit; or unless the range of the vibration itself exceeded the limit of elastic range of the particles; or the velocity of the applied force exceeded that of the pulse period of the given elastic solid; he believed that the truth would be found within that proposition. With respect to the effects of vibration, where the extraneous strains were great, as in the case of the neck of an iron railway axle, which was, as it revolved under its load, continually bent round new axes, the part exposed to tensile strain at one instant being almost directly after exposed to compressive strain. When that was long continued the axle was broken, and the fracture showed more or less change in the molecular structure of the iron, whenever those rapidly changing strains were severe enough to approach very near to the elastic limits of the material; but if the maximum strain were very far within these elastic limits, the axle might be constantly bent thus without undergoing any change, whether the strain were accompanied by vibration or not.

Some remarks had been made during the earlier part of the discussion as to the finding of a surer basis for the elastic limit of structural materials, and a better expression for the elastic modulus of those materials. With regard to the modulus he would observe that, while he agreed with Mr. Reilly that the modulus of elasticity as defined by Dr. Thomas Young led to some anomaly, which had not been removed by the French modification of the definition of the modulus, still Dr. Young was far too acute a man so to define the modulus of elasticity as to involve any real absurdity, and, in fact, there did not seem to be any, although there was an incongruity, in taking the modulus in extension as represented by 2, and that of compression by the reciprocal $\frac{1}{2}$. When the modulus came to be used in calculation, it appeared to him that, notwithstanding this, there was great advantage in the simple form of Young's expression, and that it was better than any more complicated algebraic expression involving logarithmic forms.

Mr. Siemens had made some valuable remarks on the importance of being able to determine in large structures, when professedly of steel, what was steel and what was iron; and he proposed a method of determining this question by reference to the specific gravity of those metals. He was sure credit would be given to him, that, in what he was about to remark, he had no wish to discourage the use of steel, inasmuch as he was one of the earliest advocates for its extended structural use, and was so still. He recognised the full importance of every engineer being able readily and certainly to distinguish in every great structure, such, for example, as that fine one just completed, Blackfriars Bridge, whether the material being put into it, was iron or was steel. Some ready mode of doing that was required. Much might be done to remove doubts by the steel-makers themselves, and at the Hæmatite Ironworks at Barrow-in-Furness, he believed the authorities were prepared to contract for steel plates, and guarantee that, if any one plate failed on test within a certain pre-determined range of tension or extension, they would forfeit a large sum, or give other equivalent commercial assurance. But that was not enough; engineers must have a rapid and accurate means of determining the matter for themselves, and he believed that the test of specific gravity alone, at least, would not answer. It so happened that several years ago, when engaged in those researches on the corrosion, &c., of iron which had been published in successive volumes of the Reports of the British Association, he had occasion to take with exactitude some thousands of specific gravities of cast iron, wrought iron, and steel. He found that the ordinary method of taking the specific gravity by weighing, in air and then in water, was utterly untrustworthy for this special purpose. A rough fragment of iron or steel plunged into water could not be freed of air, even by a vacuum, and it must not be filed, cut with the chisel, or "hammered" smooth, for he found that those operations altered the specific gravity materially, and to an uncertain extent. He believed that a great number of the published specific gravities of British iron, such as those given by Messrs. Hodgkinson and Fairbairn, had been taken by cutting out, by means of the hammer and chisel, a known volume, 1 cubic inch, and weighing it in air. That method, besides being coarse and inaccurate, was for the reasons given fallacious. He had found that the only correct method of obtaining the specific gravities of iron, steel, &c., was this:—to cut out with the planing-machine, without blows, and by a very gentle cut, a cube of the metal the specific gravity of which it was wanted to determine. The surface must be made as smooth as possible, and care must be taken that, in the case of cast iron, there were no concealed blow-holes; and in the case of wrought iron, no included cinder or slag. He then had a small and light glass cylindrical vessel, just able to hold the cube of $\frac{3}{4}$ of an inch on the edge, which was the invariable size employed. The weight of the glass cylinder and its thin, ground-on, plate-glass cover, when the former was filled with distilled water at a temperature of 60°, was accurately determined. Then the cube, after proper preparation, and after having been weighed in air, was immersed in water above which a vacuum was formed, transferred in the same water into the cylinder, covered with the plate, the external surface dried with bibulous paper, and the whole weighed, which afforded all the data for determining the specific gravity. Change of temperature during weighing had no disturbing effect, owing to a peculiarity resulting from the apparatus, but on that and other details he would not enlarge further than to add, that the surface of the cube should be quite smooth and rendered chemically clean. Now it was obvious that these precautions were tedious, belonged to the laboratory, not the workshop; and even when every care had been taken he was satisfied the specific gravity test would often leave the question—steel or iron—still in doubt. M. Phillips, however, had shown a new method of determining, not only the modulus of elasticity, but the limit of elasticity of all metallic and some other substances, of great simplicity and admitting of surprising exactitude, which appeared to offer, just at the right moment, extremely feasible results, if applied to this question—"steel or no steel," as a workshop tool. M. Phillips' Memoir, which was contained in the last volume of the "Annales des Mines,"[1] and had but just reached this country, was probably as yet known to but few of the members.

M. Phillips had been long engaged with questions relating to springs, and their horological applications, and out of these had grown his Memoir "On the application of chronometric springs to the determination of the co-efficients of elasticity, and the limits of permanent elongation of various bodies." In the ordinary balance-wheel and spiral spring of a chronometer, M. Phillips had obtained as an expression for the time of one oscillation (corresponding to one-half vibration of the simple pendulum):

$$T = \pi \sqrt{\frac{A\,L}{M}} \quad \ldots\ldots\ldots\ldots\ldots \quad (1).$$

A being the moment of inertia of the balance-spring,
L its developed length between the attached points at its extremities, and
M its moment of elasticity.

[1] *Vide* tome xv., pp. 65-84. Paris, 1869.

It would be seen that this expression corresponded to that for the half vibration of the simple pendulum:

$$T = \pi \sqrt{\frac{L}{g}};$$

and as by taking a given value for L, say that of the seconds pendulum at London, and causing it to swing in different latitudes, there resulted—

$$T : T' :: g : g',$$

so that it was possible to determine the variation of the co-efficient of gravity,—or, in other words, the relation between gravity and centrifugal force at different latitudes,—so here, by altering the material of the elastic pendulum, *i.e.*, the balance-spring, the modulus of elasticity of the material of which it was formed could be determined.

In a previous Memoir M. Phillips had shown that, if the balance-wheel and attached spring were rotated through an angle, and the corresponding arc being taken as of the circle whose radius was unity, then the shortening of the spiral was given by the equation—

$$i = \frac{e}{2L} a \quad \ldots\ldots\ldots\ldots\ldots \quad (2),$$

in which e was the thickness of the spring in the plane of the spiral, and L its length between the points of *encastrement*.

This equation it was evident afforded the means of determining the limit of elasticity of the material of the spring. But the value of M (in Eq. I.) was given by

$$M = \frac{E \pi d^4}{64} \quad \ldots\ldots\ldots\ldots\ldots \quad (3),$$

d being here the diameter of the cylindrical wire of which the spring was formed (or $d = e$), and E the co-efficient or modulus of elasticity of its material.

Hence, with a given balance-wheel supposed always the same, and any spiral spring, differing in material, the co-efficient of elasticity of the latter was

$$E = \frac{64 \pi A L}{d^4 T^2} \quad \ldots\ldots\ldots\ldots\ldots \quad (4).$$

Now, in order to apply this to the "steel or iron" question, it was only necessary to knock off a corner of the plate, as Mr. Siemens had said, heat the bit to a low red-heat and roll it by hand if necessary, at once into a small cylinder, always of one diameter, pass this then through a few successive holes in the steel draw-plate until it was reduced to a suitable diameter; and from the wire so produced form the spiral, to be vibrated for a known time, in connection with the standard balance-wheel, and T to be observed.

As regarded T the exactness of observation might be increased to any extent by merely increasing the total time of observed vibration—the number of vibrations being recorded by a "counter." Probably in no other form could the dimensions of a solid be so precisely determined, both by measurement and by weighing, as in that of a wire; so that the value of d could be had very precisely, indeed within less than the one-thousandth of a millimètre, as M. Phillips had shown. The moment of inertia of the spiral was also rigidly obtainable.

There remained, therefore, no source of discrepancy, unless it were that of the state of hardness or softness of the material of the wire; and to secure that this should be always comparable, it would probably prove quite sufficient, along with the method of preparation of the wire already indicated, merely to anneal fully the wire when brought within one or two holes in the draw-plate of its final diameter, and then to re-harden it, always to the same proportional amount, by passing it, at the same velocity, through those final holes, the last of which should be pierced through a ruby or other hard gem, as was well known to horologists.

The specific gravity of the wire might be readily and accurately taken by the method for iron cubes already described; and as in the end there were thus obtained three data of difference, viz., specific gravity, modulus of elasticity, and elastic limit, and as both the latter differed very materially for iron and for hardenable steel, and as a fourth condition might be readily added here—viz., the actual tensile breaking strain of the wire,—it would seem that these together afforded the most ample means of determining the physical qualities of, if necessary, every plate in the largest alleged steel structures. For it need scarcely be added that, when once the apparatus necessary was put into its best and most convenient shape, the application of it would become mere routine; and like the operations of the assayer of gold, though these were exquisitely exact, would be capable of being conducted by persons of ordinary acquirements.

Mr. R. P. Brereton noticed that the title of the Paper was "On the present state of knowledge as to the Strength and Resistance of Materials;" but it did not give additional knowledge as to the strength of materials; and although it afforded many theoretical deductions relative to strains and stresses to which materials were subject, and the mode in which they were applied, it gave no information as to what strains structures could undergo, nor in what way they actually failed.

He had no doubt, with such authorities as M. Gaudard and Mr. Pole, that the mathematical theories were right, yet there was not always certainty of the accuracy of printed figures. Books frequently contained material errors arising, not from the inability of mathematicians to give the figures rightly, but from subsequent inattention in revision. In a Paper of a mathematical character, once read before a learned society, upwards of forty errors in mathematical signs were printed in the statement sent out to the members, and which were not corrected till a subsequent volume appeared twelve months afterwards.

The first point on which he would remark was in Section I. of the Paper, in which the Author said, "The elastic change of form interests him (the engineer) less directly, because he knows beforehand that this change is generally very small, and presents no great inconvenience." Elastic change of form was frequently considerable and led to serious consequences; for instance in the use of iron rods, or bolts of different lengths applied together for carrying a load, and in timber used as props, the weights with these elastic materials came almost entirely on the shorter pieces. The sailor was particularly liable to deduce erroneous opinions from this change of form. He might lay out two ropes of equal size, one long and the other short, and imagine that the strains would be equally divided between the parts.

