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BIOLOGICAL PRINCIPLES

A Critical Study

J H WOODGER

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1690

That which I have here to do is, to inquire whether, if it be the readiest way to knowledge to begin with general maxims and build upon them, it be yet a safe way to take the principles which are laid down in any other science as unquestionable truths; and so receive them without examination, and adhere to them without suffering to be doubted of, because mathematicians have been so happy or so fair to use none but self-evident and undeniable. If this be so, I know not what may not pass for truth in morality, what may not be introduced and proved in natural philosophy.'

Locke, ESSAY CONCERNING HUMAN UNDERSTANDING,
Bk. IV., xii, 4

1843

In the progress of science from its earliest to its more difficult problems, each great step in advance has usually had either as its precursor, or as its accompaniment and necessary condition, a corresponding improvement in the notions and principles of logic received among the most advanced thinkers. And if several of the most difficult sciences are still in so defective a state; if not only so little is proved, but disputation has not terminated even about the little which seemed to be so; the reason perhaps is, that men's logical notions have not yet acquired the degree of extension or of accuracy, requisite for the estimation of the evidence proper to those particular departments of knowledge.'

J. S. Mill, LOGIC, Intro. 6

1926

The progress of biology and psychology has probably been checked by the uncritical assumption of half-truths. If science is not to degenerate into a medley of ad hoc *hypotheses, it must become philosophical and must enter into a thorough criticism of its own foundations.'*

A. N. Whitehead, SCIENCE AND THE MODERN WORLD, p. 25

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CONTENTS

PART I

THE DATA OF NATURAL SCIENCE AND THE PRINCIPLES OF SYSTEMATIZATION OF SCIENTIFIC KNOWLEDGE

CHAPTER I

Phenomenalism and Kindred Doctrines

CHAPTER II

An Alternative to Phenomenalism

CHAPTER III

The Categories of Substance and Causation and their Use in Modern Natural Science

CHAPTER IV

Demands, Postulates, and Subjective Factors in Knowledge

PART II

PROBLEMS OF BIOLOGICAL KNOWLEDGE

CHAPTER V

The Antithesis between Vitalism and Mechanism

CHAPTER VI

The Theory of Biological Explanation

A—General:

CHAPTER VII

THE ANTITHESIS BETWEEN STRUCTURE AND FUNCTION

CHAPTER VIII

THE ANTITHESIS BETWEEN ORGANISM AND ENVIRONMENT

CHAPTER IX

THE ANTITHESIS BETWEEN PREFORMATION AND EPIGENESIS

CHAPTER X

THE ANTITHESIS BETWEEN TELEOLOGY AND CAUSATION

CHAPTER XI

THE ANTITHESIS BETWEEN MIND AND BODY

CHAPTER XII

THE FUTURE OF BIOLOGY

INTRODUCTION

In the original writing of this book a great deal was contributed by the methodological wisdom of A. N. Whitehead, which has many applications to biological methodology. Among the ideas employed by Whitehead which are frequently mentioned in *Biological Principles* perhaps the most important is the idea of *abstraction*, and this is used by Whitehead in at least two quite distinct senses. I propose to take this opportunity to elucidate the sense in which this is used in the following impressive passage on p. 23 of Whitehead's *Science and the Modern World:*

> 'Thought is abstract; and the intolerant use of abstractions is the major vice of the intellect.'

It has taken me much time and effort to discover exactly what this passage means, and to recognize examples of what is being referred to in biological writings. The impressiveness of the passage is not a whit diminished by this discovery.

Very briefly we can say that abstraction (in one sense) occurs when a relation of *n* terms is treated as though it were a relation of *less than n* terms. To take a very simple and homely example; when we say:

Tom is boiling a kettle

and:

George is boiling a pint of water

we are treating a relation of at least four terms as though it were one of only two terms. The first sentence omits reference to the liquid which is being boiled, the second says nothing about the receptacle in which the boiling is taking place, and neither mentions the source of heat which is being used to raise the liquid to boiling point. In his *Survey of Symbolic Logic*, (1918), p. 198, C. I. Lewis writes:

> 'It is characteristically human to think in terms of dyadic relations: we habitually break up a triadic relation into a pair of dyads. In fact, so ingrained is this disposition that some will be sure to object that a triadic relation *is* a pair of dyads.'

It is a remarkable fact that relations were not taken seriously until the middle of the last century, and both of the early works devoted to the subject—Schroder's *Algebra der Logik*, and Whitehead and Russell's *Principia Mathematica*, deal only with two-termed or dyadic relations. But by no means all triads can be represented by a pair of dyads, and biology involves relations of as many as five terms at least. We are still without a systematic study of these many-termed relations.

The above example illustrates the omission of reference to one or two terms of a tetradic relation, but it is not an impressive example, because it is so easy to see what is missing and to supply it; although it is not unknown for an absent-minded person to place a well-filled kettle on a gas ring and omit to light the gas. In natural science the occurrence of abstraction is often more subtle and less easy to detect. A very good example is provided by the early history of the mercury thermometer. When this was first used in the study of the changes of temperature of water in the transition from fluid to vapour, it was found that the column of mercury ceases to rise above 100° on the centigrade scale as soon as boiling begins. This level was accordingly called the boiling point of water. Here we appear to have a *functional* relation of two terms: the substance boiled and the reading of the thermometer at boiling point; the former being the argument, and the latter the value of the function for that argument. But when, later, the observations were repeated up a mountain, it was found that the boiling point of water was *lower* at greater heights. It was then recognized that a functional relation of *two* arguments was involved: one being the substance boiled, the other the atmospheric pressure. Here, in contrast to our first example, we have an example of *unwitting* abstraction. The mercury thermometer was not a sufficiently delicate instrument to detect variations of atmospheric pressure at sea-level, and so there was nothing to suggest to those who first used it that atmospheric pressure was a term in the relation which they were studying. One obvious methodological lesson to be learnt from unwitting abstraction can be expressed as follows: Never mistake the *latest* word on any topic for the *last* word. As we do not *know* what discoveries are in store for us we do not *know* what is the last word on any topic. This is well illustrated by the history of Dalton's atomic theory. Dalton assumed that the atoms of any single element were all indistinguishable from one another. This sufficed until the discovery of radio-activity and the devising of tests which led to the recognition of the isotopes of the elements.

Having learnt a little about the nature of abstraction (in one sense of the word) we can now return to the passage from Whitehead quoted above and consider what is meant by the *intolerant use* of abstractions. It is most important to note that it is not abstraction itself which is being criticized, but the *misuse* of the results of abstraction. With our present linguistic apparatus science without abstraction would be impossible. We can say that an abstraction is used *intolerantly* if it is treated as though it were *not* an abstraction, and this occurs when what is abstracted *from* is regarded as non-existent, or unimportant, not only to the person who is abstracting from it, but to everyone else! When the operation of pride in human relations is borne in mind (not abstracted from!), it is not difficult to recognize a tendency in people to exaggerate the importance of the things in which they are interested, and to despise, denigrate, ignore or reject some things in which other people are interested and therefore do not abstract from. Specialization is an inevitable outcome of the process of abstraction, and if the ' major vice of the intellect ' is to be avoided, specialists must learn to recognize and respect each others' abstractions and to co-operate rather than to compete.

After these general preliminaries let us now turn to consider abstraction in biology. First of all, biology, like every other branch of natural science abstracts from persons and from what I call cognitive gots. This can be made clear by the following example: When we say

> Tom is seeing the moon

we are treating a relation of *five* terms as a relation of only two terms. The expanded statement would be (say):

> Tom is getting a view of the moon from his bedroom window at midnight on New Year's eve 1965.

