ter sometimes exhibited, is only developed, after being kept some time, probably influenced by changes of temperature. It also seems that the effective force of the oil decreases in proportion as the sensitiveness to ignition increases.

The object of the inventors, however, has not been the production of an explosive oil of greater strength or better quality than the nitro-glycerine heretofore produced, and though some of the results of experiments tried by them would seem to indicate that it might be so, this is no part of their claim, their object being to render the manufacture of the article so simple and safe that it can at all times be made in all desirable quantities, in the immediate vicinity of the points where it is to be used, thus securing the following direct and unquestionable advantages.

1st. Removing the necessity of packing, storing, and transporting a material which grows more and more dangerous the longer that it is kept.

2d. Supplying a means of manufacture unattended with a shadow of danger, other than in the mere handling of acids.

3d. Dispensing with the labor and time required to thoroughly wash the material, as it may be poured directly from the precipitating jar, into the hole or centre, where it is to be exploded, any time within ten days after the oil has been mixed.

5th. The opportunity afforded to use the oil, when it is unquestionable that its effective force (whether this is due to the process of manufacture, or merely because newly made), is very much greater than when it has been kept or packed, while the danger of handling it is proportionally less, as it then *cannot* be exploded when *unconfined*, and with difficulty (as compared to powder or oil that has been packed), when confined.

HISTORY OF THE INVESTIGATIONS ON THE RESISTANCE OF SOLID BODIES.

By De Volson Wood.

The French have made a very full investigation of this subject, and, in their records, have not been satisfied with merely sketching its outlines, but have entered very fully into its details, and given all the points of interest brought forward by each writer and experimenter. An abridgment of this history, covering 220 pages, is given in the introduction to *Navier's Résistance des Corps Solides*,* an abstract of which constitutes the following article. The history of the early developments of a science, even in its minutest details, is more interesting than the voluminous productions which trace it in its broader growth. Accordingly, I have given quite a full abstract of the early history, and barely touched upon the later portion.

The architects of the middle ages have not left any traces in their writings of the manner by which they determined the proportions of their structures. All their investigations were upon the equilibrium of external or applied forces, without even considering the resisting forces at the supports, or at the fixed points, and much less the internal resistances of the fibres or elastic resistances.

For instance, they did not consider that *ten-pins* were maintained in a vertical by their proper position, but by the equilibrium of all the external forces.

They were fond of determining the resistance of blocks to crushing; and before using a new kind of stone, they probably made experiments by the aid of levers.

In regard to carpentry, they reasoned upon the combination of timbers which constituted frames; but the low price of timber permitted them to use pieces of large dimensions. But we may presume, however, that they knew that a prismatic piece, loaded transversely, was stronger in proportion to its depth than to its length; for the principal object of Galileo seemed to be, at first, to determine the *theoretical law* of this resistance, as well as the ratio between the *absolute* resistance which is exerted when a piece is broken by a tensile strain, and the *relative* resistance when it is broken by a force perpendicular to its length. After his visit to the arsenal at Venice, he considered the cause of the failure of large machines when small ones of the same kind had succeeded.

At first he considered a horizontal cylinder or paralopipedon, having one end fixed firmly in a wall and a weight acting on the other. He supposed that all the points of the beam in the section of the plane of the wall resisted rupture equally, and hence the horizontal resultant passed through the centre of this section, and was in equilibrium with the weight at the other end of the beam, by means of a right angled lever, of which the point of support was at the lower edge of the section of rupture.

* Resistance of Solid Bodies, with notes and appendix, by M. Barré-de-Saint-Venant. Published by Dunod. Paris, 1864.

The forces being reciprocally proportional to the arms of the lever, he concludes that the *absolute* resistance is to the *relative* as the horizontal arm, or the projecting length of the beam, is to its vertical arm, or half depth of the beam.

