

claim to the improved form and construction thereof, by which a much larger portion of heat is saved than by any of those formerly known. I also claim the employment of a portable oven, such as I have described, to be placed over an open fire, and to be used in the manner, and for the purposes set forth; and likewise, the general construction, arrangement, and combination of the various parts of the fireplace and the apparatus described, as contributing to the general effect intended to be produced by the particular parts.

JAMES COX.

Remarks by the Editor.—Although there is nothing new in the general principle of introducing air from without, and warming it by the waste heat from the fire, we are of opinion that there are, in the above, some points in the general arrangement of the whole, which must be productive of decided advantage. We entertained the idea that a grate placed in a sitting room, and provided with the requisite arrangements for cooking, must necessarily be unsightly; we have seen one of them set, however, and were much pleased with its appearance, and think that in those families where it is necessary to economise room, and to turn all the apartments to the best advantage, this fireplace will be productive of real benefit.

Hot Air Blast.

At a meeting of the Chemical Section of the British Association for the Advancement of Science, Dr. Clark gave an account of Mr. Nixon's process for smelting iron by aid of the hot-blast, and exhibited numerical results of the advantages derived from the new process. The saving is so great, that the total amount of coal now necessary to produce one ton of iron, amounts only to two tons and fourteen hundred-weight, whereas formerly it required eight tons and one and a half hundred-weight, being a saving of five tons and eight hundred-weight for each ton of iron produced. This subject was discussed at considerable length.

[*Ed. New Philos. Journ.*

¶ *Report on the Present State of our Knowledge respecting the Strength of Materials.* By PETER BARLOW, Esq., F. R. S., Corr. Memb. Inst. France, &c. &c.*

[Made to the British Association for the Advancement of Science.]

The theory of the strength of materials, considered merely as a branch of mechanical or physical science, must be admitted to hold

* The volume of reports made to the British Association for the Advancement of Science, in the session of 1833, has just reached this country, and we hasten to lay before our readers the report of Mr. Barlow, on an interesting branch of practical science. This will be followed by Mr. Rennie's report on Hydraulics, considered as a branch of engineering.

only a very subordinate rank; but in a country in which machinery and works of every description are carried to a great extent, it certainly becomes a subject of much practical importance; and it was no doubt viewing it in this light which led the committee of the British Association, at their last meeting, to do me the honour to request me to furnish them with a communication on the subject. In drawing my attention to this inquiry, the committee have subdivided it into the following heads: 1. Whether, from the experiments of different authors, we have arrived at any general principles? 2. What these principles are? 3. How modified in their application to different substances? And what are the differences of opinion which at present prevail on those subjects?

To these questions, without a formal division of the essay, I shall endeavour to reply in the following pages, by drawing a concise sketch of the experimental and theoretical researches which have been undertaken with reference to these inquiries.

The subject of the strength of materials, from its great practical importance, has engaged the attention of several able men, both theoretical and practical, and much useful information has been thereby obtained. As far as relates to the mechanical effects of different strains, every thing that can be desired has been effected; but the uncertain nature of materials generally will not admit of our drawing from experiment such determinate data as could be wished. Two trees of the same wood, grown in the same field, having pieces selected from the same parts, will frequently differ from each other very considerably in strength, when submitted to precisely the same strain. The like may be said of two bars of iron from the same ore, the same furnace, and from the same rollers, and even of different parts of the same bar; and so likewise of two ropes, two cables, &c. We must not, therefore, in questions of this kind, expect to arrive at data so fixed and determinate as in many other practical cases; but still, within certain limits, much important information has been obtained for the guidance of practical men; and by tabulating such results in a subsequent part of this article, I shall endeavour to answer the leading questions of the committee of the British Association, as far, at least, as relates to experimental results. In reference to theory, it must also be admitted that some uncertainty still remains; but this, likewise, is in a great measure to be referred to the nature of the materials, which is such as to offer resistances by no means consistent with any fixed and determinate laws.