The five terms in the expanded sentence are: (i) Tom, a person, (ii) a cognitive got, a view in this example; (iii) the moon, a physical thing; (iv) a bedroom window, a place; and (v) midnight on New Year's eve, 1965, a date. From the point of view of natural science Tom's body would be recognized as a thing, but not Tom as a person and owner of a body; cognitive gots as such are not recognized; but the moon, and place and the date would not be abstracted from. If it is asked: why mention views, why not be content to say that the moon is being seen; the answer is that seeing the moon and getting a view of it are two different processes, as we recognize when we remember that one can occur without the other. Tom can get a view of the moon when he is tucked up in bed with the curtain drawn over his bedroom window. He may announce at breakfast that he has had a dream in which he saw the moon. But to see the moon is to get a view of it when you are looking towards it, when it is *not* screened from you by bedclothes and curtains. In commonsense situations we usually do not distinguish between views and the things *of which* they are views; but we do not make the same mistake regarding smells, tastes, sounds or feels. But the notion of a cognitive got is of little use in biology because it is impossible to check whether animals get views, etc. The abstraction from persons in biology has many interesting consequences. It explains why anthropomorphism and teleology, as well as vitalistic theories are excluded. All theories of this type involve the notion of persons; and because purposes are the unfulfilled wishes of persons, purposes, and therefore teleological explanations, are also excluded. For the same reason divine intervention in such processes as evolution is also excluded. Such matters can be discussed in philosophical language, but not in the language of scientific biology. For the same reason, biological evolution, abstracting from persons, concerns the bodies owned by persons, but not the persons themselves. An interesting example of the confusion that can arise in these circumstances is furnished by a passage in H. W. B. Joseph's *Some Problems in Ethics*. T. H. Huxley, we are told, held that ' if evolution showed the groundlessness of moral distinctions, it was our duty to tell the truth and say so '. But if biology abstracts from persons, how can a biological theory have any comment to make about the groundlessness of moral distinctions? Moreover, if moral distinctions *are* groundless what can it possibly mean to say that it is *our duty* to tell the truth?

Regarding the exclusion of anthropomorphism from biology the connexion between this and the abstraction from persons is illustrated by the following examples. When someone introduces personal considerations into biology for explanatory purposes he is accused of being anthropomorphic; but when he explains some feature of human behaviour by reference to the behaviour of sea-gulls he is not accused of being ornithomorphic and his hypothesis may be accepted. This is because we have no grounds for supposing that the bodies of birds are owned by persons, and therefore the appeal to bird behaviour does not bring with it the notion of intentions or purposes.

We turn to some examples of abstraction in genetics. The following passage occurs in an article on genetics:

> We may differ from one another either because the genes we have received from our parents are not the same or because our environments have not been alike—or by reason of both causes acting together.

Anyone reading this might suppose that all we receive from our parents is a set of genes. But in fact each parent produces a gamete, these gametes unite to form a single cell, and this cell in an environment develops into a human body. But neither gamete is simply a mass of genes. Each has parts which are neither genes nor parts of genes. If we can abstract from these other parts in gametes this may be because in groups within which breeding is possible these parts do not differ genetically and can therefore be abstracted from. Or it may be that they sometimes differ but that the statistical methods employed do not enable their effects to be recognized. Moreover, not only are such gamete parts abstracted from, but all references to the *environments* of the gametes are omitted. And this may also be because they are not easily under experimental control.

A genetical example analogous to that of the mercury thermometer is provided by the saying that sex is determined by the X chromosome. This suggests that we are dealing with a functional relation of only one argument, namely, the number of X chromosomes in the cells concerned. But subsequent investigation has shown that it is one of at least *three* arguments, because in addition to the number of X chromosomes, the resulting sex depends on the number of autosomes and on the taxonomic position of the organisms concerned.

Yet another example of abstraction in genetics is furnished by the following quotation:

> It is well known how the introduction of an organized assembly of genes in the form of a goat into an oceanic island may completely change its flora, fauna and physiography.

This statement abstracts from the whole hierarchy of spatial parts which constitutes a living organism. A goat is not an organized assembly of genes in the sense in which a snow crystal may be called an organized assembly of water molecules. It is not even an organized assembly of cells. For there are parts which are neither cells nor parts of cells, and there are levels of organization above the cellular level and complicated relations between the members of these different levels: organ-systems, organs, tissues, cell parts (of various levels) among which some are genes. A feature of those modes of abstraction which lead to the pursuit of particles, is the danger that in the process of abstraction those deductive links which connect statements about the particles with the observable objects *of which* they are particles, may be lost or obscured, thus rendering the testing of the hypotheses difficult or even impossible. For it should not be forgotten that not only the initial data which suggested the hypothesis were obtained on the observational level, but it is only by return to this level that any hypothesis of this kind can be tested. This seems to be what Whitehead is referring to in the following passage from *Process and Reality* (1929), p. 5:

> 'The true method of discovery is like the flight of an aeroplane. It starts from the ground of particular observation; it makes a flight in the thin air of imaginative generalization; and it again lands for renewed observation rendered acute by rational interpretation.'

I hope enough has now been said by way of example to illustrate clearly what Whitehead meant by the passage quoted at the beginning. In conclusion, attention is drawn to some other effects of unwitting abstraction or of its intolerant use. A successful explanatory hypothesis can bring order out of disorder, introduce new insights where before there was darkness, and so stimulate research in new directions, thus leading to discoveries which might not otherwise have been made. But there is another effect which sometimes follows success. Because it has involved certain modes of abstraction the success of a new hypothesis will tend, while opening doors leading in some directions, to keep others firmly closed, and so discourage research along the lines to which they lead. It is thus extremely important that deliberate efforts should be made to discover as far as possible the factors which a new hypothesis is abstracting from, so that we may be warned about possible limits to its safe application in technology and medicine. Related to this is another lesson to be learned from the occurrence of abstraction, and this is the desirability of trying to disclose the modes of abstraction which are characteristic of each branch of natural science. Because, if a branch of science S_1 abstracts from a topic T, and another branch of science S_2 does *not* abstract from T, then clearly, in applying one branch to the other, difficulties and dangers may result if their different relation to topic T is not borne in mind. And this may easily happen if the specialists of science S_1 do not appreciate the importance of topic T for the work of the exponents of science S_2; or if the specialists of S_2 fail to recognize the possibility, or the reasons for the possibility, of abstracting from T in S_1.

The laws of mechanics provided by physics do not enable us to predict the course taken by the ball in a game of tennis. This is because a game of tennis involves persons, their aims, temperaments and skills, and all these are completely abstracted from in physics. Chemistry, as such, abstracts from organisms as viewed by biologists, and hence from pregnancy. Consequently in the testing of drugs from a chemical standpoint there is a possibility of the pregnancy of the patient being overlooked, together with possible effects on the foetus. Owing to the complexity of organization encountered among living things, and the occurrence in them of parts which are existentially dependent upon other parts, constituting the whole, we have the possibility to be reckoned with of the occurrence of so-called internal relations between such parts. That is to say, it may be that some parts can only be satisfactorily studied from some points of view while they *are* parts, and not in isolation. For example, a member of a football team is a part of an organized system which has human beings as top-level parts. To discover how such parts behave we must study them while they are actually functioning as members of the team; not while they are detached. Similar considerations may apply to the parts of organisms at many different levels, and if this is the case it will mean that the chemical outlook will not always suffice but will require supplementing and checking from the biological standpoint. The occurrence of abstraction renders independent observation and thinking necessary on the part of biologists. Finally, we can conclude with one more gem of Whiteheadean wisdom; after the foregoing reflections on abstraction, it is not difficult to appreciate the point of: Seek simplicity, but distrust it!

Epsom
February, 1966

J. H. W.

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PREFACE

MODERN natural science may be likened unto a crab which has grown too fat for its shell. The process of ecdysis is slow and painful. The old shell, which has matured and hardened for some three hundred years, has done good service. No wonder the crab is loath to part with it. But it has already begun to crack, and some bits have even dropped off. What is to be done ? Should the crab go on getting fat and take no thought for the raiment of the morrow ? Or should it resolutely face the situation, heave off the remains of the old shell with a sigh, and set about making a new one in earnest ? There is a great deal of uncertainty about the precise form and texture of the future new shell. But the evil day cannot be delayed much longer, and if it is put off too long the process of growth may suffer, or the whole may fall to pieces for lack of support.

