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# IV.—MECHANICAL EXPLANATION AND ITS ALTERNATIVES.

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§ 1. THE controversy which has long raged as to the applicability, or, at any rate, the adequacy, of mechanical explanations to biology still continues and seems likely to be always with us. Evidently, to settle it two questions must be answered: (i) What precisely do we mean by a mechanical explanation and how do we suppose it to differ from any alternative kind of explanation? and (ii) Can the phenomena dealt with by biologists be fully accounted for mechanically in the sense defined? To answer the second question profound knowledge of the details of biology would be needed, and such knowledge I make no claim to possess. But it is perfectly useless even for the most eminent biologist to attempt to answer the second question till he and his opponents have agreed on their answer to the first. Now the first question is very largely a logical and philosophical one; no doubt to deal with it adequately a knowledge of the special sciences is also needed, but a mere knowledge of the special science of biology without any philosophical or physical training is a very slender equipment for meeting the difficulties involved in the problem.

§ 2. I have two main complaints to make against most of the discussions between mechanists and their opponents with which I am acquainted. First and foremost, the combatants all assume that everyone is agreed as to what is meant by a mechanical explanation, and, presumably in consequence of this assumption, never condescend to inform the reader what *they* in particular mean by it. I strongly suspect that this belief in a general agreement indicates nothing but a general haziness.

Secondly, it strikes me as strange and unfortunate that the controversy should always be conducted about biology in particular. One is tempted at once to ask: Why biology? Is the accepted, but carefully concealed, definition of mechanical explanation such that no one but a lunatic would suggest of any other science, say chemistry, that mechanical explanation may be inadequate to its subject matter? To confine the question to biological ground is not merely, on the face of it, strange and unwarranted, it is really misleading. Biology is not a particularly advanced science compared with physics and chemistry; it has discovered few general laws as yet, and none, I imagine, of the same range and certainty as those of gravitation or constant proportions. Hence, by confining the question to biology, the opponent of the adequacy of mechanical explanation needlessly prejudices his own case. He lays himself open to two alternative retorts. One sect of mechanists will tell him that the problems of biology are evidently so complex that, even if many biological facts can be produced of which no satisfactory mechanical explanation has been given, this offers hardly the least presumption that no such explanation is possible. Another sect of mechanists will tell him that he may learn from the state of his own study that where mechanical explanation stops there science stops also. Now suppose that, instead of confining the question to biology, we had propounded it about chemistry. And suppose, for the sake of argument, that we had found that, when *mechanical* is defined in an intelligible and acceptable way, some facts of chemistry are not susceptible of complete mechanical explanation. The second objection would now be useless. Chemistry undoubtedly is a science with definite laws of great certainty and wide range Hence, if it be incapable of complete mechanical explanation, we can see at once that science does not end where the possibility of mechanical explanation stops. The first objection would not indeed vanish, but it would lose much of its force, and it would be a problem to see how much force it had left. For undoubtedly much of its appeal to scientists comes from their combining the two propositions (*a*) all regions of phenomena are susceptible of scientific treatment, and (*b*) nothing is susceptible of scientific treatment unless it be capable of mechanical explanation. With the downfall of the second of these premises this particular argument would collapse, and it is doubtful how much solid reason would remain for the view that when a subject *appears* to be incapable of complete mechanical explanation this appearance is *always* an illusion due to the complexity of the phenòmena.

If there be anything novel in the following discussion it consists in the fact that I am concerned rather to define mechanical explanation, and see what are the alternatives to it, than to discuss whether some particular region of phenomena, such as biology, be mechanically explicable, and in the fact that I shall try to take illustrations rather from chemistry and physics than from biology.

§ 3. An explanation of any phenomenon always involves two factors:—general laws and a specified set of entities subject to these laws. For what is to be explained is a definite particular state of affairs, and you cannot explain this merely by a set of general laws. Of course the set of phenomena to be explained may be more or less general; you may, *e.g.*, wish to explain reproduction in general, and not that of camels in particular. In proportion as the phenomena to be explained are general so will the specifying features of the entities involved in the explanation be general. But the phenomena dealt with by a given science will always be considerably specialised or they could not fall under a single science. And so the entities involved in an explanation will always be more or less specialised as compared with the general laws employed. To put the matter in a different and probably more satisfactory way, laws assert correlations between attributes. What has to be explained is some more or less specialised instances of correlated attributes. Laws alone will not explain this; one specialised instance can only be explained in accordance with a general law by another specialised instance.

If this be true of explanation in general, there will be two questions about mechanical explanations in particular: (i) What is the peculiar nature of the entities employed in them? and (ii) What is the peculiar nature of the laws? Of course these two questions are never completely independent, since one important characteristic of the entities is that they are the kind of entities mentioned in the laws.

§ 4. It seems plausible to suppose that it is a necessary condition of a mechanical explanation that the laws employed shall be those of Mechanics, *i.e.*, Newton's three laws of motion or some substitutes for them. The characteristic peculiarities of mechanical explanation will therefore depend on the special character of the laws of motion, and of the entities presupposed by them. We must therefore begin by asking ourselves what are the special characteristics of the laws of motion. I do not think we need trouble ourselves here with the question as to what is meant by space and time as used in the laws of motion. This question is of the utmost importance in a special study of the philosophy of mechanics, but it is not so important here because any difficulty or obscurity about space and time in mechanical explanation would be equally present in any alternative kind of explanation, since all would presumably use these concepts.

The fact is that questions about absolute *v.* relative position or motion, the precise meaning of simultaneity, etc., appear to be peculiar to mechanics and electrodynamics, not because other sciences do not use these notions, but because they have used them without that resolute attempt to clear up their obscurities which physicists have had to undertake. All alternative kinds of explanation use the space and time of mechanics, whatever that may be, and what characterises mechanics as regards these concepts is not the fact that it employs them, but that it tries to be clear about their implications. I shall, therefore, simply refer to the space and time of mechanics as the dynamical reference system without discussing the precise nature of this system, and shall assume that this is what other natural sciences have in mind when they talk about space and time, position and motion. I think we can make this assumption without unfairly prejudging the case even of Bergsonians, for their "duration," so as far as I can see, is not a new and different kind of time, but an alleged property of what exists in time in the dynamical sense.

§ 5. To state the characteristic features of the laws of motion, it seems best to express them in the concentrated form of Lagrange's equations. We are here much nearer to laws in terms of quantities that can actually be observed and measured than when we express the laws in terms of particles, which are, of course, pure mathematical fictions. Lagrange's equations for a system, as everyone knows, are in terms of generalised co-ordinates, *i.e.*, measurable magnitudes which between them fix the position and configuration of the system at a given moment. Let us denote them by the letters $q_1 \ldots q_n$. Then Lagrange's equations for the system consist of $n$ simultaneous differential equations of the form

$$\frac{d}{dt}\left(\frac{\partial T}{\partial \dot{q}_r}\right) - \frac{\partial T}{\partial q_r} = P_r.$$

Now let us consider the terms in this equation. $q_1 \ldots q_n$, as has been marked above, are measurable spatial magnitudes sufficing to fix unambiguously the configuration and position of the system at a given moment. T is a function of these co-ordinates of a certain definite form. In the most general case it takes the form

$$M\left[A + \sum_{r=1}^{r=n} B_r \dot{q}_r + \sum_{r=1}^{r=n} \sum_{s=1}^{s=n} C_{rs} \cdot \dot{q}_r \dot{q}_s\right]$$

when A and the B's and C's are functions purely of the $q$'s and of time, and M is a pure scalar constant characteristic of the system. M has the further property of being an additive constant. By this I mean that, if we have two systems $S_1$ and $S_2$ and their separate Lagrangean equations involve $M_1$ and $M_2$ respectively, then the Lagrangean equations for the two, regarded as forming a single system $S_3$, will be characterised by a constant $M_3$ such that $M_3 = M_1 + M_2$. T is, of course, really an old friend, the kinetic energy of the system. The forms of A and of the B's and C's are of the following nature. They are always sums or integrals carried out through the whole region of the system. The term under the integral sign always contains, among other things, a function of the co-ordinates and the time expressing the distribution of density, and such that this function when integrated over the whole volume is independent of time and equal to M. Apart from this limitation, the function is characteristic of a given system, and nothing further can be said of it in general. Similarly, of course, the limits of the integration vary from system to system, and nothing can be said of them in general.

