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XXIII. *On the Hypotheses of Dynamics.* *By* Prof. J. G. MacGregor, *D.Sc., Dalhousie College, Halifax, N. S.*[*]

PROFESSOR LODGE'S paper on the Foundations of Dynamics[†], in which he criticises an Address of mine on the same subject[‡] and replies to criticisms I have made[§] on a series of papers by him in this Magazine[‖], contains so much debatable matter that it would require more space than is available to give it full discussion. There are some points, however, which are of so much importance in the clearing up of our conceptions of the fundamental assumptions of Dynamics, that I venture in as brief a manner as possible to draw attention to them.

(1) *The Relativity of the First and Second Laws of Motion.*

Prof. Lodge completely misunderstands the objection which was urged in my Address against the usual statement of the first and second laws of motion, and which had been previously urged by various writers[¶]. He states it to be "that

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[*] Communicated by the Author.

[†] Phil. Mag. current volume, p. 1.

[‡] Trans. Roy. Soc. Canada, vol. x. (1892), sec. iii. p. 3.

[§] Phil. Mag. vol. xxxv. (1893) p. 134.

[‖] Vols. viii. (1879) p. 277, xi. (1881) pp. 36 & 529, xix. (1885) p. 482.

[¶] The list of writers which Prof. Lodge gives is obviously not intended to be complete. It omits C. Neumann (*Ueber die Principien der Galilei-Newtonschen Theorie*, Leipzig, 1870), Prof. J. Thomson (Proc. R. S. Edin. vol. xii. pp. 568 & 730), Prof. Tait (ibid. p. 743 and 'Properties of Matter,' 1885, p. 92), H. Streintz (*Die physikalischen Grundlagen der Mechanik*, uniform motion is unintelligible or meaningless, unless you specify its direction and velocity with reference to a set of axes," whereas the real objection is that the laws themselves, in their usual form, are unintelligible, unless the axes are specified, to which the uniform motion or acceleration mentioned in them is referred. His criticism is therefore necessarily somewhat wide of the mark. It may be summarized thus:—(1) Uniform motion is perfectly intelligible; and therefore no specification of axes is necessary in the enunciation of the first law. (2) The difficulties in the way of specifying axes are practically insurmountable.

With regard to (1), it will be noted that it rests entirely on the intelligibility of uniform motion, and does not therefore touch the necessity of the specification of axes in the case of the second law* or of the first law in the form which Prof. Lodge has given it himself:—"Without force there can be no acceleration of matter"†. For in neither case is there any reference to uniform motion.

With regard to the intelligibility of uniform motion, while it cannot be admitted that "such notions as axes of reference are not at all necessary for the apprehension of what is meant by a uniform velocity" (seeing that a uniform velocity is one whose magnitude and direction do not change relatively to the axes employed in its specification), it is nevertheless obvious that the specification of particular axes is not necessary for this purpose. But the intelligibility of the first law requires more than the mere apprehension of what is meant by uniformity of velocity. For it is not a mere statement *about* uniform velocity, but an assertion that a particle in given circumstances must *have* a uniform velocity. Now a velocity which is uniform with respect to one set of axes may be variable with respect to others. It is therefore at once obvious that, if we employ the ordinary conception of force, the assertion which the law makes cannot hold for all axes, and consequently can have no definite meaning, unless

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Leipzig, 1883), L. Lange (*Ber. d. K. Sächs. Ges. d. Wiss. zu Leipzig, Math.-phys. Classe*, Bd. xxxvii. 1885, p. 333, and *Die geschichtliche Entwickelung des Bewegungsbegriffes*, Leipzig, 1886), and Muirhead (Phil. Mag. [5] vol. xxiii. 1887, p. 473), the last, however, being mentioned subsequently in a footnote.

* Mach, Streintz, Lange, and other German writers refer to the relativity of the first law merely, because they employ as second law Galileo's law of the "physical independence of forces" (Unabhängigkeitsprincip). The second law to which I refer is Newton's second law.

† 'Nature,' vol. xlviii. p. 62.

we are told what are the axes by reference to which it does hold*.

Much may of course be derived from the first and second laws without specification of axes. The whole science of dynamics bears witness to that fact. But, as Streintz has shown in the work referred to above, much practical inconvenience and much unnecessary complication have arisen from the employment of these laws in their vague form; and I shall have occasion to refer below to one paradox, the absoluteness of rotation notwithstanding the relativity of motion, which receives its solution when the relativity of these laws is recognized.

The specification of axes by reference to which the first and second laws hold, or of what may be called dynamical reference systems, is thus no mere refinement of the pedantic mathematical mind. On the contrary, it satisfies a felt want. The want is not felt indeed in dealing with the simple problems of the common school. For the rough experiments which are usually cited in elementary text-books as suggesting the laws show that it is by reference to axes fixed in the earth that they are supposed to hold; and this tacit specification is quite sufficient for the discussion, *e. g.*, of the inclined plane and the wheel and axle. But when we come to treat the problems of theoretical Astronomy, it is at once obvious that we cannot assume the laws to hold with respect to these axes; and the question forces itself upon the attention: What are the axes by reference to which they must now be considered to hold? And the question having been raised must be answered. The critical student who has seen in his study of kinematics that velocity and acceleration are relative conceptions, will not be convinced by Prof. Lodge's "opprobrious or perhaps complimentary epithets" that they lose their relativity when applied to the motion of bodies.

Turning now to the second criticism, it is obvious that to one who thinks it is proposed to specify axes by means of which the magnitudes and directions of velocities may be described absolutely (p. 8), the difficulties in the way must appear

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* Should Prof. Lodge, therefore, endeavour to crush a doubter of the first law, as he tells us he would, by saying to him:—"If the speed and direction of a freely moving body vary they must vary in some definite manner; very well, tell me in what manner they are varying. You cannot, unless you can show me absolutely fixed lines of reference,"—the doubter need be at no loss for a reply. He has but to say:—If you will kindly tell me what are the axes by reference to which you hold the velocity of the body to be uniform, I will then tell you how I suppose it to vary. But unless we agree upon axes of reference it is impossible for us to compare our respective axioms.

insuperable*. They would be in that case the difficulties attending the solution of an inconceivable problem. That the actual problem has only recently been attacked is due not so much to its difficulty as to the fact that the necessity of its solution has been apparent only since the full recognition of the essential relativity of velocity and acceleration, whether uniform or variable. That there are difficulties, however, is obvious from the fact that only some of the methods employed appear to be sound, and that a number of writers have attacked the problem and left it only half solved †. What the difficulties are may be shown best by a sketch of the efforts made to overcome them.

There would seem to be two legitimate ways of finding dynamical reference systems:—(1) by re-studying the experimental results for the deduction of which the laws of motion were enunciated, and re-formulating these laws; and (2) by assuming that, since the laws of motion in their vague form have been abundantly tested in the hands of men enabled by a kind of dynamical instinct to use them aright, there must be axes by reference to which they hold, and proceeding to determine these axes by the aid of the laws themselves.

The former method, the historical-critical, is that employed by Prof. Mach ‡. He points out that Galilei observed the first law to hold, by reference to points fixed in the earth, for motions on the earth's surface of small duration and extent, and that, when Newton came to apply it to bodies moving in space, he generalized it, showing that, so far as could be determined, it held for the motions of the planets by reference to the distant and to all appearance relatively fixed celestial bodies. And he holds that the first law, when referred, so far as space is concerned, to the fixed stars, and, so far as time is concerned, to the earth's rotation, is to be regarded as a sufficient approximation to accuracy for practical purposes, and as forming as close an approximation as it will be possible to obtain until a considerable widening of our experience occurs.

It seems to me that the historical-critical method might carry us farther than this. For we now know that the so-called fixed stars are not fixed; and means have been devised of correcting observations made on this assumption. We also know that the laws of motion do not hold when referred to a time-scale determined by the earth's rotation; and a rough correction has been determined for application

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* The fact that Prof. Lodge regards motion with respect to the æther as absolute motion (p. 30) perhaps renders this statement doubtful.

† Neumann, J. Thomson, and Muirhead. See works cited above.

‡ *Die Mechanik in ihrer Entwickelung*; Leipzig, 1889, pp. 217 & 481.

to this time-scale, in the case of motions extending over long periods of time. The first law, when expressed by reference to the fixed stars and the earth's rotation, is therefore no longer regarded as sufficiently accurate for all purposes; and the exact expression of the law, as empirically determined and employed in actual work, changes from day to day or at least from decade to decade. The question therefore arises: Can we put the laws of motion into general forms such that the empirical forms which they may have at any time may be regarded as special cases determined by the state of knowledge of the time?

