The Mechanical View of Life*

By DR. J. GRAY, F.R.S.

ALTHOUGH it is impracticable to review, even in the most general terms, the progress of zoology as a whole, it is perhaps possible to take stock of one particular branch of the subject and to discuss its contributions towards problems which are of some general scientific and human interest. To an increasing extent, experimental zoologists are borrowing the weapons of physical chemistry, and possibly the time has come to consider the general point of view which underlies this type of attack on zoological problems. What is our conception of the essential nature of the living organism ? Do we believe that the activity of living matter and its potentiality for change can be expressed adequately in terms of physical units ? Do we incline to the belief that living animals have been evolved from inanimate matter ?

When, as biologists, we are asked to define our conception of the nature or origin of living matter, we must confine ourselves to views which are based on the facts of observation. The more accurate and extensive are our observational data, the more precise and the more satisfying will be our conclusions. The material with which the biologist must deal is of extreme diversity and complexity, and we naturally turn to the physical world for standards of measurement which will help us to arrange our material and to place our observations in a reasonable relationship to each other.

As I understand it, the age-long discussion between the mechanistic and vitalist schools of thought turns on how far we believe—on the basis of observation—that the facts of biology can be sorted out into a harmonious and satisfying series, without invoking conceptions which are found to be unnecessary in dealing with the facts of observation within the physical world. The centre of gravity of the problem shifts from time to time; but for many years two concepts appear to have influenced the discussion to a marked extent.

First, the synthesis of organic compounds from inorganic material suggests that there is no fundamental difference between the type of substances found in, or made by, living organisms and those which are found in, or formed by, purely inorganic systems. Secondly, the inferences drawn from the theories of organic and terrestrial evolution suggest that these two processes are fundamentally similar and involve the operation of fundamentally comparable forces. Not a few biologists have, in fact, maintained that living matter "owes its origin to causes similar in character to those which have been instrumental in producing all other forms of matter in the Universe" (Sharpey-Schafer, 1911). This was the view of Ray Lankester, who elaborated a series of intermediate steps whereby the first type of living organism was evolved from inanimate matter. I imagine that not a few modern zoologists would tolerate, if not actually accept, a similar view. From this it is often, but not always, implied that there is a fundamental continuity in the properties of all matter and that the only properties which a living organism can possess are those which can be defined in physico-chemical terms.

Opposition to such a view has not been wanting. In 1912, Sir Oliver Lodge replied to the views set forth by Sir Edward Sharpey-Schafer and stressed the existence in organisms of a principle, not easy to define, which is absent from the world of physics and chemistry. From time to time the battle has been renewed, and both biologists and physicists have taken an active part. It is a curious but pertinent fact that the most far-reaching mechanistic views have been and are being put forward by biologists; the more cautious views or the vitalistic views are held by physicists and chemists.

The exponents of the mechanistic view have been curiously indefinite in the exposition of their opinions. I confess that a study of the more popular works on physical science leads me no nearer to an understanding of those "causes" which, according to Sir Edward Sharpey-Schafer, "have been instrumental in producing all other forms of matter in the Universe" ; nor have such chemists as I have had the good fortune to meet been very familiar with the concept of "co-ordinated series of self-regulating and self-propagating chemical reactions", such as are described by Prof. Lancelot Hogben. According to Prof. Hogben, we may look for a complete solution to the nature of life within a mechanistic framework, fortified by the conviction that "The mechanist has a cheerful attitude to knowledge and refuses to capitulate to the fear of the Unknown : the vitalist, a sadder but not necessarily a wiser type, finds balm in the limitations and failures of human effort". So far as I have been able to observe, it is by no means obvious to note in the writings of Dr. J. S. Haldane, Prof. A. V. Hill or the Bishop of Birmingham those signs which are usually associated with a contemplation of the failures of the human intellect.

The mechanistic view of life seems to imply that if, at any instant of time, we were to know the precise distribution of the matter and energy which are present in an organism, we would have a complete understanding of all its properties. In other words, the behaviour of living systems can be completely defined in terms of laws which are fundamentally similar to those which describe the behaviour of inanimate systems. It is of interest to consider how far this conception is based on

* From the presidential address before Section D (Zoology) of the British Association, delivered at Leicester on September 7.

the results of observation, and how far it rests on a rather indefinite foundation of intuitive belief.