We may say, then, with regard to the left-hand side of Lagrange's equations, that what characterises a mechanical system is (*a*) the general form of the function T as regards its degree in $\dot{q}$; (*b*) the fact that the coefficients in T other than M are functions of purely spatio-temporal magnitudes, that they are always sums or integrals taken throughout the volume of the system, and that they always involve, among other things, under the integral sign a function which when integrated through the volume is equal to M; and (*c*) that this quantity M is a pure scalar, independent of time, and additive from one system to another in the sense defined above.

§ 6. We can now turn to $P_r$, the term on the right-hand side of Lagrange's equations. The P's are called generalised components of force; and there is a very marked difference between them and T, both in their form and in their independent variables. The variables in T were only the $q$'s and $\dot{q}$'s of its own system, S, and the form of the function was fixed by the laws of mechanics. But the P's are functions which always involve variables belonging to other systems, since they represent the mechanical effect of the rest of the world upon S; and the form of the P's cannot be laid down beforehand, but depends upon the special natures of S and of the parts of the world that affect it mechanically. *E.g.*, if S contains pieces of iron, and S′, the part of the world whose effects on S have to be considered, contains electrically charged bodies, and is moving relatively to S, the form of the P's will depend on the laws of electro-magnetism. If both be uncharged and unmagnetic, the form of the P's will depend on the laws of gravitation. At present, then, we can say that, so far as the laws of mechanics alone are concerned, the functions on the right-hand side of Lagrange's equations are practically unlimited, both in their form and in the number and kind of their independent variables. These are determined by the special laws of nature and will vary with the chemical, thermal, electrical, or magnetic state of S and S′. Their independent variables will therefore (*a*) not be confined to geometrical and temporal magnitudes, the first differential coefficients of the former with respect to the latter, and a single additive constant; as was the case with T. Variables such as temperature or electrical charge may enter, and new constants, such as the gravitational constant or the elasticities of various kinds of matter may be involved. Again, (*b*) the geometrical variables may be present as differential coefficient with respect to time of any order. In some theories of electrodynamics, *e.g.*, the P's would contain accelerations as independent variables, and mechanics has nothing to say against differential coefficients of any order you please figuring here, if the facts of physics are found to demand them.

We thus reach a rather interesting conclusion. If by "mechanically explicable" we mean "obeying Lagrange's equations," we are tied down pretty tightly by the left-hand side, and allowed almost unlimited latitude by the right-hand side as to the forms and the variables of the functions which express the laws of nature. This difference between the two sides of the equation becomes less startling when we remember that the left-hand side never professed to contain ultimately any variables except spatio-temporal ones, since the laws of motion by themselves never pretended to inform us about any other characteristics of material systems beside their configurations and positions at every moment of time. In fact, the laws of motion, whose form is summed up in the form of Lagrange's equation, and of the function T, and whose restricted subject-matter is indicated by the restriction in the nature of the independent variables in T, may profitably be compared in their relation to the movements of matter with the laws of logic in relation to our reasonings. The laws of logic will not guarantee our premises or our conclusions; but they forbid some conclusions to be drawn from some premises. Similarly the laws of motion will not tell us that matter will move or the particular way in which the state of one part of the world will determine motion in other parts; but, if true, they restrict possible movements within certain wide limits.

§ 7. Now I think that most people who speak of mechanical explanations and hold that they are always possible in theory mean by them something in one respect more rigid, and in another respect less rigid, than the mere obedience to Lagrange's equations which we have so far described. To put the matter figuratively, they would be prepared somewhat to loosen the rigidity of the left-hand side, and would insist on greatly tightening the laxity of the right-hand side. Let us consider these two points in turn.

It would be justly counted unfair to tie mechanists down to a slavish adhesion to the precise form of Lagrange's equations. If the theory of relativity be true, for instance, Lagrange's equations, as we know them, cannot be strictly correct, though their deviation from correctness would be in practice negligible in most cases. Now, it would be absurd to say that a man was inconsistent in holding that everything was mechanically explicable merely because he had deserted the mechanics of

Newton for that of Minkowski or Einstein. As to the form of the fundamental equation and of the function T, then, we must be reasonably charitable; we must allow that an explanation is mechanical even though the forms of the equation and function differ from those in Lagrange's equations, provided that they tend to approach Lagrange's forms indefinitely under normal conditions.

Again, we know that Maxwell tried the very interesting experiment of removing the restriction of the variables in Lagrange's equations to geometrical magnitudes, and replacing them by generalised co-ordinates defining the electromagnetic state of systems. In certain cases and with suitable interpretations of his terms he found that Lagrange's equations held, *i.e.*, that his equations had the same form as Lagrange's and expressed the observable facts, whilst the function T was of the same form as in the ordinary Lagrange equations. Now no one can deny the interest and importance of this fact; but are we to define "mechanical" in such a sense that any region of phenomena in which the laws can be expressed by equations of the form of Lagrange's, even though the generalised co-ordinates be not merely spatial magnitudes, shall be counted as mechanically explicable? Well, we are simply seeking for a definition, and we have a shrewd suspicion that most people who say that they are (or are not) mechanists are far from clear as to what it is precisely that they are asserting or denying. Hence we shall do best at this stage to distinguish two senses of mechanical explanation, a milder and a more rigid one. If the magnitudes which Maxwell took as his generalised co-ordinates be correlated with spatial magnitudes defining the minute structure of the electromagnetic system with which he was dealing, and if these invisible parts obey Lagrange's equations in the strict mechanical sense, it is natural that the correlated observable magnitudes taken by Maxwell as co-ordinates should obey the equations. But the converse does not hold. Hence we can distinguish a milder form of mechanism, which merely asserts that any region of phenomena is to be called mechanically explicable if the measurable magnitudes which define the state of a system within this region obey something like Lagrange's equations, and a more rigid form which will only consent to call the phenomena mechanically explicable if (to use the convenient expressions of Lorentz) the "macroscopic" obedience to Lagrange's equation be due to the "microscropic" obedience of the minute particles of the system to these equations in the strict sense in which all generalised co-ordinates are spatial magnitudes.