The data of natural science constitute the meat. The shell in the parable represents the general philosophical background and the theoretical basis upon which the data have been systematized. Periods occur in the life of every branch of natural science when revisions and stock-takings of its foundations become necessary, but in recent years changes of a much more deep-seated and consequently far-reaching nature have been in progress, involving foundational beliefs which have scarcely been questioned before from the standpoint of natural science. So far this revolution appears to have had little influence on biology but has been confined to physics. Changes in the foundations of physics, however, may be expected sooner or later to concern biology, and especially those now in progress, which involve the foundations of our knowledge of nature in general. As long ago as 1887 Huxley wrote :

> ' Boyle did great service to science by his " Sceptical Chemist," and I am inclined to think that, at the present day, a " Sceptical Biologist " might exert an equally beneficent influence.'

So far as I am aware such a ' Sceptical Biologist ' has never been written, at least in the English language, but the present time appears to be a particularly favourable one for carrying out Huxley's suggestion, and the present work is a tentative attempt to do so.

In this Preface it will be desirable to explain the scope of the book a little so that a prospective reader may know what it will involve and how far it is likely to interest him. And it is important to emphasize the fact that such an undertaking as this involves a frame of mind and a way of thinking which is somewhat foreign, and perhaps even repugnant, to that proper to the scientific investigator. To write a book on how to conduct scientific investigations would be as silly as writing a treatise on how to paint pictures or write poetry. The 'Sceptical Biologist' has nothing to do with teaching investigators their business. He is concerned with interpretations: not with weighing empirical evidence upon which they are based, but with the most general assumptions, presuppositions, postulates, etc., which underlie them.

If we take such a scientific proposition as 'Adrenaline causes rise of blood-pressure' there are two directions in which our understanding can be extended. On the one hand we can repeat the experiment and try to determine more precisely what happens when adrenaline causes rise in blood-pressure. On the other hand we can ask ourselves more resolutely than the investigator usually needs to do what precisely we *mean*, what exactly we assert, when we say that adrenalin causes a rise of blood-pressure. Adrenaline is spoken of as a 'chemical substance' and rise of blood-pressure is referred to as a 'process'. What *exactly* is the difference between a chemical substance and a process, and what *exactly* is meant by saying that a substance causes a process? What in general is meant by substances and causes, and how is our knowledge of nature expressed and promoted by the use of these fundamental notions?

Now it happens (perhaps unfortunately if it suggests that they are unrelated) that when we follow the first course we are said to be pursuing 'science', and when we take the second we are pursuing 'philosophy'. It happens also that the people who follow the one alternative are not the people who follow the other. It results that the people who use these and other fundamental notions are frequently not at all clear about what precisely they mean by them. This may be expected sooner or later to have unfortunate consequences.

If such notions are used uncritically a time may come when, with the progress of investigation, and the increase of data obtained from sources far removed from ordinary experience, we may say more with them than we intend. We may suppose ourselves to be using them when we are in fact being misled by them. And yet, to be meticulously exacting about care in observation, and slovenly and careless in thought and the expression of thought is—to say the least—to spoil the ship for a hap'orth of tar. But this does seem to be the case in biology. Nothing is more striking in this science than the contrast between the brilliant skill, ingenuity and care bestowed upon observation and experiment, and the almost complete neglect of caution in regard to the definition and use of the concepts in terms of which its results are expressed.

Now if we are to call the investigation of such fundamental notions as causation, etc., 'philosophy', it is important to understand that it is only a part of philosophy. Some philosophers also employ the data of the various branches of learning in order to develop constructive schemes of thought about the general nature of existence and our own position in it. This is by far the most popular meaning of the word 'philosophy' and also the most popular *kind* of philosophy. Consequently it is the kind of philosophy which is most often meant when the term is used. Men of science usually take one of two attitudes towards this kind of philosophy. They either regard it with suspicion as a bad way of doing the same job as that upon which they are themselves engaged, or they regard it as a way of settling or at least studying certain interesting and important problems which are beyond the scope of natural science. Those who take the latter course often make a sharp distinction between philosophy and science—sometimes so sharp as to remove all possibility of any mutual influence between the two. Nothing that natural science discovers has any influence on their philosophy, because science is said to be 'abstract' or that it only deals with 'appearance'. Similarly, their philosophy never ventures to criticize or otherwise impinge upon their science. This may be called the 'Bradleyan view' of the relation between science and philosophy.[1] Those who follow the other attitude towards philosophy in regarding it as attempting to do what

[1] This view is expounded in F. H. Bradley's *Appearance and Reality*, London, 1908, pp. 283-5.

they are doing but by a wrong method usually have a constructive metaphysic of their own based upon natural science, often rather uncritically interpreted.

Between these two views (which only apply to constructive or speculative philosophy) the other kind of philosophy which is more modest in its aim (since it only tries to be clear about the exact significance of the fundamental notions and methods of inference which we employ in systematizing our knowledge) is apt to escape attention. And if it does receive notice it is usually mistaken for an idle discussion about words. But this is to confuse the symbol with what is thereby symbolized—a not uncommon mistake.

It seems, then, that the 'Sceptical Biologist' will not be able to avoid 'philosophy' in its more modest critical aspects, and at the same time he will be exposed to misunderstandings based upon the traditional scientific distrust and dislike of speculative philosophy. But, according to Professor Whitehead: 'To neglect philosophy when engaged in the re-formation of ideas, is to assume the absolute correctness of the chance philosophical prejudices imbibed from a nurse or schoolmaster or current modes of expression. It is to enact the part of those who thank Providence that they have been saved from the perplexities of religious inquiry by the happiness of birth in the true faith.'[1]

Broadly speaking we can now distinguish three main periods in the history of European thought: (1) that dominated by Aristotle, which came to an end with the Renaissance; (2) that dominated by Descartes, which reached its high-water mark in the nineteenth century; and (3) the transition period in which we now stand. The first and second periods may be described as dogmatic in the sense that they had cut and dried answers to most questions; the third may be called critical inasmuch as it has not so far been dominated by any one constructive scheme, but has chiefly been concerned with a general overhaul and stock-taking of fundamental ideas. Those who dislike dogmatism and delight in 'transition periods' will be glad to be alive in this new springtime of thought—when 'all the wood stands in a mist of green and nothing perfect'. But, as I have said, the only science which has been influenced to any extent by this change so far is physics. It is in the mathematical physicist that we most

[1] *The Principle of Relativity*, Cambridge, 1922, p. 6.

often find a union of the man of science with the critical philosopher, although by no means all physicists are critical. Biology is still in that phase which, according to Comte, all sciences pass through, and which he called the metaphysical stage. Among English biologists the late William Bateson seems to have come nearest to understanding the critical standpoint. Consequently the need for a critical review of its principal difficulties and fundamental notions is perhaps greater in biology than in any other science.

The mutual relations of the two great branches of inquiry—those of critical philosophy (i.e. logic and theory of knowledge or epistemology) on the one hand, and the experimental investigation of nature, on the other, as seen in the history of physics, have been well sketched by Professor E. Cassirer in the following passage :

> 'The working together of the two points of view has always come to light with special distinctness at the decisive turning points in the evolution of theoretical physics. A glance at the history of physics shows that precisely its most weighty and fundamental achievements stand in closest connexion with considerations of a general epistemological nature. Galileo's *Dialogues on the Two Systems of the World* are filled with such considerations and his Aristotelian opponents could urge against Galileo that he had devoted more years to the study of philosophy than months to the study of physics. Kepler lays the foundation for his work on the motion of Mars and for his chief work on the harmony of the world in his *Apology for Tycho*, in which he gives a complete methodological account of hypotheses and their various fundamental forms ; an account by which he really created the modern concept of physical *theory* and gave it a definite concrete content. Newton also, in the midst of his considerations on the structure of the world, comes back to the most general norms of physical knowledge, to the *regulae philosophandi*. In more recent times Helmholtz introduces his work, *Über die Erhaltung der Kraft* (1847), with a consideration of the causal principle as the universal presupposition of all " comprehensibility " of nature, and Heinrich Hertz expressly asserts in the preface of his *Prinzipien der Mechanik* (1894), that what is new in the work and what he values is " the order and arrangement of the whole, thus the logical, or, if you will, the philosophical side of the subject." But all these great historical examples of the real inner connexion between epistemological problems and physical problems are almost outdone by the way in which this connexion has been verified in the foundation of the theory of relativity. Einstein himself . . . appeals primarily to an epistemological motive, to which he grants, along with the purely empirical and physical grounds, a decisive significance.'[1]

[1] *Einstein's Theory of Relativity considered from the Epistemological Standpoint*, Eng. trans. by W. C. and M. C. Swabey. Chicago, 1923, pp. 253-4.