From this Galileo easily deduced that a rectangular piece pressed sidewise resisted more than the same piece pressed flatwise, in the ratio of the depth to the breadth of the *section of rupture;* that the weights capable of being supported by pieces which have their ends fixed and the weights supported at the free ends, or whose ends rest on two supports, and the weights supported in the middle, *are as their breadths, as the square of their depths, and inversely as their lengths;* also, *that hollow cylinders, such as bones, quills, the stalks of plants, &c., resist much more to transverse stress than solid cylinders of equal volume*, in a proportion which he determined, which caused him to reflect on the works of creation and considerations of an elevated order.

Finally, flowing from the same principles, he showed that a piece which has one end firmly fixed, and whose inferior surface is a horizontal plane, and whose vertical longitudinal sections are equal parabolas, the vertices of which are at the end where the weight is suspended, is equally strained at every vertical section, from one end to the other. Such a piece is called a *solid of equal resistance*, and gives the most economical distribution of the material.

Galileo's hypotheses were erroneous in supposing that the strains were equal at all the points in the section of rupture, and in placing the support of the lever at the base of the section of rupture; and yet all the theorems which he deduced have been confirmed by later researches, except that which gives the ratio between the resistances to rupture by a longitudinal and by a transverse pressure.

Galileo has been charged with ignorance of the principles which enables us to determine the form of beams of uniform resistance, because he stated that "We may diminish the volume of a rectangular beam more than $\frac{33}{100}$ without diminishing the strength, which is a great consideration in the construction of ships;" but the architect, Francis Blondel, detected the error of this statement, by showing that if the beam is to resist a load applied *at any point*, the longitudinal section should be elliptical; but, in this case, as Navier has remarked, there will be an excess of strength at every section, except the one directly under the load.

The principles of Galileo, as thus understood, were applied by P. Fabri, Vincent, Viviani, and especially by his follower, P. Grandi; but about the same time, Hooke, a noted physicist of England, and Marriotte, a noted physicist of France, announced the basis of the true theory of the rupture of solids.

In 1678, Robert Hooke, in pursuing his studies of steel springs, especially of watches, gave his famous principle *ut tensio sic vis*, which he says he discovered in 1660, but which was first announced in 1676, under the anagram *c e i i i n o s s s t t u u*. He based the theory of elasticity on the proportionality of the extensions or contractions to the forces which produced them, and thus explained the vibratory movements of bodies, and added the fruitful remark, *that when elastic bars are bent, the material resists elongation on the convex side, and compression on the cancave side.*

Near the same time, in 1680, Marriotte, a Frenchman, having discovered, by several experiments on wooden and glass rods, that the ratio between the resistance to rupture by a pull and by a transverse strain was greater than that given by Galileo, ascertained *that hard bodies, elongated more or less under the action of different weights, and very nearly proportioned to those weights*, and *when the weights were removed they returned to their original length*, and that they are also compressible, so that a stick which is bent contracts on the concave side, and extends on the convex side; and he added, that *it is reasonable to suppose that this compression resists as much as the extension;* finally, the extended parts rupture only because they are *extended* beyond what they are able to resist.

We here see a correct statement of the essential principles of flexure as now understood. Galileo did not recognize any extension of the fibres; and Marriotte supposed that extension always preceded rupture. But the latter made an error in the application of his principles; for he supposed that the resistance to rupture is the same as if the section turned about the base, and the strains increased uniformly from zero, on the lower side, to that producing rupture on the upper side; and hence, in a rectangular section, the resultant will be at $\frac{2}{3}$ the depth, and the total resistance $\frac{1}{2}$ what it would be if all the fibres were equally strained. Accordingly, the *absolute* strength would be to the *relative* as the length to $\frac{1}{3}$ the depth, instead of $\frac{1}{2}$, as found by Galileo. Afterwards he definitely stated *that the axis of equilibrium, on which the fibres are neither extended nor compressed, is at the middle of the depth;* but he still erred in assuming that the resistance was the same as if all the fibres were all extended. The axis of equilibrium M. Dupin called the *line of invariable fibres*, and Tredgold, and more recent writers, *the neutral axis.*

