after Galois' death, his theory of the resolution of algebraic equations was for the first time made intelligible to the general public and established with complete rigor. A few words more on the further history of Galois' theory will complete my account. According to Weber,* Kronecker probably first became acquainted with it during his visit to Paris in 1853 where he was associated intimately with Hermite, Bertrand, and other leading French mathematicians. The first mention Kronecker makes of Galois' name is in a letter to Dirichlet in March, 1856. Dedekind also became very early acquainted with Galois' theory since it is known that he lectured in the winter of 1857–58 on higher algebra and in particular on Galois' theory. According to Weber † this was probably the first extensive account of Galois' theory given at a German university. The first account of it given in a text-book on algebra is in the third edition of Serret's algebra (1866). This, together with Jordan's classic treatise which appeared in 1870, made a knowledge of Galois' theory possible to all the world.

Perfectly just was Galois' estimate of his own discoveries when he said shortly before his death: "*J'ai fait des recherches qui arrêteront bien des savants dans les leurs.*"

YALE UNIVERSITY,
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LOVE'S THEORETICAL MECHANICS.

Theoretical Mechanics, an Introductory Treatise on the Principles of Dynamics. By A. E. H. LOVE, M.A., F.R.S., Fellow and Lecturer of St. John's College, Cambridge. Cambridge, The University Press, 1897. 8vo, xiv + 379 pp.

THIS is a text-book on dynamics intended for the useof students who have some knowledge of differential and integral calculus and coördinate geometry. The statements of first principles necessarily relate to motion in three dimensions, but the systematic development of the subject is for the most part confined to the motion in two dimensions of particles and rigid bodies. A notable feature of the book is the careful attention which is paid to the statement of the theory of dynamics, not merely as a basis for mathematical problems, but also as a branch of science. In this respect it stands in marked contrast to most other textbooks of similar scope.

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* *Mathematische Annalen*, vol. 43, p. 1.

† Algebra, vol. 1, Einleitung, p. 7.

Newton's theory is one which, partly on account of the fundamental character of it, has always been in danger of becoming a superstition, supposed to have some higher validity than the facts which it summarizes. Newton himself, though he discusses the evidence within his reach, did not indicate the position of the theory very clearly; and the discussion of the subject in Clerk Maxwell's "Matter and Motion" shows how, in this connection, science is in danger of being tinged with mysticism. Yet the correctness of the theory is very far from being obvious; and, to bring it into harmony with known facts, it requires more careful statement than it receives at the hands of most writers on the subject. One fundamental idea of it, that of the base relative to which motion is to be measured, has frequently been stated in such a form as to be unintelligible; and another point involved in it, namely the division of matter into particles, is not supposed to be in agreement with the structure of any actual body. Mr. Love has no superstitions; and, though we may not agree with him on all points, he understands how the subject should be approached in order that the theory may be put on a sound basis.

The first question which arises is that of the scientific measurement of time. It may be doubted whether Mr. Love treats this in the best way. He makes too much of our freedom to select at pleasure a standard process by means of which to measure time; a freedom which, though of course it may be claimed, has never been exercised freely. And he does not appear to appreciate the fundamental character of the notion of uniform time which is based upon the agreement of all methods of measuring time which depend upon repetitions. A repetion method is one according to which a period of time is measured by the number of times a given physical operation can be repeated in the course of it, allowance being made for variations in the conditions. There is some indication, in the Principia, that Newton had the notion of repetitions being the test of his so-called absolute time; and this notion lies at the root of all physical theory in which time is a factor. If a lump of sugar takes longer to melt in a cup of tea than a lump of the same size did yesterday, the difference has nothing to do with the fact that the two experiments are not carried on simultaneously, but is to be traced entirely to other differences in their circumstances and surroundings. Of course we can imagine this view being upset, as it would be if it were shown that the tests by two different repetition methods disagreed irreconcileably. In the meantime the theory that there is no such disagreement is so well established that it forms a possible basis for a definition of uniform time. A clock is a machine expressly designed for the performance and counting of repetitions ; but the comparison of the results given by the best clocks establishes the successive rotations of the earth, relative to the stars, as being so trustworthy a series of repetitions that no artificial method can at present compete with it. Thus the rotation of the earth is, from this point of view, established as a practical standard ; and we are fortunate in having so good a one, which can be referred to so easily and accurately. But the choice of it is presented to us by Mr. Love as if it were merely arbitrary. He says that it is desirable that the choice should be so made that a number of processes uncontrollable by us should be uniform, or approximately uniform; but he does not say that we are guided by the aim of making those processes most uniform in time which are, so far as our knowledge goes, most uniform in their other conditions. We come to the root of the matter when it is suggested that the adoption of a standard slightly differing from that given by the earth's rotation may be advisable, in order to enable the law of gravitation to account for one of the moon's inequalities. Mr. Love says that this may be advisable because it may be a simpler statement of the facts to change the standard of time and keep the law of gravitation than to keep the standard of time and throw over the law of gravitation. We do not wish to suggest any other standard for the choice of a system of "laws of nature" than the simplicity of the statement which it gives of the facts. But we think that Mr. Love's way of putting the case is rather misleading ; for it can hardly be questioned that we should wish to save the repetition test of equal times, rather than the law of gravitation, if it should turn out that these come into conflict. Mr. Love, in his last paragraph, mentions the retardation of the earth's rotation by tidal friction, and says that it may be advisable to save the laws of energy at the expense of the uniformity of the earth's rotation relative to the stars. He might have added that, since the moments of inertia of the earth certainly undergo some change, the consideration of angular momentum shows that its rotation cannot give, with perfect accuracy, a measure of time with reference to which the laws of motion can be accurately true. It may be said that this inconsistency is so minute that it does not affect the agreement of the theory with all that we know about the facts. All the same it is making the statement of a theory rather puzzling to give it, without warning, in a form in which it is self-contradictory. If the repetition test is adopted, the question of consistency with the law of gravitation becomes a matter of speculation, for we cannot at present apply the test accurately enough to settle it. Nevertheless we are inclined to regard this test as affording the most satisfactory basis for the definition of uniform time.