Those biologists who admire the methods of the physical sciences would do well to consider carefully the implications of the above passage for biology.

What I have attempted to do in this book is to give a general sketch of the whole wide field which is involved when we try to dig down to scientific foundations. Consequently the proportion of 'biology' to 'philosophy' is very small. The General Introduction is devoted to introducing the topics to be discussed in an elementary way, and to removing certain difficulties which would be liable to arise later and confuse the issues. Part I deals with the general problems of the theory of knowledge which are involved in the interpretation of the results of *any* branch of natural science, although I have used biological examples to illustrate general principles as far as possible. In Part II I have picked out what appear to be the most general and most deep-seated difficulties and peculiarities of biological knowledge, and I have attempted to use the results of Part I to elucidate them. But it is necessary to repeat that even in this part the critical biologist is not primarily concerned to discuss rival *biological* theories, or to attempt to decide between them, on the basis of an estimate of the relevant empirical data. His duty is simply to examine the logical procedure and ontological assumptions involved in them, and to show how far the difficulties ordinarily felt in regard to such rival theories depend upon such assumptions. Those philosophers who interest themselves in the methodology of natural science usually confine their attention to physics. I hope they may find something of interest in Part II of this book which will persuade them to devote some attention to the interesting and largely unexplored requirements of biological thought.

In the course of Part II I have ventured to put forward some suggestions towards a resolution of certain of the traditional biological conflicts which are there discussed, but my primary aim has been to introduce and expound a new[1] method of approach to such questions—a method which has not yet received the care and attention among biologists which it deserves. Consequently it will be a matter of little importance what becomes of any constructive suggestions which may here be brought forward (and this applies especially to Chapter IX) so long as the point of view from which the difficulties themselves have been approached has been understood. But this

[1] 'New,' that is to say so far as English biology is concerned.

can only be done if the reader is prepared to stand aside, so to speak, from his ordinary scientific way of thinking and subject it to a critical and disinterested examination.

Criticism in the common meaning of the term may be either hostile or friendly. The hostile critic has an axe to grind. Criticism as here intended (in the sense in which it was first introduced into philosophy by Kant) is a disinterested examination of traditional conflicts with a view to the discovery of their roots, and the removal of difficulties created by an uncritical use of the notions of unreflective thought. Criticism of this kind is friendly in so far as it seeks to remove difficulties and not merely to point them out. It has no axe to grind because it does not aim at speculative constructive schemes. It deals only with foundations, leaving the superstructure to others. In the words of Locke: 'It is ambition enough to be employed as an under labourer in clearing ground a little, and removing some of the rubbish that lies in the way to knowledge.' A great deal of the discussion that goes on in biology around the traditional controversies seems to be vitiated partly because it is not taken deeply enough, partly on account of the almost universal neglect of the elementary precaution of defining the meaning of the terms used, and partly because it seems always to be conducted from the standpoint of one side or the other rather than from one of neutrality. These are three defects which I have tried to remedy in what follows. One consequence of these defects is that appeal is made to *convictions* instead of to *reason*. In regard to the celebrated quarrel between 'mechanists' and 'vitalists', for example, it is not a bit of use appealing to 'instincts' and 'intuitions' to reveal the shortcomings of one side or the other. It is necessary to analyse the precise nature of the strong and weak points of each side so that we can make a rational judgment of their relative merits. What is here offered, therefore, is a study of the central problems and basal difficulties of the biological sciences not from the standpoint of one or another of the traditional theories, but from that of disinterested critical reflexion, utilizing any aid that modern investigations of this kind in other spheres may be able to offer. And if, in relation to a particular controversy, I have criticized one side more than another I would ask the reader to believe that this is not because I wish to favour one side more than another, but because one seems to be in more need of critical examination than another, and from considerations of space it is necessary to concentrate on points which require most attention.

In the search for aids to the elucidation of the difficulties peculiar to biological thought I have turned modern developments in many branches of inquiry to account—particularly in logic, theory of knowledge, and physics. Accordingly it has not been possible in Part II to pursue the problems dealt with there in any great detail. But I venture to hope that the method of approach here advocated will be capable of much further development, and that the reader who is interested in this aspect of biological inquiry will be aided and encouraged to undertake such further development. In this connexion I would especially draw attention to the need for a critical analysis of biological concepts with a view to freeing them from historical accretions which are not in harmony with modern knowledge, and to the need for more boldness and originality in developing new ones. I have devoted special attention to the important notion of 'organization' which seems to have been singularly neglected. Another topic of equal importance is the ancient antithesis between 'structure' and 'function' to the removal of which, as I have tried to show, modern developments appear to furnish the key. The consequences of a more detailed working out of these developments in relation to this and other biological problems should be important and interesting. But I would point out that it will be quite impossible properly to follow the arguments of Part II if due attention is not paid to the difficult but quite fundamental problems discussed in Part I. I must also state clearly at the outset that what is here offered does not profess to be in any sense exhaustive or complete. Even in regard to some of the most general problems which I have singled out for discussion it has not been possible to do more than analyse and state the difficulties clearly and so to leave them for the present. This applies with special emphasis to Chapters IX and X. My feelings on this point are echoed in the words of the preface of another author: 'What I have here in some measure set in order is adjoined on all sides by thickets abounding in monstrous doubts and difficulties. There are complications which I have not followed out, assumptions which I have not followed back, and after-thoughts which I can already anticipate.' But something is accomplished even when difficulties are merely brought to light and stated clearly, and if more people are persuaded to set about removing some of the 'monstrous doubts and difficulties' with which biological thought abounds, the purposes of this book will have been accomplished. It is necessary first to make clear to biologists the existence of these difficulties, and then to bring home to them the nature of the intellectual weapons they are using and, by showing them something of their origin and of the uses for which they were originally intended, to persuade them to consider more critically their use and limitations in the biological field. Having in this way fostered a wholesome spirit of scepticism and a dissatisfaction with their 'current modes of abstraction' on the part of those interested in theoretical biology, the desire for something more satisfactory will lead to the exploration of other possibilities, and the exploitation of the rich potentialities of human thought which have not yet been drawn upon.

My indebtedness to other authors will be sufficiently plain from the references given in the text. I have learnt most from the writings of the Cambridge school of logicians, particularly from the works of Messrs. C. D. Broad, W. E. Johnson, G. E. Moore, Bertrand Russell, and A. N. Whitehead. And of these the first and the last have been of most help from the biological point of view. Professor Whitehead's views are specially sympathetic to a biological application. I am greatly indebted to Miss L. S. Stebbing for twice reading the book in manuscript from the logical standpoint. I have profited much from her criticisms and advice. To Dr. E. S. Russell I am indebted for reading the manuscript from the biological point of view and making a number of helpful criticisms. Similarly, Mr. G. C. Robson kindly read Chapters VI and IX. Acknowledgements are due to the editor of *Science Progress*, for permission to make use, in Chapter IV, of parts of a review which has already been published in that journal. Finally, I am obliged to my wife and to Dr. S. Wright for help in proof-reading.

J. H. WOODGER.

Middlesex Hospital, Medical School,
March, 1929.

'If anyone does not care for knowledge for its own sake, then I have nothing to say to him; only it should not be thought that a lack of interest in what I have to say is any ground for holding it untrue.'—G. E. Moore.

