THE PHYSICAL FOUNDATION OF BIOLOGY

An Analytical Study

Walter M. Elsasser

The Physical Foundation of Biology

AN ANALYTICAL STUDY

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PREFACE

Mais il faut parier. Cela n'est pas volontaire: vous êtes embarqué.

PASCAL

ONCE upon a time Disraeli remarked on two qualifications indispensable in a statesman. In the first place he must have a characteristic without which he cannot survive: he must be prudent. But another characteristic is no less important: he must be imprudent. Perhaps this description is true of anyone whose work confronts him often, or even now and then, with the unforeseeable; and research is surely such an occupation. In writing the present volume the author has tried to have consistent recourse to prudence after having been imprudent enough to become engaged in an analytical study on a subject known as one of the controversial battlegrounds of scientific philosophy. It is true that we have tried to resist the temptation to sacrifice clarity of expression altogether to academic reserve; we have cut down the use of the 'if', 'perhaps', 'conceivably', and so on, when feasible. We are also keeping certain technicalities to a minimum since the topic is of interest to a variety of specialists with almost any kind of background. The fact that we prefer reasonably plain language should not, however, lead one to think that our aim is popularization. The reader will have some difficulties unless he possesses a moderate familiarity with the more quantitative forms of scientific reasoning; but with this proviso we have tried to avoid a style that would be too troublesome to the practitioner of the less mathematical sciences.

A writer tackling this subject, unless he looks at it from the viewpoint of the pure philosopher, must be a physicist as well as a biologist. I should here introduce myself as being a theoretical physicist fairly familiar by long practice with various aspects of this specialty. I am not in any sense of the word a biologist. This has led me to spend a vast amount of thought and effort in order not to violate either the facts or the spirit of experimental biology, in this exposition. I shall not try to make excuses, but shall leave it to the reader to see how well I have succeeded. The main effort of the book is not to rehash old paradoxes, but to pose in clear and distinct terms a problem; and we hope it to be true, here as elsewhere, that establishing the precise definition of a scientific problem goes some way toward its solution. Also, the job is essentially one for a mathematical physicist, because the problem of the foundations of biology (as distinct from the directly observed phenomena) is highly analytical, bordering moreover on epistemology. My philosophical attitude in this inquiry is positivistic and operational to the limits of the possible. It is not too much to say that every sentence (with the exception of a few brief excursions into philosophic generalization) has been carefully surveyed so as to be operational, that is to have a definite meaning in terms of laboratory or other observational procedures. If this should not invariably be found true we hope that the defaults will be taken as involuntary omissions.

Physics, in the course of the last few generations, has developed a remarkable method to modify established philosophical concepts, which has not perhaps been posited from first principles, but has in fact been most successfully applied in the development of modern theoretical thought. It consists in identifying certain phenomena whose existence is required by current concepts but which seem always to escape observation. By eliminating these 'unobservables' from the analysis one may arrive at a satisfactory formal description of the observed phenomena, but only after a revision of the more conventional conceptual schemes originally involved. The idea underlying this essay is that an essentially equivalent method can be successfully applied to biology. This makes the inquiry of these pages somewhat philosophical, but only in the sense in which the term 'natural philosophy' is still used on occasion to designate physics, that is, indicating inductive generalization on an analytical basis, not to emphasize the subjective element.

The approach taken here was made possible by a threefold development in theoretical science that has occurred in recent years: our background is taken from the theory of automata (often designated as computers) the theory of information (mainly developed in communication engineering) and the theory of microscopic measurement in the atomic and molecular domain (based largely on quantum mechanics). These are described at various places, the presentation taking up about two thirds of this book. Interspersed among this material are stretches dealing with applications to and conclusions about biological theory. In dealing with such a theory nobody could possibly claim to have novel ideas or to be original, seeing that it has been the center of inquiry of many wise men over a long time; and we are intensely aware that they have not come to a unified conclusion. The justification for reassessing an aged problem can *only* be found in the recent development of novel techniques and novel conceptions which can be brought to bear on the problem. We hope to show that many interesting questions can be raised in biology on the basis of our increased understanding of the above-named three subjects. Since this essay involves of necessity a conglomeration of heterogeneous topics, nobody could be more aware of its shortcomings and its superficiality than the author, and we can only ask the indulgence of our colleagues, be they physicists, chemists, or biologists. We shall be satisfied if some old questions of theory can be reopened with more up-to-date equipment, and we hope that our imprudence in so doing will be charitably received.

