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The Laws of Dynamics, and Their Treatment in Text-Books (Continued)

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## THE LAWS OF DYNAMICS, AND THEIR TREATMENT IN TEXT-BOOKS.

*Continued from page 389.*

IN the preceding discussion of the Galileo-Newton theory, force has been introduced as measured by mass-acceleration, in accordance with the generally accepted procedure. The existence of laws, connecting force with the conditions accompanying it, has been referred to as a fundamental fact which makes the theory possible; though, as a matter of logical order, all details as to such laws have been put into a secondary position. The recognition of the fact that acceleration was the thing, which could be connected by laws with the conditions under which a body moved, has been mentioned as Galileo's great achievement. This was historically the first and most important, and was probably the most difficult, step to be taken. There is no record of any suggestion of it before Galileo's time, and no theory of motion of any value had been constructed on any other basis. Perhaps the nearest approach to such a theory was the study of the celestial motions on the basis of circular motion; and this was not capable of progressing beyond the empirical stage.

There are two general laws of force; the first of which may be briefly referred to as being that under constant conditions force is constant, and the second that, if two sets of conditions are superposed, the forces accompanying them are added. Galileo's chief experimental result was the establishment of the first law approximately, in the case of the approximately constant conditions attending the motion of a falling body; and one great merit of this investigation consisted in the insight which led to the elimination of such disturbance as the resistance of the air. The existence of a disturbance of this character

introduces the natural objection that we find forces depending on the velocities of the moving bodies. But the view is that, in such a case as the resistance of the air, the velocity effect is due to the consequent attitude of the surrounding air, and that the statement in terms of the velocity of the moving body is merely a convenient expression of the way in which such surrounding conditions vary. The second law is commonly stated as the principle of the physical independence of forces. There are certain cases in which the approximate superposition of conditions without interference can be readily arranged, and for such cases the law can be tested. The case in which the conditions imposed upon a small body consist of its attachment to another small body by a uniformly stretched elastic thread may be looked upon as a model of the sort of class here referred to, being one in which the physical surroundings, which appear to be material to the question, can be sufficiently perceived and isolated. It is not easy to specify the scope of the law completely, for it should be noted that, in the case of some special laws of force, such as the law of gravitation, this general law may be considered to be included as part of the special law. The comparison of forces by balancing has already been discussed, as being, where it is applicable, a method of comparison independent of an investigation of what has been called a Newtonian base; and this, combined with the law of addition of forces by superposition of conditions, gives what is commonly known as the statical measure of force. It has been the fashion with some writers to discredit the use of this measure of force; but nevertheless it is not, and is not likely to be, wholly discarded in elementary books, since the provisional use of it permits some progress to be made in the subject without the complications attending the introduction of mass and base.

Before pursuing this point further, let us consider what position we are in when we pass from the Galileo-Newton theory of the motion of particles to the study of the statics of rigid bodies. The step which has to be taken is the establishment of the principle of transmissibility of force, or of something equivalent to it. This cannot be regarded as altogether easy, and by some writers seems to have been rather slurred over. Professor Love, in his *Theoretical Mechanics*, has recognized what is needed for its logical treatment; but his argument, though brief in form, is of a kind which may perhaps not be very easily appreciated by a beginner. The following, though longer, may be regarded as, in some respects, a more direct investigation of the point in question.

The motion of a rigid system of points, relative to a given base, may be specified, at any moment, by the velocity of one point and the angular velocity; that is to say, by six quantities.

Let us exclude the case of the points all lying in one straight line as inapplicable to any actual body. Take any set of rectangular coordinate axes, moving in any manner relative to the base. Let  $u_0, v_0, w_0$  be the components, in the directions of the axes, of the velocity of the point, either belonging to the system, or rigidly attached to it, which happens to be passing through the origin; let  $p, q, r$  be the components of the angular velocity of the system; and let  $u, v, w$  be the components of the velocity of a point  $x, y, z$  of the system. Then

$$u = u_0 + zq - yr$$

$$v = v_0 + xr - zp$$

$$w = w_0 + yp - xq.$$

There are three times as many quantities  $u, v, w$  as there are points in the system; and if any six independent linear functions of these quantities are known to be zero, it will follow that  $u_0, v_0, w_0, p, q, r$  must all be zero, that is to say all the quantities  $u, v, w$  are zero. Let numerical quantities  $m_1, m_2 \dots$  be assigned arbitrarily to the several points of the system; then, if  $\Sigma mu, \Sigma mv, \Sigma mw, \Sigma m(yw - zv), \Sigma m(zu - xw), \Sigma m(xv - yu)$  are all zero, it will follow that the velocities are all zero, provided that the six equations thus given for  $u_0, v_0, w_0, p, q, r$  are independent. We shall find that this is certainly the case if the  $m$ 's are all positive.