§ 8. There is, however, as it seems to me, a *via media* between these two views, which it is very important to discuss. We talk of defining the state of a system by magnitudes like temperature, electrical charge, and so on, and we distinguish such generalised co-ordinates from the purely geometrical ones contemplated by the laws of motion. But, if we consider what it is that we actually measure when we say that we are measuring temperature or charge, we find that it nearly always is a spatial magnitude or the change or rate of change of one. To take an example. Maxwell found that he could express the effects of two circuits on each other in terms of Lagrange's equations if he took for generalised co-ordinates not merely the geometrical quantities defining their positions and shapes, but also electrical charge as a co-ordinate $q$ and current as $\dot{q}$. If we consider, however, what is directly measured when we say that we measure a current or a charge, it is always a spatial magnitude, such as the deflexion of a galvanometer needle. Suppose then that he had reckoned in with his system of circuits the galvanometers with which he measured the current and the ballistic galvanometers with which he measured the charges, all his generalised co-ordinates would have been spatial, and the only outstanding feature of the so-called non-spatial ones would be that, whilst in themselves spatial, they were supposed to stand for a certain physical state of matter. Here, then, we have got back to the strictly mechanical Lagrangean equations, but without making any assumptions as to the microscopic accompaniment of macroscopic phenomena.

I do not wish to contend that this invariably removes the distinction between mechanism in the milder and mechanism in the more rigid sense, for I am not sure that it does. The difficulty remains that what from the spatial point of view is a co-ordinate may need to be regarded from the physical point of view as the differential coefficient of a co-ordinate with respect to time if the form of Lagrange's equations is to be kept. Thus a constant current means a constant deflexion of a galvanometer needle; for mechanical purposes the latter would be regarded as a spatial co-ordinate defining the state of the galvanometer; from an electrical point of view it would have to be regarded as the time differential-coefficient of a co-ordinate, if the form of Lagrange's equations is to be kept. Still, this is largely a matter of means of measurement chosen. If charge be measured statically, and current by the rate of decomposition of water, the directly measured magnitudes would respectively be a spatial magnitude and the rate of change of one. Even in purely mechanical problems there is sometimes a difficulty in hitting on the right generalised co-ordinates, and a danger of mistake through taking as a co-ordinate some variable that contains a differential coefficient with respect to time.

All things considered then, it seems not unreasonable to suggest that wherever Lagrange's equations are obeyed in the extended sense which seems to involve non-geometrical generalised co-ordinates for the specification of a system, they are also obeyed in the more restricted mechanical sense, if we substitute for the supposed non-geometrical coordinates the actual readings on some instrument which is said to measure the latter magnitudes, and remember that such readings are always ultimately lengths or angles or their rates of charge. If this be so the milder sense of mechanical explanation involves the more rigid without necessitating any doubtful assumptions as to the microscopic accompaniment of macroscopic phenomena.

§ 9. It is now high time to return to the right hand of the Lagrange's equations. We have seen that many people who would call themselves mechanists would allow a certain laxity in the form of Lagrange's equations and in the nature of the variables taken as generalised co-ordinates on the left hand side. But we suggested that a mechanistic view is generally considered to impose restrictions on the functions $P_r$ which are in no way necessitated by the laws of motion. Let us now consider what these restrictions are. I think that what might be called a "high and dry" mechanist would impose very severe restrictions, both on the form of the P's and on the nature of the variables and the constants contained in them. It is reasonable to suppose that phenomena are typical instances of mechanically explicable ones if they are treated in books on abstract dynamics and not in books on general physics. Now the two main examples of such phenomena are gravitational attraction and the impact of bodies. What is there peculiar to them which makes them typically "mechanical" transactions? Let us take gravitational attraction first. There are four peculiarities about the form of the P's here. (*a*) All the variables involved in the P's are of the same kind as those involved in T. We simply need to know the shapes, sizes, distances, and distribution of mass in the two systems. No property other than these, which may differ from one system to another or in the same system from time to time, is needed. (*b*) One constant beside mass is needed that does not appear in T, viz. $\gamma$, the gravitational constant; but it is supposed to be the same for all systems at all times and in all conditions. (*c*) The P's contain no differential coefficients of geometrical magnitude with respect to time. (*d*) The P's in all cases whatever are vector functions, but in the present case they are vector functions of a special kind. They are compounded vectorially from vector functions which contain the distances between points of S and points of S′ by pairs, the gravitational constant, and the values of the density-distribution functions for these pairs of points, and which have for direction that of the line joining the two points. This last characteristic is sometimes thought to be guaranteed by the laws of motion themselves, but this does not seem to me to be true. Suppose we take a magnetic pole as our system S and a straight wire carrying current as our system S′. Then the P's are vector functions compounded vectorially out of functions involving the distance between points on the wire and the pole taken by pairs. But these vectors are never in the directions of the lines joining the point-pairs. They are always at right angles to the plane containing the wire and the pole, and hence at right angles to these lines.

The condition (*d*) is a very important one from our point of view. When it is fulfilled we may say that the P's are "mechanically analysable." When this is the case the action of a whole system S′ is connected in a perfectly definite and extremely simple way with that of its parts. S and S′ can be regarded as divided up into mass-points and the total action of S′ on S can be regarded as the vector sum of a set of infinitesimal vectors each involving a point in S and a point in S′, their masses, and some other constant. These infinitesimal vectors will involve the co-ordinates of the point-pairs as differences, and these differences will appear in the function in the same way for all point-pairs. The absolute co-ordinates of each point in the pair may enter through the functions expressing the density-distribution in the two systems, but no co-ordinate of any third point will enter.

§ 10. The imparting of motion by impact, the other kind of transaction which is regarded as typically mechanical, now demands attention. Any case in which bodies hit each other or slide over each other requires for its complete determination a knowledge of two special sets of natural laws beside the laws of motion. We need to know the laws of elasticity and those of friction. And these laws involve constants which are not the same for all kinds of matter, but differ from one system to another. In the artificially simplified cases of perfect smoothness or roughness or of perfect elasticity, we do not avoid these special laws of nature; we assume them, but we also assume that in the systems under consideration the constants become 0 or 1 or $\infty$. Thus, impact, which is sometimes regarded as *the* mechanical transaction *par excellence*, seems to me less "mechanical" in a perfectly definite sense than gravitation. For it has to take account of properties which vary from one kind of matter to another, whilst the gravitational constant, so far as we know, is independent of all circumstances. When people take the view that all action is by impact, and this is considered a typically mechanical view, they always make the assumption that friction and imperfect elasticity are merely the macroscopic appearances of microscopic transactions between systems which are frictionless and perfectly elastic. The remaining important factor in impact is that there is no necessity to analyse the action of a system $S'$ on a system $S$ into actions between their parts. The peculiarity of gravitational action was the simplicity of the relation between the action of a whole and that of its parts; the peculiarity of impact is that no analysis into the actions of parts is needed to explain the different actions of systems with different configurations and distributions of mass. The difference can be put still more accurately as follows. When two systems act gravitationally on each other six triple integrals are needed whose arguments refer to points in each system and whose limits involve the boundaries of both. When two systems act on each other by impact or friction we do indeed need two triple integrals, for we shall need to find the mass-centres of each. But the arguments and limits of each of these refer only to one of the systems respectively. Apart from these we merely need to know the points at which the bodies hit each other, their elasticities and coefficients of friction, and their translational and angular velocities just before impact.

§ 11. We are now in a position to see what meanings can be attached to the phrase "mechanically explicable." Several possible meanings have emerged, some making the phrase involve much more than others. Our next task will therefore be to arrange them in order from those which are least to those which are most rigid in their demands.