BIOLOGICAL PRINCIPLES

GENERAL INTRODUCTION

1

BIOLOGY is a science of antitheses. If we make a general survey of biological science we find that it suffers from cleavages of a kind and to a degree which is unknown in such a well unified science as, for example, chemistry. Long ago it has undergone that inevitable process of sub-division into special branches which we find in other sciences, but in biology this has been accompanied by a characteristic divergence of method and outlook between the exponents of the several branches which has tended to exaggerate their differences and has even led to certain traditional feuds between them. This process of fragmentation continues, and with it increases the time and labour requisite for obtaining a proper acquaintance with any particular branch. But whereas in some sciences this process has been accompanied by the attainment of generalizations which have tended to knit the several branches into a single whole, in biology the disruptive process has not been compensated by the help of any principle of such unifying power, and the possibility of a unified biology seems to recede more and more from our grasp.

At the bottom of these difficulties we find the fundamental antitheses of biological thought. Structure and Function, Organism and Environment, Preformation and Epigenesis—these are some of the antitheses which determine the lines along which biology is divided in more senses than one. There are, moreover, lines of cleavage which cut deeper still. Where, in the physical sciences, shall we find such a total divergence on a fundamental matter of principle as that which separates those who profess some form of 'vitalism' from the exponents of 'mechanism'? This ancient controversy continues *ad* *nauseam* and shows no sign of abating. Biology has been cleft by this antithesis at all stages of its history. The same arguments are repeated by successive generations without any resolution of the conflict. Each side belabours the other without—if we may judge by the confidence with which each continues to assert its convictions—making any impression on its opponents.

Involved in this controversy are others which have their roots in the antithesis between 'Body' and 'Mind'—an antithesis which is not a purely domestic one for biology but which links it with psychology. Much help has been furnished by physical and chemical knowledge in the investigation of biological problems. This aspect of biological inquiry stands in no danger of neglect. It enjoys a wide popularity to-day, especially in view of its applications in medicine. But although biology appears to have clear points of contact with psychology this relation receives comparatively little attention. Far from psychology being *sought* as an aid and ally, it seems to be *avoided* as more likely to create difficulties than to remove them. And yet, in the human organism at least, the spheres of physical, biological, and psychological science seem in some way to coalesce, and the critical inquirer should not allow this unique circumstance to be set lightly aside. The origin and significance of the prevailing point of view call for his attention.

But without going beyond the boundaries of our own science there are problems enough when we try to bring the results of its main branches together. The general theoretical results which have been reached by investigation along the lines of physiology, experimental morphology, genetics, cytology, and the older descriptive morphology are extremely difficult to harmonize with one another, even although, for various reasons, these difficulties are not apparent on a *prima facie* view. As soon as we do attempt such a synthesis we are confronted with contradictions which appear to rest on the fundamental biological antitheses. Instead of a unitary science we find something more approaching a 'medley of *ad hoc* hypotheses'. Moreover, the fundamental cleavages of opinion are reflected in the exposition of the results of this department of science according to the beliefs to which a given author subscribes. An author will tend to give more emphasis to those facts which support the view he favours than to those which are antagonistic. Consequently the reader of biological literature is compelled to bear this constantly in mind and must seek to correct one exposition with another. And this presupposes some acquaintance with the antitheses of biological thought and their consequences. How is it possible to improve upon such an unsatisfactory state of affairs ? Would it not be possible to take up an attitude of neutrality towards the traditional conflicts in order to study the nature of the antitheses upon which they appear to rest ? If we do this we may find that the roots of these antitheses themselves do not lie wholly within biology but are to be sought elsewhere, and in that case where are we to seek them ? In other words : what is to be our standpoint for such an inquiry ?

2

If we ask ourselves what exactly we are doing as biologists we may find some indications of how to set about answering these questions. It will be generally agreed that biologists are primarily concerned with the investigation of those constituents of the world which we call animals and plants. Biology, it will be said, is that branch of science which deals with these things. We are therefore referred to something wider, called science, of which biology is a part. What, then, is science ? The word has a diversity of meanings. In one sense—as when we speak of this or that science—it stands for a body of knowledge : a systematized body of propositions about some subject matter or other, enshrined in books or in the minds of individual men. We ought, therefore, to speak, not of science but of the scienc*es*, or, if the term is used in the singular, it would seem to be a collective term for all the branches of science taken together. But the word science is by no means universally employed simply as a general name for all systematized knowledge. It is doubtful, indeed, whether it is ever used in this sense at the present day. Often it appears to be used not as a name for *knowledge* about some subject matter but as a name for a particular *attitude* or activity towards nature. Not infrequently it appears to be confused with a certain *theory* about the nature of existence in general—the theory more properly referred to as ' naturalism '. Again, many writers seem to restrict the term science so far as to include in it only such knowledge as is systematized with the aid of mathematics, in which case a great part of biological knowledge would be excluded. But by far the commonest usage is in its application to what is also called 'natural science', the adjective 'natural' frequently being omitted. If we follow this usage we shall be restricting the application of the term to knowledge about nature, instead of making it equivalent to the whole of knowledge—unless, of course, we believe that 'nature' is equivalent to all that there is to know, and in that case science, natural science, and knowledge would be synonymous terms. But many people would not agree to this synonymy. They would say that knowledge is wider than natural knowledge, since there are branches of science which do not have 'nature' for their object in the same sense as physics, for example, does. To clear up this difficulty we should obviously have to decide what we mean by 'nature'. But for the present we are trying to be clear about what we mean by science. Biology, we say, is a branch of science, and science, whatever else it is, is knowledge; whether it is to be regarded as a part or the whole does not concern us for the present. Now the possibility suggests itself that the roots of those biological antitheses do not lie wholly in the nature of the *organism* and are not, therefore, to be studied as part of the subject-matter of biology, but that they arise partly out of the nature of *biology* itself as knowledge. This further suggests that by the study of biological thought from this point of view we might throw some light upon the nature of biological controversies. What, then, is knowledge? Biologists often speak of our knowledge of this or that, but they rarely discuss knowledge itself. They are concerned with *getting* knowledge *about* animals and plants. They are interested in such knowledge, but not in it *as* knowledge. They may, in fact, forget that they are dealing with knowledge at all, and suppose themselves to be dealing not with knowledge but simply with animals and plants. But this is clearly not the case, otherwise what would distinguish a biologist from a landscape painter? The latter might also claim to be dealing with animals and plants, and so he is—but not by way of knowledge. Both are dealing with animals and plants but they are exercising different activities, the biologist's activity is primarily intellectual and its outcome is knowledge. This being understood we now have to inquire what is involved in biology as a branch of knowledge, in the hope that such inquiries may help us towards a better unification of biology.

3

I have referred to a science as a 'systematized body of propositions about some subject matter or other, enshrined in books or in the minds of individual men'. This will serve as a provisional analysis of what is involved in knowledge, and as a starting point for further inquiry. We can now (i) examine some particular instances of biological *propositions*, then try to determine (ii) how they are systematized, and (iii) consider how they are related to minds. What the propositions are about we need not now inquire because we know that this forms the subject-matter of biology, but we shall have to study (iv) the relation of propositions to this subject-matter as well as their relation to minds. It seems from this that propositions have a double relation: on the one hand to minds, and on the other to subject-matters. A detailed study of these questions will form the topic of Part I. Foı the present it is only necessary to pursue them a little further.

(i) The following propositions are taken at random from a biological book:

> 'Isolated rudiments of the eye, the nose, and the ear of the chick differentiate independently on the chorio-allantoic membrane. The same is true of rudiments of pronephros, mesonephros, neural crest, liver, pancreas, intestine.'