The treatment of the base (or axes) of reference, established by Newton's theory of kinetics, for the measurement of motion is on the whole good. But it is a little lacking in simplicity, in consequence perhaps of the author's desire not to countenance too much extension of the theory into the unknown. Moreover, one point in the theory is missed. No doubt the omission is intentional, but as no facts are produced in support of it, we do not see why the theory should be robbed of a point which has hitherto belonged to it. The situation is this: Newton presumably enunciates his theory for the whole universe, at any rate he does not define any limits for the dynamical system to which it is to apply. The theory is that there is one base such that the motion of bodies relative to it is that which is expressed by the "laws of motion," mass being suitably distributed. Two bases are regarded as identical from this point of view if one moves relative to the other uniformly without rotation. For the discovery of a base satisfying the theory some knowledge would be needed of the relative motions of all bodies belonging to the system. Motion relative to this base is what Newton calls "absolute" motion. But our knowledge is practically almost entirely restricted to the motions of bodies within the solar system. For this system, regarded as isolated, we can find a base which, so far as it has been tested, satisfies the theory. It is usual to assume, in the absence of any indication to the contrary, that the theory would hold for a more extended system, and accordingly that, relative to a more general base, the accelerations of all points of the solar system are approximately equal. Of course, the theory might eventually be found to break down under such extension. It is, however, convenient to regard the theory as having the more extended application, and the base employed for the solar system by itself as a provisional one. But this provisional base is regarded as unique, with the proviso mentioned above. The uniqueness is what Mr. Love omits from his statement. He approaches the question by discussing what he calls the "relativity of force." He points out that, as acceleration has no meaning unless a base is specified relative to which it is to be reckoned, so force would appear to be in the same case. He then has to go on to say that, by virtue of the requirement of the theory that forces are to occur as interactions between pairs of particles, force does not hold quite the same position as acceleration. Any base (or frame, as he calls it) will do to refer acceleration to, but a frame of reference for force must be properly chosen. But he tells us that there is a choice among several suitable frames, and that that one should be chosen which gives to forces the simplest expression. This view may perhaps be correct, but we are not furnished with any evidence in support of it. The old established theory would be better suited for an elementary treatise.

Mr. Love has a note on the subject of the conception of matter as consisting of particles which is good and suggestive. He points out the provisional character of a theory which is dependent on particles. The importance of widening the basis of the theory to meet the requirements of physics cannot be overrated, and ought to be kept constantly in view.

Regarding the book as a whole, the arrangement strikes us as rather ponderous. There are too many provisional statements which require subsequent modification. It is tiresome not to be able to distinguish at once between those definitions and explanations which are final and those which are going to be modified. Moreover, the task sometimes put upon the reader of working backwards, and seeing how the modifications affect the intermediate argument, may be a difficult one. It is part of the scheme of the book to postpone till the end the consideration of the way in which motion must be measured in order that the system of "rational mechanics," which is built up from definitions, may have any application to real bodies. In the meantime we have the weight of a body explained to be the attraction between the body and the earth, which strikes us as being most unfortunately misleading; and we have examples involving all the apparatus of ordinary life, from railway trains to billiard balls, as if the application of the ideal system to the motion of bodies relative to the earth could be assumed without more ado. When we come to the further discussion of the subject we are told that, for the motion of a body near the earth's surface, it will be sufficient to consider the body and the earth as an independent system, and there is no reference to the remarkable and instructive exception afforded by the tides.

One of the points ignored by some writers, to which attention should be called, is the proof of the "transmissibility of force" for a rigid body. The proof given, which cannot be regarded as very easy, might have been usefully supplemented by a direct investigation of the combined effect of velocities distributed among the particles of a rigid body, and the conditions which they must satisfy. This is the most direct way of obtaining the rules for compounding the forces acting on a rigid body, though it may not be the shortest.

Turning to the more mathematical parts of the book, we are rather disappointed that there is not a greater variety of method. A text-book like this should satisfy reasonable requirements as a book of reference. Thus from this point of view, if from no other, we should have expected more prominence to be given to the expressions for velocities and accelerations in terms of coordinates referred to moving axes. One case is given, as a note in small type near the end of the book, namely, that in which the coordinates are referred to a pair of rectangular axes rotating in their own plane about the origin. Mr. Love refers in the preface to the preference shown to analytical as compared with geometrical methods, nevertheless it is a little surprising to find no use made of the hodograph for obtaining expressions for accelerations. The author has clearly made up his mind that the book should be elementary, and we find frequent evidence of the self restraint which he has practised in keeping out of it every thing of an advanced character, but a little expansion throughout would make the book much more useful. The motion of a rigid body in two dimensions appears to be well done. In this connection the term "kinetic reaction" is used, which is an improvement on the old expression "effective forces." We are inclined to grudge the large amount of space taken up by the mere enunciations of examples which are not worked. They have been collected together rather wholesale, and occupy about one hundred and thirty pages, or more than a third of the book. Indeed, taking account of the difference of type, they appear to contain nearly as much matter as the whole of the text. A more select collection would have satisfied all ordinary requirements.

On the whole, the book is both useful and interesting, and is in some respects better than any similar book in common use.

W. H. MACAULAY.

KING'S COLLEGE, CAMBRIDGE,
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