Then, again, the Author said in the same section—"It may happen, also, that a change of form may present in itself a direct practical interest: thus in the testing of a bridge, if the observed deflection agrees with that predicted by the calculation of the elasticity, it will be inferred that the beams do not possess any hidden defect capable of producing an abnormal deflection." That might be so; but a beam which gave no indication of failing would sometimes fail suddenly. An example of this was afforded in the case of the Joiner Street girder bridge of the South-Eastern Railway, crossing the thoroughfare at London Bridge. The bridge had been in use for traffic, when suddenly some of the girders broke down without any predication of the cause, but on examination it turned out that the pins of the diagonal braces had been left out by some oversight. The calculations for that bridge might have been perfect enough, but the accident showed how little reliance could be placed upon previous signs giving indication of an impending failure. What held the girders up so long was unaccountable; several of them did not give way at all.

In another instance, where a railway was about to be opened, the Government inspector examined and would have passed as satisfactory all the bridges, some of them consisting of large wrought-iron girders. They had stood the required tests; but the day after, a ballast train in passing over one of them fell through into the turnpike-road below. No unusual deflection had indicated any danger; and, on examination, inside the box-girder, it was found that a butt plate of the bottom web had been left out and the girder had given way.

The deflection of a girder built of riveted boiler-plates could not be relied upon as giving a proper indication of the strain. Fracture generally took place at a line of rivet holes of a butt joint of the tension member, where the iron might be on the eve of breaking, without the remainder of the plate being subject to such stretching as would produce material deflection.

Under the head of 'Extension' the Author did not appear to attach importance to the force of molecular adhesion. The text, though not quite clear, seemed to imply that one particle derived no assistance from its neighbour in carrying weight; that if in a rod of twenty fibres a ton were suspended from each fibre separately, the result would be the same as if 20 tons were suspended from the rod considered as a whole. But it was important to know that, in practice, individual weights could not be suspended from individual fibres, and that the only means of utilizing the whole of the rod was by laying hold of it or attaching the load at particular points. In the case of bolts these points were at the nuts or heads, where there was an attaching force by friction or adhesion to the adjoining particles, otherwise there could be no shearing of bolt-heads or stripping of the screw threads. The strains upon long bolts or rods or iron plates became more equally distributed amongst the fibres towards the middle of their length, or where farthest removed from the points of application of the loads, and at joints which were indispensable in the building up of structures, some loss of strength ensued from the unequal distribution of the strains. The Author probably referred to that, when he said, the middle of the rod might have greater strength than the ends; for in the middle there was a more uniform carrying of weight by all the fibres than where reliance was placed upon only part of them for distributing the strain amongst their neighbours. He had found that the strength per inch of bolts carrying heavy loads might be much increased by forging the ends, so as gradually to diffuse the strains amongst the fibres of the shank. The Author might have intended whatever was derived from the friction of the fibres when nipped together by the stretching of long rods, particularly where they had a twist, as in the case of cables.

To take the example of a shroud-laid rope and a hawser-laid rope, friction could not be much increased by pulling taut the fibres of the shroud-laid rope; but the hawser-laid one acted in the same manner as the finger tube, consisting of four-threaded spirals, in which the more the ends were pulled asunder the tighter the threads were jammed together. It had been observed in the breaking by tension of iron rods, that the apparent section at the point of fracture had been so reduced by the closing together of the fibres as to carry 50 per cent. greater strain per inch than for the remainder of the rod.

The Author next said in Article 3, "Certain facts seem to establish, that time intervenes as an enervating cause, by giving permanence to elongations, which are at first elastic, and by consequently diminishing the vigour of the material to react against derangements." No doubt time had a material influence upon the permanent strength of materials, particularly where there was motion; but happily failures seldom ensued without warning, which were generally made evident by the gradual increase of sagging or deflection in roofs or beams. He could give an instance of a long continuation of small forces producing extraordinary results. He had carried in a pocket-book for about twenty years, when he was travelling, a paper of pins. These pins did not adhere to their original shape, but became bent at right angles in the middle, all exactly alike, and remained in that form. Now, if one was pressed upon a hard substance, it took considerable force to bend it to a right angle. How, then, was that force applied to a paper of pins in a pocket-book by the mere vibration of travelling on a railway? He found it took seven years, travelling from 500 miles to 1,000 miles a-week, to bend medium-sized pins to a right angle; but even the largest showed bending in about twelve months, and acquired their full bend in about seven years, without subsequent increase. If the material of a pin attained such a form, there might be some weight in the theory, that the continued vibrations of a railway carriage occasionally affected the brains of human beings.

He was glad to find that M. Gaudard was alive to the conclusion that nothing could fail from actual compression. It was one of the few points of the subject that appeared not to be generally understood. Compression usually meant a driving apart of the fibres, or a splitting and wedging asunder of the particles of the material under pressure, and these would probably carry most after being crushed to powder. Water, when confined, had frequently to carry from 300 tons to 400 tons per foot of pressure. Clays and sands carried hundreds of tons per foot, provided the load was put upon them far off from the edges, and by that means the spreading out here spoken of was avoided. He did not agree with the theory of length compared with thickness. The Author gave a length of ten times the thickness up to which the strength of a rod was assumed to be the same as if in tension; and further on, that a rod, if prevented from bulging or bending laterally, would carry four times as much as when in tension, but the strength should have been infinitely greater under these circumstances. In experiments made with large timbers, with lengths of from ten to forty times the thickness, he had found that timber 12 inches square and 10 feet long bore a weight of 120 tons; when 20 feet long it bore 115 tons; when 30 feet long 90 tons; and when 40 feet long it carried 80 tons. At the latter length the resistance was still two-thirds of what it was in the first case; and whereas the length had been increased four times, the strength only appeared to have been diminished as the cube root of the length inversely. Instances occurred of boiler plates carrying heavy weights, where the thickness had been only $\frac{1}{10000}$ of the length. In the Saltash Bridge, with tubes 16 feet in diameter, the thickness was about $\frac{1}{10000}$ of the length. There were diaphragms at intervals; but $\frac{1}{2}$-inch plates 500 feet long sustained the weights put upon them, equal to a pressure of 5 tons per inch.

As regarded the "limits of safety" the Author had assumed that $\frac{1}{10}$ of the breaking weight should be applied to timber and to stone, and $\frac{1}{4}$ to $\frac{1}{6}$ for iron. With considerable experience in the use of timber on a large scale for railway viaducts, it had been found that with proper attention to the joints and framing, it would carry safely up to $\frac{1}{4}$ or $\frac{1}{5}$ of the breaking load. The limitation to the use of these materials was principally from the injuries they were liable to from weather and decay.

As to the forces employed in the shearing of rivets, he did not agree that one edge only would be crushed before the other edge had moved. It might be so to some extent if done by sudden blows, but with a steady force of shearing or punching there was no reason to suppose that one edge would give way before the other.

He had observed, from experiment, that the force required in punching or shearing was in proportion to the surface cut, irrespective of the thickness of the plates. For instance, with a punch 1 inch in diameter it took about 20 tons to punch a $\frac{1}{4}$-inch plate, 40 tons a $\frac{1}{2}$-inch plate, 60 tons a $\frac{3}{4}$-inch, and about 80 tons a plate 1 inch thick. Different diameters of punches did not appear to influence the result. For instance, a $\frac{1}{4}$-inch punch required a force of about 5 tons to penetrate a $\frac{1}{4}$-inch plate, and about 10 tons a $\frac{1}{2}$-inch plate. The forces required with the two punches and the surface sheared were directly as their circumference, this being one-fourth with the smaller punch, whilst the surface pressed was one-sixteenth.

The question of torsion was a matter of importance to civil engineers, principally in connection with the shafts of marine engines and the rudders of ships. No theory that could be applied to the enormous sizes now used was of much value. The great difficulty was to obtain perfect welding of the iron. In the case of the "Great Eastern" steam-ship, where a paddle shaft was required to resist a force of about 10,000 horses applied as torsion at its circumference, it was some time before a sound forging could be obtained 2 feet in diameter, in consequence of the difficulty of welding, and after several years of wear it was found necessary to replace this by another. Torsion in these cases was unavoidable, and was aggravated by alternations of turns and twists. Another instance was found in the failure of the rudder head of the same vessel. The rudder was 9 feet in breadth of blade, and the shaft 10 inches in diameter, and apparently well manufactured, but in a gale of wind the rudder shaft was entirely twisted off. The ends presented the ragged appearance of a couple of Bramah keys, and he was able to push a goosequill its whole length between the staves where there had been defective welding. Rudders were still subject to the same kind of twist, and twenty years ago the balance rudder was introduced to remedy it, but nautical objections had prevented its coming into general use. In all cases of steering by steam or hydraulic gear there was a tendency to aggravation of risk from torsion in these large masses of material.

As to the continuous beam, M. Gaudard, Mr. Heppel, Mr. Pole, and others attached more importance to the perfection of theory than he thought there was occasion for. He had found with moderate lengths of 400 feet to 600 feet, an extent to which it was convenient to use iron in one length when there was much change of temperature, it had only to be considered where the piers or supports could most conveniently be placed and to make a preliminary calculation of what were the most advantageous spans that should be adopted.

The determining of the most economical strains the bridge should practically be subjected to, and the sections of iron required at different parts of the girders under the varying conditions of the load, need not involve abstruse mathematical investigation. All calculations of continuity, however, became vitiated unless care was taken by proper adjustment of the girders upon the piers and abutments after they had been erected; the circumstances differed according to the mode originally adopted in putting up the bridge. If the girders of each opening were separately built in place on scaffolding, or separately raised complete upon the piers, or if built out as balanced cantilevers, or if the more common mode were followed of connecting the girders of two or more openings together and rolling into place, no subsequent riveting or connecting of the different lengths would insure efficient action as a continuous beam, and the strain might be enormously increased beyond the calculations. In the final adjustment there were simple methods not dependent upon any calculations of deflection for securing that the strains should be as contemplated. These depended upon the positions of the points of contrary flexure of the beam, which themselves depended upon the weights to be carried upon the abutments and the piers. In the case of railway bridges, with two or three continuous openings of equal span, the most advantageous points for contrary flexure varied from about $\frac{3}{4}$ to $\frac{4}{5}$ of the span, measured from the abutment, and all that was necessary was that the weight of half of the above length of girder should be put upon it; this, with moderate spans, did not amount to many tons, and in such cases as the Britannia or the Saltash bridges could be easily handled with the hydraulic presses used in their erection.

