Anglian coasts. Perhaps some of your readers could inform me whether the following difficulty, which has occurred to me, has been already raised, or has received a satisfactory answer. A submarine channel, some 400 fathoms deep, sweeps round the southern coast of Norway from the Cattegat to about the 62nd parallel of latitude, whence it gradually opens out into the deeper water further north. If the 100 fathom-line of soundings were to become the coast margin of north-western Europe, this channel would form a fjord, considerably broader than the straits of Dover, and for the most part 1800 feet deep. A further general upheaval, amounting in all to some 2500 feet, would convert this fjord into a wide valley, sloping gently towards the north, which was bounded on one side by the Scandinavian mountains (then commonly rising to a height of about 5000 to 9000 feet); on the other by a nearly level plateau (with a yet slighter slope, but in the main northward), elevated generally some 2000 feet above the bed of the valley. In such cases, if any trust can be placed on the evidence afforded by Greenland at the present day, the drainage of Scandinavia would obey the law of gravitation, even when in the form of ice, and would be diverted down the fjord or valley towards the northern Atlantic.

T. G. BONNEY.

The Nomenclature of Radiant Energy.

REFERRING to Prof. Simon Newcomb's letter in your issue of November 30 last (p. 100), suggesting a nomenclature for radiant energy—if no one else has already pointed it out, I would suggest that the word *irradiate* might be used in place of *illuminate*. It would be just as expressive, and would have the advantage of consistency; and its use would leave the word "illuminate" to its proper sphere. A. N. PEARSON.

Melbourne, January 9.

THE FOUNDATIONS OF DYNAMICS.

IT is rather curious that at the present time, when applied dynamics embraces so wide a range, so much attention should be directed to its foundations. One would have thought that the basis of a department of science which is used and used successfully in the investigation of the motion of vortex rings in a fluid, and the propagation of waves of electromagnetic disturbance, had been fully understood, and that no doubt of the firmness of the logical structure on which so huge a weight is laid, was entertained by those who are most active in turning it to practical account. If, as some appear to believe, our dynamical methods are founded on a vicious circle, how is it that the same men have been so successful in applying them to the elucidation of physical phenomena? Surely the repeated attempt to do this ought only to have led, if not to confusion of contradictory results, to continual failure to obtain any explanation at all.

On the other hand the extended use of dynamics has led scientific men themselves to a more general familiarity with dynamical processes. The study of dynamics is now a recognised part of scientific education, and the exigencies of teaching the subject have rendered necessary a much more complete examination of its fundamental assumptions than was usual before, when a few gifted mathematicians, by the force of their own genius, were led, almost "by a way they knew not," to the glorious results of physical astronomy. Again the recognition, more or less clear, that the old action-at-a-distance theories are really mathematical shortcuts, each gathering up into a single formula the result of the physical actions on molar matter of a medium in which it is immersed, has directed attention to the ether, and raised many questions of extreme interest as to the localisation of energy, and the conditions of its transference from place to place. Though a whole race of subtleties has with the new views sprung into being to mock our attempts to find firm footing, we are forced to the conviction that in this action of a medium lies the best means of scientific progress at the present time. As a consequence we are led to the reconsideration of the theory of energy, and therefore also of the conceptions of force, &c., and discussions as to the foundations of dynamics have been revived and carried on with a keener interest.

No one has worked with more zeal at the task of restating the doctrine of energy on anti-action-at-a-distance principles than Dr. Oliver Lodge, and it happens that recently his views have again been brought to the front by an address on the Fundamental Hypotheses of Dynamics delivered in 1892 by Prof. J. G. MacGregor before the Royal Society of Canada, and an article by the same author in the *Philosophical Magazine* for February 1893. An instructive paper has been presented by Dr. Lodge to the Physical Society, in which he has re-stated and defended his position. The discussion which took place on that paper, and the divergence of opinion then manifested, showed how wide is the interest in this subject, and how far it is still from being completely settled.[1]

The chief points in Dr. Lodge's papers are his insistence upon contact action as the cause of all action between bodies, and his re-statement of the principle of the conservation of energy. Only incidentally and as a preliminary, in his last paper at least, are the laws of motion touched upon. On the other hand, the chief burden of Dr. MacGregor's address is the laws of motion, and an attempt so to formulate them so as to give a logical basis for the science of dynamics in its application to physics. In his *Phil. Mag.* paper, however, he deals with Dr. Lodge's views with respect to energy.