James Bernoulli, in his writings of 1705, claimed that he was the first to give the true hypothesis for calculating the resistance of solids. Until recently, writers generally gave him the credit of being the first to recognize the compressive resistance. But his theory was the same as Marriotte's, with one unfortunate change, consisting *in regarding the extensions and compressions as augmenting in a less ratio than the forces which produce them.* He endeavored to establish his position by saying, that if compressions were proportional to weights which cause them, a rod would be compressed more than its whole length; and he added that it would be the same with extensions, as they are only negative compressions; and he tried to confirm it by some slight experiments on certain cords, which he found did not follow the law of proportionality rigidly. His reasoning would have been correct, were it not that the elasticity of all materials is changed for a sensible amount of extension or compression.

Bernoulli also erred the same as Marriotte, in affirming that the transverse force of rupturing was the same, whether the fibres are all extended or all compressed, and that it made no difference where the fixed axis is supposed to be, whether at the top of the section or bottom, or at any point between. He failed to perceive the equality of the extensive and compressive forces, and hence could not fix the position of the neutral axis.

In July, 1684, two months after the death of Marriotte, Libnetz, who had learned of his predecessors' investigations and experiments, admitted, like Marriotte and Hooke, the law of the proportionality of the longitudinal stresses, but placed, like his predecessors, the axis of rotation at the base of the section.

Varignon, in 1702, presented a *Memoir on the Resistance of Solids*, in which he placed the neutral axis at the base of the section, and assumed the extensions an unknown function of strains, and deduced a general formula of resistance; then *assumed* different laws, and compared the results with previous experiments, and thus tried to ascertain the most probable one.

The acamedician, Parent, who has not received the notice which he merits, appears to have been the first to make a correct application of the principles of Marriotte. In his earliest memoirs he made the same errors as his predecessors; but, in a later work, about 1713, he took the moments about the *neutral axis*, and showed that the *sum of the moments of extension and compression gave a result only half the amount found by Marriotte and Bernoulli. He also proved that the sum of the resistances of the compressed fibres would equal that for the elongated ones*, a property before mentioned, and which enables us to establish the position of the *neutral axis*.

Bilfinger, a few years later, gave to Marriotte the credit of being the first to consider compressions; and Coulomb, in 1773, presented his celebrated memoir on the resistance of solids, in which we find laid down nearly all the bases of the theory of the stability of constructions. He established himself on correct principles by using the principle of Statics—then but little known—that the algebraic sum of all the forces must be zero on the three rectangular axes; and the sum of the moments about a fixed point are zero. With the former principles he determined the position of the *neutral* axis; and, with the latter, the ratio between the *relative* and *absolute resistance.* He entirely rejected the perfect rigidity of the material, and considered it elastic, and stated *that if the law of proportionality of extensions and compressions did not hold good up to the point of rupture, the neutral axis would change positions.* No real advance was made in the *theory* for forty years after this essay.

Barlow, in his "Essay on the Strength of Timber," as late as 1817, to determine the position of the *neutral axis*, assumed *the erroneous principle that the sum of the moments of resistances to compression equalled those to tension.* This he corrected in his work in 1837.

Tredgold placed the neutral axis at the centre of *rectangular* sections, but did not seem to understand the simple principles of Parent and Coulomb, which would have enabled him to establish the correct position for all forms of sections.