It appeared to have been considered indispensable that the points of support for a continuous beam should be all originally built and continue horizontal or at the same level, but this was not essential. The bridge might be horizontal, or it might be built upon an inclination, or, what was often seen in bridges over rivers, with inclinations in both directions towards the centre opening, without affecting the calculated strains. Nor was absolute rigidity of foundation imperative although desirable; general settlement might take place at the abutments and at the piers, or they might settle unequally towards one end of the bridge; but if there was no distortion the continuity would not be vitiated.

At the conclusion of the Paper, the Author appeared to assume an ignorance of the theories of the strength and the resolution of materials amongst the majority of constructing engineers. He believed it would not be found that failures frequently occurred from defective designing of works, arising out of want of theoretical knowledge amongst constructors. Whilst on the other hand instances constantly occurred where the behaviour of structures under accidents turned out entirely at variance with calculations. For example, the well-known instance of the falling of one of the tubes of the Britannia Bridge during its erection, when several hundred tons were for some time supported where the calculated strength was insignificant. Occasions had come under his own observation. The large entrance gates of the Plymouth Docks were carried away before the storm gates had been put up. The entrance was 80 feet in width, closed with a pair of wrought-iron meeting-gates opening inwards, against which there was a head of water of about 30 feet. The gates were held by the two closing chains used for the working of the gates, and under those circumstances were struck by a sea from without during a gale of wind. The result to be expected would have been the forcing open of the gates without disaster by the parting of the $1\frac{1}{4}$-inch chains from the broadside blow against the mitre of the gates; but the water, although striking obliquely near the hollow quoins, sheared off the tops of both cast-iron heel-posts at the collars, each 9 inches by 7 inches square, or 63 square inches area of iron, instead of breaking off the closing chains, each with less than $2\frac{1}{2}$ inches area of section.

Another instance occurred with the sea gate of the dock at Briton Ferry, in South Wales. The entrance was 50 feet in width, with a wrought-iron gate, in a single leaf opening inwards, 53 feet wide and 30 feet in height, working upon a heel-post. Storm chains, 3 inches in diameter, fixed into the masonry, had been provided for attaching to shackles at the four corners, for holding the gate against the sea in gales of wind. On one occasion, before the water was finally shut into the basin, the pin of the upper shackle of the meeting post was inadvertently left in and the flood-tide rose to about 17 feet, with a head outside of about 12 feet, when the storm chain gave way, and the gate flew open with no other damage than the swamping of a dredger by the rush of water. The pressure on the lower part of the gate, held only diagonally at its corners, must have amounted to 250 tons, with probably 300 tons or 400 tons upon the storm chain. The gate was built with horizontal decks, and only occasional vertical bulkheads, and should have had no calculated stiffness in a diagonal direction, yet it showed no signs of weakness under the enormous wrenching strain it experienced.

Mr. G. H. Phipps said there were two points in M. Gaudard's Paper on which he would offer a few remarks. The first had reference to the large and uncertain margin in practice between the utilized and the final strength of materials; and the second to the interesting facts connected with transverse strain, brought to light by Mr. W. H. Barlow's Paper, read before the Royal Society in 1855, some explanation of which he hoped to have found in the Paper.

On the first of the above points, he would say, without in the least undervaluing the scientific character of the Paper under discussion, that it seemed somewhat like labour lost to pursue that part of the subject to such an extreme degree, while on iron, for instance, the strain to be applied was apparently only guessed at about $\frac{1}{4}$ of the breaking-weight.

On the other point, he had listened with great attention to the explanation offered by Mr. W. H. Barlow of the singular difference in the strength of any fibre in a body under transverse strain, to the strength of the same fibre when under direct strain. He was sorry to say, however, that the explanation failed to clear up the difficulty he had previously experienced in understanding the matter, and accordingly he had cast about for some more intelligible cause. It was curious to observe that the facts noticed by Mr. Barlow tended towards the theory of Galileo, which gave to every particle, in any given transverse section of a body subjected to transverse strain, an equal value, a tendency remarked to have place in actual experiment by Professor Barlow.[1] But although the tendency

[1] *Vide* "A Treatise on the Strength of Materials." New edition, p. 29. London, 1867.

appeared to be in the direction of the theory of Galileo, the difficulty was to account for the enormous disparity, as ascertained by Mr. Barlow, between the strength of the fibres of a body under transverse strain, and similar fibres under direct strain; namely, for cast iron, as 40,000 lbs. to 17,000 lbs. on the square inch, or as 2·35 to 1. Under the theory of Galileo, even if any fibre were taken to have a value equal to the most strained fibre, by the ordinary theory the respective moments would only differ as 50 to 33, or about as 1·5 to 1.

Further investigation appeared, therefore, necessary to place this interesting question on a satisfactory basis.

Mr. E. A. Cowper, submitted with all deference that abstruse formulæ were not necessary for the purpose of building continuous girders of proper form. He confessed to a little disappointment when Mr. Heppel said that, in the case of a girder bridge of several spans, certain of the formulæ became almost "unmanageable," by which he presumed was meant, that it was not possible quite clearly to see that every item was absolutely correct. Now it was just that absolute certainty that was wanted, when building a structure of any importance. It was necessary thoroughly to understand the calculations, and to see clearly through them to be sure that all was right. He would refer to two points in which he thought French engineers had not taken the practical view of the question, if question there was, in so simple a matter.

First. They had attempted to calculate a girder of "uniform section." Now this, he conceived, was not of the practical importance that they seemed to think, for no continuous girder ought ever to be of "uniform section."

Secondly. They had not said what the deflections ought to be both over the piers and between the piers, and as these two points were so intimately connected, he would take them together, and attempt to show in a practical way what the curvature ought to be, and why a "continuous girder" ought not to be of "uniform section," as it would cause great waste of metal.

He must first guard himself against being supposed to recommend continuous girders, as there were few situations in which they were to be preferred; and, generally speaking, the circumstances were such, that there would be no saving of money in their use.

Supposing, now, he bent a straight girder or stick to a given curve, it was clear that he strained the fibres on one side and compressed them on the other side, to a certain definite given amount; and if they were strained as much as was good for them, then that curve was the right curve to which to bend the girder or stick, to make the material do its full duty. If it were bent too much the fibres were crippled or broken; if bent too little the material did not do its full duty; thus the curve was a measure of the work the girder was doing. Of course, with every different depth of girder a different curve was the right one to bring out its strength.

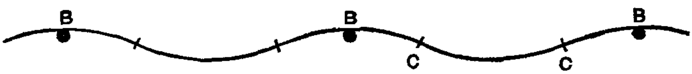
Now in the case of a model of a continuous girder of uniform section (Fig. 27), it would be seen at a glance that the curvature

Fig. 27.

of such a girder, when uniformly loaded, was anything but right; and in fact that it would be bent to breaking over the piers A, A, A, long before it was fully bent at any other place; in other words, it was excessively weak over the piers, in consequence of being of uniform section.

Now if the curvature ought to be at all parts the same, the form the girder should take, throughout, was at once arrived at, viz., similar arcs of a circle both for the centre half of the span, and for the half over the piers, or a regular undulating line (Fig. 28). To obtain

Fig. 28.



this effect, he should say that the girder must be several times as much in section at one part as it was at another, the greatest strength of all being required just over the piers, B, B, B, and the section being reduced to a minimum at one-fourth of the span from either side, just where the contrary flexure took place; indeed, at this point there might be a simple connection of the two parts of the girder together, were it not for the moving load (that, it was assumed as a matter of course, was to be brought upon the girder when in use) causing a variation in curvature.

It was quite clear, that as soon as it was known how much load was to be brought upon the girder, and how much it was to be deflected, and its length and depth, a calculation could be made of how much material must be put into each part of it.

For instance, the middle half C, C was a simple girder of a length equal to half the span, and having a distributed weight on it, whilst that part over the piers was a girder or double cantilever of the same length, having a weight on each end equal to one end of the central girder and its load, and also a distributed weight over its length as well, thus making the strain several times as much over the piers as in the centre of the span.

He submitted that in cases of this kind, the proper form of construction might with advantage be considered first, and then the material necessary to give the requisite strength at all parts of the structure be calculated; but, as a rule, he thought there was often much waste of time in investigating how materials would behave under circumstances which would never occur.

He repeated that there were but few cases in which it was advisable to adopt continuous girders; and he was well aware of the difficulty there was sometimes in keeping the bearings all exactly to a level, particularly where the supports varied in construction or quality, or stood in the bed of a river.

He agreed with Mr. Barlow, and thought that iron must be treated and considered as a solid, and not as a bundle of parallel fibres. Several thousand pounds might be economically spent in a good series of experiments on materials under circumstances such as occurred every day. As to testing iron or steel in girders or other structures, he would only add what was suggested to his mind by Mr. Mallet's remarks. He would take a "punching," or piece of metal punched out of a plate, and simply draw it down at a smith's fire into a little bar $\frac{1}{8}$ of an inch square and 3 inches long, and then test it for strength and ductility. This would give some idea of the quality of the steel over and above all other proofs.

Mr. Andrew Murray, C.B., expressed his regret at the omission, from the list of names given by Dr. Pole of those to whom the profession was indebted, for their labours and researches on this subject, of one name, which certainly ought not to be passed over, viz., that of Fairbairn. Having assisted in some of the early experiments conducted by Mr. Eaton Hodgkinson for Sir W. Fairbairn and his partner at their works, Mr. Murray knew how much was due to Sir W. Fairbairn, for the practically useful form into which the formulæ, deduced from those experiments by Mr. Hodgkinson, were brought. It had been said in the course of the discussion, that some of the later experiments of Fairbairn were slovenly; but he could not hear that term applied to an old and experienced experimenter without protesting against it. The experiments on the internal tubes of boilers were objected to, but he had unfortunately occasion to reinvestigate that subject lately, and as a manufacturer of boilers, he had obtained a great deal of information from Sir W. Fairbairn's experiments. The results of each experiment were given in detail, and engineers who did not approve of the deductions were at liberty to draw their own from the facts laid before them, and it could not be denied that they were sufficiently ample for the purpose. True, in some cases, the experiments might have been carried farther than they had; but the expense and other reasons might not have allowed of it. Again, all knew how much was due to mathematicians, as distinguished from practical men, more especially to those who had gone further than Mr. Hodgkinson in deducing formulæ; having these formulæ, it was for practical men to bring them into such a form as would be useful in practice. Mr. Heppel, in speaking of the theory of the three moments, said there were simple beams which engineers knew how to deal with: but it was by the labours of such men as Hodgkinson and Fairbairn that this was so. Mathematicians were first wanted to draw deductions; and it was for practical men to adapt and modify them, to meet the cases which they were called upon to deal with.