Let  $\Sigma m = M$ ,  $\Sigma mx = M\bar{x}$ , etc., then our six equations are of the two types

$$M(u_0 + \bar{z}q - \bar{y}r) = 0,$$

$$M(\bar{y}w_0 - \bar{z}v_0) + p\Sigma m(y^2 + z^2) - q\Sigma mxy - r\Sigma mzx = 0.$$

Assuming that  $M$  is not zero, and eliminating  $u_0, v_0, w_0$ , we get three equations of the type

$$p\{\Sigma m(y^2 + z^2) - M(\bar{y}^2 + \bar{z}^2)\} \\ - q(\Sigma mxy - M\bar{x}\bar{y}) - r(\Sigma mzx - M\bar{z}\bar{x}) = 0.$$

Write these equations in the form

$$Ap - Fq - Er = 0 \\ - Fp + Bq - Dr = 0 \\ - Ep - Dq + Cr = 0;$$

eliminating  $p, q, r$  we get

$$ABC - 2DEF - AD^2 - BE^2 - CF^2 = 0;$$

but this, in the language of dynamics, is equivalent to saying that the product of the three principal moments of inertia of the system at the centre of gravity is zero; and, if the  $m$ 's are all positive, this cannot be the case, nor can  $M$  be zero.

We can now prove the proposition that a system of external forces (components  $X, Y, Z$ ), satisfying the six equations of the types  $\Sigma X=0, \Sigma(yZ-zY)=0$ , do not affect the motion of a rigid body. To prove this, notice that the internal forces, called into play, will by themselves satisfy equations of the same type, since they occur in pairs; hence the whole system of forces applied to particles of the body satisfies these equations. Such a system of forces implies the generation, in a time  $dt$ , of a velocity distribution  $u dt, v dt, w dt$  relative to a Newtonian base, satisfying the six equations of the types  $\Sigma mu=0, \Sigma m(yw-zv)=0$ , where  $m$  is positive; that is to say the combined effect of the forces upon the motion is nil. Thus the equations used in statics for a rigid body are proved. It is clear that they are not only sufficient, but also necessary.

The point to be insisted upon is that, if we are to derive the mechanics of rigid bodies logically from a theory of the motion of particles, the theoretical step, of introducing the relations between the motions of the points of a rigid system, is of such a character as to be rather awkward to deal with in an elementary fashion. Thus we are led to make an independent appeal to experiment; and this is an additional reason for beginning elementary statics on a somewhat independent footing. If, by the methods of balancing and superposition, we get a measure of force, by means of which the subject can be developed within a certain range, within which the ideas of mass and Newtonian base have no place, the simplest and most scientific course is to carry out the development within that range without bringing in this machinery. It seems to be possible to maintain, in fact, that the modern procedure of basing the whole of Mechanics, from the beginning, on the laws of motion is inadvisable from the points of view both of simplicity and of logic.

In the definition of any physical quantity we are concerned only with the identification of it and the measurement of it. A definition may embrace several alternative measurements; and these may either be mathematical functions of each other, or may be dependent for consistency upon some physical law. Allowing, if we do so, the definition of force to embrace a superposition measurement, is a case of the latter sort. It appears also that a superposition measurement holds a subordinate and provisional position, for the scope of it is practically limited, so that the subject could not be developed fully with it alone.

Elementary statics might be introduced somewhat as follows. The causal relations between the motions of portions of matter, analysed into relations between the motions of particles, are to be dealt with fully by the Galileo-Newton theory. But, as a preliminary step, the existence of some of these relations may be recognisable without the aid of this theory. The contact at a

point between two solid bodies is obviously a case in which a relation exists, and given cases of such contact can be reproduced with some precision. Without going into the way in which the motions are to be measured, we can study the condition under which they are neutralised. The establishment by statical experiments of the fact that the pressure effect at a point has a direction is not found to be a difficulty, and superposition gives a measurement of magnitude. A quantity with this direction, and with a magnitude thus measured, in terms of any recognisable unit, may be called a force. Other forces may then be recognised by the method of balancing against a certain identifiable pressure.