All mechanical explanations imply that the phenomena under discussion obey either Lagrange's equations or some substitute for them which approximates indefinitely to them for ordinary velocities. But, as we have seen, Lagrange's equations can be interpreted in a more or less rigid way: (1) The mildest form of mechanism would simply maintain that all systems can be determined by sets of co-ordinates, $q$, which may include time, and must exclude differential coefficients of co-ordinates with respect to time, but may include other than geometrical magnitude. That with these co-ordinates a function, T, can be found of Lagrange's form, and a function, P, such that Lagrange's equations hold, and describe fully all the changes of the system. No special form is assumed for the function P. This mildest form of mechanism I will call "descriptive macroscopic mechanism." (2) At this stage a man who still refuses to set limits to the form of the P's may yet make more rigid demands about the generalised co-ordinates. He may insist that ultimately they must be only times and geometrical magnitudes. This view itself may take two forms, a milder and a more rigid one. (*a*) It may simply mean that, whatever we may choose to *call* our generalised co-ordinates, what we actually measure are masses, geometrical magnitudes, and times. These magnitudes are *called* currents, or charges, or temperatures, because of their relations, but in themselves they are geometrical. The view then comes simply to this, that in every region of phenomena the behaviour of any system which includes the measuring instruments by which the phenomena are investigated obeys Lagrange's equations in the strict sense in which all generalised co-ordinates are geometrical magnitudes or times. This view I shall call "metrical macroscopic mechanism." But (*b*) the more rigid view may be taken that the obedience of the non-geometrical generalised co-ordinates of a system to Lagrange's equations is due to the correlation of the macroscopic phenomena with microscopic transactions, which obey these equations in a form in which all the generalised co-ordinates are geometrical magnitudes or times. This view, unaccompanied by any special limitations on the form of the P's, I shall call "heterogeneous microscopic mechanism." This is as far as we can get without imposing restrictions on the form of the P's.

(3) The functions P might evidently be restricted by confining the independent variables to certain kinds of magnitude, or by making some special assumption about the constants, or by imposing some limit on the form of the function. The rigid mechanist would subject the P's to all these restrictions: (*a*) In the first place, he would only allow P to contain independent variables of the same kind as are admitted in T, viz., geometrical co-ordinates, their first differential coefficients with respect to time, and time. There is something specially "mechanical" in an explanation which only allows on the right-hand side of Lagrange's equations variables of the same kind as those to which the laws of motion confine us on the left-hand side. The theory that all action is by impact or by central forces conforms to these conditions. (*b*) Again, the rigid mechanist would wish to assume that the distinctions between one kind of matter and another, *e.g.*, wood and iron; or between one state of matter and another, *e.g.*, between an unmagnetised piece of iron and the same piece magnetised, are only macroscopic differences, and that their microscopic correlates are always differences of number, configuration, and density. This means that he believes that the P's ultimately contain no constant, other than mass, which differs from one system

to another, but only some universal constant, like $\gamma$ in gravitational theory. The theory, if it were sincerely entertained, that the microscopic correlate of all the macroscopic phenomena of matter was a set of perfectly similar electrons would fulfil these conditions, particularly if we hold mass to be an electromagnetic phenomenon. In this case the universal constant would be C, the velocity of light *in vacuo*; the only constant that would vary from one system to another, and would take the place of mass, would be electric charge. All variables would be geometrical magnitude or their first derivatives with respect to time, and all the macroscopic differences between one kind of matter, or one state of matter, and another, would be those based on differences in respect of these variables among qualitatively homogeneous electrons. Of course the form of Lagrange's equations would have altered somewhat, but only in ways which we have allowed to be compatible with a mechanical view.

Lastly, the rigid mechanist would impose restrictions on the form of the P's. He would insist either that all action is by impact, or that it is all by central forces. Now either of these hypotheses involves a particular view of the connexion between the behaviour of a whole system and that of its separate parts. The hypothesis of central forces, as we have seen, implies the possibility of a mechanical analysis in the sense defined above. The hypothesis of impact removes the necessity for any analysis at all of the whole into the action of its parts. The view which would restrict the variables and constants in the way described I will call "homogeneous microscopic mechanism," and the most rigid view of all, which also restricts the form of the P's, I will call "pure mechanism."

§ 12. We have thus distinguished five meanings of mechanism. Two are macroscopic and make no assumption about the invisible correlates of observable physical phenomena. These are descriptive and metrical mechanism. All more rigid forms are necessarily microscopic. Heterogeneous mechanism is so from its definition, since it offers itself as a microscopic explanation of descriptive mechanism. Homogeneous and pure mechanism, if held at all, must be held in a microscopic form, since they fly in the face of the observable facts if you interpret either of them macroscopically. Macroscopically there are different kinds of matter with different specific constants and capable of different physical states which modify their mechanical action on each other. And the mechanical effects of whole systems (*e.g.*, of a heated mixture of oxygen and hydrogen) are not macroscopically connected with that of their separate parts by any laws which allow of a "mechanical analysis" in the sense contemplated by pure mechanism.

The philosopher or scientist, then, who asserts that everything must be mechanically explicable or denies that some region of phenomena such as growth or reproduction can be explained mechanically, must be reminded that his statement is susceptible of at least five different interpretations, and may be invited to tell us which of the five correspondingly different propositions he is intending to assert or deny. In the meanwhile our best plan will be to ask ourselves the following questions: (1) Is there any reason to suppose that everything must be mechanically explicable in one of these senses, and, if so, in which? (2) Is there any reason to believe that some things are not mechanically explicable in any of these senses? As we cannot ask this question at random of everything under the sun, I will chiefly discuss chemistry and then glance at life. (3) Has the possibility of a mechanical explanation in one of these senses (and if so, in which of them?) anything to do with the possibility of treating a set of phenomena scientifically? Of course these questions are very closely connected with each other, but they are independent enough to be separately discussed. *E.g.*, if we found that there was no reason to believe that every set of phenomena could be explained mechanically, it would not necessarily follow that there was any reason to believe that any region of phenomena cannot be explained mechanically. For there may be no good reason to believe either of two propositions, one of which must be true and the other false. The proper order seems to be to consider the third question first. For, as we have seen, if it could be shown that the possibility of scientific treatment involves the possibility of mechanical explanation in some sense, this would provide a strong and perhaps justifiable presumption that, in this sense, everything must be mechanically explicable.

§ 13. What is needed for the possibility of scientific explanation is that all phenomena should obey *some* laws, and that these laws should not be too complex for us to be able to discover them. Apart from this latter condition it is quite unimportant what in particular the laws may be. Now we have already seen that mechanical explanation never means in any sense explanation that uses no laws except the laws of motion. Lagrange's equations always involve among the P's some special law of nature. It follows that the subjection of a region of phenomena to Lagrange's equations cannot be a *sufficient* condition of its being scientifically explicable, since the laws of nature involved in the P's might be too complex for us to unravel. Neither does it appear to me that subjection to Lagrange's equations is a *necessary* condition of scientific explanation. Roughly speaking, the laws of motion, as embodied in Lagrange's equations, assert that all motions, however caused, and in whatever system, are subject to certain formal laws. There are two factors then to be distinguished (1) the fact that all motions are subject to a single set of conditions, and (2) the fact that these conditions are summed up in the form of Lagrange's equations and in the form of the function T in them. Now it would certainly be difficult and perhaps impossible to have developed scientific explanation to any great length if the first condition had not been fulfilled. If, *e.g.*, movements due to impact obeyed quite different laws from movements due to electrical attraction and the movements of iron systems from those of golden ones, the world would perhaps have been complex beyond all hope of unravelling. Again, if these laws, though common to all movements, had been excessively complex instead of having the simple form and involving the simple functions of Lagrange's equations, scientific explanation would perhaps have been beyond our powers. We may admit then that probably the possibility of scientific explanation does depend on the existence of *some* general laws of motion, and on these laws being mathematically of a tolerably simple form.