Mr. Hemans thought it was a pity the discussion should close without doing justice to the distinguished foreigner who had furnished the communication. It might not be known by the Members generally, that the Paper was sent in response to an invitation from the Council for communications on subjects for which premiums were to be awarded. This must be regarded as a handsome contribution in answer to that invitation. A general exception had been taken to the supposition that M. Gaudard had intended to convey that, with regard to treatment of bars of iron as bundles of fibres, no strength was derived from the cohesion of these fibres, and that, therefore, he was justified in treating the subject as if the bar was simply a bundle of threads and fibres; and, moreover, that he had enunciated the principle that time was no element in the strain upon the bars under experiment. But Mr. Hemans, having read over the Paper in the original, thought no such expressions were meant by the Author: and though the mathematical portion of the Paper was so abstruse that he had not been able to follow a great part of it, he thought he was justified in the opinion that M. Gaudard had not enunciated that principle, and that he only separated the bars into imaginary fibres in order to treat them more conveniently mathematically. It was hardly possible to apply that sort of calculation, or to treat of this subject, without such ideal divisions. Mr. Brereton's observations were most interesting on practical points, and showed the difficulty of bringing mathematical deductions to agree with practical results; but, at the same time, the Institution should receive with gratitude all communications tending to unite the higher mathematics with practical points.

Professor Sir W. Thomson expressed his pleasure, through the Secretary, at seeing the powerful analysis and very important practical results of M. de St. Venant, in his theory of the torsion of prisms, so prominently brought under the attention of British engineers. Sir W. Thomson sent for exhibition a box of specimens and some photographs illustrating a Paper of his "On the Fracture of Brittle and Viscous Solids by Shearing."[1] Some of the specimens were the originals from which the photographs were taken. He

[1] *Vide* Proceedings of the Royal Society, vol. xvii., p. 312.

also observed that the subject of elastic fatigue, which had been treated by him in a Paper "On the Elasticity and Viscosity of Metals,"[1] had no doubt important relations with the effect of long-continued vibrations of solids on their molecular structure and strength, referred to by M. Gaudard in Section 32 of his Paper.

Mr. B. B. Stoney remarked, through the Secretary, that he had already published most of what he had to communicate on the Strength and Resistance of Materials in the second volume of his treatise on "The Theory of Strains," the molecular physics of materials under transverse strain being specially alluded to in Art. 539, p. 447. Mr. Blood, many years since, made a very elegant analysis of the strains in continuous girders in three spans, which Mr. Stoney successfully applied, in the year 1854, to the calculation of the points of inflection of the Boyne Viaduct. He also wished to draw attention to the practical method of fixing these points by severance of the flanges, which was adopted at Mr. Stoney's suggestion.[2] Two joints in the upper flange of the centre span, as close as possible to where theory indicated the position of the points of inflection to lie, were selected for section. The rivets were cut out, and drifts inserted in their place. The drifts were then cautiously struck out with a light hammer, and a slight closing of the joints proved that a certain amount of compression had previously existed in place of perfect freedom from strain. The extreme ends of the side spans were then lowered one an inch, and the other half an inch, which caused the joints to open about $\frac{1}{64}$th of an inch. When thus slightly open, it was clear that no strain, either of tension or of compression, was transmitted across the joints, and they were thus riveted up, the exact level of the extremities of the side spans being maintained by rollers of suitable diameter accurately turned to the proper size. While he fully agreed with M. Gaudard that, in general practice, there was more or less doubt regarding the exact position of the points of inflection, it was obvious that the practical method just indicated reduced this doubt to a minimum. It was, of course, not applicable to girders with continuous plate webs.

Mr. C. Brooke observed, through the Secretary, that the determination of specific gravities seemed likely to become an important means of distinguishing steel from iron in finished structures. The chief source of error in weighing small masses of solid matter in water was the adhesion of air-bubbles to the surface, or their entanglement in minute cavities. The best mode of obviating this inconvenience was to fasten the solid in a loop at the end of a bit of fine platinum wire, by which it might be conveniently attached to

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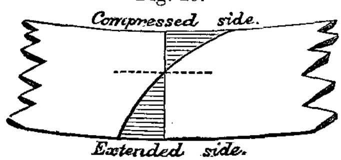
[1] *Vide* Proceedings of the Royal Society, vol. xiv., p. 289.

[2] *Vide* Minutes of Proceedings Inst. C.E., vol. xiv., pp. 456, 457.

a hook under the bottom of a scale-pan; then to boil it in distilled water in a suitable glass or other vessel, in which it could be subsequently weighed without removal. A small wedge of the material to be tested should be removed by two fine saw-cuts, without applying either hammer or cold chisel, in order that the molecular disturbance might be the least possible. Any adhering grease, oil, paint, or varnish, might be removed by boiling in a solution of caustic potash (liquor potassæ of the druggists), which did not act on iron or steel, in preference to using strong acids, which acted unequally on different points of the surface, in proportion as their conversion was more or less complete.

Mr. R. H. Bow observed, through the Secretary, that he had been very favourably impressed with the value and soundness of the Paper. It appeared to him, however, that this class of subject should be treated in a manner less condensed, and more explanatory. Fig. 29, according to his view, would better represent the stresses produced in many substances from extreme flexure than either Fig. 8 or Fig. 9 in the Paper.

Fig. 29.



Mr. JOHN JONES observed, through the Secretary, that one method of distinguishing iron from steel consisted in pouring the filings over a gas flame, when, if the substance was steel or cast iron alloyed with carbon, each atom would scintillate like a star; if wrought iron, the atom would simply become luminous. Again, in the combustion of steel filings, a noise was produced as though each grain exploded, while in the combustion of iron a very gentle sound was given off. Then, the combustion of cast-iron filings yielded a red colour as the general effect of the multitudinous stars, whereas steel filings had a white brilliancy. Also the filings of steel were grey, those of cast iron black.

Mr. BRAMWELL said that reference had been made to a Paper which he had read in another place, upon the relation of form to the strength of materials; and it was in reference to this subject of form that the paragraph in the Paper under discussion had excited the attention of Mr. Barlow and of himself. That paragraph was to be found in Section 2 of M. Gaudard's Paper, and was as follows:—"We are content to assimilate the compact rod to the assemblage of its elementary fibres, close but unconnected, and thus exercising no mutual lateral actions." The Author then said he thought that was an unimportant matter. To his mind, it was by no means so, as even before he heard Mr. Barlow's explanation of the way in which lateral action assisted in giving strength to a beam, it had occurred to him that lateral action was important in that view of the case to which Mr. Unwin had referred. He had endeavoured elsewhere to establish this proposition, that if there were two bars suspended, one of which was of uniform section throughout, and strained by a quiescent load at the bottom, while the other bar had a section the same at the lower part as the first bar, but had at the upper part an enlarged section, the enlargement being made quite abruptly, then, although the load suspended by the second bar should be exactly similar to that suspended by the first bar, there was not only relative weakness in the lower part of the second bar, as compared with its upper part, but that there was absolute weakness in that lower part; in other words, that there was more strain per unit of section where the small section joined on to the larger section than at any part of the section lower down, or than at any part of the first bar, which was uniform from top to bottom. To his mind, this excessive strain at the point of junction was due entirely to the lateral connection of the particles, and could not be explained on any other ground; and were it not for the fact that there was a lateral elastic connection between the fibres, there would be no more strain per unit of section of bar at the junction of the small part of the second bar with the large part than there would be upon the uniform section. He believed this fact of the increase of strain per unit of section at the sudden junction of dissimilar areas to have been the cause of many instances of failure, the sources of which were for a long time obscure. He thought, therefore, if he were right in the suggestion that this effect was due to lateral connection of the fibres, the Author of the Paper had overlooked the important part which this lateral connection really played.

Mr. Callcott Reilly in reply, observed, that Dr. Pole's sketch of the progress of discovery in this important department of philosophy was necessarily condensed; yet he had pointed out some of the salient landmarks of the road travelled by Galileo, Descartes, Newton, Leibnitz, the three Bernoullis, Hooke, Mariotte, Euler, D'Alembert, Lagrange, Coulombe, Legendre, Young, Navier, Poisson, Cauchy, Poinsot, Clapeyron, Barlow, and many others; not to speak of many still living. To the student of engineering, there was no department of literature more to be commended than the history of scientific achievement, as unfolded in the dissertations of Playfair, Leslie, and Forbes, the histories of De l'Hôpital, Whewell, and M. Michael Chasles, and the Historique Abrégé of M. de St. Venant. It would stimulate honourable ambition, and would teach the student to appreciate his profession as a branch of exact science, as a liberal art, in which success required at once, the accurate learning and the well-disciplined mind of the philosopher, and the generous enthusiasm of the artist. An intimate knowledge of the resources of exact science, and the power of using them with discriminating intelligence, was now more necessary than formerly, and in the future was likely to be absolutely requisite.

Dr. Pole had also referred to the importance of the experimental branch of the subject under discussion, and on that head some suggestions would hereafter be offered.