I do not propose to restate the positions of the parties to the present controversy, but to endeavour to say how the question appears to an outsider who has felt keenly the difficulty of teaching the elementary principles of dynamics without introducing confusion by unnecessarily obtruding the fundamental *cruces* of the subject; or, on the other hand, slurring over matters of really vital importance.

In the first place, it seems to me that there is in general no sufficiently clear recognition of the fact that abstract dynamics is really abstract, and depends upon certain ideal conceptions just as much as does geometry, and that its application to practical problems must be made on certain assumptions, axiomatic in the proper sense or not, which must be justified by the results of experience. Abstract dynamics is a purely ideal science, geometric in a somewhat extended sense, caused by the introduction of certain notions not ordinarily employed in purely geometrical processes. So long as we confine ourselves to the ideal as we do in geometry, there are about it only difficulties of the same kind as we have in geometrical conceptions, and these I do not here propose to discuss. It is only when we apply the science to the interpretation of nature that we meet with the difficulties that every one must admit do exist, and which there is no blinking if we want to be straightforward, as to absolute direction, uniform motion, &c.

In this application we take some standard for the measurement of time. In this we are guided by the idea derived from the first law of motion, that any body in relative motion, which there is reason to conclude is not changed by the action of other bodies, may be taken as timekeeper. In practice we have recourse to a joint result of this idea and the equality of action and reaction, and take as our standard the rotation of the earth on its axis. [Of course this standard may not agree with some other and preferable standard means of time reckoning, but this will not affect the argument.]

In abstract dynamics we can and do imagine a system of axes of reference of some kind or other, but quite ideal so far, and agree upon or assume the existence of some mode of measuring intervals of time. We then consider the velocities and accelerations of different particles rela-

1 A rejoinder to this paper appeared in the September number of the *Philosophical Magazine.*

tively to those axes. We suppose different particles to have any accelerations relative to those axes which may be assigned, or which are deducible from data given, and so from the configuration at any given epoch that at any other, that is, to speak shortly, the motion, can be found. If the particles do not change their configuration relatively to one another a limitation is imposed on the motion, the particles constitute a rigid body. Thus we may consider any conceivable cases, and the science which deals with them is one of pure kinematics.

Now we may suppose our reference system, which we may call A, to have a motion relatively to some other reference system B, and the motion of the particles considered if referred to that other system will be compounded, for any instant, of the motion which the particles would have with respect to B, if they were rigidly connected with A, *in the positions they have at that instant*, and of the motions which the particles then have with respect to A. There is no difficulty, if the motion of A with respect to B is specified, in determining the former part of the motion of each particle. It will vary, of course, with the changing positions of the particles in consequence of their motions with respect to A.

Similarly we can push the reference still further back, and so from reference system to reference system whenever we find it desirable to do so. Of course we should never by any such process as this reach axes absolutely fixed; but it is the process by which we introduce corrections suggested by experience, as explained below.

It is, then, a result of observation that we can stop at some reference system, it may be the first A, which is suggested to us by the circumstances of the case. To a certain extent we can consider the effect of referring our chosen reference system to other reference systems naturally suggested, and be sure that the additional motions necessary for the parts of our system are negligible.

In practice we generally make the supposition that we may refer to a naturally suggested system of reference and find in what manner the results deduced require correction. For example, we refer the motion of a projectile to axes fixed in the earth, say one vertically upwards, and two others, one north the other west, and consider the motion. We find that the results only approximately coincide with experience, and we have to correct them on account of the earth's rotation. It may be that there are other corrections which on account of their smallness relatively to unavoidable errors of observation we can take no account of.

So far we have made no mention of mass or inertia. This idea is derived from experience of physical phenomena.

If we wish to apply our ideal science to the investigation of physical relations from experimental or observational data, we can only do so on certain assumptions tacitly or explicitly made, and these are to be regarded as postulates to be justified by the consistency and accuracy of our results when tested in their turn by observation. The term axiom, it may be remarked, seems inapplicable to many of these unproved assumptions, inasmuch as though they are simple concise statements, neither their truth nor their falsehood commends itself at once to the mind.

Now, with reference to our naturally chosen system of axes, we find that different bodies have, *in the same circumstances*, different accelerations, and hence we get the idea of the masses of bodies. In estimating similarity of circumstances we assume the constancy of the physical properties of materials, such as constancy of the quantity of matter in a body, the elastic properties of a spring, and the like. Thus, if we take a given spiral spring and apply it repeatedly to the same body with the same stretch, we find the same acceleration given to the body each time. Of course this result might be produced by a *pari passu* variation of the mass of the body, and the properties of the spring, but since we find the results consistent with those obtained with different masses and springs, the possibility of such variations need not be discussed. To this ideal method of comparing masses, the ordinary method by weighing is shown to be equivalent by Galileo's experiment with the falling bodies, Newton's pendulum experiment, &c.