The latter of the two methods* referred to above is intended

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* Mach, though holding, as seen above, to the empirical result of the historical-critical method, gives in his *Mechanik* (p. 218) an interesting "remnant," as he calls it, of his efforts to apply the second method. He holds that, in using the first law in its Newtonian form, we may be regarded as employing the universe, or a sufficiently large portion of it, as our reference system, and on the following grounds:—"Instead of saying the velocity of a mass μ remains constant in space we may also employ the expression, the mean acceleration of the mass μ, relatively to the masses m, m', &c., at the distances r, r', &c., is zero, or

$$\frac{d^2}{dt^2}\frac{\Sigma mr}{\Sigma m}=0.$$

The latter expression is equivalent to the former, provided we take into consideration a sufficient number of sufficiently distant and great masses, the mutual influence of the nearer small masses being in that case negligible." If this be so, the first law may be expressed as follows:—The mean acceleration of any particle, relatively to the other particles of the universe, or of a sufficient portion of the universe, is zero, provided the particle is not acted upon by force,—an expression which obviously has not the same vagueness as the Newtonian form of the law, though practically, as Mach points out, it is not more readily applicable, on account of the impossibility of making the summation necessary for the determination of the mean acceleration.

How this result is arrived at, Mach does not say. But it is easy to prove it to be one of the properties of the centre of mass, that the component acceleration, in any direction, of any one particle of a system, relatively to the centre of mass of the system, is equal to the mean component acceleration of this particle, in the same direction, relatively to all the other particles of the system, provided the mass of the particle is small compared with the mass of the system. In making the above statement, therefore, Mach would seem to assume that the uniform velocity contemplated in Newton's form of the first law is a velocity which is uniform relatively to the centre of mass of the universe, or of a sufficiently large portion of it; and if that be so, he assumes a partial specification of a dynamical reference system. It would also appear that the portion of the universe taken into consideration need not consist of numerous and distant particles, but must simply have sufficient mass.

It is obvious that if the above assumption be made, not only may the first law be thrown into the above form, but also the second law may be thrown into a corresponding form. The making of this assumption, however, introduces a complication. If we assume merely that there are axes to give laws of this kind. Prof. James Thomson may be said to have employed it when he showed how, by observation of successive relative positions of particles given as moving in straight lines, the axes by reference to which their paths are rectilinear may be determined geometrically*. Thomson and Tait may be said to employ it also, when they show, by a deduction from the first law, how we may imagine ourselves as obtaining "fixed directions of reference" †. But these authors make no attempt to give a formal specification of a dynamical reference system.

Lange employed this method in the paper referred to above, basing his suggestion as to specification on a kinematical result, viz. that for three, or fewer than three, points, which are moving relatively to one another in any way whatever, it is always possible to find a system of coordinates, indeed an infinite number of such systems, by reference to which these points will have rectilinear paths; while for more than three such points this is possible only in special circumstances. It follows that the law of the uniformity of the direction of motion of particles free from the action of force is, for three such particles, a mere convention, and that it is a result of experience only in so far as it applies to more than three particles by reference to one and the same system. Hence just as the dynamical time-scale is defined as a time-scale by reference to which a particle free from the action of force moves with a uniform speed, so the dynamical reference system may be defined as a system by reference to which three particles free from the action of force move in rectilinear paths. Following out these considerations he finally proposes to enunciate the first law in the following form:—Relatively to any system of coordinates by reference to which three particles projected from the same point in space and thereafter

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by reference to which the first and second laws hold, it may be proved by means of the second and third laws that relatively to these axes the centre of mass of a system of particles will have no acceleration, provided no external forces act on the system. While, therefore, the assumption that the centre of mass of the universe may be employed as the origin of a dynamical reference system is justified, it is obvious (1) that if, in employing Mach's expression of the first law, we restrict ourselves to a part of the universe, it must be a part on which no external forces act; and (2) that since, in obtaining this form of the law and the corresponding form of the second law, we employ the third law of motion, the new laws are not merely new expressions of the old laws, but involve the third law in addition.

* See also Prof. Tait's solution of this problem by Quaternions in the paper cited above.

† 'Treatise on Natural Philosophy,' vol. i. part 1 (1879), § 249.

left free from the action of force, which do not, however, lie in a straight line, describe any three straight lines intersecting in a point (the axes of coordinates for example), the path of any fourth particle free from force will be rectilinear. And relatively to any time-scale, by reference to which one particle free from force will, when its motion is referred to the above axes, move with uniform speed, every other particle free from force will move with uniform speed, if its motion be referred to the same axes.

It was this method also which I employed in my Address, when I had not yet met with Lange's paper, the conclusion reached being, that the 1st and 2nd laws hold relatively to any particle not acted upon by force, as point of reference, and to lines drawn from it to other particles which are unacted on by force and have the same velocity as the first particle, as axes of reference. I showed also that it followed from this, that in dealing with the ordinary problems of the motion of bodies on the earth's surface, axes fixed in the earth might serve practically as a dynamical reference system*.

With regard to all such modes of specifying axes as those referred to, Prof. Lodge asks, "How can we utilize as axes the trajectories of particles free from force, without tacitly assuming the first law continually?" A criticism in the form of a vague question is hard to meet, because indefinite. If the first law is assumed in its own enunciation, when such trajectories are employed in the specification of axes, it should be easy to point out exactly where the assumption seems to be made; and a definite criticism of that kind might be met at once. But, judging from the context, the question is probably suggested by the mistaken notion, that when such trajectories are employed they must be assumed to be straight lines in absolute space,—a notion which springs directly from the belief that the object of specifying axes is the description of velocities absolutely. The object of specifying axes, however, is not "to attempt the impossible." And when the trajectories of particles free from force are employed as axes, or for the specification of axes, no assumption is made as to their form. Indeed it is recognized that they cannot be said to have any definite form except by reference to other axes; and that they may be made to take an infinite number of forms by varying the axes by reference to which their forms are specified. And no assumption being made as to

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* I need hardly refer to Prof. Lodge's objection to such statements of the first law on the ground of their complexity. If intelligibility is consistent with simplicity, well and good. But if not, it is of course the simplicity which must be sacrificed.

their form, no assumption of the first law is made in utilizing them.

We are also asked how we can appeal to the experience of the human race with regard to such axes *. It must be admitted that we cannot make any direct appeal. The only dynamical axioms which can make such an appeal are axioms applicable within, but not beyond, the narrow range of direct experience. When we pass from the discussion of the motion of bodies on the earth's surface to the motion of bodies in space, we enter a region which is outside our direct experience; and the human race, if it is interested in such things, must learn that the hypotheses made by philosophers to coordinate dynamical phenomena generally, must be judged solely by the accuracy of the deductions which flow from them.

Possibly Prof. Lodge is not thinking of the human race generally so much as of the race of young students. And it is at once obvious that such an enunciation of the first law as that suggested, for example, by Lange, is not suitable for use in an elementary text-book or before a class of beginners. But no one has proposed to use it in either case. The object of writers who have sought to solve the problem under consideration has been logical, not pedagogical. The beginner deals with simple motions of bodies on the earth's surface. He is led to see from his own experience that, relatively to axes fixed in the earth (the north-south, east-west, and up-down lines, say, at his place of observation), the first and second laws hold for such simple motions. All that is necessary at this stage is to make clear that it *is* relatively to such axes that in such cases these laws are found to hold. When he reaches such problems as those of theoretical Astronomy, he will see at once that the laws of motion, as first enunciated, are insufficient and that they must be generalized. And by that time he will have learned that axioms are not to be accepted or rejected according as they do or do not appeal directly to his experience, but according as the deductions which flow from them do or do not stand the test of observation.

Mach's objection † to such modes of specifying axes as those just considered is more to the point. While he admits that the first law may be expressed definitely by means of them, he holds that in using them we only apparently avoid

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* This objection ought surely not to be urged by a writer who holds to the third law of motion as an axiom, and yet tells us that he is constantly meeting with engineers (whose dynamical experience is of course wider than that of most members of the human race) who refuse to admit it.