The author is indebted to a number of colleagues and friends for stimulating and critical advice. These are named in footnotes to the text at appropriate places. Our special thanks are due to Miss Margaret Culbertson for a thorough revision of the manuscript for style.

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INTRODUCTION

We intend to analyze from a present-day viewpoint a very ancient problem: the relationship of biology to physics. There are two main schools of thought, the mechanists and the non-mechanists. On closer sight it appears that the latter group has no coherence, but represents a variety of divergent philosophies, differing from each other as much as any of them differs from the purely mechanistic view.

The mechanical philosophy of life goes back to classical antiquity, but it is perhaps fair to say that such thinkers as Democritus and Epicurus were interested in a scientific approach to natural phenomena generally speaking, but were not yet far enough advanced to clearly perceive the specific issue with which we shall be concerned here. The same is no doubt true of the thinkers of the Renaissance and even the succeeding period. When scientific knowledge began to increase substantially, the problem came into sharper focus. Usually Descartes is credited with being the first to have proffered fairly clearcut ideas. Descartes' view on the problem is interesting. He tries to adhere to a strictly mechanistic interpretation of bodily functions in terms of chemistry and hydraulics. This was for him largely a matter of method, since the physiological knowledge of his time did not permit him to carry this view into much detail. Let us say, on deliberately oversimplifying the historical situation, that Descartes considered the organism as a machine so far as the knowledge of his age allowed. On the other hand, he admitted the existence of a soul or spirit in man as intrinsically different from matter, and therefore not properly situated in space. But this soul must be able to act upon the body. Descartes' view is that this action is concentrated in a small region; he hypothesizes this region to be in the pineal gland. Thus the soul exerts a control over the body which is transmitted from the center of control to the other organs and extremities by purely physical messengers, in modern language, by hormones and nerve impulses. This view is clearly a reflection of the traditional, dualistic Christian doctrine. Descartes, however, did not come to it by mere expediency; he remained a devout Catholic all his life. Now since according to such doctrine humans partake of a soul but animals do not, it appears that with respect to animal physiology Descartes was a mechanist, the rigor of his mechanistic determinism being limited only by the narrowness of the physical and chemical knowledge he could muster and by the inadequate elaboration of biological problems possible at his time.

Going from the seventeenth to the eighteenth century we encounter some intriguing developments in biological theorizing pertinent to our problem. We shall pass over these for the moment, planning to revert to them shortly. Next, in the nineteenth century we find the great unfolding of experimental physiology. The dominance of large numbers of highly skilled specialists armed with the then well advanced tools of physics and chemistry began to assert itself. A mechanistic view of biology became extremely widespread. Still there were some reservations. Perhaps a typical representative of this period is the German physiologist Dubois-Reymond who about the middle of the century complained, not perhaps without some affectation, that 'the human mind will never be able to penetrate the riddles of the universe'. Specifically he meant, and said, that no matter how far science may go in explaining the workings of the cell and the organism in terms of pure physics and chemistry, it would not be able to encompass the phenomenon of consciousness. Consciousness implies introspection and therefore belongs, according to Dubois-Reymond, to a category of phenomena of a quality different from the substances and forces, atoms and molecules of the physicist and chemist. This gap, he claims, can never be bridged. His attitude clearly implies that anything which is 'below' this gap is some day likely to find a purely mechanistic explanation.

It is remarkable how little this point of view, apparently rather widely held at the time, has gone beyond that represented by Descartes two centuries earlier. We might call this the philosophy of the Little Difference. Pragmatically it is indistinguishable from pure mechanistic thought so long as we remain below the level where psychophysical relationships become important. While the Little Difference is all-important of course from a philosophical point of view, it is little indeed in the daily work of the practical investigator.

There is also the philosophy of the Big Difference between an organism and a machine. Its most familiar representatives are the vitalists of the late nineteenth century. Their ideas have perhaps found their most pungent expression in the writings of Hans Driesch who started his career as an experimental embryologist, but later became a professor of philosophy, devoting himself to the elaboration of his vitalistic doctrines. According to this group of thinkers there exists in the living being a non-physical, organizing, purposeful principle which Driesch calls 'entelechy'.

On closer view this idea appears rather complex. In the first place there is the notion of purpose. In the second place one might justifiably ask whether and to what extent the vitalist position excludes the application to biology of the universal language of physical science, which is mathematics. Finally there arises the difficult problem of how one can justify the coexistence of two sets of natural law, one applying to inorganic matter and another apparently more extensive one to the organism. We shall briefly discuss these three questions in turn, not to analyze the vitalist's views, but mainly to propound some general comments that are useful in our further study.