Mr. Siemens had objected to the prominence given by the Author to the subject of ultimate strength, and he was bound to admit that, in his opinion, such objections were well founded. He thought that the subject of ultimate strength, so far as concerned all materials (such as iron and timber), that admitted of treatment as elastic solids within certain well-defined limits, was of little interest to the engineer, except as sometimes affording knowledge, that would enable him to discriminate between different commercial varieties of the same material. There was as yet no theory of the ultimate strength of natural bodies, notwithstanding M. de St. Venant's efforts in that direction, nor indeed was it an important desideratum, and the Author would perhaps have done well to have put the results of that investigator's researches in this department in an appendix, as a matter interesting to the student of molecular physics, but entirely beyond the scope of the limited theory of the resistance of materials, as taught to the pupils of the engineering schools of France.[1] That limited theory was strictly confined to the effects of what were relatively very small deformations, produced by the action of molecular forces, or stresses, whose intensities did not exceed the limits, determined by experiment, where the elasticity of the material ceased to be sensibly perfect. Whatever was known of the effects produced beyond those limits, if mentioned at all, was dismissed with a passing allusion. Of course it became necessary, in treating the subject upon this basis, to include all the effects produced; not only the easily-understood statical effects of forces gradually applied, but also the more obscure effects of shocks and of external forces more or less suddenly applied, whether with or without velocity; so that the aggregate of all those effects should not exceed the prescribed limits. If this was done with success and completeness, the limited theory was sufficient for all the purposes of the engineer; and researches, whether experimental or theoretical, into the condition of materials subject to stress beyond their known limits of elasticity, were to him comparatively matters of indifference. True, it might occur that a complete solution of the problem, presented by some complex or otherwise difficult case, was unattainable in the present state of the theory, either from absence of data, or from insufficient development in the

[1] This remark was not intended to apply to M. de St. Venant's profound researches on the effects of elastic torsion. His researches on this question, and on sliding and shearing stress, were without doubt the most important contribution to the Theory of Resistance ever made by a single individual.—C. R.

theory itself. In such a case an empirical solution might be the only one attainable, but such a treatment of the problem should be regarded as merely provisional, to be tolerated only pending the collection of necessary data, the formation of a sufficient hypothesis, or the requisite development or simplification of the theory. But even then the empirical solution should avoid, if possible, absolute conclusions respecting the working or the elastic condition of the materials, founded on experiments upon ultimate deformation and resistance; and in fact such absolute conclusions were almost as irrational as it would be to infer the strength of a piece of copper from experiments upon that of a piece of timber; although relative conclusions were, no doubt, legitimate within carefully defined limits. For instance, supposing it were desirable to ascertain which of two differently shaped or proportioned pieces of the same material, was best able to resist the action of certain applied forces, and that the theory failed to solve the problem of internal stress satisfactorily, by reason of its difficulty or complexity; then an experimental comparison of the ultimate strength and deformation of the two pieces, under the action of perfectly analogous forces, might afford sufficiently accurate conclusions respecting their relative merits. But it would afford no information respecting the absolute intensities of stress between contiguous particles of the material, in its working condition, and within its limits of elasticity. It was usual to designate the theory as that of the strength, or resistance, of materials. That title was given by the older writers, who had not learnt to discriminate sufficiently between the elastic and the ultimate resistance of natural bodies, and Mr. Reilly thought that its want of precision was responsible for some of the confusion of ideas that prevailed upon the subject; the same might be said of the frequently careless use of the phrase "moment of rupture," when the writer meant sometimes "bending moment," and sometimes "moment of elastic resistance." He suggested that the title should be, the "Theory of the Elastic Resistance and Deformation of Materials;" it would then precisely indicate the nature of the subject.

Mr. Young had mentioned difficulties he felt in admitting some of the results stated by the Author, and he seemed disappointed that the Author's explanations were insufficient to resolve his doubts. Mr. Reilly would remark that the memoir did not pretend to be a treatise on the subject. A complete treatise, demonstrating and explaining systematically all the results stated by the Author, would fill many hundred pages. It was simply a résumé, and a very good one, although, as admitted by the Author himself, by no means perfect, concentrating into one short and readable synopsis, results that must otherwise be sought for in the pages of voluminous and recondite treatises. Mr. Young's difficulties appeared to refer chiefly to the effects produced, by the combination of shearing stress and longitudinal stress in a loaded solid beam or cantilever. It would be futile on that occasion to attempt a demonstration of those effects, but Mr. Young might be referred to Professor Rankine's "Applied Mechanics," Articles 309, 310, and 312, where would be found a concise demonstration of what was most useful to be known on that head. More extended information, and indeed all that was known of this subject, would be found in "Notes sur les Leçons de Navier sur les Résistance des Corps Solides," by M. de St. Venant,[1] and in a memoir, "On the Strains in the Interior of Beams" by the Astronomer Royal,[2] who had extended his researches into that subject, much beyond the limits attained by other writers.

Mr. Brooke had attacked the nomenclature used by the Author, somewhat unjustly, as Mr. Reilly thought, and he therefore conceived that a few explanations in defence of the Author on that head would not be out of place. It was true that the older writers divided the science of Theoretical Mechanics (*Mécanique Rationelle*, as it was called by the French) into two branches, Statics and Dynamics. But that division of the subject was now tending to become obsolete, the modern writers, especially the French, following Ampere, dividing the science into two principal divisions; viz., first, "Cinematics," or the "theory of pure motion;" secondly, "Dynamics," or the "theory of the equilibrium and motion of material points and systems." The second division was further sub-divided into three parts,—first, the "Equilibrium and motion of material points;" secondly, "the Equilibrium of material systems;" thirdly, the "Motion of material systems;" the second sub-division being the subject of what was formerly called "Statics." That arrangement was now almost universally adopted by the most advanced French writers, and in the "Mécanique Rationelle,"[3] of M. Ch. Delaunay, Membre de l'Institut de la France, probably the best elementary work extant on the subject, the term "statique" was ignored throughout. The same writer and all of his school defined "weight" as a "force" of a particular kind,[4] so did Rankine, while Sir W.

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[1] Paris, 1864. [2] *Vide* Phil. Trans., vol. cliii., p. 49.

[3] 4[me] edition, Paris. Victor Masson et fils, 1866.

[4] *Ibid.*, p. 105, "La chute des corps que l'on abandonne à eux-mêmes, à une certaine distance au dessus de la surface du sol, est due à l'action d'une force qu'on nomme la pesanteur." . . . . P. 107, "Il est naturel de prendre le *poids* d'un corps pour mesure de l'intensité de la force qui tend à faire tomber ce corps. La force de la pesanteur agissant sur un corps sera donc représentée par un certain nombre de grammes ou de kilogrammes." . . . . "Ainsi, une force quelconque peut toujours être mesurée par une poids, et en conséquence évaluée en nombre au moyen de l'unité de poids."

The French writers employ "force" to express ideas essentially different, believing themselves compelled to follow established usage in such matters, although quite alive to the objections against that custom. In reference to that, M. Delaunay

Thomson and Professor Tait employed "weight" as a measure of "force," the "absolute unit of force" in the latitude of Edinburgh, being stated by them to be $\frac{1}{32 \cdot 2}$ of a pound. "Reaction" was a short and convenient term employed by writers on applied mechanics to denote any resisting force, for instance, a supporting force which balanced a corresponding loading force. Again, "force of inertia" was a phrase universally adopted by the modern French writers, (who were quite aware that "inertia" was not a force) and it was carefully and strictly defined by them, to mean the resistance to acceleration; for example, a force of inertia of a body was equal in magnitude and contrary in sign, to the difference of the forces required to impart to that body, successively, a given uniform motion and a given accelerated motion. Mr. Reilly hoped that these explanations would suffice to show, that the eminent writers who adopted the phrases attacked by Mr. Brooke, did so with a full apprehension of the ideas which they intended to convey.

Mr. Unwin had pointed out what he considered a possible error in the Author's statement of the ratio of linear lateral contraction to elongation, per unit of length, of an isotropic elastic prism stretched by tensile stress. In justice to the Author it should be noticed that he had only followed M. de St. Venant in adopting the value considered to have been established by Navier and Poisson, and that their theory, first attacked by Professor Stokes in 1845, was only attempted to be finally disproved for an isotropic solid, by Professors Sir W. Thomson and Tait in their work published but two years ago,[1] and therefore not likely to have been read by the Author at so early a date, as that of the composition of this memoir. But Mr. Reilly was inclined to think that the attempt had not yet succeeded, and that its apparent success was due to a certain begging of the question on the part of those distinguished writers. The truth of the theory was only maintained by its supporters with respect to isotropic *solids*; an isotropic solid being defined as a solid of "equal contexture in all directions," and in a *perfect* state, viz., free from all deviations from that quality, due to the accidental effects of the processes by which the solids were produced; as a consequence of which, St. Venant denied the quality of perfect isotropy to all bodies *regularly* crystalline, and to all such solids as brass, either drawn or cast, glass, tin, cast iron, rolled or hammered iron; also to such substances as cork, indiarubber, and jellies, either natural or artificial. But the attempts to disprove the theory

says, "La manière dont le mot force se trouve introduit dans une phrase indique toujours suffisamment a laquelle de ces acceptions il se rapport." P. 529.

[1] *Vide* "A Treatise on Natural Philosophy," vol. i., art. 684. 8vo. London, 1867.

rested on results of experiments on those and similarly defective substances, and their validity had yet to be established to the satisfaction of the eminent writers who maintained the theory. Among their names St. Venant's stood conspicuous, as that of "beyond all doubt the greatest authority living on the subject of elasticity,"—to quote the words of one of the most eminent English writers who had opposed his school on this particular branch of theory. But whatever the value of this disputed ratio, or its importance in the study of molecular physics, it had no practical application that Mr. Reilly was aware of in the limited theory established for the use of engineers, and it was merely alluded to in the courses taught in the French schools of engineering, as having no appreciable effect on the small deformations, tolerated in the working condition of the materials of construction. The only use made of this ratio in applied mechanics, had been in the theoretical determination of the co-efficient of transverse elasticity, called G by the French writers. But such determination required that the body be isotropic, according to the preceding definition, and that the displacements of the particles be exceedingly small, two conditions not generally occurring in experiments. Professor Rankine had informed Mr. Reilly that the value of that co-efficient for the different materials of construction, could only be ascertained by experiments upon torsion, owing to its great variation in different materials. Nevertheless experiments made in France, had given the mean values of G for fine cast steel and for wrought iron as about $\frac{2}{4 \cdot 42}$ E, and $\frac{2}{6}$ E respectively,[1] values which differed from the theoretical value $\frac{2}{5}$ E, not more than the conditions of the materials and of the experiments, differed from the theoretical conditions above mentioned, fine cast steel probably approaching nearer to the condition of isotropy than any other material used in construction. He ventured to think that no one would allow that Sir W. Thomson's somewhat dogmatic statements had finally closed this celebrated controversy, who had read St. Venant's minute, courteous, and impartial examination of the hostile arguments, and his discriminating defence of those of Navier, Poisson, and Cauchy,[2] until their validity had been admitted by the other side. Nevertheless, Mr. Unwin deserved thanks for having noticed a celebrated controversy, for a long time occupying attention in the abstract science of molecular physics. Mr. Unwin had mentioned

[1] *Vide* Morin, "Résistance des Materiaux." 3me edition, Paris, Hachette, 1862. Vol. i., p. 61, and vol. ii., p. 328.

[2] *Vide* "Annotations sur les leçons de Navier," par M. de St. Venant. Paris, Dunod, 1864. Appendix 5, extending from p. 645 to p. 762.

to Mr. Reilly a new instrument of research, which, when developed, promised to facilitate the solution of obscure practical problems, difficult to treat by the ordinary methods of the theory. He alluded to the Principle of Least Action discovered by Mr. J. H. Cotterill, of the Royal School of Naval Architecture. That principle, stated in general terms amounted to this,—that the effect of any given force was produced when the work done by the force was a minimum.