† *Die Mechanik*, p. 484.

a reference of motion to the fixed stars and the earth's rotation. No doubt, in practical observation of motions on the earth's surface or in space, we must still employ, immediately, points fixed in the earth, or the fixed stars, respectively, as reference systems, and the earth's rotation as giving us our time-scale, applying to the crude observations, when necessary, the corrections which may have been determined. But the laws as expressed above both give a theoretical justification of this course and indicate the way in which the necessary corrections may be made more and more accurate. If the laws of motion in the form referred to be assumed, it may readily be shown that in the circumstances in which we find ourselves, surrounded on all sides by bodies, at vast distances, which are moving with velocities of apparently the same order of magnitude as our own, these bodies may be employed as a rough reference system; that the earth, constituted and situated as it is, must be rotating with a roughly uniform angular velocity relatively to these bodies, and that therefore we are justified for most practical purposes in using the fixed stars as a reference system and the earth's rotation as a time-scale. Moreover, with this assumption it becomes apparent that the corrections to be applied to the crude observations made relatively to this reference system and this time-scale, must become more accurately known as we acquire increased knowledge of the motions of the stars and of the masses and motions of the members of the solar system *.

It may turn out, of course, that the assumption on which the above method rests is untenable, that in fact there may be no axes by reference to which Newton's laws hold. In that case other axioms will have to be formulated. Meantime the above general form of the laws may be said, at least qualitatively, not only to give us as a particular case the empirical expression which they have at present, but also to account for those of the past and to indicate the lines on which we must work that they may be improved in the future.

I mentioned above two legitimate ways in which dynamical reference systems may be determined. The attempt has been made to determine them in what seems to me an illegitimate way, viz. by assuming *à priori* that such systems must have certain characteristics, notably that they must have no rotation. This is the only kind of method to which Prof. Lodge seems to refer in the paragraph in which he gives a sketch of the difficulties in the way of specifying axes (p. 7). It is

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* Thus this mode of expressing the relativity of the laws of motion has, in addition to the advantages just mentioned, that which Mach claims for his, that its tendency is to stimulate the progress of science.

based upon the belief that dynamical reference systems are means of describing velocities absolutely.

This is the method which Streintz employs in the work referred to above. He accepts Newton's conclusion* that it is possible to recognize absolute motion of rotation, and possible, therefore, apart from all reference to surrounding bodies, to recognize a body as not rotating. He consequently assumes that the fundamental body, by reference to which the motion of other bodies must be specified in order that the first law may hold, must be a body which is undergoing no rotation †; and he holds that such a body may be recognized by the application of the Foucault's pendulum test and by other similar experimental tests.

While Streintz's method leads him to a correct, though a particular, result, as tested by the results of legitimate methods, it is based upon an assumption which, when the relativity of the laws of motion is admitted, may readily be shown to be erroneous, viz. that the absence of centripetal forces enables us to recognize a body as being absolutely without rotation. The widespread confidence in the conclusion which Newton drew from the behaviour of his rotating bucket of water seems to me to be an instance of the confusion of thought which has its origin in the non-recognition of this relativity ‡. That a particle which is moving in a curved path must be acted upon by a resultant force which has a component directed towards the centre of curvature, is a deduction from the second law of motion. Without specification of axes the deduction has no definite meaning; for a path which is curved relatively to one set of axes may be differently curved or even straight relatively to others. Obviously, however, the axes by reference to which motion is assumed to be specified are axes by reference to which the second law holds. Fully enunciated, the proposition would therefore read thus:—A particle which, relatively to a dynamical reference system, is moving in a curved path, must be acted upon by a force having a component directed towards the centre of curvature. If, therefore, in any case we can detect the action or the non-action of such force, we may assert that the particle is moving or is not moving, respectively, in a path which is

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* *Principia*: Scholium to Definitions.

† Prof. Tait proposes a similar mode of specification (Properties of Matter (1885), p. 92).

‡ Writers who accept Newton's conclusion usually deny the possibility of recognizing absolute translation. Yet if we regard the laws of motion as holding for absolute space, which we must do in order to accept his conclusion, it follows that the translational acceleration of a body determined by the second law must be an absolute acceleration.

curved relatively to a dynamical reference system; but we have no means of making any assertion as to the absolute motion of the particle. The rotation of a body about an axis may be regarded as the motion of its particles in circles about that axis. If, therefore, we observe any body which from our point of view seems to rotate about an axis, and if we are able to recognize the non-action of centripetal forces on its particles, we may assert that, relatively to a dynamical reference system, the body is not rotating about this axis; while if we can recognize and measure the centripetal forces, we may be able to assert that relatively to such a system it is rotating about this axis. But we can make no statement about its absolute rotation*.

It follows that the experimental tests which Streintz proposes to apply to his fundamental body would enable him to select a body which was not rotating relatively to a dynamical reference system, but would not enable him to select one which was absolutely without rotation†.

(2) *The Independence of the First Law of Motion.*

Prof. Lodge agrees with me in holding the first law to be a particular case of the second, but imagines that, for reasons I need not quote, I will not seriously adhere to the view that the first law gives us no more useful definition of time than the second. I do, however, seriously adhere to it, for the obvious reason that, if the first law be a particular case of the second, we must be able to obtain from the second all that we can obtain from the first. I quite admit, of course, that for educational purposes it is desirable to give separate enunciation

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* The application to the old problem of the rotation of the earth is obvious. By reference to axes fixed in the earth, the fixed stars rotate about the earth's polar axis, while by reference to axes fixed relatively to the stars, it is the earth which rotates. Which is the real motion? Both motions are real, as real as any motions can be. But Foucault's, and other similar experiments, are held to show that it is the earth which is really rotating. According to the above they do not prove this; but they do prove that relatively to a dynamical reference system it is the earth and not the system of fixed stars which is rotating. Motion when specified relatively to such a system is no more real than when specified otherwise. But when it is specified in this way, we find that we are able to represent our dynamical experience by means of simpler formulæ than when we specify it otherwise. And thus we come to regard motion specified in this way as being real.

† On absolute rotation, see Prof. J. Thomson (Proc. R. S. E. vol. xii. p. 577), Prof. Mach (*Die Mechanik*, p. 216), L. Lange (*Bewegungsbegriff*, p. 64), Muirhead (*loc. cit.* p. 475), and Prof. K. Pearson (Grammar of Science, Appendix, note 1).

to the first law. But that is quite consistent with a denial of its logical independence*.

(3) *The asserted Deduction of the Third Law of Motion.*

The question of the possibility of deducing the third law from the first seems to me of such importance† that I shall discuss it at some length.

The common belief that the deduction is possible arises probably from the fact that Newton is supposed to have made it ‡. I can best examine Newton's argument § by writing it

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* Prof. Minchin is reported in 'Nature' (vol. xlviii. p. 166) as not admitting the first law to be a particular case of the second, on the ground that "unless force was postulated (the function of the first law), the second became a mere definition, and not a law." Obviously, if the first law be regarded merely as a postulate it cannot be a particular case of a law. But Prof. Minchin states that it gives also "the criterion of the presence of force;" and it is this aspect of the law which is held to be a particular case of the second. Prof. Henrici is reported (*ibid.*) as saying that "in passing from geometry to kinematics the idea of time presented itself, and the appropriate axiom was contained in Newton's first law." He would thus make the first law a kinematical axiom, though why such an axiom should be expressed in terms of dynamical conceptions is not apparent. It would follow, however, from this position that the first law ought not to be enunciated among the dynamical axioms, in which case the question of dependence or independence would not arise.

† Prof. Lodge now considers it of minor importance. But he has insisted upon the possibility of deducing this law, not only in his book on Mechanics, but also in papers in this Magazine and in 'The Engineer.' Indeed he still considers the deduction so important for the conversion of examination candidates and engineers as to justify us in pretending, as it were, to make it, by means of a non-rigorous proof (p. 10).

‡ "That Newton really regarded himself as having deduced the third law from the first is rendered extremely doubtful by the fact that he retained this law as one of his axioms. But it seems clear that he regarded part of what we now consider to be included in the third law to be capable of deduction. That Newton regarded the third law as less general in its applicability *as an axiom* than we do may be gathered from his comments on it. He illustrates it by reference to the finger pressing a stone, a horse hauling a stone by means of a rope, and bodies impinging upon one another,—all cases of palpably contact-actions. And he concludes his illustrative comments by saying:—"This law holds also in cases of attraction, as will be proved in the following Scholium." The fact that his third law states action and reaction to be equal and opposite but says nothing as to their being in the same straight line, forms corroborative evidence that he regarded his law as applicable directly to contact-actions only. For in such actions it would follow, from the opposition of action and reaction, that they must be in the same straight line. It would thus appear that Newton regarded the application of the third law to attractions as capable of deduction."—My Address, p. 10.