First as to 'purpose'. Here, the scientific developments of recent years have dealt the vitalistic scheme a severe if not fatal blow. If one looks into one of the older, nineteenth-century textbooks of biology, he finds in the introductory chapter a tabulation of specific properties which are supposed to set an organism apart from an inanimate device. Among these one finds the ability of the organism to respond to an external stimulus in a manner which is not proportional, or quantitatively related to, the energy of the stimulus; the ability to perform purposeful actions, as in defense, gathering of food, and so forth; the capacity for growth and self-duplication. Now electronic devices, 'robots', do perform many of these functions in present-day technology. The characteristic and purely physical property which enables them to do this is the presence of feedback, and we shall discuss at length these mechanistic devices and their potentialities. (The term mechanistic is used here to describe any device which operates according to physical causality, whether it be mechanical in the strict sense of the word, or electrical, or even chemical as in organisms.) In order to emphasize the analogous operation of machines and organisms Norbert Wiener has introduced the term 'cybernetics', meant to designate a generalized science of feedback and control. At first sight the properties of growth and self-duplication seem to be foreign to this concept. Closer analysis shows, however, that these processes also can be reduced to, and understood in terms of, feedback and other properties of mechanistic robots.

It is clear that with respect to the notion of 'purpose' the vitalistic approach has been a resounding failure. Its proponents did not have the analytical insight to realize how far mechanistic principles can be pushed; for this they can hardly be blamed. It is true that they use the term purpose also in a different sense. Dealing with embryology they say for instance that the whole complicated structure of the eye is assembled before birth, at a time when it is completely useless functionally. Thus it is being built for the 'purpose' of later use. This latter use of the term purpose is logically different from, and more subtle than, the one mentioned before. It cannot be refuted quite so easily and we must delay its discussion until later.

Let us now look into the second question raised above, namely, the limitations of the mathematical method of description. Here we do not get a very clear answer from the vitalists, which is not surprising since they are recruited almost exclusively from the ranks of empirical biologists rather than from mathematicians. But the question is one which deserves close attention, especially since it is connected with many prejudices in the popular mind. People often question whether complicated concepts can be expressed in mathematical terms. Nobody will doubt that the concept of a triangle can be expressed mathematically, although there are triangles of all sizes and of many different shapes. As the mathematician says, those properties which characterize the concept of a triangle are 'invariants' relative to those other properties that change from one triangle to the next. How about a more complicated concept, say that of a horse? Assume that we have a horse on a stage with a curtain behind it, so as to eliminate all extraneous matter, and that we get a full-size picture of it by means of a television camera. Any five-year-old child sitting in front of a television screen when he sees this picture will immediately respond with the word 'horse'. But the picture of the horse has been transmitted in the form of an electrical signal, and the latter can be expressed in purely quantitative terms, for instance as a sequence of numbers that give the intensity of the signal every millimicrosecond. We have here the seed of what we shall call the principle of the transformability of information: all scientifically relevant information can be changed into a variety of forms, one of them being certainly a sequence of numbers. The implications of this principle will be discussed at length in a later chapter. To return to the horse, there are of course fat and slender, large and small horses, draft horses and ponies. Again, as in the case of the triangle, there are certain properties that are invariant. In the case of the picture of the horse on a screen for instance, this would no doubt be a matter of bodily proportions. In biology a horse is defined genetically and not pictorially, but this does not alter the basic nature of the problem we have in mind, that of concept formation. The difficulty of defining a horse already has a simple analog in the case of the triangle: shall we consider a straight line going through three points as a degenerate form of a triangle or shall we exclude this case? These are problems of definition, of concept formation, but they do not at all preclude the use of quantitative methods; only that these methods will in biology be far more complicated than those used in the elementary branches of mathematics. What we claim is that with some effort all scientific description can be expressed in quantitative form provided certain statistical elements are included. This is exactly what we intend to express by the principle of the transformability of information. This principle is closely related to the positivistic and operational approach to science. The latter has become too well known to need detailed discussion here; we may point to the work of Bridgman* as a concise expression of this philosophical attitude.