There seems no necessity whatever that the form should be that embodied in Lagrange's equations. Really satisfactory scientific explanation must be in terms of measurable magnitudes and their correlations. Now, it is only geometrical magnitudes, lapses of time, and masses which can be directly measured satisfactorily. The measurement of time by clocks and of mass by weighing ultimately comes down to the measurement of geometrical magnitudes. This is obvious in the case of time; and, in weighing, what we actually observe is the levelness of the beam or the equality of the swings of the pointer of our balance under given conditions. Hence it is very important for the possibility of scientific explanation that at any rate something like metrical macroscopic mechanism should be true, though it need not take the precise form of Lagrange's equations. It would be bad enough if many substances had specific properties like magnetised iron, even though all the forces called into play by them obeyed the laws of mechanics; for I should then have to take endless precautions in weighing a substance A on a balance made of substance B. But the complications would become fearful if, beside this, the laws of statics and dynamics differed according to the nature and state of the substance that I was trying to weigh; for I should then have to work out the whole theory of the balance separately for each class of substance.

We see, then, that the relevance of metrical macroscopic mechanism to the possibility of scientific explanation involves nothing of profound metaphysical importance, but depends on two limitations of the human mind: (1) the limitation of our senses which prevents us from making very accurate measurements of anything but spatial magnitudes, and (2) the limitation of our understandings which prevents us from dealing with very complicated and non-analysable laws. The fortunate fact that this form of mechanism does seem to hold very widely, and perhaps universally, in spite of there being no trace of logical necessity that it should, may be presented to aspiring Gifford Lecturers, who will, doubtless, know what to do with it.

§ 14. Metrical macroscopic mechanism is then in some form probably a necessary condition of scientific explanation. Is it sufficient, or does science demand mechanism in some more rigid sense? Certainly it is not sufficient, for the special laws of nature, which, as we know, are always needed in any explanation in addition to the laws of motion, might be too complex in form for us to divine and unravel them. If this further demand is to lead us to one of the other kinds of mechanism at all, it will not let us stop merely at heterogeneous microscopic mechanism. For this only offers a special explanation of such macroscopic mechanism as is found, and puts no limitations on the form, the constants, or the independent variables of the special laws of nature.

Before we go any further it will be well to make quite clear the relation between microscopic explanations and mechanism in the more rigid sense which we have now to consider. Logically, there is no necessary connexion between the two, and the fact that the more rigid forms of mechanism are all microscopic is due to the constitution of the actual world, not to the laws of logic or mathematics. A microscopic explanation simply means that directly observable macroscopic phenomena and their laws are explained hypothetically as the results of systems of particles which are too small to be directly observed. It is thus obvious, at any rate, that a microscopic explanation does not imply mechanism in any sense: since the microscopic movements might not obey Lagrange's equations, and the microscopic special laws might not be of the type demanded by the more rigid forms of mechanism. Thus the atomic theory in chemistry is microscopic without being mechanical in the strict sense, for it assumes different kinds of microscopic particles, and it does not make any special assumption that the laws of their interaction are mechanically analysable. Similarly, there is nothing in the definition of the more rigid forms of mechanism to imply the necessity or even the possibility of a microscopic explanation of macroscopic phenomena. In practice all the more rigid forms of mechanism require a microscopic analysis of phenomena, but this is simply because they are palpably false if asserted to apply directly to all macroscopic phenomena. Macroscopically there are different kinds of matter with different specific properties, and capable of differences of state which can be perceived by the senses, and so homogeneous mechanism is certainly false if applied macroscopically to the whole universe. Again, macroscopically, there are laws of nature, which are not capable of a mechanical analysis, *e.g.*, the laws of electro-magnetics. Hence pure mechanism is certainly false if it be asserted to hold macroscopically of everything in the world. Thus the connexion between homogeneous or pure mechanism and microscopic explanation is that, if these forms of mechanism be true at all, they must be true microscopically, since they are certainly false macroscopically.

It is exceedingly important to be clear on this point; for, if we are not, mechanism may get the credit of the successes of microscopic analysis. Now there is no doubt that pure mechanism deserves a certain reflected credit from the success of the dynamical theory of gases, for that theory does, so far as I know, always assume that the action between molecules is either by impact or by central forces, and thus fulfils the main demands of pure mechanism. The only demand that is left unsatisfied is that of homogeneous mechanism, since the dynamical theory of gases does sssume ultimately different kinds of molecules. But I do not think that pure mechanism deserves to shine in the light reflected from the successes of the atomic theory in chemistry or of the electron theory. The atomic theory contradicts homogeneous mechanism and makes no assumption in favour of pure mechanism. It is useless to say that perhaps the differences between an atom of oxygen and one of hydrogen are merely differences between the number and configuration of two different groups of precisely similar particles, whose laws are mechanically analysable. Perhaps they are. But since chemistry has no need to make any assumption on the question one way or the other, the success of the atomic theory up to the present can have no tendency to support this view, and therefore can reflect no credit on homogeneous or pure mechanism. Again, the fundamental laws assumed on the electron theory are not of the nature of central forces, so that whatever credit the success of the theory may reflect upon homogeneous mechanism it reflects none upon pure mechanism.

I think, then, that we are justified in saying that the possibility of dealing scientifically with a given region of phenomena does not imply that it must be *known* to obey even microscopically the more rigid forms of mechanism. And if anyone says that its explicability must depend on its *actually* doing this, whether the fact be known or not, he is asserting a pure dogma, for which, from the nature of the case, there can be no evidence. What is necessary is that the ultimate laws of nature and kinds of matter should not be too numerous or too complicated. No doubt pure mechanism with its perfect qualitative homogeneity and its mechanically analysable ultimate laws represents the simplest conceivable assumption as to entities and their connexions. As such it has an elegant simplicity which we cannot too highly admire on æsthetic grounds. But we do ourselves an injustice if we think that we cannot get on with somewhat more complicated entities and laws than this; and we perhaps pay Nature too high a compliment by assuming it *must* be as logically beautiful as we can imagine that it *might* be. We may admit with Mr. Dombey that "Nature is a highly respectable institution," but we need not stake our faith in science as its being so terribly respectable as that mathematical Mrs. Grundy—pure mechanism—demands.

§ 15. An objection which I imagine might be made at this point is the following: No doubt the example of chemistry shows us that a large region of phenomena can be scientifically dealt with without making any assumption that it is even microscopically mechanistic. But genuine scientific explanation will not be content with dividing the world into regions with special entities and laws, and dealing with each region separately. It will want also to see and explain the connexion between the different regions of phenomena. And this is impossible, unless the more rigid forms of mechanism be true of the world, at least microscopically.