Hence, some authors have assumed the fibres or crystals composing a body, to be perfectly incompressible, and others as perfectly elastic; whereas it is known that they are strictly neither one nor the other, the law of resistance being differently modified in nearly every different substance; and as it is requisite theoretically to assume some determinate law of action, it necessarily follows that some doubt must also hang over this branch of the subject. It is, however, fortunate, that whatever may be the uncertainty on these points, the relative strength of different beams or bolts of the same material, of similar forms, and submitted to similar strains, is not thereby affect-

ed; so that whatever may be the law which the fibres or particles of a body observe in their resistance to compression or extension, still, from the result of a well conducted series of experiments, the absolute resisting force of beams of similar forms, of the same materials of any dimensions, submitted to similar strains, may, as far as the mean strength can be depended upon, be satisfactorily deduced. An examination of these different views taken of the subject by different writers, will, it is hoped, be found to furnish a reply to the other queries of the committee. The first writer who endeavoured to connect this inquiry with geometry, and thereby submit it to calculations, was the venerable Galileo, in his *Dialogues*, published in 1633. He there considers solid bodies as being made up of numerous small fibres, placed parallel to each other, and their resistance to separation to a force applied parallel to their length, to be proportional to their transverse area—an assumption at once obvious and indisputable, abstracting from the defects and irregularities of the materials themselves. He next inquired in what manner these fibres would resist a force applied perpendicularly to their length, and here he assumed that they were wholly incompressible; that the fibres under every degree of tension resisted with the same force; and consequently, that when a beam was fixed solidly in a horizontal position, with one end in a wall, or other immovable mass, the resistance of the integument fibres was equal to the sum of their direct resistances multiplied by the distances of the centre of gravity of their section from the lowest point, about which point, according to this hypothesis, the motion must necessarily take place.

The fallacy of these assumptions was noticed, but not corrected, by several subsequent authors. Leibnitz objected to the doctrine of the fibres resisting equally under all degrees of tension, but admitted their incompressibility, thereby still making the motion take place about the lowest point of the section; but he assumed for the law of resistance to extension, that it was always proportional to the quantity of extension. Accordingly, as the one or the other of these hypotheses was adopted, the computed transverse resistance of a beam, as depending on the absolute strength of its fibres, varied in the ratio of 3 to 2; and many fanciful conclusions have been drawn by different authors, relative to the strength of differently formed beams, founded upon the one or the other of these assumptions, which, however, it will be unnecessary to refer to more particularly in this article.

We have seen that each of these distinguished philosophers supposed the incompressibility of the fibres; but James Bernouilli rejected this part of Leibnitz's hypothesis, and considered the fibres as both compressible and extensible, and that the resistance to each force was proportional to the degree of extension or compression. Consequently, the motion, instead of taking place, as hitherto considered, about the lowest point of the section, was necessarily about a point within it; and his conclusion was, that whatever be the position of the axis of motion, or, as it is now commonly called, the neutral axis, the same force applied to the same arm of a lever, will always

produce the same effect, whether all the fibres act by extension or by compression, or whether only a part of them be extended, and a part compressed. Dr. Robinson, in an elaborate article on this subject, also assumes the compressibility and extensibility of the fibres, and, as a consequence, assumes the centre of compression as a fulcrum, about which the forces of extension are exerted, and the resistance of both forces to be directly proportional to the degree of compression or extension to which they are exposed; that is, he assumed each force, although not necessarily offering equal power of resistance, to be individually subject to the law of action appertaining to perfectly elastic bodies. In carrying on the experiments which laid the foundation of my *Essay on the Strength of Timber, &c.*, in 1817, I was led, by several circumstances I had observed, to doubt whether, in the case of timber, this assumption of perfect elasticity was admissible. And as some of the specimens used in my experiments showed very distinctly after the fracture, the line about which the fracture took place, I thought of availing myself of this datum, and of that which gave the strength of direct cohesion, in order to deduce the law of resistance from actual experiments, instead of using any assumed law whatever.