§ "In attractionibus rem sic breviter ostendo. Corporibus duobus quibusvis A, B se mutuo trahentibus, concipe obstaculum quodvis interponi, quo congressus eorum impediatur. Si corpus alterutrum A magis trahitur versus corpus alterum B, quam illud alterum B in prius A, out in detail. If I understand it aright (it is so condensed as to be somewhat obscure) it is as follows :—Let A and B be two mutually attracting bodies, and let F and F′ be the attractions on A and B respectively. Imagine any obstacle, O, interposed between them so as to prevent their approach. Then, provided the attractional stress between A and B be independent of the existence of other stresses between them and other bodies (assumption No. 1, the "physical independence" of stresses), F and F′ will still be the attractions on A and B respectively. Let R and R′ be the action on A and the reaction on O, respectively, of the contact stress between A and O ; and let R_1 and R_1' be the corresponding forces for B and O respectively. Since there are no external forces acting on the system of A, B, and O, it follows from the law of the conservation of the motion of the centre of mass* (assumption No. 2, which we may call the generalized first law of motion), that the centre of mass of A, B, and O will move uniformly. If A, B, and O be rigid bodies (this restricts the argument to the case of attracting bodies kept at a constant distance from one another) O's motion will also be uniform. Hence the resultant force on O must, by the first law of motion, which is a particular case of the generalized first law, be zero. But, by the law of the composition of forces, which is a deduction from the second law of motion (assumption No. 3), this resultant force is $R' + R_1'$†. Hence $R' = -R_1'$. Now, by the third law of motion regarded as applicable to contact stresses (assumption No. 4) we have $R = -R'$ and $R_1 = -R_1'$. Hence $R = -R_1$. But the motions of A and B must be uniform for the same reason as that of O. Hence by the first and second laws as above, $F + R = 0$ and $F' + R_1 = 0$. Hence also $F = -F'$.—If this is a correct statement of Newton's argument it is obvious that it does not make the deduction which is claimed for it ‡.

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obstaculum magis urgebitur pressione corporis A quam pressione corporis B ; proindeque non manebit in æquilibrio. Prævalebit pressio fortior, facietque ut systema corporum duorum et obstaculi moveatur in directum in partes versus B, motuque in spatiis liberis semper accelerato abeat in infinitum. Quod est absurdum et legi primæ contrarium. Nam per legem primam debebit systema perseverare in statu suo quiescendi vel movendi uniformiter in directum, proindeque corpora æqualiter urgebunt obstaculum, et idcirco æqualiter trahentur in invicem."—*Principia* : Scholium to Axiomata.

* Newton had previously (Cor. 4 to Axiomata) proved this law, assuming, in the proof, the third law as applicable to all stresses.

† I assume, as Newton does, for simplicity, that the forces are all in one straight line.

‡ See Lange, *Bewegungsbegriff*, p. 57.

Maxwell's version of Newton's argument*, which he regards as "a deduction of the third law of motion from the first," may, as I showed in my Address (p. 12), be attacked on two grounds. First, it assumes that the attraction between the mountain and the remainder of the earth is the only stress between them, ignoring the stress at their surface of contact, an inequality in the action and reaction of which might obviously neutralize the "residual force" due to the assumed inequality in the action and reaction of the attraction. Secondly, the conclusion, thus illogically obtained, is not the third law of motion. For the former asserts the equality and opposition of the action and reaction of the stress between two parts of a body, to which body, *as a whole*, the first law has been assumed to apply, while the latter makes the same assertion for two bodies, *to each of which* the first law is applicable. That this criticism is sound becomes especially apparent, if we reflect that when dealing with rotation and strain we must regard the laws of motion as applicable to particles or elements, the first and second laws being held to apply to each particle, and the third law to the stresses between pairs of particles. Maxwell's deduced law would apply only to the actions and reactions of the stresses between the parts of single particles which, as Prof. Lodge says (p. 11), are "not worth troubling about." It would tell us nothing about the stresses between pairs of particles, and would thus be of no use in the solution of dynamical problems.

Prof. Lodge's version of Newton's argument :—"Jam the bodies apart with a rigid obstacle, then you have reduced their action to contact action" &c., (p. 10), is so condensed that it is hard to analyse. But it is easy to see that its first statement is incorrect. For when we "jam the bodies apart" we do not reduce their action to contact action. The attraction continues. We have simply introduced, in addition, two contact stresses. The premisses, therefore, being thus faulty, the conclusion cannot be warranted.

Prof. Lodge appears to have abandoned the deduction of the third law from the first in its usual form. "Whether," he says, "it is deducible from the first law or not may be

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* "If the attraction of any part of the earth, say, a mountain, upon the remainder of the earth, were greater or less than that of the remainder of the earth upon the mountain, there would be a residual force acting upon the system of the earth and the mountain as a whole, which would cause it to move off with an ever-increasing velocity through infinite space. This is contrary to the first law of motion, which asserts that a body does not change its state of motion unless acted upon by external force" ('Matter and Motion', Arts. lvii. and lviii.).

held to depend on how general the terms are in which that law has been stated. If it can be axiomatically asserted that the *centre of mass of a rigid system* moves uniformly until an external force acts on the system, and also that the system does not begin to spin, then the third law is established. For since zero acceleration means zero force, it follows that all the internal forces add up to zero, and have no moment; and since the system can be dissected bit by bit without ceasing to be a system within the scope of the first law, it follows that no stress can contain an unbalanced force or couple." Here, then, we have a new deduction, on which I would make two remarks :—(1) The conclusion is obviously too general. For since the assumptions specified are made for a rigid system only, the "no stress" of the conclusion should clearly be—no stress between the parts of a rigid system. The conclusion would thus become only a particular case of the third law. (2) That even this modified conclusion cannot be obtained without additional assumptions, and, even with them, by the method of dissection, may readily be shown. "All the internal forces add up to zero and have no moment." How do we know this? Only by the aid of familiar deductions from the second law of motion. Thus the second law is assumed. Dissect away one particle from the system. By the second law, as above, the internal forces of the remaining particles now add up to zero and have no moment. But we cannot assert this to have been true before the removal of the particle, unless we assume the physical independence of stresses. If this second additional assumption be made, though we now know that the actions and reactions of the stresses between any one particle and the remaining particles add up to zero and have no moment, we cannot conclude that "no stress can contain an unbalanced force or couple," because they would add up to zero and would have no moment, also, provided any inequality in the action and reaction of one stress, and their resultant moment, were neutralized by inequalities in the actions and reactions of other stresses and by their resultant moment, respectively.

While Prof. Lodge's method of dissection will not give even the modified conclusion, even with the aid of the above additional axioms, the reverse process will give it without his assumption as to spin. For in a system of two particles the conservation of motion of the centre of mass and the second law together tell us that the action and reaction of the single stress in the system are equal and opposite. And in a system of any number of particles, the axiom of the physical independence of stresses tells us that the stress between any two particles is the same as if there were no others acting, and that, therefore, in the case of all the stresses action and reaction are equal and opposite.

It is at once obvious that this argument will hold also, whether we restrict ourselves to rigid systems or not. Prof. Lodge does so, probably because he feels he cannot appeal to the experience of the human race with regard to the motion of the centre of mass of a non-rigid system. Had he adopted as his axiom the generalized first law (which he would be justified in doing according to my conception of an axiom as a proposition by means of which it is found possible to coordinate dynamical phenomena generally), then, with the aid of the second law and the physical independence of stresses, he might have deduced the equality and opposition of the action and reaction of all stresses *. But even then he would have deduced only what is explicitly stated in the third law and not the whole law. For just as the second law, by the generality of its assertion, implies the "physical independence of forces," so the third law implies the physical independence of stresses, at least so far as the equality and opposition of their action and reaction are concerned. This implied part of the third law is assumed in the above deduction.