Let us consider the limitations of the subject as developed in this way. We at once meet with weight, and are led to regard it as a force, but have no means at our disposal for investigating the question of its reciprocal character. So this question must be left in abeyance; and in fact we know that, when studied by the light of a more complete theory of motion, weight is found not to possess this character completely. Moreover, such examples as the relative equilibrium of the governor of a steam engine would introduce centrifugal force in a prominent manner. This could not be reconciled with the idea of mutual relation with which we started, if a more complete theory did not come to the rescue. It seems to be the most natural procedure, in an elementary development of the subject, that a theory of motion with mass and base should come in at this particular stage, the superposition measure of force at the same time giving place to a measure based on acceleration.

The parallelogram of forces makes its appearance in connection with elementary statical experiments; and to make this, at this stage of the subject, dependent on a theory of mass, as appears to be usual in modern text-books, somewhat grates upon one's sense of logical order. It seems rather confusing to hang it on to a crude and provisional theory of the universe, seeing that it is capable of being dealt with without so elaborate a preface. The old-fashioned statical proofs of this proposition seem to give a better view of its true position. Duchayla's proof, which held its own in text-books for many years, has something to be said for it, in spite of the extraneous feature which the appeal to transmissibility of force introduces. Logically the best form of proof seems to be one on the lines adopted by Laplace; but the fact that he uses a differential equation to determine the law of composition may be regarded as a disadvantage. Mr. W. E. Johnson's proof, published in *Nature*, vol. XLI., p. 153, is one of the same class. It is a simple geometrical proof, and is in all respects admirable. It puts the proposition on its narrowest logical basis. All that has to be assumed is that the directed quantities, which are to be combined, have an unique resultant,

whose direction and ratio to one of them depends only on the angle between them and the ratio of their magnitudes.

The procedure to be followed practically in elementary teaching is a matter as to the details of which it would be idle to dictate. But, on the whole, it seems good to isolate any group of facts capable of isolation, and to encourage any ramifications of treatment that are available, rather than follow only one narrow path of deduction. At Winchester, thirty years ago, Mr. George Richardson, when lecturing on Mechanics, began with the principle of virtual work, as exhibited by elementary examples in statics. The present writer can testify to the fact that this procedure had the merit of arousing immediate interest in the subject.

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## REVIEWS.

**Leçons sur la Théorie des Formes et la Géométrie Analytique Supérieure, à l'usage des Etudiants des Facultés des Sciences.** Par H. ANDOYER. Tome I. pp. vi., 508. Paris (Gauthier Villars), 1900.

It is a surprising fact that the projective invariant theory should have remained, till the very end of the century, unrepresented by any systematic treatise emanating from a French source. Quite early after its inception the study was benefited by the attention of French mathematicians; Hermite's striking contributions to it followed close on the original investigations of Boole, Cayley and Sylvester. Moreover French refinement in Analysis, and French leadership in Projective Geometry, more especially as developed from a few principles which are in effect the counterparts of facts of algebraical invariancy, led to the expectation that French Analytical Geometers would take the lead in expounding the algebraical theory as the key to the geometrical. But isolated memoirs, and partial consideration incidentally given in *Cours d'Analyse*, etc., have remained all that French authorship has provided.\* Two years ago M. Andoyer put an end to the anomaly by the publication, for the use of candidates for "Agrégation," of a concise but comprehensive course of introductory lectures, *Leçons élémentaires sur la Théorie des Formes et ses Applications Géométriques* (Paris, 1898, lithographed), and the promise of an extended manual of which the first volume is now before us. Examinations and authorship act beneficially on one another in France as here.

We have had long to wait; but ought now to be more than satisfied. M. Andoyer's aim is to present didactically the theory of Forms, and develop its application to a perfectly general geometry; and he is singularly successful. His plan is marked by great breadth of view. Possibly he is too chary of space when general lessons could be enforced and illustrated by particularisation. To be at once precise and general

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\* Faà de Bruno's *Formes Binaires* (Turin, 1876) is credited to Italy.