The result of this investigation implied that the resistance was nearly as first assumed by Galileo, and although very different from what I had anticipated, yet, as an experimental result, I felt bound to abide by it, attributing the discrepance to the imperfectly elastic properties of the material. Mr. Hodgkinson, however, in a very ingenious paper, read at the Manchester Philosophical Society in 1822, has pointed out an error in my investigation, by my having assumed the momentum of the forces on each side the neutral axis as equal to each other, instead of the forces themselves; consequently, the above deduction in favour of the Galilean hypothesis fails. This paper did not come to my knowledge till the third edition of my essay was nearly printed off, and the correction could not then be made; but being made, it proves that the law of actual resistance approaches much nearer to that of perfect elasticity, than from the nature of the materials there could be any reason to expect; so that, in cases where the position of the neutral axis is known, and also its resistance to direct cohesion, a tolerably close approximation may be made to the transverse strength of a beam of any form, by assuming the resistance to extension to be proportional to the quantity of extension, and the centre of compression as the fulcrum about which that resistance is exerted. But I have before observed, and beg again to repeat, that by far the most satisfactory data will always be obtained by experiments on beams of the like form, (however small the scale,) and of the same material as those to be employed, because then the law of resistance forms no part of the inquiry, and does not necessarily enter into the calculation, the ultimate strengths being dependent on the dimensions only, whatever may be the absolute or relative resistance of the fibres to the two forces we have been considering.

At present, I have only considered the resistance of a beam to a transverse strain; but there is another mode of application, in which,

again, the law of resistance necessarily enters, and has led to many curious and mysterious conclusions. This is when a force of compression is applied parallel to the length. In the case of short blocks, the resistance of the material to a crushing force is all that is necessary to be known; and in the *Philosophical Transactions* for 1818, we have a highly valuable table of experimental results on a great variety of materials, by George Rennie, Esq., which contains nearly all the information on this subject that can be desired. But when a beam is of considerable length in comparison with its section, it is no longer the crushing force that is to be considered; the beam will bend and be ultimately destroyed by an operation very similar to that which breaks it transversely; and the investigation of these circumstances has called forth the efforts of Euler, Lagrange, and some other distinguished mathematicians.

When a cylinder body, considered as an aggregate of parallel fibres, is pressed vertically in the direction of its length, it is difficult to fix on data to determine the point of flexure, since no reason can be assigned why it should bend in one way rather than in another; still, however, we know that, practically, such bending will take place. And it is made to appear, by the investigations of Euler and Lagrange, that with a certain weight this ought, theoretically, to be the case; but that with a less weight, no such effect is produced—an apparent interruption of the law of continuity not easily explained, which exhibits itself, however, analytically, by the expression for the ordinate of greatest inflection being imaginary, till the weight or pressure amounts to a certain quantity. Another mysterious result from these investigations is, that while the column has any definite dimensions, and is loaded with a certain weight, inflection, as above stated, takes place; but if the column be supposed infinitely thin, then it will not bend till the weight is infinitely great. These investigations of two such distinguished geometers, are highly interesting as analytical processes; but the hypothesis on which they are founded, namely, that of the perfect elasticity of the materials, is inconsistent with the nature of bodies employed in practice; they form, therefore, rather an exercise of analytical skill, than of useful practical deductions. There is, however, one useful result to be drawn from these processes, which is, that the weight under which a given column begins to bend, is directly as its absolute elasticity; so that, having determined experimentally the weight which a column of given elasticity will support safely, or that at which inflection would commence, we may determine the weight which another column of the same dimensions, but of different elasticity, may be charged with without danger.

M. Gerard, a member of the Institute of France, aware of the little practical information to be derived from investigations wholly hypothetical, has given the detail of a great number of actual experimental results connected with this subject, on oak and fir beams of considerable dimensions, carried on at the expense of the French government—from which he has drawn the following empirical formulæ, viz:

$$1. \text{ In oak beams, } \frac{P f^3}{3 b} = \frac{11784451 (f + .08) a h^3}{1.3}$$

$$2. \text{ In fir beams, } \frac{P f^3}{3 b} = 8161128 a h^3$$

where P = half the weight in kilogrammes, a the less, and h the greater sides of the section; f half the length of the column, and b the versed sine of inflection, the dimensions being all metres.