Navier is one of the most noted authors who has written upon this subject. At first he accepted the statements of Bernoulli and Marriotte, relative to the indifference of the position of the *neutral axis*, but showed that if it be assumed at the base of the section, or at the middle of the depth, that for *flexure the moment of resistance would be as the cube of the depth, and not as the square, as in the case of rupture.* Afterwards, he established an erroneous equation on the hypothesis that the resistance to flexure was proportional to the curvature of the fibre; and also to its relative elongation, which gave him a compound expression of two terms for the sum of the moments of resistance. But he corrected this expression in his first course to *L'école des Ponts et Chaussées*, and gave a correct expression for the resistance of the fibres when the position of the nextral axis is shown; but, like Duleau, he determined this axis by assuming that the sum of the moments of resistance to compression equaled those for tension. On the 14th of April, 1821, he presented his celebrated paper "On the Laws of Equilibrium and Movement of Solid Elastic Bodies," which was the foundation of *Molecular Mechanics*, or the general theory of elasticity. This was a thorough analytical investigation. In his course, in 1824, published in 1826, he corrected all his previous errors, and was the first to establish the principle *that the neutral axis passed through the centre of gravity of the section when the material is of uniform texture, and the strain is within the elastic limit.* In the same course he brought out more fully the distinction between the law of resistance to flexure and ultimate resistance to rupture, and solved many problems not before attempted. *He also made an essential distinction between gradual and sudden rupture;* but in regard to the latter, it is nearly or quite impossible to assign the law, and is of little practical importance. He also showed how the formulas for a limited strain can be deduced from those for flexure, and added the remark, which has since become very useful, that *it was often better, in practice, to use for safety the elastic limit than a fractional part of the resultant strength.* In the same course he solved the problem of finding the reaction of the supports when a straight piece rests on several horizontal supports and is loaded at different points; *and also the resistance of a piece fixed at both ends, and loaded at any point between the ends.* But one of the greatest steps towards making a practical use of the theoretical investigations of flexure, was made by this noted scholar, in making an approximate solution of the elastic curve. The exact expression of the elastic curve was first given by James Bernoulli, in an enigma, in 1691, and explicitly given in 1694; which is *that the radius of curvature is inversely as the moment of the force producing flexure.* This principle gives rise to a differential equation of the second order, which may generally be integrated once directly and in finite terms; but the second integration is only effected by quadratures, or elliptic functions, or by series. But Navier assumed that $DS = dx$ for small deflections, or what is the same, *that the tangent of the angle which the curve of the neutral axis makes with the axis of x is practically unity for small deflections.* This approximation enabled him to make an easy solution of a large class of problems not before attempted. Problems of elastic curves were discussed long before the laws of elasticity were known. Galileo *supposed* that a piece slightly bent was a parabola; and P. Pardis and Laius considered it a catenary. In 1744, Euler made a complete enumeration of elastic curves, and made nine classes, in the first of which the force is nearly or quite parallel to the axis of the piece, and has given rise to much discussion as applied to columns.

M. Persey, in 1834, stated that a single equation of moments about the neutral axis is not sufficient to establish the equilibrium of the exterior and interior forces of tension and compression, unless the neutral axis is one of the principal moments of inertia passing through the centre of gravity. If the resultant of all the forces is not perpendicular to one of the principal axes, the beam will be sprung sidewise. It is then necessary to take two equations of moments of the forces, and it is most simple to take them about the principal axes. If the principle axes of a straight piece—*i.e.*, the axis of the piece is straight, but the sides warped,—are not in a plane, the piece will be twisted at the same time that it is bent.

In 1840, M. Poncelet reduced, theoretically, the danger of longitudinal compression to that of transverse dilations.

Slipping and Tangential Forces.

Beams subjected to flexure experience two other kinds of strain.

1. That which results from slipping of the transverse sections upon each other, and of the slipping, necessarily simultaneous, of the longitudinal fibres upon each other.