Now mathematical relationships can be expressed either in terms of numbers or as mere ordering relations (such as $a > b$). If the data are given as numbers we can change them in various ways into different forms. Thus in the above example of a pictorial representation of a horse, what is given originally is the light intensity on a screen, that is, a function $f(x,y)$ of two variables. By means of the 'scanning' process to be discussed in more detail later this is converted into a television signal which is essentially equivalent to a sequence of numbers. It is true that order is not a numerical concept, although the two are closely related, as mathematics shows. By means of the auxiliary device of a metric (a 'grid') an ordering relation, $a > b$, may be replaced by a distance between a and b which is a number. This might require additional, perhaps somewhat involved assumptions, but we cannot enter into the details of mathematical theory without going beyond the confines of our subject. It must suffice to say that *in practice* scientific data can be expressed in numerical form. One may rightly ask how such a concept as 'oxygen' can be expressed in purely numerical terms. According to the positivist, oxygen is merely an abbreviation for a reproducible set of experiences in the laboratory; with sufficient effort, albeit often a major effort, these experiences also could be expressed in quantitative form. One does not usually do this because conventional concepts are such very convenient tools of description. They take account of the fact that most objects of experience occur in classes, that is repetitively (e.g. chemical elements and compounds, biological

*For instance: P. W. Bridgman, *The Logic of Modern Physics*, Macmillan, New York, 1927; *The Nature of Physical Theory*, Dover, New York, 1936; *The Nature of Some Physical Concepts*, Philosophical Library, New York, 1952. The writer wishes to use this opportunity to express his gratitude to Professor Bridgman for reading the manuscript of this book and making a number of valuable suggestions.

species) the concept being a code name for an often very cumbersome operational description.

Ultimately of course all such description must be referred to some basic body of immediate experience. There must be a 'dictionary' defining the basic terms of our scientific universe of discourse. But fortunately this dictionary can in principle be rather brief and the basic terms of reference rather simple and little subject to controversy. The activity of the scientist can ultimately be referred to the basic concepts of classical physics, measurements of length, time, mass, and some others. It is a matter of convenience just how long one would want to make this dictionary to which quantitative description refers. In practice it would have to be fairly lengthy if description is not to be extravagantly cumbersome; thus one would have to include a number of electrical terms as well as much of chemistry, the elements and simpler organic compounds.

Perhaps we are here rather extreme and a bit pedantic in stressing the possibility of making all scientific description purely quantitative. We must justify this by the dangerous nature of our subject, which lends itself so readily to mere verbalizations and has so many metaphysical connotations. The vitalists cannot be quite absolved from the accusation of having been lax in this respect. We can avoid these pitfalls and achieve whatever progress there may be, only by adhering as closely as we can to quantitative formulations. Thus consider the following statement: 'The whole is more than its parts.' What does this mean in precise terms? Assume we have a set of elements, a, b, c, d, e, f, . . . This set is made into a structure, that is a 'whole', by postulating certain relationships between the elements. To take a rather simple example, assume that the elements are numbers and that $a + b = c$, $a + d = e$, $a \cdot b = f$. Now if we consider a smaller structure, retaining only the elements a, b, c, and dropping all the others, we lose the last two of the relationships written down; the original structure has been reduced to a simpler and less complex structure by merely reducing the number of elements. Without such an effort at being quantitative and operational we could not hope to tackle the subject before us.

We come now to the third point raised above. How can one claim that there are laws of nature different from the laws of physics and not contained in them? It is important to distinguish clearly between this question and the preceding one. Previously we asked whether there can be a description of nature other than in quantitative terms and we concluded that this is inadmissible; to concede this would be tantamount to a declaration of bankruptcy of the scientific method. But this is radically different from the question of whether the agencies and principles so far discovered in the physicist's and chemist's laboratory are sufficient for a quantitative description of biological experience. The vitalists claimed of course that physics is inadequate. But in the process of making this claim they committed a triple mistake. In the first place they identified their non-physical principle largely with purposeful action, and in this they were shown wrong by a succeeding generation which, on constructing elaborate robots, demonstrated how much of the notion of purpose can be expressed by the functioning of purely mechanistic devices. In the second place they failed to emphasize the quantitative character of all scientific description, in the absence of which metaphysical elements are bound to creep in. In the third place they failed to enter into the logical analysis which becomes indispensable when one claims that physics and chemistry are inadequate. One cannot simply say that the organism is 'physics plus' or 'chemistry plus'; this is all too primitive an approach. If one abandons pure physics and chemistry one must do more than introduce some heterogeneous principle; one must try to explain why it is that physics and chemistry should fail and not simply postulate that they fail.

There have been many distinguished biologists at various times who believed that a mechanistic view of life is inadequate; that biological phenomena are not simply specializations of physics and chemistry. As Nordenskjöld* in his History of Biology points out, these men thought that a biological theory must be *broader* than the conventional physics and chemistry; it must be constructed so that there are no seams where physical and biological principles are patched together; it must encompass a concept of matter broader than that with which the physicist is wont to operate in his laboratory. *The first step in this direction must be a thorough analysis of the assumptions implicit in the usual applications of the methods of physical science to biology.* From such a viewpoint some of the claims of the vitalists may eventually be vindicated, although perhaps in a context and with a meaning radically different from what they had expected.