So much for the asserted possibility of deducing the third law from the first. Prof. Lodge has held also that it may be deduced from the second †. Divested of its "muscles and clothing," his argument is as follows:—Action may be taken to mean simply the whole force applied to the body considered. The reaction of a body is defined as equal to the product of its mass into its acceleration. The second law of motion may be expressed in an equation on the one side of which we have the resultant force on a body or the action, on the other side of which we have its mass multiplied by its acceleration, which we have agreed to call its reaction. Thus action is equal to reaction. After a few paragraphs of explanatory matter he changes the expression of this result, without any attempt at justification, to the following:—The reaction or mass acceleration of a body is equal and opposite to the resultant of all the forces acting on it. It is hardly necessary to discuss this argument. It will be sufficiently obvious that if the definitions of action and reaction be accepted, reaction, if assumed to have direction, must be co-directional with action, not opposite to it, and that there-

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* Streintz (*loc. cit.* p. 131) and Muirhead (*loc. cit.* p. 477) point out the possibility of deducing the third law from the generalized first law, but do not perceive the necessity of assuming the physical independence of stresses.

† 'The Engineer,' vol. lix. (1885), pp. 217, 311, 380.

fore the conclusion which ought to be drawn is not even expressed in the same words as the third law. It is also just as obvious that even if the conclusion drawn had been warranted, though expressed in the same words as the third law it would not have been the same law, because the term reaction would be used in entirely different senses in the two laws.

That the above efforts to deduce the third law from the first or second should thus prove futile need not surprise us. For the second law gives us a quantitative statement as to the effect which is produced in a particle by a force; while the third tells us that forces always occur as one-sided aspects of stresses, and gives us the relation between the two forces of which every stress consists. Had these laws been recognized as being thus complementary to one another, efforts to deduce either from the other would have been seen beforehand to be doomed to failure *, and the above dreary refutations would not have been called for.

(4) *Prof. Lodge's Deduction of his Law of Conservation.*

Though Prof. Lodge still holds (pp. 11 & 14) that the conservation of energy (as defined by him) "can be deduced from Newton's third law and from the denial of action at a distance," and indeed gives a new version of this deduction, he admits that his deduction applies only to conservation during transfer, and that conservation during residence or "storage" is incapable of deduction †. How he reconciles

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* This seems, at first sight, not to agree with what Mach says (*Mechanik*, p. 226) after having referred to the subject matter of Newton's first and second laws, viz.:—"The third law contains apparently something new. We have already seen, however, that without the correct conception of mass it is unintelligible, and that on the other hand, through the conception of mass, which itself can be obtained only through dynamical experiences, it is rendered unnecessary." As, according to the ordinary interpretation, the idea of mass is given in terms of force by the second law, Mach would seem to hold that the third law is not independent of the second. This is not the case, however. Mach had previously shown that if we interpret Newton's second law by the aid of his definitions, this law does not give us a clear conception of mass. He himself obtains the conception, without reference to force, by an appeal to experience, which takes the place of the appeal made in the third law and thus renders it unnecessary.

† After dismissing, with some hesitation, the "plausible" method of establishing a law of nature by appeal to definition, he suggests that conservation during storage should be adopted as an axiom; but he does not meet the argument given in my Address to show, that if we retain Newton's laws, it is illogical to employ the law of conservation as an axiom, and that if we adopt the latter law as an axiom, Tait's suggestion (*Ency. Brit.*, Art. Mechanics, § 299) is the only logical one, viz., that Newton's laws should be abandoned and the law of transference of energy adopted instead.

the reiteration of his old claim with this admission we are not told.

With regard to my criticism of his earliest mode of making this deduction, he replies that the appeal to experience which I pointed out as having been made in his argument was a mere piece of politeness, which might have been omitted without affecting the argument. I think if he will look into the matter he will find that if he had been less polite his reasoning would have been faulty. It is unnecessary to occupy space in proving this, however, because the new deduction, given in the present paper and referred to below, embodies exactly the same fallacy as the old one.

In reply to my criticism of the law of conservation deduced by the argument of his third paper (quite a different law as I pointed out from that obtained in the earlier paper, though Prof. Lodge does not seem to realize this), viz., that it was of the same nature as the law of the conservation of momentum, his energy as defined in that paper being constant in quantity, because equal quantities of positive and negative energy must always be produced together, he states that his law is deduced from a less obvious assumption than the conservation of momentum. This is possibly true; but it does not affect the nature of the law deduced. The law of the conservation of electrical quantity is obtained in a different way from either, but is nevertheless a law of the same kind.

The new version of the deduction of the conservation of energy from the third law and the assumption of contact-action, is based upon a new definition of energy as "the result of work done," or "the result of activity lasting a finite time." As this is rather vague, work done on a body having a variety of results, Prof. Lodge proceeds to expound his definition and tells us that energy "is a name for the line-integral of a force, considered as a quantity that can be stored"*. Here, again, is the appeal to experience,—that the line-integral of force *may* be considered as a quantity which can be stored. If it is introduced merely out of politeness, it must not be used in the argument. If used in the argument, it forms an unacknowledged assumption.

The argument is as follows:—"Bodies can only act on one another while in contact, hence if they move they must move over the same distance; but their action consists of a pair of equal opposite forces; therefore the works they do, or their activities, are equal and opposite; therefore, by defi-

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* His comments show that he should have added here the words "in a body." But this does not affect the present argument.

nition, whatever energy the one loses the other gains. In other words: in all cases of activity, energy is simply transferred from one body to another, without alteration in quantity." Now it is obvious that we cannot pass *by definition* from work done or activity to energy, for energy has been defined as the *result* of work done or activity, the line-integral of a force considered as a quantity which can be stored. We must first pass from the equality and opposition of the works done (or line-integrals of the forces) to the equality and opposition of *the results* of the works done (or of the line-integrals of the forces *considered as quantities which can be stored*). How is this passage made? We are not told. But it is obviously by the assumption that work done on a body is equal to the result of work done, or that the line-integral of a force *may be considered* as a quantity which can be stored. And as obviously, this assumption is the law of the conservation of energy. Thus the conclusion of the argument, which, as in former deductions, is clearly conservation during transference only, is obtained by assuming the law of conservation generally.

It would indeed be a remarkable thing if it were possible, in the case of systems whose parts act upon one another only when in contact or at constant distance, to deduce the conservation of energy during transference from the third law of motion alone. When we make no assumption as to the distance at which action may occur, we require, in order to obtain the law of transference, to obtain first the general law of conservation, for which purpose we have to assume the second law of motion, and some such axiom as the impossibility of the perpetual motion. Having obtained from these axioms the general law of conservation, the third law then gives us the law of transference. Why, then, when we restrict our attention to systems exhibiting constant distance action * only, should it be possible to deduce the law of transference independently of the law of conservation? This is a logical question to which it should be possible to give a clear answer, if it is possible to make the deduction referred to.

(5) *Prof. Lodge's Deduction of Contact-action.*

In former papers † Prof. Lodge claimed to prove the

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* Prof. Lodge's argument assumes constant distance action, not specifically contact-action; for "if they move they must move over the same distance" is true of actions at all constant distances, not merely of actions at distance zero.

† Phil. Mag. [5] viii. p. 279, xi. p. 36.

incompatibility of action at a distance (1) with the law of the conservation of energy and the third law of motion, and (2) with the former law alone, and to be able to prove its incompatibility with the third law alone. In my paper (p. 139) I showed that the incompatibility with *the* law of conservation and the third law had not been proved, and that the incompatibility with *his* law and the third law was a necessary consequence of his definitions. I showed also that the argument by which he had sought to prove the incompatibility with *the* law of conservation alone (expressed in the form of the impossibility of the perpetual motion) was not sound. I also called attention to the fact that no attempt to substantiate the third of the above claims, a very important one, had been made. In the present paper he makes no reference to all this, but he makes a dogmatic statement which appears at first sight to be a reiteration of the second claim mentioned above, viz., "Energy is only really conserved under conditions of universal contact-action" (p. 16). Prof. Lodge is obviously under the impression that in making this statement he is taking up a position which is opposed to that of all his "prehistoric" dynamical brethren. In reality, however, any one who holds to the ordinary law of conservation and is able to regard action at constant distance as "practically" contact-action, must admit it; for, subject to the last proviso, it is an obvious deduction from the ordinary law. We may express that law as follows:—In any isolated system of bodies, the sum of their kinetic working-power, the working-power they may possess because of their being individually in a state of strain, and the working-power of the system due to actions between the bodies at variable distance, must be constant. Now the first two kinds of working-power constitute what Prof. Lodge calls energy, and the third must vary with the configuration of the system. Hence Prof. Lodge's energy can be conserved only subject to the condition of action at constant distance. If, therefore, action at constant distance be admitted to be "practically" contact-action, Prof. Lodge's statement is seen to be an immediate deduction from, and therefore merely a particular case of, the ordinary law of conservation.