Let us look for a moment at the theory of the evolution of animate from inanimate matter. From a biological point of view it seems at first sight reasonable—it seems to be the natural conclusion to draw from the process of evolution which characterises the world of living organisms and the universe as a whole. The theory gives us a comfortable feeling of continuity of thought. Let us look at the position from a physical point of view. As a physical phenomenon it is undoubtedly *possible* for a living organism to have been evolved spontaneously from inanimate matter. It is also possible for a stone to leap spontaneously from the surface of the earth. These things are possible, but are they probable ?

It is *possible* for all the particles in a suspension of Indian ink to move simultaneously in one direction. It is also possible for all the molecules of a pebble to perform the same feat—but in view of the very large number of other possibilities, the *probability* of simultaneous coordinated movement is very, very small unless we are dealing with very small numbers of molecules. The degree of smallness can be judged by putting ten black and ten white balls into a box and drawing them out at random in lots of ten. The probability that we will draw ten white or ten black balls is five times in one million. If we increase the numbers and draw one hundred balls, the probability of drawing balls all of one colour is so small that we say that anybody who expected it to occur must be slightly demented. In practice we sum all this up by saying that so long as we are dealing with reasonably large numbers of molecules, the events which we observe are the most probable events, and we assume that the improbable events do not in fact occur. On this arbitrary but effective basis rest most, if not all, the laws of physics and chemistry which we apply to the study of living matter.

The organisation of the simplest living organism is clearly more complex than that of a stone or of a motor-car, and it carries out processes which are infinitely more complex than the sorting out of black from white particles. What, in fact, is the probability that any chance distribution of molecules should lead spontaneously to the dynamically active mechanism of the living organism ? Would any serious credence be given to the suggestion that a motor-car or even a footprint on the sands came spontaneously into existence without the intervention of directive forces ? Why, then, should we accept the spontaneous origin of living matter ? It is possible, but it is so improbable that, if considered as an observable phenomenon, in any other sphere of human thought it would be discarded as a figment of a deranged brain.

Left to himself, the chemist does not seriously consider the *spontaneous* origin of proteins from carbon dioxide, water and simple salts, nor does the physicist admit the spontaneous origin of organised machines. Biology itself provides not one shred of observational evidence to support the spontaneous origin of living matter in the world to-day, and yet not a few biologists are prepared to postulate the spontaneous origin of intermediate stages between the living and the inanimate worlds—to my mind, the spontaneous origins of these stages represent physical events which are so improbable that we cannot describe them in terms of 'laws' which only apply to events of an entirely different order of probability : if these intermediate stages actually occurred they must be classified as miracles, not as 'natural' events. We may be told that in past ages, events which are now very improbable were, in fact, of quite frequent occurrence. As men of science, we cannot accept this statement without some assurance as to what were the nature of the conditions which made the origin of life inevitable or even probable. The distribution of energy and of matter in past epochs may have been different ; but if such conditions produced the living organism, is it not strange that every attempt to reproduce them in the laboratory has completely failed ?

We can put the facts in another way. Within the physical world all systems appear to move towards the state of greatest probability, and the events which take place within a dynamic system are those which tend to destroy structure and not those which elaborate it. Is there any evidence which suggests that, within the physical world, a dynamic machine has spontaneously come into existence ? That such an event might happen is true, but has it, in point of fact, ever occurred under the observation of mankind ? Unless a positive answer can be given to this question, the belief in the spontaneous origin of living matter seems to be a negation of the principles which underlie scientific thought.

If we decline to accept the spontaneous origin of living from non-living matter, there is no particular reason why we should hope to express all the properties of an organism in terms of physical laws ; we might just as reasonably try to express physical phenomena in terms of biological conceptions. It seems more logical to accept the existence of matter in two states (the animate and the inanimate) as an initial assumption. Some properties are naturally common to matter in either state, and it is therefore legitimate to study the so-called physical properties of living matter ; but just as the fundamental concepts of physics are based on observational facts, so those of biology must conform to the same conditions. The physicist is not concerned with the origin of inanimate matter ; he is content to investigate it as he finds it. The biologist must likewise accept the living state as he finds it and not allow his science to rest on theories, however spectacular or attractive.

It is not easy to define life, but in practice most people will admit that matter in the living state possesses characteristics which are fundamentally different from those of inanimate objects. The central characteristic of living matter is its state of organised dynamic structure.