These will illustrate some of the properties of propositions in general. Something is *asserted* of something else. We are told that certain objects (isolated eye rudiments) *do something* (differentiate independently) in a certain place (chorio-allantoic membrane). The second of the two sentences quoted reminds us that such assertions make a certain claim—they claim to be *true*. Thus biology consists in making true assertions about organisms, or parts of organisms. It will be noticed that although explicit reference appears to be made to a certain *place* no such reference is made to *time*. The proposition asserts that whenever certain objects occur in a certain situation a certain event happens, and it is therefore implied that *when* it happens is a matter of indifference. In a sense this is also true of place. The proposition says that the eye rudiment is in a certain place—the chorio-allantoic membrane, but it does not say where the chorio-allantoic membrane has to be. Is this also a matter of indifference? The author assumes quite legitimately that the reader knows that what is meant is that the membrane is part of a chick, that the chick is alive and in an egg-shell, and that the egg-shell is either in an incubator or under a hen. Is it then a matter of indifference where the hen or the incubator is? Could they, for example, be at the North Pole? What is usually meant is that it does not matter *where* the membrane is so long as certain unspecified, because unknown, *conditions* are realized. What the proposition wants to assert is that *if* such conditions are realized the assertion will hold good, no matter where or when they happen to be realized. Thus the proposition, in spite of the form in which it is stated, is really *hypothetical*, and involves reference to an ever widening circle of conditions--recalling the 'conditions' which prevented the old woman from driving her pig home from market—about which it says nothing. We cannot say that where and when do not matter because at every time and place of which we have any knowledge *some* conditions prevail, and there may be times and places at which hens and incubators cannot function. It seems to be begging the question to say that, *if* the same conditions are realized when *you* try the experiment as were realized when *I* tried it, then you will get the same result, because I do not know what conditions were realized in my case and you will not know what conditions will have been realized in your case. But if two experiments give the same result it is said that it is because the conditions were the same and if the results are different this is ascribed to the supposed fact that the conditions were different.

The same difficulty arises in connexion with the *objects*. We know, of course, that only a certain limited number of eye-rudiments have actually been placed in the situation mentioned, in a certain laboratory at a certain time. But the proposition does not restrict itself to those objects, any more than to that laboratory or to that time, and it may be asked: How is it possible to make true assertions about other eye-rudiments when you have only observed a few? What does it mean to claim *truth* for such propositions? If we knew that all eye-rudiments and all chorio-allantoic membranes differed from one another only numerically then it would suffice to try the experiment once only, but we are told that 'variation' is an important and common characteristic of living things—so important and frequent that Darwin founded a theory of evolution on this fact as a basis. Thus we not only do not know that all objects of the kind mentioned are merely numerically different, but we should be contradicting an important general biological principle even in assuming that they were.

What, then, do we mean in claiming truth for such a proposition? If the author had merely said that certain events *had* happened in certain objects at certain times and places we should know that by this being true simply meant that such events had actually been observed, and a reputable investigator who made the statement would have no difficulty in convincing reasonable people of its truth by showing the specimens he had obtained. But such a proposition would be an historical proposition—it would merely record a fact. The biological proposition clearly wants to do more than that. For reasons already noted we could not test this assertion by doing the experiment ourselves because this would still leave an enormous number of cases still untested, and if the experiment were unsuccessful it could still be said that this was because the conditions were not favourable, as so frequently happens in biological experiments. But if a number of careful operators repeat the experiment unsuccessfully then we are inclined to say that the proposition is false, and we should reject it as worthless (although it would still remain true as a historical record). Thus the number of times the experiment has been repeated is considered to be of importance. But even if these further experiments confirmed the original proposition we should still be in doubt about the time question. How far, for example, into the past may we suppose such a proposition to have been true? When we have to invoke present conditions about which we know so little, what can we be justified in saying about past conditions about which we know nothing? This is evidently a difficulty which we ought to bear in mind when we make statements (sometimes using the word 'must') about organisms at enormously remote epochs.

Yet in spite of all these difficulties the procedure of natural science seems to work well enough to sustain our hopes that our inductive propositions say something meaningful and form a worthy foundation for the erection of speculative theories.

These difficulties, which come to light as soon as we begin to reflect, and which appear to be particularly acute in the biological sciences, will have to be removed if we are to find a logical justification for our scientific procedure, if, that is to say, it is to be valid.

(ii) What do we mean by systematized propositions? By a body of propositions being systematized we mean that it possesses some sort of order, unity, form, or organization. What, then, determines this form? Upon what principles does it rest? Here we have to remember the double relation of propositions to minds on the one hand, and to a subject-matter on the other. We may expect that both of these relations will have an influence upon the structure of our knowledge. It seems clear enough that the character of the subject-matter will determine, in part at least, the mode of sub-division, for example, of a science. But it is also equally clear that minds have some say in the matter. Some thinkers have even gone so far as to suppose that *all* the systematization our knowledge possesses is the ' work of the mind '. But quite apart from such an extreme it is evident that minds make certain *demands* which knowledge is expected to fulfil, such as generality, simplicity, etc. These are characteristics of propositions. But before all else we demand that our knowledge shall be free from contradiction and that it shall be true. The nature of the subject-matter, however, may be such as to set certain limits to the attainment of some of these ideals. What we discover about organisms, for example, conflicts very much with the demand for simplicity. ' Seek simplicity ', says Professor Whitehead, ' and distrust it '. Consequently, if we find that the nature of the subject-matter conflicts with a demand we shall feel that in order to reach the ideal of truth we shall have to relinquish the demand in question. It will be generally agreed that in science we are not concerned with making our knowledge conform to our wishes but primarily in assuring ourselves that it tells us *what is in fact the case* in the particular sphere in which we are interested.

Then there are certain extremely general notions such as that of a ' thing ' having qualities and properties, and the notion of cause, which play a fundamental part in the organization of knowledge. Biology too, as a branch of natural science, is under the dominion of those most pervasive features of our experience which we call space and time. The study of these and of their significance for science has occupied a great deal of attention in recent years. Closely connected with them is the problem of induction to which we have already referred. Moreover the whole of our scientific procedure involves certain universal assumptions and beliefs which require examination. A study of these matters—all of which are implicated in the 'principles of systematization'—will be undertaken, from the standpoint of biology, in Part I. We shall see that an examination of these principles of systematization is of great importance in understanding the biological antitheses. The traditional contrast between structure and function, for example, which may seem at first sight to be determined solely by the nature of the subject-matter will be shown to depend on our attitude to some of the general concepts to which we have been referring.

(iii) With regard to the relation of minds to propositions it will suffice for the present to say that, except for an extreme behaviourist, propositions are the outcome of mental operations of the kind called intellectual. If you are asked what is the sum of 25 and 18 you will probably say, after an interval, 43. And what you did during part at least of that interval would be called, by most people, an intellectual operation. The propositions of a science refer, we said, to a subject-matter, and hence minds are indirectly related to the subject-matter through propositions. But minds are also directly related to subject-matters. In the case of natural science this relation is always in the first instance through sense-perception, and the relation is usually described as one of awareness. Perception is also regarded by most people as a mental operation. Thus in natural science we first become aware of our subject-matter through perception, and as a result of subsequent operations we give utterance to propositions about it. The nature of perception and its relation to knowledge will be the first topic for Part I. But intellectual operations are not the only mental processes. When anyone 'gets heated' in an argument it is usually believed that he is the subject of mental processes of the kind called affective or emotional. Moreover, some people, in the course of an argument, may deliberately *appeal* to such processes in their hearers in order to influence the kind of propositions they will make about certain subject-matters. Thus there are other mental processes which may interfere with the intellectual operations, and in discussing a theory we ought to take into consideration the possibility of such influences in its composition. Such possibilities will be discussed in Part I. under 'subjective factors' in knowledge.

4

This first brief examination of knowledge brings us back to the question whether science as knowledge could be regarded as equivalent to 'natural' science. We have been taking *knowledge itself* as our subject-matter—making *it* the object of inquiry. Now knowledge is not usually regarded as a 'natural object' in the same sense in which a rabbit is regarded as a natural object, and hence if we are going to use the term natural in its common sense, natural science will not be equivalent to all knowledge. But the branch of science which takes knowledge for its object is usually called logic, so it seems that what we are seeking as a basis for the study of the biological antitheses is the logic of biology. Are there other branches of knowledge which do not come under 'natural' knowledge? The reference to minds suggests that a systematized knowledge of such objects would also be a science, but would it be a 'natural science'? We see that this expression 'natural science' is constantly forcing us to ask what we mean by nature in order to determine whether a particular bit of subject-matter is to be referred to nature, and so to natural science, or not. But it is evident that to decide such a question we should have to make a survey of the subject-matters of a number of sciences and study the relations between them.