Thus applying *similar circumstances* (which we may typify by a spring with a given stretch) to different bodies, we find their accelerations different, and we are led to a comparison of their masses, and thence to a prediction of the accelerations which in different circumstances will be produced in the same mass or in different masses, that is to the comparison of rates of change of momentum or of force. For example, suppose a spring with a given stretch in it to be applied for a second to each of a number of masses, and let the accelerations produced be a_1, a_2, a_3, &c. Then if we take quantities inversely proportional to a_1, a_2, a_3, &c., say μ/a_1, μ/a_2, μ/a_3, &c., and multiply each of these by the accelerations produced, we obtain, of course, the same product μ in each case, and we take this as a measure of the stress in the spring regarded as the producer of motion in bodies. In the ordinary system of measuring forces we take μ as ma, where m is the mass of the body reckoned in terms of a chosen unit of mass. This gives the dynamical method of comparing the masses of bodies. The masses of the bodies here considered are μ/a_1, μ/a_2, &c.

On the other hand, when we have to compare the motion-producing powers of springs having different stretches, that is, the forces they exert, we may use the same system of bodies if we please (or any system of which the masses have been compared as just described), and suppose that accelerations a'_1, a'_2, a'_3, &c. are produced by different springs applied to the bodies. Thus applying the method of reckoning explained above, we are led to measure the forces exerted by the springs by the products $\mu a'_1/a_1$, $\mu a'_2/a_2$, &c.

Thus from the point of view here adopted, Newton's second law sets up this mode of comparing masses and forces, and thereby furnishes a perfectly simple and consistent method of writing in a form ready for solution the equations of motion of a body relatively to any system of axes which we know from experience we may regard as at rest.

Here I wish to remark that when we write such equations as

$$m\ddot{x} = X,\ m\ddot{y} = Y,\ m\ddot{z} = Z,$$

the quantities on the right, commonly called the applied forces on the particle of mass m, are, it seems to me, merely put provisionally for values of the quantities on the left, which from the given circumstances of the motion, that is from the relations and data given, we may be able to calculate, or to supply from the results of experiment or observation. There is not any necessity for considering them as the *causes* or the measures of the causes of the accelerations $\ddot{x}$, $\ddot{y}$, $\ddot{z}$, of the particle.

The idea of force as cause of acceleration is useful as enabling us to speak and write with brevity about dynamical problems, and so to arrive quickly at the necessary equations. For example, take the problem of the motion of a particle of mass m hung by a massless spiral spring which the weight of the particle stretches by a length s. Then we know (1) that the stretch of the spring if not counteracted by the weight mg of the particle would cause the particle to receive an upward acceleration g, and since experiment shows that different weights stretch the spring by amounts proportional to them, we infer (2) that when the spring is stretched by an amount $s + x$, the elastic reactiom would produce an acceleration $g(s + x)/s$. Hence an upward acceleration of amount

gx/s will be produced, and if $\ddot{x}$ represent downward acceleration, we get the equation of motion :—

$$m\ddot{x} = -mg\frac{x}{s}.$$

which is ready for solution, and gives the well-known result.

We greatly abbreviate the above statements by saying that the upward "force" exerted by the spring in the first case is mg, and in the second, from the experimental result, $mg(s+x)/s$. This gives at once $-mgx/s$ as the *downward* force on the particle, which being substituted for X in the formal equation of motion, $m\ddot{x} = X$, puts the latter into a form adapted for solution.

Thus, though we may use, and do use constantly, the language of cause and effect in this connection, it ought to be remembered that when matters have been reduced to the solution of a dynamical problem, we have a purely mathematical process to carry out, by which we render explicit only that which is already implicitly involved in our equations.

This does not exclude or do away with the consideration of stresses as physical realities, it only states what I believe is substantially involved in the application of dynamics to physical problems. The objectivity, in the metaphysical sense, of force does not concern us, and discussions regarding it are, so far at least as physical results are concerned, not likely to be profitable.