Now this really is an important assertion. In order to test it let us see how far a general scientific view of the world would remain possible if we accepted the milder kind of mechanism, viz., that something like Lagrange's equations hold for all movements, whether microscopic or macroscopic, but dropped the more rigid views. We are to suppose then that there may be ultimately different kinds of matter, and that the laws of nature may not be of the kind contemplated by homogeneous or by pure mechanism, and we are to see how much unification would remain possible. This is practically where we should stand if we accepted the present chemical elements as ultimate, and made no special assumptions as to the kind of laws governing the interactions of atoms, over and above the general condition that their movements were subject to Lagrange's equations. The macroscopic world would then consist of various arrangements of various kinds of ultimate atoms moving about in various ways. The nature of the laws between the ultimate kinds of atoms, whatever they may be, renders only certain kinds of grouping stable. These stable groups are compounds. Now what we should presumably find in such a case would be a hierarchy of laws rising from those which deal with the most abstract and general characteristics to those which only deal with characteristics peculiar to certain kinds of groups. At the bottom of the hierarchy would come the laws of motion which only refer to configuration, position, and motion, and do not by themselves suffice to determine any motion. These, we have assumed, are common to the atoms themselves and to all aggregates of them. But there might also be some particular laws of nature which only involve the same characteristic in all groups. For instance, all our groups will be aggregate in space with certain positions, motions, and configurations. Now there might quite well be some laws which only depended on these properties and did not depend on the particular kind of atoms which were contained in a group. Such laws would be less general than the laws of motion, but more general than any that depended on the particular nature of the atoms in a group. For all groups have *some* configuration; hence these laws would show themselves macroscopically in *some* form in any group of any order. Of course they might show themselves macroscopically in very different forms in different groups. Groups whose structure was similar might be expected to obey the same forms of these laws ; those whose structure was different, different forms ; but, in any case, the different forms would all depend on the one characteristic of structure in a uniform way, and thus the various laws could in theory be united in a single explanation. As a special case there might be laws which were precisely the same (and not merely different specifications of a single general law) for all possible groups. The latter, if such there be, would be the next most general laws in the hierachy. An example is the laws of constant and of multiple proportions in chemistry. The next set of laws in the hierarchy would be those which depended on structure in a uniform way, but took different forms for different kinds of structure. These would be the laws of what we call the physical as distinct from the chemical properties of bodies. An example would be the rotation of polarised light by compounds containing an asymmetrically linked atom. It is now clear why such laws will reappear in some form in every group of every order.

§ 16. Next would come those properties of first order groups which, while they may depend partly on the configuration of the groups and on the motions of their constituent atoms, also depend on the particular nature of the atoms in them. Such properties, and laws in terms of them, will not be able to be regarded as instances of a single law if, as we are supposing, the differences between the various kinds of atoms be irreducible. The characteristic behaviour of each chemical compound will then have to be studied separately. This, however, does not preclude all hope of further unification. *E.g.*, it is quite open to us to take a series of compounds of the same type, *i.e.*, of the same structure, to replace a given atom by others in turn, and to see if we can find any general laws. The laws connecting the properties of wholes with those of their parts will be far more complicated than those contemplated by pure mechanism with its mechanical analysis, for they will be a joint function of the structure of the compound, the nature of the atom under investigation, and the natures of the remaining atoms in the compound. But there is nothing theoretically hopeless in the task of trying to find general laws connecting the properties of compounds with those of their constituents when it is once clearly understood that this simply means changing one independent variable at a time in a function which involves several, and whose form is unknown to us and seeing what general laws emerge. Further unification than this in this particular direction will, of course, remain imposible if the differences between different atoms be ultimate.

Now it is obvious that there might be groups of higher order than the first with special laws of their own. Let us see what this means. There might be certain groups of compounds maintaining for a time, within certain limits, a characteristic structure and a characteristic proportion between the amount of the compounds in them. A very simple example would be a crystal with water of crystallisation; a very complex example would be an organised body. Now, in the first place, we should expect these groups to exhibit the laws of mechanics, physics, and chemistry. They would obey the laws of motion by our fundamental assumption. Again, they have configurations and motions, and so they will obey all those laws which only refer to such properties. They might conceivably exhibit the latter laws in new forms, because, *ex hypothesi*, we are dealing with a characteristic kind of structure; but still the new forms are not ultimately new laws but only special results of a common principle concerning the relation of structure to properties applied to a specially complex kind of structure.

§ 17. But it seems to me that they might also have properties and obey laws of their own which were not deducible from any we had learned by studying mechanics, physics, and chemistry. The properties of compounds, as we saw, are doubtless functions of their structure and the motion of their atoms, and of the peculiar properties of the atoms themselves. The laws of such compounds have been studied by isolating the compounds as much as possible from everything else, and so dealing as far as possible with pure cases where only the structure and components of the particular compound under investigation were likely to be relevant. Of course you never can in practice study one compound in the absence of all others, since there will always be others present in the vessels that you use in your experiments, in the room where you perform them, and so on. Still we can learn by varying the conditions that variations in a vast number of factors are irrelevant to the properties of a compound, and when we assert in chemistry that compound C has properties $p_1 \, . \, . \, p_n$, we must be understood to mean that it has these under all those conditions which are conveniently, but not with strict accuracy, summed up by the phrase "in isolation." Now in a group of the second order our compound is in special conjunction with other compounds, and it is in a different conjunction from any under which it was tested in the laboratory when we said it had such and such properties. This is especially true if the second order group be a living organism. Hence, all that we are justified in saying is that the properties and laws of a second order group will be functions of the structure of it, and of its subordinate groups, and of the special elements in all these groups. We are not at liberty to assume that each subordinate first order group will obey precisely the same laws as it did when we investigated it under quite different conditions. To assume this is to assume that the function connecting the structure and components of a second order group with each of its properties is analysable into a sum of functions each involving only the structure and components of one of its subordinate first order groups. This may be true, and it will be very nice if it is; but we have no right to assume it without investigation.

Perhaps I can make the point clearer to some people in the following way: Let A, B, C be compounds in the chemical sense, *i.e.*, first order groups. Let X be a second order group consisting of A, B, and C in certain definite proportions and positions, and with a definite structure in space. Let the atoms in A be $\alpha_1 \;.\;.\; \alpha_p$, those in B be $\beta_1 \;.\;.\; \beta_q$, those in C be $\gamma_1 \;.\;.\; \gamma_r$. Let us call the structures of A, B, and C, $\sigma_A$, $\sigma_B$, and $\sigma_C$, respectively, and the state of their surroundings $S_A$, $S_B$, and $S_C$, respectively. Then presumably the chemical behaviour of A is $f_A(\alpha_1 \;.\;.\; \alpha_p, \sigma_A, S_A)$, that of B is $f_B(\beta_1 \;.\;.\; \beta_q, \sigma_B, S_B)$, and that of C is $f_C(\gamma_1 \;.\;.\; \gamma_r, \sigma_C, S_C)$. What we know from ordinary chemistry is that over a very wide range of variation a change in the variables $S_A$, $S_B$, $S_C$ is irrelevant. Naturally, we never know that all possible changes in them will be irrelevant. Now take the behaviour of the second order complex X. In the first place, we can write this as $f_X$ (A, B, C, $\sigma_X$, $S_X$). Here $\sigma_X$ refers to the structure of the second order complex in terms of the first order complexes taken as elements, and by $S_X$ to the surroundings of the complex X taken as a whole. Now let us consider, *e.g.*, the behaviour of A in this complex. B and C, with their structures and components, $\sigma_X$, the structure of the complex, and $S_X$, the surroundings of the complex, will now all be lumped together as $S_A$, the surroundings of A in the function $f_A(\alpha_1 \ . \ . \ \alpha_p, \sigma_A, S_A)$, which expresses A's chemical behaviour. Now all that we know from chemistry is that the value of the latter function is unaltered or alters in certain known ways over a wide range of variation of $S_A$; we do not know that it will remain unaltered or will alter in any of these ways if $S_A$ be varied beyond these limits. Now in some second order complexes, such as living organisms, $S_A$ will be very different from any of the surroundings which have been tried in ordinary chemistry, and it will not, therefore, be surprising if A should exhibit new and unexpected properties. The same remarks of course apply to B and C. We should doubtless express this fact, if it proved to be a fact, verbally by saying that A, B, and C had *latent* chemical properties, which were always present, but only appeared in certain special surroundings. There is no objection to this mode of expression so long as we remember that it *is* purely verbal, and that it does not alter the fact that some part of the behaviour of the second order complex could be neither deduced nor suspected from a knowledge of the behaviour of its parts in other surroundings.