It might seem that such questions are of no importance for our present purpose. But it is absolutely essential at this stage to consider the relations of the various branches of knowledge to one another in order to avoid confusions and difficulties which would otherwise present themselves later. We have seen that each science has a certain field marked out for it by its subject-matter. What is the relationship between the subject-matter of physics and that of biology? or between the subject-matter of the science which studies minds and that which studies animals? Most people believe that although there are many sciences there is only one universe, and that the results of all the sciences taken together ought to tell us something about that universe and about our own position in it. Moreover it is felt that, although there is such diversity between different sciences, and so much difficulty in finding any principle of unity between them—often we find contradiction and incompatibility instead—it is felt that, in spite of all this, there is unity of some sort and non-contradiction in the universe itself if only we could discover it. But if we are *in* one science how can we answer such questions ? Is it even possible for one man to know the field of his own science properly, let alone enough about another to talk about the relationship between the two ? Even in a single science there are many sub-divisions, and in order to *add* to the knowledge of a particular division it is necessary, at the present day, to know a great deal about it, and this usually means that there is no time left to know much about the others. What does the average zoologist know about botany ? or the average animal morphologist know about animal physiology ? A specialist has been described as one who knows more and more about less and less. But we have seen something of what 'systematized' means, and how each science strives after a theoretical interpretation of its own field in which the details will be organized in accordance with certain principles. Most people will agree with Poincaré that a collection of facts no more makes a science than a heap of bricks makes a house. Thus there is not only a process of adding to knowledge but also one of organizing it. But a specialist in one branch would only be able to do the organizing of the branch with which he was familiar, he would not have time left for attending to other branches if he were also engaged in adding to his own. One can imagine a highly perfected science in which it would be necessary to have not only 'adders' who also did the organizing each in his own sub-division, but also 'organizers' who did nothing but study the mutual relations between the sub-divisions. It might be necessary to have hierarchies of 'organizers' to deal with groups of sub-divisions, according to the degree and manner in which the subject-matter was divided. The only science which approaches such a state at present is physics, and here the 'organizers' have been mathematicians who perhaps have never been in a laboratory in their lives. One thinks of Leverrier predicting the discovery of a new planet and being too bored—so the story runs—even to look through the telescope when his prediction was verified.

Moreover in physics what I have been calling 'principles of systematization' have been the subject, all through its history, of lively discussions. When we turn to biology we find, as I have said, a large number of branches pursued by specialists who are too busy making new additions to knowledge each in his own branch to attend to other branches, still less to devote any time to the study of the principles of systematization. Moreover the difficulties of biology are still further exaggerated by those deep cleavages of opinion to which reference has been made and with which every biologist is familiar.

Now the question: What do we mean by nature? is one of those which appear to necessitate a consideration of more—many more—than one science. It could not be decided by a *specialist* in one science. All *he* could tell us would be about the present state of *his* science, or rather his particular branch of that science. It is also clear that what he had to tell us would be valuable just in proportion as he was *disinterested* in the general questions. His function is just to tell us what he finds to be the case in his own sphere with the minimum of distortion. Thus it would seem that there ought to be a most general science, not immersed in a particular subject-matter, but dealing with the relationship between the various special sciences, and trying to synthesize their most general results. And the function of such a general science would not necessarily be exhausted by attempting to make a synthesis of knowledge. It might also be able to help the special sciences by pointing out contradictions between them, and thus suggesting new lines for investigation with a view to their removal. It is obvious that such a general science would differ in a number of important respects from a special science and would probably require a different type of mind for its pursuit. It could not, for example, make experiments because it would be dealing with the whole. It would be utterly dependent for its data upon the special sciences, but it would also preserve a strict impartiality between them, just taking their data as it receives them and making what it could of them by way of synthesis. Its success would depend on the care taken in making the subsidiary syntheses in each special science, since it would have to assume that such syntheses had been carefully and disinterestedly carried out. Being dependent in this way, the conclusions of such a universal science could not be more probable than those of the sciences upon which it was based.

Like their conclusions its own would, therefore, always be tentative and subject to revision.

Such a general science would also serve as a corrective to certain defects which inevitably attach to the procedure of the special sciences. The whole success of the latter depends upon their way of dealing with their problems piecemeal—postponing difficulties until the easier tasks have been completed. In other words they proceed by abstraction—a most important feature which will have to be considered in detail later. Now in our universal science this would obviously be impossible. Since it has undertaken to give some account of the whole it could not abstract but would have to ensure that the consequences of one mode of abstraction were duly compensated by the use of others. It thus becomes, in the words of Dr. Broad, the residuary legatee of all the difficulties which the special sciences have found it convenient to neglect.

5

From the earliest days of systematic reflexion there has always been an ideal of such a science and it is commonly called metaphysics. Whether it has always supposed its task to be such as I have depicted it may well be doubted. All I have been doing has been to state *my* purely personal opinion about it. There appear to have been two principal types of metaphysics in the past: one, which we may call the a priori type, which works deductively from agreed 'first principles'; the other, 'empirical', attempting to reach a general synoptic interpretation of the results of the special sciences. Among recent thinkers the work of M'Taggart might be cited as an example of the first, and that of Professor Alexander as an example of the second. But such a division makes the situation appear to be simpler than it really is because it is difficult to be purely a priori or purely empirical. Now from time to time we hear about a conflict between 'science' (meaning natural science) and metaphysics. There seems to be a tradition among men of science that there is something disreputable about metaphysics—something which makes it a topic which it is desirable to avoid. But people who hold this do not by any means make it quite clear what they mean by metaphysics. This is not a satisfactory state of affairs, because if you do not know clearly what metaphysics is you may easily fall into it unawares, and if metaphysics is something it is desirable to avoid it will be a misfortune to fall into it unawares. It seems very frequently to be the case that what a person means by metaphysics is the opinion on certain topics of other people who do not agree with him. His own opinion he calls 'science'. This, clearly, will not do. It reminds us too much of the story of the man in *Punch* who was explaining the difference between 'doctrine' and 'dogma': 'What he thocht richt was doctrine, and what the ither yins thocht richt was juist dogma.' If metaphysics is defined in this way there will be as many definitions of metaphysics and of science as there are different opinions. It will, moreover, have the serious consequence of leading to perpetual confusion between a particular science and metaphysics in the sense in which I have described it. It will usually be found that people who make the division in the above way have already made up their minds on certain metaphysical questions without being aware of the fact—owing to a failure to distinguish clearly the problems and methods of science, on the one hand, and metaphysics on the other. From what has been said it will be clear that they differ in many important respects.

Put in the way I have attempted to put it, it seems absurd that there should be any conflict between metaphysics (in the sense of a most general synthesis of the special sciences) and a special science which furnishes the general one with data. It would seem, rather, that they should be mutually complementary. But there certainly is a tradition of such a conflict, just as there is a tradition of a conflict between 'science' and religion—as though science were always quarrelling with something or other How has this tradition arisen? Such quarrels—like disputes between nations—not infrequently result from lack of mutual understanding, and in a world of knowledge divided into thought-tight compartments it would not be surprising to find such a lack.