How far these formulæ are to be trusted in practical construction, is, however, I consider, rather doubtful, because they are drawn from a number of results which differ very greatly from each other; and in one case in particular, the result, as referred to the deflection of beams, has been satisfactorily shown to be erroneous by Baron Charles Dupin, in vol. x. of the *Journal de l'Ecole Polytechnique*, as also by a carefully conducted series of experiments in my *Essay on the Strength of Timber*, &c. I conceive it, therefore, to be very desirable that a set of experiments on this application of a straining force on vertical columns, should be undertaken; and it is, perhaps, the only branch of the inquiry connected with the strength of materials, in which there is a marked deficiency of practical data; at the same time, it is one in which both timber and iron are being constantly employed. We see every day in the metropolis, houses of immense height and weight being built, the whole fronts of which, from the first floors, are supported entirely by iron or wooden columns; and all this is done without any practical rule that can be depended upon for determining whether or not these columns are equal to the duty they have to perform.

I say this with a full knowledge that Mr. Tredgold has furnished an approximate rule for this purpose; but the principle on which it is founded, has no substantial basis. The extraordinary skill which Mr. Tredgold possessed in every branch of this subject, and the great ingenuity he has displayed in investigating and simplifying every calculation connected with architectural and mechanical construction, certainly entitle his opinion to high consideration; but still, on a subject of such high importance, it would be much more satisfactory to be possessed of actual experimental data. The supposition he advanced was made entirely as a matter of necessity, and I am confident that no one would have been more happy than himself to have been enabled to substitute fact for hypothesis, had he possessed the means of adopting the former. But unfortunately such a series of experiments is too expensive and laborious to be undertaken by an individual situated as he was, having a family to maintain by his industry, and whose close and unremitting application to these and similar inquiries, in all probability shortened his valuable life.*

* Mr. Tredgold's *Principles of Carpentry*, and his *Treatise on the Strength of Iron*, ought to be in the possession of every practical builder; besides which two works, he published many separate articles on the same subject, in different numbers of the *Philosophical Magazine*.

At present, I have referred principally to experiments made with a view of determining the ultimate strength of materials; and with data thus obtained, practical men have been enabled to pursue their operations with safety, by keeping sufficiently within the limits of the ultimate strain the materials would bear, or rather with which they would just break, some working to a third, others to a fourth, &c., of the ultimate strength, according to the nature of the construction, or the opinion of the constructor. But it is to be observed, that although we may thus ensure perfect safety, as far as relates to absolute strength, there are many cases in which a certain degree of deflection would be very injurious. It is therefore highly necessary to attend also to this subject, particularly as the deflection of beams, and their ultimate strength, depend upon different principles, or are at least subject to different laws. Hence, most writers of late date give two series of values, one exhibiting the absolute or relative strength, and the other the absolute or relative elasticities. These values will, of course, be found to differ in different authors, on account of the uncertainty in the strength of the materials already referred to; but amongst recent experiments, the difference is not important; they will also be found differently expressed, in consequence of some authors deducing these numbers from experiments differently made. Some, for example, have drawn their formulæ for absolute strength, from experiments made on beams fixed at one end, and loaded at the other, using the whole length; some, again, from experiments on beams supported at each end, and loaded in the middle, using the half length. Some take the length in feet, and the section in inches; others, all the dimensions in inches; and a similar variety occurs in estimating the elasticity. Also, in the latter case, some authors employ what is denominated the modulus of elasticity, in which latter case the weight of the beam itself, and consequently its specific gravity, enters. These varieties of expression, however, are not to be understood as arising from any difference of opinion amongst the authors from whom they proceed, but merely as different modes of expressing the same principles; indeed, in reply to that inquiry of the committee in reference to this point, I may, I think, venture to say, there is not at present any difference of opinion on any of the leading principles connected with the strength of materials, with the exception of such as are dependent entirely upon the imperfect nature of the materials themselves, and which, as we have seen, will give rise to different results in the hands of the same experimenter, and under circumstances in every respect similar.

As I distinguish the doctrine of the absolute resistance, or strength of materials, which is founded on experiment, from that which relates to the amount and resolution of the forces or strains to which they are exposed, which is geometrical; and as I confine myself to the former subject only in this essay, it is not, I conceive, necessary to extend the preceding remarks to any greater length. I shall therefore conclude by giving a table of the absolute and relative values of the ultimate strength and elasticity of various species of timber and

other materials, selected from those results in which I conceive the greatest reliance may be placed.