2. That which results from tension or the unequal rotation of the sections about the axis of the piece.

M. Vicat called attention to the former in 1833, and called it a *transverse force.* Coulomb, in 1773, gave an expression which could easily be interpreted to mean the same thing. Young spoke of detrusion. Rankine calls it *transverse shearing* and *longitudinal shearing.* The latter is easily calculated when it is evenly distributed over the surface; but it is known to diminish from the centre to the base of the section. In the practical investigations of flexure, we assume that sections originally perpendicular to the axis of the piece remain normal to the neutral axis during flexure, and this is true only when the flexure is equal from one end to the other, or is the arc of a circle. In all other cases the sections cut the axis at a greater angle than they do the surfaces of the piece, and are warped.

Torsion.

Torsion is to the slipping of a section over its adjacent one, what flexure is to the extension or compression of the fibres. Torsion was studied for the first time by Coulomb, in 1784, and afterwards the theory was discussed by Lagrange, who gave incorrect equations, as was indicated by Binet, in 1814, and which were corrected by Poisson, in 1816 and 1833. Cauchy made an analysis of this subject, and considered the effect of warped transverse sections. His formulas formed the basis of the more thorough analysis of the subject, both theoretical and practical, by Chevandier and Wertheim, the results of which are published in several numbers of the *Annals de Chemie et Physique.* Notwithstanding the exact analytical solution of the problem of torsion is complicated, yet the exact problem of flexure by the mixed method is more complicated, and has been obtained only after long researches.—*Louisville Journal*, 1856.

Resiliance.

To determine whether the limits of cohesion when a body is subjected to a shock is reached, it is necessary to calculate what M. Poncelet has called *résistance vive*, and Young the *resiliance* of the piece or system—that is, to know the live power possessed by the system to resist the blow or shock. To do this we must know the greatest elongation $i=\frac{R_o}{E}$ beyond which it is not safe to pass, and then calculated the total dynamic work of the elastic resistance which is offered by the piece or system. The effect of the shock of a body very *resistant*, such as a spherical shell against a rod, is easily calculated when the weight of the rod is neglected; but in most practical cases the problem is a very difficult one to solve.

Molecular Mechanics.

Without any reference to the practical application of molecular mechanics, the subject has received the attention of the ablest mathematicians since the days of Bernoulli. It would extend this article much beyond suitable limits, to give a sketch of the analysis upon this topic by such noted men as Boscovich, Clairaut, Laplace, Newton, Fresnel, Cauchy, and Poisson.

Their discussions of such subjects as light, heat, capillary attraction, and the like, lead to practical results; but their discussions of the resistance of solids was of little practical value until the principles of elasticity were known and recognized. Of late the conditions of stability are founded upon the limits of elasticity—that is, the strain should not be so severe as to damage the elasticity of the material; but formerly it was established upon the cohesive resistance of the particles. These principles, in some cases, give very different results. In 1855, W. J. Macquorn Rankin gave an article on potential work, strains, stresses, &c., which was published in the *Transactions of the Royal Society* for that year.

The article from which these facts are taken closes with a detailed account of the experiments which have been made upon solids from the time the subject has been treated as a science to the present time; and although reference is made to the *Civil Engineers' Journal* of 1862, yet the author has strangely ignored the theory and experiments of Barlow on the "Resistance to Flexure," published in the *Civil Engineers' Journal* for 1856 and 1858. Barlow, as well as other writers, had observed that the modulus of rupture for transverse stress was not the same as the absolute strength or tenacity of the material when ruptured by tension; and he proposed a new theory, called by him "*Resistance to Flexure*," which was intended to explain the discrepancy. The term I consider unfortunate, for all resistances to bending may be considered a *resistance to flexure*, whereas he intended to include only a certain class of strains. The stress which he considers is the same as that called by Rankine "*Longitudinal Shearing Stress.*" Whether his theory be correct or not, the spirit of the article cannot be too highly commended. He presented the results of a large number of experiments, some of which were made by himself, and others were selected, and their agreement with the results as given by his theory, went far towards confirming his views. We can expect to establish the true theory of the resistance to rupture of solids under all the conditions to which they may be subjected, only after a long series of scientific experiments.

University of Michigan, December 3, 1868