If we base our conception of the structure of protoplasm on the facts revealed by physical methods we must imagine a system, of great chemical complexity and of great potentiality for self-differentiation, within a fluid framework. So far as we know, specific chemical reactions are seldom restricted to localised regions of the cell; on the contrary, substantial mechanical disturbances can often be induced in the cytoplasm without affecting its biological or chemical activities. We can, however, look upon a mass of protoplasm as a very fine emulsion, the fundamental units of which are extremely small. If we assume that the properties of the system as a whole are essentially those of each individual unit, then we have no great difficulty in seeing how mass disturbances fail to affect the properties of the whole system. The displacement of the particles by diffusion, or other causes, throughout the mass of the system will not influence the fundamental properties of the cell or nucleus if these properties are essentially those of the small individual units.

The conception of the living cell as an aggregation of a very large number of fundamental units is in keeping with the fact that small fragments of egg cells retain some, at least, of the properties of the whole system. It is also in keeping with the very small dimensions (as in viruses) within which living phenomena have been observed. There is some evidence to support the view that single differentiated cells also represent aggregates of very small living units. For example, a suspension of the spermatozoa of the sea-urchin *Echinus* in sea-water, after a period of maximal activity, enters a phase of declining mechanical and respiratory activity. If we consider a single spermatozoon during this period of senescence, we find that the intensity of its mechanical and respiratory activity declines in a way which is characteristic of a population of units which differ from each other in their viability—the single cell behaves, in fact, as though it represented a large population of much smaller units of activity.

If we accept the view that the fundamental unit of life is extremely small, we can see that mechanical disturbances throughout a suspension of such units may induce no very far-reaching results. The conception of protoplasm as an emulsion of small vital units suspended in a fluid system is perhaps the most satisfactory picture we can derive from available facts; but it breaks down when we try to think of the mechanism whereby the cell differentiates itself as a whole—for here we must postulate some form of co-ordinated relationship between individual units. If, however, we shelve this difficulty for the moment and accept the general conception that 'vital' properties are associated with very small units of structure, a variable number of which are normally aggregated together as a suspension to form a single cell—it is obvious that we must exercise very great caution in the application of the statistical laws of physics in describing the properties of the fundamental units of life. The only legitimate laws are those applicable to the behaviour of single units of activity.

So far as I can form an opinion, such determinate laws have not yet been forthcoming. I am inclined to think that the intrinsic properties of living matter are as mysterious and as fundamental as the intrinsic properties of the molecule of a radioactive substance: when the physicist can tell us why one particular molecule explodes and why another goes on existing, I venture to think that we can begin to consider the possibility of defining the fundamental properties of living protoplasm in physical terms. At present, however, the physicist seems more inclined to define physical phenomena in terms of biological conceptions, for, according to M. Poincare and others, "modern physics is presenting us with apparent examples of spontaneity and foresight". For the moment, however, we must conclude that although physical methods have provided important facts concerning the state of living material, they have not as yet thrown much light on its fundamental properties.

After cell division has been in progress for a very short period, the cells which are formed by an egg of a sea-urchin begin to show a marked difference in arrangement from those of a polychæte worm. At the end of the third cleavage cycle, the cleavage pattern of a sea-urchin is seen to be orthoradial—the cleavage furrows between the upper quartet of cells lie immediately over the furrows of the lower quartet. In the polychæte, however, the arrangement is spiral, not orthoradial, for the furrows of the first quartet of smaller cells lie between the furrows of the basal quadrant cells. By experimental means we can force the sea-urchin egg to divide in a way characteristic of the worm. This is done by increasing the centripetal force which tends to press one cell against another, and we can show that the arrangement in the polychæte worm is that assumed by a system of spheres so arranged as to pack together within a minimum volume. The arrangement in the polychæte is essentially the same as in the egg of the mollusc or polyclad turbellarian. The classical interpretation of this fact associates the similarity in the cleavage pattern with a common phylogenetic relationship. From an experimental point of view one is inclined to a totally different view, namely, that the similarity in form is due to a similarity in the intensity of the mechanical forces operating on the cells. In the worm, mollusc, or turbellarian, the centripetal pressure acting on the cells is sufficient to force the cells to occupy a form in which a maximum volume is enclosed by a minimum area of surface. In the sea-urchin this is not the case. The pattern as such plays no essential rôle in determining the fate of the egg. A spirally cleaving sea-urchin egg develops normally; it does not develop into a worm or mollusc. The mechanical view is peculiarly attractive, but it has one serious objection.