Formulæ relating to the ultimate Strength of Materials in cases of Transverse Strain.—Let l b d denote the length, breadth, and depth, in inches, in any beam; w , the experimental breaking weight, in

pounds; then will $\frac{lw}{b d^2} = S$, be a constant quantity for the same

material, and for the same manner of applying the straining force; but this constant is different in different modes of application. Or, making S constant in all cases for the same material, the above expression must be prefixed by a coefficient, according to the mode of fixing and straining.

1. When the beam is fixed at one end, and loaded at the other,

$$\frac{lw}{b d^2} = S.$$

2. When fixed the same, but uniformly loaded,

$$\frac{1}{2} \times \frac{lw}{b d^2} = S.$$

3. When supported at both ends, and loaded in the middle,

$$\frac{1}{4} \times \frac{lw}{b d^2} = S.$$

4. Supported the same, and uniformly loaded,

$$\frac{1}{8} \times \frac{lw}{b d^2} = S.$$

5. Fixed at both ends, and loaded in the middle,

$$\frac{1}{6} \times \frac{lw}{b d^2} = S.$$

6. Fixed the same, but uniformly loaded,

$$\frac{1}{12} \times \frac{lw}{b d^2} = S.$$

7. Supported at the ends, and loaded at a point not in the middle. Then, n m , being the division of the beam at the point of application,

$$\frac{n m}{l^2} \times \frac{lw}{b d^2} = S.$$

Some authors state the coefficients for cases 5 and 6 as $\frac{1}{8}$ and $\frac{1}{16}$; but both theory and practice have shown these numbers to be erroneous.

By means of these formulæ, and the value of S , given in the following table, the strength of any given beam, or the beam requisite to bear a given load, may be computed. This column, however, it must be remembered, gives the ultimate strength, and not more than one-third of this ought to be depended upon for any permanent construction.

Formulae relating to the Deflection of Beams in cases of Transverse Strains.—Retaining the same notation, but representing the constant by E , and the deflection in inches by δ , we shall have,

<p>Case 1. $\frac{32}{1} \times \frac{l^3 w}{b d^3 \delta} = E.$</p> <p>2. $\frac{12}{1} \times \frac{l^3 w}{b d^3 \delta} = E.$</p> <p>3. $\frac{1}{1} \times \frac{l^3 w}{b d^3 \delta} = E.$</p>	<p>Case 4. $\frac{5}{8} \times \frac{l^3 w}{b d^3 \delta} = E.$</p> <p>5. $\frac{2}{3} \times \frac{l^3 w}{b d^3 \delta} = E.$</p> <p>6. $\frac{5}{12} \times \frac{l^3 w}{b d^3 \delta} = E.$</p>
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Hence again, from the column marked E in the following table, the deflection a given load will produce in any case, may be computed; or, the deflection being fixed, the dimensions of the beam may be found. Some authors, instead of this measure of elasticity, deduce

immediately from the formula $\frac{l^3 w}{3 b d^3 \delta} = E$, substituting for w , the

height in inches of a column of the material, having the section of the beam for its base, which is equal to the weight w , and this is then denominated the modulus of elasticity. It is useful in showing the relation between the weight and elasticity of different materials, and is accordingly introduced into the following table.

The above formulæ embrace all those cases most commonly employed in practice. There are, of course, other strains connected with this inquiry, as in the case of torsion in the axles and shafts of wheels, mills, &c., the tension of bars in suspension bridges, and those arising from internal pressure in cylinders, as in guns, water-pipes, hydraulic presses, &c.; but these fall rather under the head of the resolution of forces, than that of direct strength. It may just be observed, that the equation due to the latter strain is

$$t(c-n) = nR.$$

Where t is the thickness of metal in inches, c the cohesive power in pounds of a square inch rod of the given material, n the pressure on a square inch of the fluid in pounds, and R the interior radius of the cylinder in inches. Our column marked C will apply to this case, but here again not more than one-third the tabular value can be depended upon in practice.