(6) *Relative generality and precision of Prof. Lodge's Law of Conservation and the ordinary law; localization of Potential Energy; "Identity" of Energy.*

Prof. Lodge makes no reference to the argument by which I sought to prove that his law of the conservation of energy is merely a particular case of the ordinary law (p. 137 of my paper.)

In reference to his statement that his law is as axiomatic as the ordinary law, I showed that the latter is the more general in its applicability to dynamical problems. Prof. Lodge declares himself to be in entire agreement with this: but he adds that it is no discredit to "the true law not to lend itself to fictions," and that, while it is often permissible to work in a fictitious or incomplete manner, ignoring communicating mechanism, such "omissions and slurrings" should not be made in laying foundations, and that the habit of thus ignoring unknown essentials may lead to the treatment of fictions as if they were realities. There seems to me to be confusion here between the fictitious and the general*. There is nothing fictitious in the ordinary law of the conservation of energy. Work being said to be done by a body when the body moves (relatively, of course, to a dynamical reference system) against an opposing force, and energy being defined as power of doing work †, the ordinary law asserts that energy is conserved. It is sometimes expressed in terms of the fiction of action at a distance and sometimes also in terms

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* It is this confusion, I think, which has led Prof. Lodge, in his comparison of our respective types of mind, to make the entirely erroneous statement that I am willing "to base Physics on action at a distance" (p. 2, footnote). To it is due also the statement of p. 16 in which the ordinary conception of potential energy is ascribed to "the believer in action at a distance."

† Prof. Lodge's extraordinary objections to this definition are easily met.—(1) It is "vague." Doubtless it is to one who can make the statements quoted below. Compare its precision with that of the definitions by which it is to be replaced:—"effect of work done;" "result of work done;" "line-integral of a force considered as a quantity which can be stored." The formal definition of his 'Mechanics':—"Energy is that part of the effect produced when work is done upon matter, which is not an accidental concomitant, but really owes its origin to the work, and could not, so far as we know, have been produced without it; and which, moreover, confers upon the body possessing it an increased power of doing work,"—would seem to imply that he rightly considers his own definition so vague as to require to be supplemented by the ordinary definition. (2) "Plenty of energy has no power of doing work, at least, no power that we can get hold of." Nor can it have according to the definition. Probably what is meant is that plenty of bodies possess energy which we cannot utilize; but our ability to get hold of power is no criterion of its existence. (3) "A given amount of energy may have an infinite working-power, since it can do work at every transfer without itself diminishing." As just stated, according to the definition, energy cannot be said to have any working-power at all. It is the body or system of bodies possessing the energy which has the power. (4) "It is bold to maintain the conservation of working-power in face of the doctrine of the dissipation of energy." The conservation of power of any kind is quite consistent with diminishing opportunity of exercising it.

of the fiction[*] of contact-action. But it may be expressed without any reference to such fictions. Moreover, it may be deduced from the second law of motion and the impossibility of the perpetual motion, neither of which axioms involves such fictions. It is thus quite general, involving no assumption as to the distance at which bodies can act on one another, and applying to all cases of action, whether at distance zero, at constant distance, or at variable distance.

It will thus be evident why "the true law" does not lend itself to fictions, and why the ordinary law does. "The true law" does not, because it already embodies a fiction. The ordinary law does, because it embodies none, and is equally applicable, whatever fiction we may find it convenient in the meantime to assume or may ultimately find apparently coincident with fact. It is no discredit to "the true law" not to lend itself to fictions, provided the fiction it embodies assists us in coordinating the whole range of dynamical phenomena[†]. But discredit must attach to it so long as there are groups of phenomena to which the ordinary law can, while "the true law" cannot, be applied.

It will also be evident that, since the ordinary law involves no fictions, there need be no fear lest the employment of it should lead to the confounding of fictions with realities.

Not only does the ordinary law make no assumption as to the distance at which action may occur, it also assumes nothing as to the mechanism of action, and holds whether bodies be regarded as acting on one another through a medium or not, and if they are, whatever the medium may be through which their action is supposed to be conveyed. To speak of the law as on this account incomplete seems to me to be incorrect. Until we find some hypothesis as to acting mechanism which will enable us to coordinate dynamical phenomena, the science of dynamics must of course be incomplete; and doubtless as soon as possible some such hypothesis should be framed. But no such axiom has yet been suggested which is capable of general application. We cannot therefore help ourselves. The foundations of dynamics must in the meantime remain incomplete, though they are none the less firm on that account. Even, however, when the time of omissions and slurrings shall have passed, the law of the conservation of energy will be no more com-

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[*] As Prof. Rücker has pointed out in 'Nature,' vol. xlviii. p. 126, contact-action is as inconceivable as action at a distance. Both are thus equally fictions.

[†] Prof. Lodge holds, somewhat inconsistently, that "in a fundamental or theoretical treatment convenient fictions are better avoided" (p. 17).

plete than it is now. We may have acquired more definite conceptions as to the character of certain forms of energy, as to the mode of transference of energy, and as to the place of residence of potential energy; but as the law of conservation makes no statement on such points which will thereby be completed, our present ignorance with regard to them does not render it incomplete.

While in the event of some hypothesis as to acting mechanism becoming axiomatic there would be some readjustment in the ordinary conception of potential energy, there would not be nearly so much as Prof. Lodge supposes; for though his account of this conception (p. 16) is obviously a burlesque, he clearly does believe that it involves an "erroneous localization of energy," that, *e. g.*, in the case of a raised stone, the potential energy must be supposed, nearly all at any rate, to be resident in the stone *. This impression, however, may readily be shown to be erroneous. The potential energy (see the definitions of work done and energy, given above) may be said to belong to the system of earth and stone, because work may be done by the earth or by the stone or by both during the approach of these bodies. That it cannot be said to belong to either, is obvious from the consideration that, if either be held fixed relatively to a dynamical reference system, the work done during approach is then done by the other. How much of the work done during approach is done by the one and how much by the other, when both are in motion, depends upon the forces against which they move and their respective displacements relative to such system. It is thus obvious that according to the ordinary conception we can assert no more than that the potential energy belongs to the system, that this conception therefore involves no localization of the energy in the system, and consequently no erroneous localization.

This of course arises from the fact that the ordinary conception of potential energy involves no assumption as to acting mechanism. Should some sufficiently definite hypothesis of this kind become axiomatic, it would then become possible to localize potential energy. If, *e. g.*, we should come to hold that bodies consist of rigid particles connected by, and acting on one another through, an elastic medium, it

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* He objects to the ordinary conception of potential energy as being "a mere receptacle for stowing away any portion of energy which it is not convenient for the moment to attend to," yet admits (p. 24) that his own potential energy belongs to the same "temporary order of ideas." He also defies "any one to realize it as a thing." If he will define "thing" we may perhaps try.

would then be obvious that the potential energy must be considered to be resident in the medium. But until some such hypothesis becomes axiomatic, no localization of potential energy is possible.

It should be noted here that the adoption of the hypothesis of contact-action alone does not enable us to localize potential energy. As an axiom of acting mechanism it is incomplete; and it involves only the residence of energy in some body or other [*]. If we are to know in what body the energy resides, the axiom of acting mechanism must be made sufficiently complete. Thus, as just stated, if we assume, in addition to mere contact-action, that material bodies consist of rigid particles, and that the medium is elastic, the potential energy in the case of the raised stone must be considered to be resident in the medium. If, however, we assume bodies to consist of elastic particles, then the potential energy must be regarded as possibly resident partly in the medium and partly in the particles of the earth and stone. Prof. Lodge does not seem to realize this; for though he has proposed no hypothesis as to acting mechanism beyond that of contact-action, he has no hesitation in saying dogmatically (for he makes no attempt to justify the assertion) that in the case of the raised stone and in similar cases [†] the potential energy resides in the medium [‡].