When the dividing cells of a molluscan egg rotate so as to reduce their centripetal pressure to a minimum, a rotation to the left is as effective as a rotation to the right—and on each occasion one would expect an equal number of rotations to the left as to the right. In a few cases this seems to occur, but in others the left-handed or right-handed pattern appears to be due to determinate and not to chance forces—for at any given stage of cleavage all the eggs show a rotation to the right or to the left. That this phenomenon is correlated with mechanical asymmetry is quite probable, and it may be that the nature of this asymmetry will eventually be observed. In the meantime, however, we seem to be faced with the fact that a mechanical condition which is satisfiable in either one of two ways, is, in fact, only effected in one way. Does it not look as though a disturbance has occurred in the probability values of the system ? It is as though we were presented with a bag of black and white balls—and each time we pick out the black balls and reject the whites. Before we attribute a determinate behaviour to the cleaving egg cell we must, of course, make certain that the chance of left- or right-handed cleavage is mechanically of equal probability. Up to the present we can only say that no mechanical difference is apparent—and in the absence of such definite evidence we are free to interpret the facts either as evidence of a deficiency in our knowledge of the mechanics of the system, or to the possibility that there exists in the egg a potentiality which makes certain events more probable than they could be in inanimate systems. One is tempted to suggest that the cells of a molluscan egg turn one way or another for intrinsic reasons : an event starts inside the cells—quite independent of any external influence—just as in the exploding molecule of a radio-active substance. In other words, the cell has an individuality of its own—which is free from the limitations of statistical laws. The field of cell cleavage is full of possibilities for future inquiry, and would well repay more intensive study.

The only laws which physics has provided for an analysis of biological phenomena rest on a statistical basis ; they only apply to systems which contain a large number of participating units and only describe natural phenomena in terms of probability and not of absolute truth. If we accept these laws as a means of describing the behaviour or the structure of an organism, we must accept the conventions attached to the laws, and agree to ignore such events as are improbable although they may conceivably occur. From this point of view, the spontaneous origin of living from inanimate matter must be regarded as a highly improbable event, and as such can be assumed not to have occurred. Similarly, the development of an organism from so-called undifferentiated protoplasm involves processes which are entirely without parallel in inanimate Nature.

So long as this state of our knowledge persists, it is dangerous to assume that the statistical laws of physics can satisfactorily describe all biological events. Our knowledge of the physical and biological properties of living matter suggests that the fundamental unit of structure is extremely small, and that it contains potentialities for change which are unique in the universe. These properties we must accept as fundamental axioms of our science which may or may not prove (in the future) to have their parallel in the physical world. It may seem presumptuous for the biologist to set up postulates peculiar to his own sphere ; it would be more fitting perhaps for him to accept, with medieval humility, the assumptions of his physical brethren. One wonders, however, at times whether the concepts of intrinsic organisation and of emergent evolution are entirely absent from modern physics. Even if this is not the case, we can fortify ourselves by the knowledge that physics has from time to time changed its fundamental assumptions with advantage to itself and to the world.

Those biologists who are inclined to accept the views I have ventured to put forward may be encouraged by a recent remark of Prof. Niels Bohr (NATURE, 131, 458, April 1, 1933) : "The existence of life must be considered as an elementary fact that cannot be explained, but must be taken as a starting-point in biology, in a similar way as the quantum of action, which appears as an irrational element from the point of view of classical mechanical physics, taken together with the existence of the elementary particles, forms the foundation of atomic physics."

Not infrequently the physiologist can restrict his interest to the physical properties of isolated organs—the origin of which does not concern him. The zoologist, on the other hand, knows that the beautifully adapted mechanism known as an 'organ' was evolved from a system unlike itself and may, in turn, initiate something new. For this reason, he cannot afford to forget what may be called the 'intrinsic potentiality of the living organism'. He may or may not be able to use this conception as a guide to more adequate observations, but it should be constantly in his mind.

Experimental zoology can be divided into two types of study : (1) the investigation of the physical and the chemical properties of living organisms ; (2) a study of the intrinsic potentialities of living matter, revealing as it does a co-ordination of events which is without inanimate parallel. In the first type of work we must use each new weapon which the physicist can give us. In the second type of work, however, biology must be the mistress and not the servant of physics or of chemistry—she must make her own foundations, and build on them fearlessly, prepared to change her views, if need be, but not prepared to force the wine of life into bottles which were designed for use in the simpler and less intoxicating fields of chemical science.