Mr. Heppel's sketch of the progress of knowledge relating to the theory and practical treatment of continuous girders, was important and interesting. It might be said that little was known in England of the proper treatment of this subject, by the easy methods due principally to Clapeyron, and known on the continent by his name. The older method, due to Navier, took for unknown quantities the vertical reactions of the supports, positive or negative as the case might be, and required the solution of, and the elimination between, three times plus one as many equations as there were points of support; and when the spans were numerous and of unequal length, the solution became very difficult. Clapeyron grappled with this difficulty, and devised in 1849 what was known as his first method, by which the number of equations was diminished from three times plus one, to twice the number of points of support. In 1855 M. Bertot enunciated what was now known universally on the continent as the theorem of the three moments, and sometimes as Clapeyron's theorem, because it was a direct inference from his first method. By it, the number of equations was reduced to two less than the number of points of support, and the equations themselves were remarkable for symmetry and facility of solution, and were applicable to any case of inequality of length of span or intensity of load in different spans. But it was wanting in generality in three respects; it implied uniformity of section of the girders throughout all the spans, uniformity of level of the points of support, or what amounted to the same thing, perfect continuity, and uniform intensity of load in any one span by itself; thus still leaving three desiderata. The second and third desiderata were subsequently supplied by M. Bresse, the latter, however, by means of an integral not very available for the purposes of computation; together with a vast amount of research, having for object and result, increased facilities for rapid and easy applications to practice. But previous to M. Bresse, M. Belanger had in 1858 published a work, containing a general solution which included, or implied, all the results published afterwards by Mr. Heppel, which he would now bring to the recollection of the readers of the Minutes of Proceedings. About the same time as M. Bresse was at work upon his later researches, or perhaps a little before, Mr. Heppel took up the subject when in India, in complete ignorance of, and isolated from, all sources of information relating to what had been

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done by Clapeyron and his followers, and knowing Navier's method only through the insufficient account given in Professor Moseley's book,[1] and the applications of it to the Britannia Bridge made by Dr. Pole, and described in Mr. Edwin Clark's work.[2] In spite of these disadvantages, Mr. Heppel not only deduced *à priori* the "theorem of the three moments," but a still more general equation including that theorem as a particular case, and completely supplying the second desideratum before mentioned, and also the first not completely, but sufficiently so as it now appeared, for all usual practical cases. His investigations and results were completely explained, and the methods of application fully described in his memoir, "On a Method of Computing the Strains and Deflections of Continuous Beams under various Conditions of Load."[3] The originality and independence of this discovery were made evident by the somewhat curious and roundabout method of demonstration employed in its development, as it now appeared, when compared with the previous demonstrations of the French writers. The merit was obvious, when it was considered that its Author had, without aid, worked out at one heat in the course of a few months, what had occupied the attention of some of the most distinguished French analysts of the present age for more than ten years.

He regretted that Mr. Heppel's work had been hitherto almost unnoticed; so far as he knew the method was unused, except by a few friends of its author, and had been passed over in silence by English writers, who had mostly treated the subject in but a perfunctory manner.[4]

Mr. Heppel had been stimulated by the appearance of M. Gaudard's memoir, to direct his attention to the supply of the first and third desiderata previously mentioned, namely, the extension of the "theorem of the three moments," so as completely to cover the case of any discontinuous variation of section in the same span, and any discontinuous variation of intensity of load in the same span; and he was happy to announce that complete success had attended these efforts. These researches and the mode of application to practical cases, would shortly be published in a memoir now being prepared by Mr. Heppel. The applications would be found to be somewhat laborious, and would require time and the skill of an expert computer; but they would only be required, if at all, in cases of

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[1] *Vide* "The Mechanical Principles of Engineering and Architecture," second edition. London, 1855.

[2] *Vide* "The Britannia and Conway Tubular Bridges." London, 1850.

[3] *Vide* Minutes of Proceedings Inst. C.E., vol. xix., p. 625.

[4] Excepting Mr. Humber, who, in his large work on Iron Bridges, published in 1861, had given a full account of Clapeyron's first method, but had been apparently unaware of its previous development into the theorem of the three moments, and of the further generalisations by the writers Mr. Reilly had named.

bridges of great and unusual lengths of span, when such skill and the requisite time could be well afforded. It would be rigorously demonstrated, that in all ordinary cases, the application of his general equation, No. 8,[1] would be sufficient to give the requisite accuracy, whatever the variation of section, and in the most usual of those cases, when the supports were level, the simple theorem of the three moments would be quite sufficient for the purpose; the reason being that the bending moments over the piers were but little affected by any practicable variation in the moments of inertia in the same span.

French engineers had satisfied themselves of the sufficiency of Clapeyron's theorem, as developed by M. Bresse and by M. Belanger, by observing that the actual deflection always coincided with that correctly computed according to the theory, with the requisite closeness of approximation. They inferred from this, and correctly so as it now appeared from Mr. Heppel's recent researches, that the influence of the variation of section actually existing in a well-designed continuous girder, might be neglected in all railway viaducts.

It had been correctly remarked by Mr. Cowper and Mr. Young that unequal settlement of the piers was to be guarded against when the girders were continuous. In fact, when such a danger was to be apprehended, continuous girders should never be used. He thought that the advantages of continuous girders, while considerable in railway bridges of large spans, say over 100 feet, and all spans where the moving load was either nil or very small compared with the fixed load, disappeared in railway bridges of smaller spans, when compared with economically designed discontinuous girders; all other circumstances being equal. The effect of the relatively very heavy moving load, when applied in the most unfavourable manner, was to assimilate the condition of a loaded continuous girder of small span, more or less, to that of a discontinuous girder; and, moreover, the short length of such a girder, afforded less opportunity for economical approximation of the sections to the theoretical proportions, when continuous than when discontinuous. The application of continuous girders should always be made with a cautious regard to the foregoing considerations.

Mr. Cowper's ideas respecting the positions of the points of inflection in continuous girders were correct for bridges, in which the conditions prevailed of equality of all the intermediate spans, a certain fixed proportion of end span to an intermediate span, and uniform intensity of load in all the spans; but he submitted that they were incorrect in the far more common case of unequal loading of the different spans, and also where the spans

[1] *Vide* Minutes of Proceedings Inst. C. E., vol. xix., p. 625.

were of arbitrary unequal lengths. In the former case, supposed by Mr. Cowper, no method of computation was easier than that of Clapeyron, Bresse, Belanger, and Heppel, the facility of application being then very great. In the latter case, it was a little more troublesome, but still perfectly easy to any computer acquainted with the simple art of algebraic elimination: moreover, it was the only practicable method applicable to those conditions, and it would give sufficiently exact results whatever the admissible variation of section. Mr. Cowper had also said that the deflection curve should be a combination of equal circular arcs arranged in a particular way, which he described; but that could only occur under the first set of conditions above mentioned, combined with perfect uniformity of strength throughout the length of the continuous girder; the last condition being most often unattainable in practice, owing to the obstacles preventing the continual alteration of the sections from point to point of the length.

Mr. Barlow and Mr. Bramwell had argued that the theory took no account of the resistance, due to the lateral adhesion of particles or fibres, and the former especially impugned the correctness of the hypotheses underlying the theory under discussion and had spoken with but slight respect of the "fibre hypothesis" in particular, thence inferring the fallacy of the theory itself. He ventured with great deference to disagree with those objections; and, as a correct appreciation of the value and utility of the fundamental hypotheses was of great importance, he would endeavour to meet them. So far from the theory disregarding the resistance of lateral adhesion, that particular kind of resistance played a conspicuous part in the modern theory, and occupied as much of the attention of its authors as any other; witness M. Gaudard's frequent reference to it in Arts. 6 to 8, 11 to 15, 17, 21, 22, 26, 28 to 30. Coming to Mr. Barlow's argument, what had been already said on the futility of attempting to deduce the conditions of stress and deformation within the elastic limits, from experiments on what happened beyond those limits, would be sufficient to show that such deductions must be discarded. An indication of the nature of the probably true explanation of apparent anomalies disclosed by such experiments, would appear in the remarks he would submit on the subject of the combination of simple sliding and simple flexure in a deflected beam. He would remark, however, that, whereas the older writers, up to and including Navier and his followers, had deduced conclusions, now known to be false, relating to the conditions of rupture and ultimate deformation of solids, on the hypothesis of perfect elasticity, yet those conclusions had long since been abandoned by the French writers. Mr. Barlow's deductions relating to flexure, as published in the 'Philosophical Transactions,' were derived from observations on the deflection of loaded beams, when the intensities of stress produced went beyond the limits of elasticity; and he had applied the results so obtained to refute the conclusions of the theory of elastic resistance. Now the fundamental basis of the theory was the condition, that no intensity of stress computed by it, should much exceed the limits admitted in the practical working condition of the material, and should at the most not exceed its least limit of elasticity. General Morin, in the last edition of his "Résistance des Materiaux,"[1] had examined in the most minute and impartial manner all the published experiments of Hodgkinson, Fairbairn, Rennie, Hosking, and others in England, and also a great number of experiments made in France with at least equal care, many of them with the aid of the great resources of the Conservatoire des Arts et Métiers. By this judicial examination he had established upon the firmest experimental basis, the conclusion that up to the highest admissible limits of working intensities of stress in cast iron, the truth of the modern theory might be relied upon with the utmost confidence, and that in the cases of wrought iron, steel, and wood, that reliance might be extended up to the known least limits of elasticity. It was impossible to give an adequate notion of the facts, and reasonings upon them, on which M. Morin founded his conclusions, extending as they did over nearly 500 octavo pages, but it might be useful to note, that the conclusions were mainly deduced from a comparison between the co-efficients of longitudinal elasticity computed from observations upon simple longitudinal extension and compression, with the same computed, according to the theory, from observations upon flexure. Mr. Reilly ventured to think that Mr. Barlow's experiments were of considerable value in verifying the theoretical position of the neutral axis in deflected prisms of cast and wrought iron, and possibly as a contribution towards an eventual theory of *ultimate* strength, but that his hostile inferences relating to the theory now under discussion were erroneous.