§ 18. We are now in a position to see what is the alternative to the more rigid kinds of mechanism, and how far scientific explanation is compatible with this alternative. If there be ultimately different kinds of matter, and if some of the laws of nature involve one irreducible property of matter and others another, the ideal of science must be a hierarchy of laws. Most general of all will be the laws of motion, which, however, are in a peculiar position, being only limitative, and not sufficing by themselves to determine any motion. Next come the special laws which depend solely on structure, and motion, and other properties common to all kinds of matter. These, in some form, will apply to all groups of all orders, for all will have structures, and they and their parts will be capable of moving. They may exhibit different forms in different groups, but all these forms will be special cases deducible from the particular structure of the group and the general laws. Then will come laws whose general form depends only on structure and motion, but which involve the particular properties of particular kinds of matter as constants. So far we have been at the level of mechanics, physics, and what is often called in physical text-books "properties of matter." Next come those laws of first order groups, which depend mainly on the nature of the constituents and the structure of the group. They will no doubt depend on the external surroundings too, but we may be able to see that, within a wide range of variation of these surroundings, the properties of first order groups remain constant or vary but slightly and in easily determinable ways. This is a really new stage; and the laws of this stage cannot be deduced or suspected from the laws in any lower stage in the hierarchy, for here we have new independent variables—the special natures of the constituent atoms—which, *ex hypothesi*, are irreducible and were not involved in the earlier laws.

This fact, however, does not prevent the discovery and correlation of laws at this stage. Our plan here is first to keep structure and surroundings constant and to vary constituents one by one; then to keep constituents and surroundings constant and vary structure, and so on. We may thus hope to obtain some general results that are not merely confined to one group, but connect the properties of groups with those of their constituents. Next, there may perfectly well be second order groups, some of whose properties depend mainly on their component first order groups, and the proportions and relative positions of these. Such groups will of course obey the laws of mechanics and physics in the sense defined. Even if they show physical properties which have not been met with elsewhere, we may suppose that this is simply due to their special structure, and that the new physical properties would be deducible from the general laws connecting structure with physical properties, and from a knowledge of the peculiar structure of the group. We may also suppose that many of the chemical properties of the constituent first order group will remain. But we are not justified in assuming this for all. The special association of first order groups to make a second order group involves a great change in the surroundings of all the first order groups. They are now in very different surroundings from those in which they were investigated by ordinary chemistry, and we have no right to assume that this change may not make a relevant difference in their behaviour. The position here is not parallel to that possible appearance of unexpected physical properties in second order complexes, which we have already mentioned. These were all, in theory at least, explicable as special cases of a general law. But the ultimately different nature of atoms, if true, prevents the laws of the various compounds being regarded as special cases of some one general law. For the differences of the atoms would be ultimate and qualitative; of what single variable, then, could atomic differences be regarded as specifications? These laws, therefore, of second order groups, would be really a fresh stage in our hierarchy. They would not be any the less *laws* for that. The only practical effect would be that second order complexes would have to be studied, for these properties at least, as a relatively new type of entity, and that we could not hope completely to explain their behaviour from the completest knowledge of their structure, their components, and the behaviour of the latter in other surroundings.

It is true, then, that unless homogeneous mechanism at least be accepted, science must take the form of a hierarchy of laws of which the higher and more specialised cannot be regarded as merely particular cases of the lower and more general. If homogeneous mechanism be accepted, we do have a unitary system of explanation holding at all levels; and all differences are due to differences of arrangement or motion in what is qualitatively alike. If in addition pure mechanism be accepted, the laws connecting structure and behaviour are of a peculiarly simple type and are everywhere the same. These seem to be the theoretical advantages of mechanism; it should now be perfectly clear that science *can* do without it, but that if it were true, there would be more unity in the world than if it were false. So far as I am aware, practically no scientist, whatever may be his theoretical predilections, actually works with the theory of pure mechanism (which indeed has begun to acquire a faintly mid-Victorian flavour like crinolines, back-partings, and the philosophy of Mr. Spencer). Even homogeneous mechanism is hardly used by anyone; the electron theory, which gets nearest to it, has its positive and its negative particles.

§ 19. It now remains for us to ask whether there be any reason to suppose that the more rigid forms of mechanism are true, and whether there be any reason to suppose that they are false. Well, there seems to be no strong reason to think that they are true. Scientific explanation, as we have seen, is by no means dependent on their truth, and, even if it were, this is no guarantee that they must be true. Is there any strong reason to think that they are false? Here we must distinguish. Macroscopically they are certainly false, and up to the stage to which microscopic explanation have so far been carried, there is no reason to think that they are true. But there are orders of microscopic explanation, as is shown by the molecules of the gas theory, the atoms of chemistry, and the electrons of physics. At none of these stages have we reached a rigidly mechanical explanation, but we cannot tell whether it might not be possible to go a stage further and analyse electrons into perfectly homogeneous particles obeying a simple law of central force. So long as this is possible, it is possible that mechanism in its most rigid form may be true of the material world.

But the material world, in this sense, is very far from being the whole known universe. I am not here referring to the fact that there are also such things as minds in the world; for I do not think that we need credit any mechanist who is intelligent enough to be worth our steel with the preposterous view that the laws of mind are and must be capable of mechanical explanation. I am referring to the macroscopic appearances: the colours, sounds, temperatures, etc., which we certainly perceive, and of whose existence, at any rate so long as we perceive them, we are necessarily more certain than of the existence of any hypothetical microscopic mechanism put forward to account for their order. These things are certainly real and they must be connected in some way with the supposed microscopic mechanism. Now there are, of course, dozens of alternative views which might be held as to the nature of these sensibilia and their connexion with the molecules, atoms, or other particles of microscopic mechanism. But these views, I think, reduce to one of four alternatives. (*a*) It may be held that colours, temperatures, etc., are properties of bodies and that one function of the microscopic mechanism of these bodies and of our body is to cause our minds from time to time to become aware of these properties. Or (*b*) it might be held that they are not properties of bodies, but are created under certain circumstances by the microscopic mechanism of our own and of other bodies and are then perceived by our minds. Or (*c*) it might be held that they are created as well as perceived by our minds when these are suitably stimulated by the microscopic mechanism of our bodies combined with, or set in motion by, that of other bodies. Lastly, (*d*) it might be held that the microscopic mechanism is a mathematical fiction and that the only existents are the sensibilia. On this view the microscopic mechanism and its laws are simply mathematical descriptions of sensibilia and their laws. Ordinary scientists appear to rest in a most unstable compromise between (*a*) and (*c*); (*a*) being held for primary qualities and (*c*) for secondaries. Between these views they oscillate as convenience or shocked common-sense may dictate, piling, as a rule, on top of these incoherences the additional absurdity that secondary qualities are nothing at all.