Prof. Lodge claims that the law of the conservation of contact-action energy is more precise and definite than the ordinary law "because it is the law not only of *conservation*, but of *identity*." As to what is meant by its being a law of identity, he gives us two statements, of which we may consider the later first. "My proposition," he says, in his

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[*] I use the word body here in Prof. Lodge's general sense as applicable to a portion of the medium as well as to a material body. When, according to the contact-action conception, potential energy is regarded as resident in a body, the body must be considered to be an elastic body in a state of strain (p. 20). Since it is thus considered to consist of parts capable of relative motion, it is a system of relatively movable parts. Thus, according to Dr. Lodge's conception, potential energy is resident in systems just as truly as it is according to the ordinary conception, only the systems in the one case are small, while in the other they may be large. See Mr. E. T. Dixon's letter to 'Nature,' vol. xlviii. p. 102, and Dr. Lodge's reply, p. 126.

[†] Prof. Lodge is not so precise in his localization in all cases. In the case of the bent bow, even the ordinary conception of potential energy would admit of our localizing it "in the bow," and in the case of the gunpowder that conception would give us a more definite localization than "in the powder."

[‡] Probably my meaning in the statement quoted by Prof. Lodge on p. 16, footnote, will now be clear.

latest paper (p. 30), "amounts to just this, that whatever energy appears in a bounded region must necessarily have passed through the boundary." It is quite obvious that the assumption of contact-action together with the law of the conservation of energy do justify this proposition. It is equally obvious that, with the ordinary conception of energy, this proposition cannot be asserted. It may hold, but we cannot assert that it does. Why this difference? Not because the law of conservation is more precise or definite in the one case than in the other, but because we have a more complete conception of transference. With the ordinary conception of energy the only source of knowledge of transference is the third law of motion. With the other conception, we have both the third law and the axiom of contact-action *; and it is because of the greater definiteness which this latter axiom gives to our conception of transference, that it enables us to assert that if energy appears within a bounded region, it must have been either conveyed or transferred across the boundary †.

The above proposition seems to Prof. Lodge "to confer upon energy the same kind of identity or continuous existence (or, if you please, objectivity) as matter possesses." What kind of identity or continuous existence matter is supposed to possess (we need not refer to anything so metaphysical as objectivity) may be gathered from the earlier of the two statements referred to above, viz. :—"On the new plan we may label a bit of energy and trace its motion and change of form, just as we may ticket a piece of matter so as to identify it in other places under other conditions; and the route of the energy may be discussed with the same certainty that its existence was continuous as would be felt in discussing the

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* It should be noted that according to these two conceptions the third law, though expressed in the same words, is not the same law. In the one case it applies to all material bodies, whether in contact or at a distance. In the other it applies to all bodies in contact, whether they are material bodies or elements of the medium.

† It should be noted, however, that the law of conservation during transference which, notwithstanding the reiteration of his old claim, is all that Prof. Lodge now considers himself to have deduced, does not of itself, as he seems to suppose, justify the assertion of the above proposition. For it is consistent with this law that energy may, as he says, "leak away in some silent unobtrusive fashion." If it may thus leak out of observation, it may also leak into observation. Since, then, some of the energy which appears within a bounded region may have got there by this process of unobtrusive leakage, and since the above law can tell us nothing as to how it got there, this law cannot of itself justify the above proposition. It cannot therefore even in this first sense be called a law of identity.

route of some lost luggage which has turned up at a distant station in however battered and transformed a condition" *.

I need not discuss here the question whether even matter can be said to have the kind of identity here specified, whether in fact we can label a bit of it and follow it in its wanderings. We certainly cannot, in general, do so practically. Whether or not we can do so ideally may, I think, be found to depend upon our hypothesis as to the constitution of material bodies.

That energy, however, has this kind of identity seems to me to be a much more definite proposition than the one considered above, and not by any means to be implied in it. Indeed, that the contact-action conception of energy does not confer upon it the capability of being thus followed in its motion may, I think, be proved. We cannot be said to be able to follow a "bit" of energy in its wanderings unless we are able at all stages to localize it. If in the course of its peregrinations it enter a system of bodies and get so hidden away that we can only say of it that it is in some body or other of the system or is distributed in unknown proportions among them, then we have lost it even more completely than we should have lost our luggage in a railway collision if we knew only that it was distributed somewhere among the *débris*. Now we have seen that the assumption of contact-action alone does not enable us to localize potential energy. While, therefore, the introduction of the axiom of contact-action confers upon energy a certain kind of continuity, telling us that it must pass from one body to another or to others, it does not enable us to follow a bit of energy and to trace its route, because it does not in all cases enable us to localize it. If we wish to be able to trace its route completely, we must introduce a further axiom which, with contact-action and the third law, will make our conception of the transference of energy sufficiently complete. Then we shall be able to localize energy under all circumstances, and the first condition of following its motion will be satisfied.

But more than mere localization is necessary in order to label and follow a "bit" of energy. We must also be able to distinguish it from other bits when several of them at the same time get into the same body; and here the chief difficulty seems to arise. It is easy enough to frame an hypothesis of acting mechanism which will localize energy in all cases, provided we do not mind much whether or not it coordinates for us dynamical phenomena generally; but how we are to distinguish between the portions of energy which are transferred simultaneously, say, to a particle by the various elements of the medium in contact with it, is not so apparent. To my

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* Phil. Mag. [5] vol. xix. p. 482.

mind they must get mixed. Yet, before we can be said to follow a bit of energy as we do a labelled portmanteau, we must either have some means of making this distinction, or we must frame our hypothesis of acting mechanism in such a way as to exclude the possibility of two bits of energy getting into the same body at the same time.

Prof. Lodge refers to Prof. Poynting's paper on the "Transfer of Energy in the Electromagnetic Field"* as an illustration of the power which the contact-action hypothesis gives us of labelling and following bits of energy. Prof. Poynting's results, however, were based not only on the assumption of contact-action, but on other hypotheses as well, which made his axiom of acting mechanism, if not complete, at any rate much more complete than contact-action alone would have made it; and, moreover, if I understand him aright, he did not profess to label and follow the bits of energy distributed in the fields which he investigated.

(7) *The complete Transformation of Energy during Transference.*

In the present paper Prof. Lodge gives a formal demonstration, and a discussion of illustrative instances, of his proposition that, according to the contact-action conception, "energy cannot be transferred without being transformed" †. I need not enter into a detailed criticism of the demonstration. It is sufficient for my purpose to draw attention to two points:—(1) The demonstration itself admits the possibility that a body may act as "a mere transmitter, not itself active, only passing on what it receives," and applies to bodies not acting in this way. But a body cannot pass on the energy it receives without the energy being first transferred to it and subsequently transferred by it. The demonstration, therefore, admits that energy may in certain circumstances be transferred without being transformed, excludes such cases from consideration, and restricts itself to other cases ‡. (2) The demonstration is entirely qualitative. It is shown that in these other cases of action between two bodies, if one body lose, or gain, kinetic

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* Phil. Trans. 1884, pt. ii. p. 343.

† Phil. Mag. [5] vol. xix. p. 486. The formal statement of the proposition in the present paper is much less precise than in the paper just cited. He says, "My proposition was that the change of form is always from kinetic to potential or *vice versâ*," though he certainly would not have set himself to prove anything so obvious. The context, quoted below, shows that it is the complete transformation of energy during transference that the demonstration is held to prove.

‡ It should be noted, however, that on p. 33 Prof. Lodge speaks of the treatment of potential energy as being "conveyed elsewhere as a simple flux without transfer or transformation," as "blindfold treatment" which "does not exhaust the matter."

energy, or potential energy, the other body will gain, or lose, potential, or kinetic, respectively, *i. e.*, that in these cases there will be some transformation. In order to prove complete transformation, it would be necessary to show that the potential or kinetic energy lost or gained by the one body was equal to the kinetic or potential gained or lost respectively by the other. Thus even if we admit the validity of the argument without criticism, all that it proves is that, except in certain specified cases in which transference occurs without transformation, transference always involves some transformation, which is equivalent to the affirmative part of my conclusion that "transference of energy will in general involve partial but not complete transformation."

Besides this demonstration Prof. Lodge gives a discussion of two examples. The first, the loaded air-gun with its muzzle plugged, is an example of the transformation of potential energy during transference; the second, the impact of a couple of equal elastic rods moving end-on, exemplifies the transformation of kinetic energy.