Now an analysis of fundamental concepts and operational procedures has preceded many, if not all, of the major advances in physics. As is well known, Ernst Mach's analysis of the fundamental notions of

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*Erik Nordenskjöld, *The History of Biology*, Tudor Publishing Company, New York, 1946.

mechanics laid the foundations on which the theory of relativity was later built. In the domain of atomic physics the development has been rather gradual, but the introduction of quantum mechanics was associated with a thorough analysis of the concept of measurement in the microscopic domain; without this quantum mechanics would make very little sense. Speaking rather schematically, the development of theoretical physics has led one to re-assess and re-evaluate our fundamental concepts and operational procedures in the realm of the very large (relativity, cosmology) and in the realm of the very small (quantum physics, elementary particles). Now it would seem at first sight that if we fully understand the physics of atoms and molecules we can also understand the physics of more complex systems built up from them. If, with enough quantum mechanics, we can compute the modes of behavior of a benzene ring, why should we not be able, in principle at least, to compute the behavior of a larger system, say a living cell? Again, what is most characteristic of a cell is not its small size; the cell is much larger than the objects studied by the atomic physicist. The prime characteristic of the cell is its immense structural complexity. Such complexity may seem a purely additive matter; one might think that it could be overcome, at least in principle, by sufficiently powerful measuring apparatus coupled to big computing machinery. But could not this view be a fallacy? *It is the aim of these pages to make clear that structural complexity requires more than a purely additive treatment.* Quantum mechanics and, more recently, information theory have acquainted us with fundamental limitations applying to measurements in the microscopic* realm. It appears that these limitations are cumulative in such a way that the more obvious methods of analyzing highly complex structures in terms of simpler constituents break down. The limitations of measurement which have become familiar from quantum mechanics, together with limitations to be discussed later which have been studied more recently in information theory, are such that the structural determination of a complex system is far less than one would expect at first sight. Thus we encounter here a form of indeterminacy. As the historical example of

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*Throughout this book the terms microscopic and macroscopic are used in the sense given them by the physicist, and not in the more conventional sense familiar from biology. Thus, microscopic phenomena are those on an atomic and molecular scale characterized by the individual electrical charges, chemical bonds, and quantum effects which appear at that level; the term macroscopic then refers to the continuum's approximation representing matter in bulk which is characteristic of classical physics and chemistry.

quantum mechanics shows so clearly, such indeterminacy can be the occasion of a thorough revision of the concept of natural law. That this may apply to biology as well as to atomic physics was first pointed out nearly 25 years ago by Niels Bohr*. This line of attack was not pursued forcefully thereafter, but it will play a fundamental role in these pages. We are saying, then, that out of the study of these indeterminacies a new concept of natural law can emerge which is a suitable basis for a positivistic biology. We are also saying that to the two realms of physical science mentioned above, in which the labors of the past two generations have effected a thorough logical analysis of procedure and concept, there must in the future be added *a third realm in which such analysis is required, namely that of systems with great structural complexity*. We use for this subject the name biophysics since all its applications so far are in biology, although it is in a sense physics (just as biochemistry is chemistry, applied to biology). The recently developed sciences of computer design and information theory constitute a beginning in the necessary direction. Although they are not in themselves concerned with the philosophical problems that arise in connection with complicated structures of molecular physics, such as are found in the living cell, they do form a basis for such studies.

For a good many years the author has had occasion to study physical problems involving indeterminacy and lack of predictability. They appear not only in atomic physics but also in such field as fluid mechanics. Now the natural thing to do is to try to simplify the problem posed here by first avoiding the organism proper and dealing with suitable models of purely physical systems such as fluids. But time and again we have found ourselves marooned in that disconcerting sterility which seems to be the curse of all purely methodological investigations. There remained nothing, at last, but to dive into biology proper. This is more than a little dangerous because what we seek are generalizations, and the only person qualified to generalize is one who has acquired a vast background of concrete experience in the particular subject. We finally tried to solve this quandary by picking out of the mountain of literature dealing with the fundamentals of biology a simple principle which we propose to discuss in these pages. The principle must not only be simple and general, it must also be well anchored in the history of biology. We can hardly expect that a

*N. Bohr, *Atomic Theory and the Description of Nature*, Cambridge University Press, 1934; see also *Nature* (Lond.), 131, 421, 457 (1933).