I have heard it said by more than one very competent judge, that there is a certain vicious circle at the foundation of dynamics which there is no avoiding. We define force by mass, and mass by force. Thus it is sometimes said in effect, "Equal forces are those which produce equal accelerations in equal masses—equal masses those in which equal accelerations are produced by equal forces." But, as shown above, if we can assume constancy of mass of a body, and of the physical properties—say of a spiral spring—there is no difficulty in getting out of this circle of definition. These are assumptions we are entitled to make as the result of experience.

It is to be observed that since the measure of force in Newton's second law, namely, $m\ddot{x}$, is relative, the forces considered must be also relative. This is noticed by Prof. MacGregor in his address (p. 4), but he states that as our idea of force is derived from sensation, force in this sense is not relative. "According to this conception a body either is, or is not, acted upon by force." It is possible that I have failed to follow Dr. MacGregor here, but it seems to me that he has confounded *real* with *absolute*. Our muscular sense certainly tells us that a force, that is a stress as distinguished from a mass-acceleration, exists, but in no case can it inform us as to what in any absolute sense are the forces acting on the body considered. The force we feel "does not depend upon our point of view," but the force we regard as acting on the body certainly does. An acceleration which we observe is also a perfectly real thing in itself, but the acceleration of the particle is altogether dependent for its value on the point of view from which we regard it.

The ordinary misunderstanding that continually crops up with respect to the equality of action and reaction is feelingly alluded to by Dr. Lodge in his paper, and perhaps as a sympathiser I may be pardoned for devoting a paragraph or two to its consideration. A recent discussion of precisely the same thing in another journal has made it clear that the difficulty felt by the beginner in this matter is not clearly appreciated by many who endeavour to remove it. Because action and reaction are equal and opposite in the case (to take Newton's illustration) of a horse pulling a stone, the student (and the would-be critic of dynamical processes!) imagines that neither the horse nor the stone can get into motion. Now the confusion arises from regarding the action which is a forward force on the stone as being cancelled by the (if for a moment we neglect the mass of the rope or chain between the two bodies) equal and opposite force which acts, and this is what is overlooked, *not upon the stone, but upon the horse*, and therefore cannot affect the motion of the stone.

There may be other forces acting on the stone, and others again acting on the horse, and the motion of each body is changed *by the forces acting on that body, and those forces alone*. Thus there are two groups of forces, one group acting on the stone, and the other on the horse, and all that is asserted in the law of equality of action and reaction, as applied in this illustration, is that that particular force of the first group, which is the force exerted on the stone by the horse, is equal to that force of the second group which is the force exerted on the horse by the stone.

Action and reaction, however, are, I believe, most properly regarded as applied at the same place, though not to the same thing. Across any cross-section of the rope in Newton's illustration a stress acts, one aspect of which is a forward force on the part of the cord immediately behind the cross-section, the other a backward force on the part of the cord just in front of the cross-section. An excellent example is the action and reaction between two links of a chain, which are exerted across the surface of contact between the links, the action being a force on one link, the reaction a force on the other link. Here, as in all other cases, the action and reaction do not cancel one another, simply because they are applied to what are here regarded as entirely different things. [Of course, if we are considering the motion which a system consisting of different parts may have *as a whole*, the actions and reactions between these parts do cancel one another.]

I agree with Dr. Lodge in believing that in a certain sense we have nothing but contact action, that is, that all radiation phenomena are propagated by contact between portions of matter (not necessarily ultimately discrete portions) filling space. Thus at every place where such propagation is going on, and consequently changes of the motions of bodies are taking place, stresses are set up, and just where we have one aspect of a stress we have its other aspect.

This view, if it is adopted, certainly seems to lead to the conclusion that a process of transformation accompanies transference of energy; but it is not, so far as I can see, inconsistent with, and does not render in any way untenable, the doctrine of conservation of energy as ordinarily stated.

The doctrine that all energy is kinetic in reality, and that transformation consists in a passage of the energy from being kinetic energy of the bodies whose velocities can be observed and measured to being kinetic energy of those parts of the system regarding which we cannot have such knowledge, or *vice versâ*, when it is more familiar, and more clearly understood in the light of further scientific progress, may possibly help to clear away some of the many difficulties which crowd round this subject.

This article is long enough, and we must defer to some other opportunity any further consideration of Dr. Lodge's theory of the transference of energy. But both he and Dr. MacGregor have done good service in discussing from their several points of view this very difficult but apparently for many minds exceedingly fascinating subject. Nothing but good can come of "a revision of the standards" in dynamics, provided it has no destructive object in view, but only the improvement and, if necessary, correction of the methods of presenting and teaching the science.

A. GRAY.