With regard to the former, Prof. Lodge says:—"The compressed air has potential energy; on its release its energy is transferred to the moving wad, which instantaneously hands it on to the air near the muzzle, compressing it, and thus retransforming itself into the potential form." This seems to me a very inadequate account of what occurs. It assumes the wad to be a rigid body. As it is not, however, the first effect of the expansion of the compressed air must be to compress the adjacent end of the wad. But compression involves the relative motion of its parts. Hence during the first small expansion of the compressed air the wad must simultaneously gain both potential and kinetic energy; and therefore the potential energy lost by the compressed air has not been completely transformed in transference. If the wad be "dematerialized" and the example treated from the point of view of the kinetic theory of gases, the energy of the compressed air is practically wholly kinetic; and if, as Prof. Lodge says, the collision of two particles is most conveniently thought of in terms of elasticity, the question which has to be settled is what transformation occurs during the collision of two elastic particles. Treated in this way, the example thus resolves itself into the second example*—the impact of two elastic rods.

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* I need not make further reference to Dr. Lodge's discussion of this example from the point of view of the kinetic theory of gases, partly for the reason mentioned and partly because a portion of it is said to be "true in one sense, but not a final or complete statement."

I need not enter into a criticism of the discussion of this example, the conclusion being quite sufficient for my purpose. "By the consideration of instances," Prof. Lodge says, "we have thus been led to the induction that energy can be transmitted without obvious change of form by substances with infinite properties, *e. g.* by an incompressible solid; all molecular processes being either non-existent or being ignored; but that with ordinary matter there is always some percentage of obvious transformation, though we may apparently have all grades of it from complete to very small." This is of course partial, but not complete transformation. "Thinking of these impact cases alone, it might appear as if I had been overhasty in saying that the whole of energy must be transformed when it is transferred*. Yet observe that it has to pass *through* the intermediate condition. A row of ivory balls in contact has another thrown against one end, and from the other end one leaps off. The energy has been transmitted through the row somewhat as it is transmitted through the compressed strata of two impinging rods. Yet if the elastic connexions of every stratum are attended to, and if these be regarded as massless, I think it will be found that all the transmitted kinetic has really passed through a momentary existence as potential." Thus, in order to uphold the complete transformation of kinetic energy during transference, it is found necessary to assume the rods to consist of strata alternately massive and massless. Similarly, in order to make good the complete transformation of potential energy it would be necessary (as in the former example) to assume the rods to consist of strata alternately rigid and elastic. "The fact of necessary transformation," he continues, "is not so obvious when you come to look into some of these special cases; but I would refer once more to the proof given at the beginning of Part IV., which seems to me conclusive as to essential fact." Prof. Lodge therefore finds it difficult to make the accuracy of his thesis obvious in such examples, and falls back upon the general demonstration, which, as shown above, even if it be admitted without criticism, proves only partial transformation. "The difficulty arises because when an elastic body is struck (say a massive molecule with a massless spiral spring connexion) it begins to move a little directly the spring is the least compressed, and is moving half speed when the spring is fully compressed." In my paper (p. 140) I pointed out this "difficulty" as standing in the way of the doctrine

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* See footnote † to p. 259.

of complete transformation, in the following words :—"If one body exert on another a certain force through a certain distance, the same work is done on it whether the former body lose kinetic or potential energy in doing the work ; while the effect produced in the latter body will in general be a change both in its motion and its state of strain, *i.e.*, both in its kinetic and its potential energy. Thus, whether the former lose kinetic or potential energy, the latter will in general gain both, or transference of energy will, in general, involve partial but not complete transformation." Prof. Lodge thinks, however, the difficulty may be met. For he observes in conclusion :—"But I venture to say that on any view of the identity of energy the bit of kinetic which it first attains is a bit of energy that has been transmitted through the elastic stress of the spring, and that just as the second half of the energy must admittedly exist in the spring before it can reach the mass, so the first half has already passed through the spring and has reached the mass only after transmutation, although the transformation is disguised while the transference is obvious." The difficulty therefore is overcome only by assuming bodies to consist of massive molecules connected by a massless but elastic medium. Compare with this the following from my paper (p. 141) :—"If we assume the particles [of bodies] to be rigid they can of course have kinetic energy only. If the medium be assumed to have no inertia, its elements can have potential energy only. Hence if both assumptions be made, transference of energy between the particles and the medium must involve complete transformation, while transference from element to element of the medium must occur without transformation. If, however, both the particles and the medium be assumed to have both inertia and elasticity, the transference of energy will, in general, involve only partial transformation, whether it occur between the elements of the medium or between the particles and the medium."

It will thus be obvious that the necessity, which Prof. Lodge here acknowledges, of making definite assumptions as to acting mechanism, when he endeavours to follow his contact-action energy and to make its transformations agree with those prescribed by his thesis, is in entire agreement with what I have pointed out above, viz., that the assumption of contact-action alone is not sufficient for the purpose of completely localizing energy. Without being conscious of the fact, he assumes, not only contact-action, but also that bodies consist of massive and rigid particles, and that the medium through which they act on one another is massless and elastic*. But even with this axiom of acting mechanism his thesis as to complete transformation during transference holds only for transferences between particles and elements of the medium, and not for transferences from element to element of the medium.

(8) *The Complete Transference of Energy during Transformation.*

Besides the proposition just considered, Prof. Lodge, in the paper referred to above, asserted also that "energy cannot be transformed without being transferred." Although I called this proposition in question also, no reference is made to it in the present paper beyond a reiteration of the assertion (p. 16). In a synopsis of the paper, however, published by Prof. Lodge himself†, the admission is made that the assertion is incapable of proof. For we find it laid down, as the fifth axiom of dynamics, that "energy which is not being actively transferred from one body to another remains unaltered in quantity and form."

Prof. Lodge's right to enunciate this proposition as an axiom may be judged by his own standard. According to him (p. 3 of his paper), in setting forth an axiom, (1) regard must be had to the experience of the human race, (2) hundreds of instances should be adduced in which it holds, (3) a few special cases should be critically examined and in no case found to fail, and (4) "contrary instances" should be called for. Most of these regulations, which are obviously intended to prevent people from carelessly and thoughtlessly enunciating axioms, are admirable. With the exception, perhaps, of the appeal to the experience of the race, such formulators of axioms as Galilei, Newton, and D'Alembert followed them, especially as to the critical examination of special cases. But Prof. Lodge, in the present instance, ignores them all. He does not show how the proposition in question appeals to the race; he gives no instances in which it holds; he examines no special cases critically; and he makes no reference to a contrary instance which I ventured to bring forward in my paper (p. 141). We are forced, therefore, to conclude that according to his own regulations he is not yet in a position to enunciate this proposition as an axiom.

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* On p. 21 he speaks of "the potential energy of the particles of a spring," thus assuming its particles to be elastic, but this occurs in a paragraph which is "true in one sense but not a final or complete statement."

† 'Nature,' vol. xlviii. p. 62.

But there is another ground on which this course is seen to be for him entirely illogical. For though his having taken it is an admission that he regards the proposition in question as incapable of deduction, he has been hasty in reaching this conclusion. We have seen that, in order to maintain complete transformation during tranference, he has confessedly to assume that bodies consist of massive particles without elasticity, and that the medium is elastic but without inertia. Now, as pointed out in my paper (p. 141):—"That energy cannot be transformed without being transferred must of course be true if bodies consist of particles with inertia but without elasticity, and if the medium connecting them possess elasticity but not inertia." This is surely quite obvious. Hence with the assumptions already made by Prof. Lodge the proposition under consideration is capable of deduction. For him therefore it cannot be an axiom.

For those of us who do not hold to his theory of the constitution of bodies and media, the proposition is of course not capable of deduction. Whether or not we are to regard it as axiomatic must depend on whether or not it may be shown to be capable of coordinating dynamical phenomena generally.

Edinburgh, July 21st, 1893.

XXIV. *A New Form of Influence-Machine.*
By JAMES WIMSHURST[*].

IN April 1891 I had the honour to submit to this Meeting a very useful form of experimental Influence-machine, by means of which I was able to show that almost every combination of glass and metal, and also that plain glass disks, when moved and suitably touched, were capable of producing a flow of electricity.

It is one of those combinations, somewhat modified and extended, which I have now the further pleasure of bringing to your notice.

The machine consists of two disks of plate-glass, each of 3 ft. 5 in. diameter and $\frac{1}{4}$ inch thickness, mounted about $\frac{3}{4}$ inch apart on one boss and spindle. This spindle is driven by means of a handle, and the disks rotate in one direction.

In the space between the disks are fitted four vertical slips of glass, two being situated on the right-hand side of the machine and two on the left-hand.

The vertical edges of the slips which come nearest the spindle are cut to an angle, leaving a rather wider opening

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[*] Communicated by the Physical Society: read June 23, 1893.