He would now attempt to indicate the nature of the reasons why the sufficiency of the hypotheses which had been attacked must be conceded. An important remark by M. Collignon might be noticed with advantage. After observing that "the science of the resistance of materials was partly rational and partly experimental," two divisions that were complementary of each other, he said "the rational part required a certain number of hypotheses, in order to submit to exact calculation the problems to be resolved. The number of requisite hypotheses tended to decrease as the science became more perfect; but that intervention of hypotheses did not, after all, diminish the positive character of the science, because they could be submitted to absolute criteria,

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[1] 3^{me} Edition, 2 vols. 8vo. Paris: Hachette and Co, 1862.

by the comparison of the computed results with the comparable facts disclosed by experiment, within the limits of application of the theory."[1] For example, if the extension of a prism, the deflection of a beam, or the angular motion of a twisted prism, under the action of given loads, as observed experimentally within the prescribed limits, agreed with that predicted by the theory, then surely the sufficiency of the hypotheses assumed in the theory must be admitted. Such agreement was well known to exist,[2] and therefore the hypotheses must be regarded as sufficient for their purpose, notwithstanding that they might be more or less false in aspects independent of their precise position in the theory. Such an hypothesis was the assumption of perfect elasticity; that was an essential basis of the science, but it might be called a false assumption, because there was no such thing in nature as a perfectly elastic solid, yet true conclusions were deduced from it, when applied within the proper limits, and with the proper qualifications, established by the authors of the theory. Another false hypothesis was that a prism of solid matter, such as iron, of uniform transverse dimensions, might be considered as a bundle of separate parallel fibres, when studied with regard only to its resistance within the limit of its elastic strength, to forces having a resultant directed along the mean fibre, and producing only longitudinal extension or shortening. The prism of iron was not a bundle of separate fibres, but the observed change of form and dimensions due to the action of those forces within that limit, agreed with that computed by theory from that false hypothesis; therefore the sufficiency of the hypothesis must be admitted, and its falsity in other aspects was of no consequence. It would be well to examine successively the fundamental hypotheses, assumed for bases of the four primary divisions of the theory, with a view of indicating that, though more or less false in their complete aspects, yet they were sufficient to lead to true results within the specified limits of their application. Those four primary divisions treated of the effects of, and the resistance to,—

1st. Simple extension or shortening of a prism of uniform cross section (*extension ou compression simple*);

2nd. Simple sliding (*glissement simple*);

3rd. Simple flexure (*flexion simple*);

4th. Simple torsion (*torsion simple*).

These four primary divisions of the subject were studied separately from each other, and some of them upon different hypotheses, and the effects thus deduced were afterwards combined, according

[1] *Vide* "Cours de Mécanique Appliquée aux Constructions," vol. i., p. 7. Paris, 1869.

[2] *Vide* Morin's work, 3me Edition.

to the dynamical principle of superposition of effects, when any problem was in question involving more than one of them.

The sufficiency of the false hypothesis of an agglomeration of parallel fibres, assumed in the first division had been already indicated. Passing to the second division, he would observe, that the subject of shearing stress or resistance to sliding, had only recently been systematically studied and introduced into the theory of the resistance of materials. It was true that Young had a notion of it, but he possessed no means of computing its effects, and it was almost, if not quite, disregarded by Navier and his followers, including Moseley, and hence many of their investigations were defective in their conclusions and were now superseded. Its importance was first pointed out by Vicat, and its present position in the theory was owing chiefly to the profound researches of M. de St. Venant.[1] The first English text-book in which it was systematically treated was Professor Rankine's "Applied Mechanics."[2] Therefore it was evident that those students of the theory, who had confined their reading to treatises published anterior to the date of the writings of M. de St. Venant on this subject, would find their knowledge of the science and their appreciation of its value, much modified by a study of modern authors. "Simple Sliding" consisted in the translation of a plane section of the body under consideration relatively to another parallel plane infinitely near to it, in the direction, parallel to the plane, of the force which produced the sliding; such a force was called a shearing force, and the resistance to the sliding or translation of the one plane upon the other, was called the shearing stress. The latter was analogous to the resistance of friction of plane surfaces, excepting that the intensity of the resistance of friction was generally uniform, on the particular pair of planes subject to it, while the intensity of the shearing stress on the surfaces in question varied from point to point according to well-defined laws, depending upon the form of the section. The hypothesis then was that a prismatic body, subject to the action of shearing forces alone, acting in parallel directions, was built up of infinitely thin planes, adhering to each other with a force measured by the resistance to the simple sliding. But such simple sliding never occurred alone, it was always accompanied by a sliding in the direction at right angles to the former, in all respects analogous to

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[1] First published in the "Comptes Rendus," 1854–5; and more fully in the "Journal de Mathématiques de Liouville," in 1856. The way had been prepared for his researches by the previous labours of Poisson, Cauchy, Lamé, and Clapeyron.

[2] Mr. Heppel had previously treated the effects of shearing stress in the web of a plate girder in an original manner, and without aid from the works of the French writers, in his memoir entitled "On the Relative Proportions of the Top, Bottom, and Middle Webs of Iron Girders and Tubes." *Vide* Minutes of Proceedings Inst. C.E., vol. xv., p. 155.

it, and of equal intensity with it, at any given point in the body. This second sliding could be separately studied and computed on a similar hypothesis to the former, and the intensity of the shearing stress, or resistance to sliding, in one of the two directions, at any given point was known, if that in the other direction could be computed. Of course the hypothesis of a pile of planes or laminæ in two rectangular directions at once was not literally true any more than the fibre hypothesis before alluded to, but the results computed by its help were true, first, because they satisfied the conditions of internal equilibrium of elastic solids; secondly, because they agreed with the facts disclosed by experiments upon elastic torsion. The false hypothesis was not absolutely necessary, the subject of sliding could be studied, and by some writers was explained, without it, but with more difficulty and complexity; it was merely an artifice for facilitating the study of a somewhat difficult problem.

Coming now to the third primary division, viz., "simple flexure" of a prism, this was studied by itself without reference to its combination, in all practical cases save one, with either or both of the first and second kinds of resistance. It was said that a prism suffered simple flexure, when any plane section originally normal to the primitive mean fibre, had, relatively to another similar section infinitely near to the first, solely a motion of rotation round an axis contained in its plane and passing through its centre of elasticity. Thus restricted, the false but sufficient hypothesis of the prism being analogous to a bundle of longitudinal parallel fibres, afforded a means of accurately explaining and computing the longitudinal extensions and shortenings, and the intensities of stress corresponding, in every practical case, within the limits of elastic deformation. But in many practical cases, the simple flexure was combined with a simple longitudinal extension or shortening, as the case might be, producing longitudinal stresses of uniform intensity to be computed by the methods of the first primary division. Such was the case (mentioned by Mr. Barlow and illustrated by Fig. 19) of a prism extended or shortened by a force whose resultant, while parallel in direction with the mean fibre, deviated from it in position to one side or the other. The effect of that deviation was to reduce the applied force to a couple, whose rotation-axis traversed the centre of elasticity of any normal cross section, and a single force acting along the mean fibre. The couple alone produced longitudinal stresses of tension and compression of the fibres on opposite sides of its rotation-axis, and of varying intensity, thereby causing a simple flexure, that was, a rotation of every normal plane cross section; the single force produced longitudinal stress of uniform intensity on all the fibres; by the principle of superposition of effects, the algebraic sum of the two intensities, at any given point in that surface, was the total stress at that point, and all the circumstances of the elastic deformation of the prism, useful to be known, could be computed from the differential equation of the deformed mean fibre.[1]

In many other practical cases, the applied forces had a resultant whose direction was normal to the primitive mean fibre of the prism, as in the case of a beam, or cantilever. Then the simple flexure was always combined with two simple slidings producing shearing stresses in two rectangular directions, of equal intensity at the same point, but varying together for different points in any given cross section.[2] The simple flexure, as before, produced solely the longitudinal stresses of tension and compression, owing to the rotation of the normal sections, which were to be computed by the methods of the third primary division; the shearing stresses were to be computed separately by those of the second primary division. It was true that the latter affected the shape of the originally plane cross sections, causing them to become curved in opposite directions above and below the mean fibre, but fortunately this circumstance did not, within the elastic limits, change the longitudinal stresses themselves, and in studying the latter alone it might be neglected. The shearing stresses however were of importance on their own account, particularly in the cases of girders with thin vertical webs. Formerly, the resistance of a solid beam or girder was supposed to consist solely in the longitudinal compression and tension above and below the mean fibre, but the existence of slidings and consequent shearing stress was now well understood. The latter resistance was known to be requisite in order to satisfy the conditions of internal equilibrium of the elastic solid, although its existence was not made evident by experiments upon the elastic deflection of beams of the ordinary materials of construction, owing to its very small influence upon the amount of deflection within the limits of elasticity, because of the comparative smallness of the deformation within those limits. Did the theory permit speculation upon the conditions of stress of a solid prism deflected beyond the elastic limit, it would possibly be found that the influence of the shearing stress in resisting great deformations largely increased, and would partly explain certain well-known apparent anomalies, exhibited by experiment, upon the rupture of solid prisms by flexure, and often cited, as by Mr. Barlow on the present occasion, in disparagement of the theory

[1] In fact this problem was one of those capable of most accurate treatment by the modern theory, within of course the limits of elastic deformation. A practical example of its proper treatment by the theory would be found in the present volume of the Minutes. *Vide* "Studies of Iron Bridges," the analysis of the piers of Bridge No. I.

[2] *Vide* "Studies of Iron Bridges" in the present volume of the Minutes, art. 35.

now under discussion.[1] But these inequalities were most likely due to the inequality of the ultimate resistances to tension and compression, and to the continual augmentation of the ratio of the change of length to the forces producing it.

Mr. Reilly had previously hinted that in one kind of practical case only did simple flexure exist alone, without combination with one of the remaining three divisions of resistance. That was when all the external forces applied to a beam, reduced to two equal and opposite couples in the same plane, and equidistant from the centre of its span. Such was the condition of a carriage axle having two wheels of equal diameter and equally loaded, at equal distances from the bearings. In such a case the bending moment, relatively to every cross section between the nearest forces of the two couples, was uniform; between those two points, the deflection curve of the mean fibre was a circular arc, the longitudinal stress upon any given longitudinal fibre was of uniform intensity, the plane cross sections, originally normal to the primitive mean fibre, remained plane and normal to the mean fibre after its deformation, and there were consequently no slidings and therefore no shearing stress.[2] The intensities of the only stresses, viz., the longitudinal compressions and tensions, were to be computed by the methods of the third primary division. Mr. Barlow contested those conclusions, but Mr. Reilly submitted that no conclusions in the theory were more truly incontestable.