Now it is clear that any view such as (*b*), which makes the microscopic mechanisms *create* sensibilia, ascribes to it properties which are flagrantly incapable of mechanical explanation. With the view (*c*), the responsibility of creating sensibilia falls on the mind and the microscopic mechanism is left with the task of stimulating the mind to this act of creation and perhaps to the act of perceiving what it has created. In view (*a*) its function is still to stimulate the mind, but now only to perception and not to creation. But in either case the laws according to which the mechanism stimulates the mind, whether to creation or to perception, can hardly be mechanical laws in any intelligible sense. In fact any theory which counts the microscopic mechanism as real and not as a mere mathematical construction must recognise three different kinds of laws:—(i) those obeyed by matter in its mutual action, (ii) those according to which matter affects mind; and (iii) those of minds and their states. The last are admitted not to be mechanical; and the more we fly to microscopic explanations to patch up the obvious rifts in the mechanism of the macroscopic world the more important and numerous will laws of the second kind become. Now these laws cannot in any reasonable sense be called mechanical, since on one side we have the movements of matter, and on the other, at least a perception of the mind, and, on some views, also a creation by the mind.

§ 20. I will conclude with a few observations on a most hackneyed subject—the connexion between mechanism and teleology. I do not profess to understand precisely what people mean by teleology; sometimes they seem to mean an observable fact, and sometimes a possible explanation of this fact. Thus we hear of internal and external teleology, and they appear to be regarded as two divisions of a single notion. So far as I can see, this is a mistake. Internal teleology seems to be no more than the statement of the fact that some systems exhibit a harmony between their parts such that the mutual actions of these tend to preserve the system as a whole and to perform some definite function. This is an observable fact, not a theory. External teleology is a special hypothesis to account for this fact, viz., the theory that systems which exhibit internal teleology in this sense do so because their parts were originally ordered with a view to this result by someone who desired it and foresaw that it would follow. It will therefore make for clearness if we drop the adjectives internal and external altogether, and call the observable fact "teleology" pure and simple, and the hypothetical explanation "design" and not teleology at all.

Now, there are two kinds of objects in the world which exhibit teleology *par excellence*: these are machines and living bodies. In the case of machines, we know two things: (*a*) that the laws according to which their parts act are, roughly speaking, mechanical, and that the co-operation of the parts to the preservation of the whole or to the performance of any definite function depends on their shapes and arrangements; and (*b*) that the parts were shaped and arranged by someone who wanted a certain result and saw that this means would bring it about. In the case of living bodies, we are uncertain of two things: (*a*) we are uncertain whether in all their behaviour they obey mechanical laws in any of the more rigid senses defined in this paper; for, if there be any complexes of higher order such as we have discussed, living bodies seem to be the most likely candidates for the position. (*b*) We have no direct evidence that the parts of any existing organism were shaped and put together by a mind which foresaw and desired a system which should behave in the way in which these systems do in fact behave. We know that no human mind designed organisms, and that no human hands constructed them, and we can trace their immediate origin back to very small (I will not be so rash as to add, very simple) pieces of matter, connected however always, so far as we know, with other organisms like themselves. There is, moreover, an additional complication about organisms which is not present in machines. Certainly many, and perhaps all, organisms are connected with minds, and we do not know of any minds except in connexion with organisms.

Teleological systems are comparatively rare; no one contemplating the known laws of matter could have anticipated their occurrence, still less could he have anticipated that minds would turn up in connexion with some of them, would apparently develop *pari passu* with them, and would seemingly not occur except in this connexion. Hence, the existence of such systems is felt to stand in special need of explanation.

§ 21. For machines we seem to have a satisfactory explanation, so far as it goes, provided we admit that thoughts and volitions in minds can be part causes of movements in matter. Otherwise I do not see that the introduction of design helps us at all.* Now I confess that all the arguments produced by parallelists and epiphenomenalists have never seemed to me to have the smallest tendency to disprove the action of mind on matter in a certain definite sense. Observation and experiments do no doubt tend to prove that, once started, the changes in a living body obey the laws of motion and the conservation of energy. But both these principles are merely regulative; they do not by themselves determine either *that* a change will happen, or *when* it will happen if it does so at all. I see no reason to doubt that volitions may be part causes of this; and, unless they be so, explana-

* *Cf.* my paper on "Body and Mind," in the *Monist*, for a fuller treatment of this point.

tions by design are simply what our naval and military friends would term "eye-wash." It follows that the more rigidly mechanical we make organisms, *i.e.*, the more we make them resemble machines, the more we shall be forced to recognise a cause of change in matter which is not mechanically explicable in the rigid sense of the term, if we want to explain the origin of such systems.

At the same time the explanation of organisms by analogy to machines seems to me unsatisfactory for the following reason. Machines are explained by the actions of minds, but minds, so far as we know, only occur in connexion with organisms, and can only act on matter through their organisms. Thus to explain organisms by design looks suspiciously circular. Suppose that organisms are machines constructed, not proximately perhaps, but in their ultimate origins, by God, according to a design in his mind. Is God's mind connected with an organism or not? If not, it and its action on matter are so unlike anything that we know, that to compare organisms as machines constructed by God with watches as machines constructed by men, seems to provide no explanation. If so, who designed and constructed God's organism? It is hardly necessary to point out at this time of day, that if you make God a creator as well as a designer and mover of matter, all analogy with human action vanishes.

§ 22. I must note in passing what seems to me a bad but easily explicable confusion. People have noticed that most and perhaps all teleological systems have something to do with minds. In the case of machines the connexion is perfectly obvious, and provides, so far as it goes, an explanation of the origin of these systems. In the case of organisms the connexion is quite mysterious, and provides no explanation whatever of their origin.

But these differences are slurred over by muddle-headed people. They combine the two facts (*a*) that machines are designed by minds, but do not have minds, and (*b*) that organisms have minds, while it is not obvious what mind, if any, designs them; and say all organisms must have minds which control their action and development. But they shall be very little ones; we will call them entelechies; and perhaps no one will notice that there is anything wrong. This is the silliest of all explanations. There is no magic in mind as such which will explain teleology; a mind does not explain anything till it has wit enough to have designs and will enough to carry them out. If you want a mind that will construct its own organism, you may as well postulate God at once; if he cannot perform such a feat, it is scarcely likely that what is hidden from the wise and prudent will be revealed to entelechies.

§ 23. To conclude, Teleological behaviour is in itself no sign that anything but mechanical laws in the most rigid sense are operating. Nothing could be more teleological than a watch or a motor-car. Whatever laws be operating, the behaviour of a system depends on its structure and its components as well as on general laws. On any view the question of teleology and its explanation comes back to the question: How did this system come to have the peculiar structure and components which determine in accordance with general laws that it shall behave in this teleological way? In some cases the proximate answer is: Because a mind had certain designs and volitions, and was able by these to determine changes, first in its own organism and then in external matter. Such an explanation involves the view that some changes can be initiated by minds, and therefore the rejection of the more rigid, though not of the less rigid, forms of mechanism. In some cases this cannot be given even as a proximate answer, and in none is it an ultimate one, since it involves reference to an organism, which is itself a teleological system. So the ultimate question is: How do these particular material systems called organisms come to have their peculiar structure and components. So long as we explain their origin by laws, whether mechanical or otherwise, we merely referred back to earlier collocations of matter and so on, *ad infinitum.* The explanation in terms of a designing mind on the analogy of humanly constructed machines seems to involve a circle or to end in a mind so different from any that we know that the analogy fails, and it is hardly worth calling it a mind. The explanation by entelechies rests on a confusion and avoids no difficulty which is raised by the notion of an external designer. The problem, so far as I can see, is extra-scientific and quite insoluble, and it has no bearing on the question of mechanism and its alternatives.