With respect to the fourth primary division, simple torsion, it would be unnecessary to enlarge upon it after what had been said respecting sliding. The hypothesis was the same as that used in explaining the second primary division, and however false in reality, its sufficiency for that purpose was fully confirmed by experiments on elastic torsion. The difference from simple sliding was that the normal transverse planes slid upon each other with a motion of rotation round the mean fibre. Only in the case of a cylindrical prism, did those originally normal planes remain normal after the deformation; in all other cases those planes became distorted by the action of the shearing stress, the effect of which was to diminish, in some forms very greatly, the total resistance per unit of section, compared with that of the cylindrical prism of equal area of cross section.

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[1] Says Morin (p. 252, 3^{me} edit. vol. 1), referring to "les limites entre lesquelles les formules déduites de la théorie sont applicables.—Mais on ne devra pas oublier, que ces formules et la théorie sur laquelle elles sont fondées, supposent expressément que les flexions sont renfermées dans des limites étroites pour lesquelles elles sont proportionelles aux charges, et qu'en poussant leur application plus loin et jusqu'à la rupture, on les étend à des circonstances où les phénomènes ne se passent plus conformément aux hypothèses admises."

[2] *Vide* Rankine, "Applied Mechanics," arts. 292, 304.

After the preceding remarks, he hoped that the fallacy of the objections made to the soundness of the theory under discussion would be evident: and that such problems as that mentioned by Mr. Bramwell, viz., the conditions of stress and deformation, within the elastic limits, in an extended prism of variable section, having re-entrant angles at the places of variation, could be solved by that theory, and by no other. He did not say that he could himself produce such a solution, either general or particular, he had never attempted it; but if he tried and failed, that would be his fault, and not that of the theory. The problem was no doubt a difficult one, but that was simply owing to the complexity of the conditions, and not to the insufficiency of the theory itself.

A few remarks on the subject of the proper functions of the experimental branch of the science might be in place in this discussion. He conceived those functions to be limited to the following inquiries, viz.:—

1st. Experiments to determine the correct value of the co-efficient, or modulus, of elasticity for each kind of resistance of all the different materials of construction.

2nd. Experiments to determine the limit of elasticity of each kind of resistance for the same bodies; such limit of elasticity being understood to be the point, where the rate of increase of the deformation, ceased to be approximately constant for uniform increments of intensity of stress.

3rd. Experiments upon elastic deformation of materials, in order to verify the sufficiency of the hypotheses upon which the theoretical branch was founded.

4th. Experiments to determine empirical methods for the provisional solution of particular cases of resistance, should such cases arise, when the theory failed to treat them satisfactorily, owing to its present insufficient development, the complexity of the question, or the absence of a sufficient hypothesis. But deductions from experiments of this class must be inferred with great caution, and meanwhile every effort should be made to extend the theory of elastic resistance to the satisfactory solution of such questions.

5th. Experiments to determine the effect of shocks, and also of repeated applications of the same load, within the elastic limits, upon the durability of materials.

6th. Experiments to determine the effect of the time during which a constant load acted upon a body, upon its co-efficients and limits of elasticity.

7th. Experiments upon the effects of different modes of manufacture of the same material.

Experiments upon the ultimate resistance and deformation of solids were of interest to the student of molecular physics; they were also valuable in enabling an engineer to discriminate between different qualities of what professed to be, in a commercial sense, the same materials. But they were of no value in assisting the construction of a true science of the elastic resistance of materials, except in cases, if such there were, where the co-efficients and the limits of elasticity had a known relation to the ultimate resistances.

In replying upon a discussion of this nature a few remarks might not be out of place, on the difference between the motives and objects guiding the pursuit of mathematical studies in England and in France. In the former, mathematics seemed to be pursued chiefly for its own sake; most eminent mathematicians, and still more, the students of the English universities, confined their studies and endeavours to enlarging the boundaries of abstract mathematics, and they seemed very much to ignore, and even to disdain, its practical applications, except to physical astronomy, to optics, and recently to magnetism and the theory of heat. The French mathematicians were not at all behind their English brethren in the success of their efforts in the cause of pure mathematics, astronomy, and optics; but most of the *élite*, the wranglers as they might be called, of their great school, the Ecole Polytechnique, directed their chief efforts, in after life, to the solution of problems in mechanical physics, and other applications practically conducive to the industrial advancement of their country, and with energy and success. But they had another merit which might be also commended to the imitation of their English brethren; they generally wrote their books and memoirs, with a degree of lucid and minute explanation calculated to render them useful to the greatest number of competent readers. Whereas most of the English works and memoirs, putting aside school and college text books, were so condensed and frequently so obscure, as to be almost unintelligible to readers less accomplished than the authors themselves.

Mr. Gregory, President, in closing the discussion, said he had hoped to have the opportunity of offering a few remarks upon some of the practical points which had arisen in the course of the discussion now brought to a close; but he felt he ought not to trespass upon the time of the Meeting. He could not help congratulating the Institution upon the discussion that had taken place on this Paper —a Paper of great value and interest in itself, admired even by those who differed from some of the conclusions; and the discussion had shown an amount of theoretical ability which he did not remember to have seen excelled, perhaps rarely equalled within those walls. He was quite sure no one undervalued the importance of theoretical investigation, or the able arguments of the last speaker and others who preceded him. At the same time all would agree that this subject might be most usefully supplemented by some such investigation as Dr. Pole pointed to, and which he gave a half promise to supply himself—he meant in the experimental branch of this subject, which many would consider to be of large importance, although those of high mathematical attainments considered it a subordinate branch of scientific investigation. Still he thought practice would sometimes lead them, if not to a complete, yet to a partial solution of anomalies which occurred, if they blindly followed the deductions of science, without studying the actual operations of the laws of nature, in the works which were carried out under their direction. Therefore he hoped they might derive some benefit from the practical consideration of the question, which had been discussed theoretically during the last few evenings. When they were told that engineers of other countries so far excelled those of England in technical knowledge, this discussion, he thought, might be held up as some proof to the contrary.

M. Gaudard said, in a letter to the Secretary, "I thank you for the printed proof of the discussion which has taken place in your Institution on the Resistance of Materials. I have read with the greatest interest this profound discussion, so rich in instruction. I really know not what to add on my part, after the lucid and complete manner in which Mr. Callcott Reilly has defended the foundations of the theory that I have attempted to sum up in my memoir. I feel, nevertheless, the worth of the objections or criticisms which have been raised. My memoir has certainly too condensed a form for an abstract subject. The excuse is, that I worked somewhat at random, knowing by experience that engineers of every country have many other matters to attend to than laborious mathematical researches, and I was far from hoping that my essay would obtain so sympathetic a reception, and be followed by so profound a discussion.

"I fully recognise that in confining myself to the purely mathematical portion I have left on one side a part of the subject no less important, more urgent and more laborious, the systematic and full discussion of the results of experience. I shall look forward with much interest to the early appearance of this important work, which appears so eminently fitted to the practical English mind.

"Mr. Reilly has clearly shown that the 'theory of the fibre' no longer misleads theorists of the present day. I ought to have explained that better; I trusted in the title alone of the chapter ('extension') to restrict the cases of application of this idea of a prism likened to a bundle of separate fibres. I have introduced it, besides, not as my personal opinion, but as the opinion of the formula $N = R\omega$ (art. 2) adopted in practice. I have omitted also to speak of the accessory circumstance raised as an objection by Mr. R. P. Brereton (page 65), namely, that in practice the inducing effort is transmitted to the fibre by the medium of certain particular points: for example, in pinching the lateral surface of the body towards its extremities, so that the distribution of the effort over the various molecules only operates more or less completely, at some distance from these same extremities. It is a point that the theorist, M. de St. Venant has well set forth and discussed in his book. It should certainly be noted, but one does not know how to impose on theory the obligation to enter into these thousand accessories, or to regard it as useless when it neglects them; it does not overlook these circumstances, but consider them as provided for in the arbitrary co-efficients of security.

"The explanations of Mr. Reilly on the small practical importance of the strains bordering on rupture are very instructive to me. I understand better now the great importance of the limit of elasticity, and of the experimental determination of its value in order to base on it, rather than on tests carried to rupture, the choice of co-efficients of practical security.

"I only meet with a single definite point on which I should be induced to modify slightly the mode of view of Mr. Reilly. This refers to the explanation (page 32) of the form of the expressions of the slidings, such as $g'' = \frac{dz}{dy} + x\phi$. Mr. Reilly appears to admit the objection of Mr. Heppel, that this value of g'' would change in changing the unit of measure. I think that this is not so, and that $x\phi$ is an abstract number, just as much as the first term $\frac{dz}{dy}$. The rotation ϕ, in fact, reckons between two transverse sections of which the distance is equal to the unit of length; so that, if the unit changes, the value attributed to ϕ should be modified in inverse proportion to the value of x. Let us admit for example, with the mètre as unit, that we have $x = 0 \cdot 03$ and $\phi = \frac{\pi}{100}$. If we wish to take the décimètre as unit we shall write $x = 0 \cdot 30$; but, on the other hand, the two sections to compare being ten times nearer together, it will be necessary to write for their relative displacement $\phi = \frac{\pi}{1000}$; thus the product $x\phi$ will preserve the same value as with the first unit.

"Mr. C. W. Siemens and Mr. R. H. Bow would wish to modify the form of the curves of inflection of the transverse sections of prisms submitted to flexure. This question becomes secondary for the practical object of science, according to the observations of Mr. Reilly, for in the ordinary case of very slight deformation all agree that these curves differ very slightly from right lines. I will therefore only point out, that the form I have reproduced in Figs. 5, 7, and 8, is borrowed from M. de St. Venant, and that this author has furnished ample demonstrations.

"Mr. R. P. Brereton contests (page 67) that the side attacked of a rivet submitted to shearing is crushed before the other side has decided to move. I have no doubt that he has good reason for it so far as is appreciable for iron, when the action takes place without shock. That which I have said is only an induction. I should be led to suppose that, in a very slight measure, one would find in a hard material something similar to what one would obviously see on a rivet of lead or of indiarubber.

"I should further point out that the augmentation of $\frac{1}{8}$ to $\frac{1}{6}$ at page 38 would result from developments furnished by M. d'Albaret (page 53, year 1866, Annales des Ponts et Chaussées de France), in the paper mentioned by M. Bresse in his letter to Mr. Heppel. It is only the question of one correction *grosso modo* by which one would modify the theoretic diagram to make it pass from the case of a beam of uniform section to the case of variable section according to the law of equal resistance.

"To sum up, I congratulate myself on the interesting facts which have been brought forward, and on the light thrown on the subject generally in the course of this discussion on the resistance of materials."