Metaphysics—like politics and theology—is one of those topics upon which many people hold very definite opinions without considering it necessary either to have any special preparation, or to attach much importance to the views of those who have devoted themselves especially to them. No-one dogmatizes about organic chemistry: it is felt that unless you are yourself a chemist it is inadvisable to hold very confident opinions on such a subject based solely on your own resources. But with metaphysics it appears to be otherwise. This is partly because most people are interested in the general problems of existence, and do in fact take up some sort of attitude towards them, whereas only a few people are interested in organic chemistry. But it is also because problems in the special sciences can frequently be put to the experimental test and there is a possibility of your erroneous opinion being refuted. Metaphysical opinions, from the nature of the case, cannot be tested in this way. Consequently whereas chemistry will be able to give a positive opinion upon many questions, metaphysics would only be able to offer a number of alternatives. But is this a sufficient reason for believing that the opinion of, say, an exponent of a special science is as good as that of one who has devoted himself to such questions ? Medical authorities are notoriously divided in their opinions, and these too are often of such a nature as not to admit of being settled by an experimental test. And yet we often consult them in spite of this, feeling that it is better to trust someone who has devoted attention to such topics rather than to rely on our own resources. Thus the fact that so many people hold decided opinions about metaphysical problems without considering it necessary to have any special knowledge of the subject, may be due to ignorance of the nature of such problems, and also to the existence of non-intellectual factors influencing their beliefs. There is a confusion between the practical attitude of everyday life which cannot wait to have its problems 'solved' for it by the laborious processes of the intellect, and the purely intellectual attitude whose concern is solely for the truth and which, if it is to be faithful to this demand must needs have infinite patience.[1]

It is not difficult to see other reasons for the existence of the traditional conflict between natural science and metaphysics, some resulting from faults on one side, and some from faults on the other. First, there is diversity of *interest*. The type of mind which is attracted to a special science will differ from one which is attracted to metaphysics. We all like to feel that what *we* are doing is important, and we are apt to think that what we do not understand or are not interested in is not important. It will be easy for one who has

[1] Cf. G. F. Stout, 'In intellectual morality the fundamental virtue is patience,' *Analytical Psychology*, I, 242.

the gifts and good fortune to make brilliant discoveries about which a great many people can be thrilled, to feel more important than one who generalizes about other peoples' discoveries, and whose theories may only be understood by a few. But there are people who are more gifted for making general interpretations than particular discoveries, and it seems absurd to make distinctions about the relative importance of workers in different branches, or at different levels of generality, of the fabric of knowledge, when they are so mutually dependent upon one another. It is largely because this mutual relation is so little realized that such distinctions are made. But the history of science is abundant with instances of this mutual interdependence of theory and investigation, and Bacon has beautifully expressed his ideal of what their relations should be:

> 'Those who have treated of the sciences have been either empirics or dogmatical. The former like ants only heap up and use their store, the latter like spiders spin out their own webs. The bee a mean between both, extracts matter from the flowers of the garden and the field, but works and fashions it by her own efforts. The true labour of philosophy resembles hers, for it neither relies entirely nor principally on the powers of the mind, nor yet lays up in the memory the matter afforded by the experiments of natural history and mechanics in the raw state, but changes and works it in the understanding. We have good reason therefore, to derive hope from a closer and purer alliance of these faculties (the experimental and the rational) than has yet been attempted.'—*Novum Organum*, lib. I, 95.

When we consider how easy it is for a specialist to get his own little bit of experience out of perspective an intellectual world peopled only by specialists becomes a horrible object to contemplate, and we turn a more sympathetic eye in any direction which offers a counterpoise to the inevitable defects of professional specialism. The need for the alliance of which Bacon speaks is even more urgent at the present day than it was at the time in which he wrote, when it was still possible for a man to 'take all knowledge for his province.' To-day when such a thing is utterly impossible the need for mutual help and understanding becomes imperative if knowledge is not to degenerate, as Professor Whitehead says, into 'a medley of *ad hoc* hypotheses' and remain so. The belief that it was possible to attain to certainty upon metaphysical questions by merely thinking about them without regard to empirical data does not appear to have been at any time so prevalent as one would suppose from those who write about a conflict between science and metaphysics. Each age does the best it can with the data at its disposal, and its metaphysic will reflect the relative importance it attaches to the different elements presented to it. It does not appear to be the duty of a particular science to decide such questions. The danger that has arisen from metaphysics in the past has depended upon the ease with which it is possible for metaphysical notions to influence empirical inquiries, and we shall find that it is possible to trace the influence of such assumptions at the present day operating unnoticed because they are entertained unconsciously, or their metaphysical character is not understood.

The truth is, of course, that it is impossible to begin work in a particular field without *some* pre-suppositions about its nature, and about how to set to work. The sciences arose on the basis of the preliminary work of this kind which had been done by pre-scientific thinkers, and is enshrined in common-sense. It is in connexion with methods of approach, methods of thinking and abstracting, that *demands* and hypotheses play such an important rôle. Some of these demands may be metaphysical in the sense that they lead us to make certain assumptions about the character of the whole field of study which cannot be put to a decisive empirical test. Instead of waiting to find out what that general character is after a good deal of work has been done, certain *assumptions* are made about it at the start. The history of science shows that progress has very largely depended upon the skill with which a few men of genius have grasped the kind of assumptions it was necessary to make. But these facts are by no means so widely understood as they should be, and consequently many people do not realize to what an extent such assumptions underlie scientific procedure, what their real character is, or how they have been arrived at. It is necessary that such buried assumptions should, from time to time, be dragged out into the light, in order to remind us how much our theoretical conclusions depend upon them, and to enable us to see whether they are still performing their proper functions and not leading us astray. Thus a science should be *conscious* of the assumptions and demands upon which it rests, but it seems that in natural science at the present day it is only physics—or rather its best representatives—that can be said in this way to be ' self-conscious '.

An important consequence of these facts is that a given set of assumptions will commit us to a certain circle of thought from which the only way of escape will be by overhauling the assumptions themselves. But when a particular way of thinking has got deeply embedded it becomes extraordinarily difficult to extricate oneself from it, and even if a man succeeds in overcoming the resistance of his own mind in this way tradition will still keep the general trend of thought in the same groove. When we hear of this or that institution creating obstacles to scientific progress we must also remember that scientific tradition may itself become an obstacle. 'The victory of the categories,' writes Professor Hobhouse, 'is not established without a struggle, and like other victories it ends in a dictatorship under which death or exile is the penalty of recalcitrance.' Thus a thinker who is not prepared to criticize his demands and assumptions or 'suffer them to be doubted of' is thought-bound. But to-day we are witnessing an attack upon the categories which is likely to have consequences as far reaching as the categories themselves are deep-seated, and we cannot meet criticism by merely turning a deaf ear. Nor shall we be able to benefit by the fruits of such criticism if we do not take the trouble necessary to understand them. It will be useful in what follows to distinguish two ways in which such fundamental notions may be entertained. For a given science they should always be adopted only as guides for investigation—not as solutions or conclusions—otherwise science would be deserting its proper function, and would merely be solving its problems 'metaphysically' in advance of empirical inquiry. Accordingly we shall say that for a given science all such assumptions are held *methodologically*, but where they are held to be true of reality then they will be said to be entertained *metaphysically*.

This distinction between the methodological and the metaphysical use of notions is an extremely important one and will play a considerable part in later discussions. But so often are these two points of view confused in biological controversy that it will be desirable to devote a little more attention to the meaning of the ambiguous term 'metaphysics'. In scientific literature it is not uncommon to find this term employed for *any* theory or notion which is at all out of the routine of common-sense, or traditional scientific, thought, and this has the unfortunate consequence of perpetuating the belief that they are in some way eternally antagonistic to one another. Sometimes, indeed frequently, the term is used as though the adjective 'metaphysical' were in same way opposed to 'materialistic'. This is illustrated by the following passage :

> 'Here we must pass by the question, to be faced in a later chapter, how we can steer between a metaphysical Scylla and a materialistic Charybdis.'[1]

This introduces confusion into the very beginning because it sets a part in opposition to the whole since materialism is a *particular* kind of metaphysical theory in the sense described above. It has, it is true, become associated with natural science with special intimacy because natural science is concerned with what is called the material world. But a theory about the material world can be scientific without being materialistic in the sense in which that term is commonly understood, as is indeed the case with much of modern physical theory. The term metaphysics or metaphysical is applied by some authors to any theory which attempts to go beyond the immediate data of sense. This appears to be the meaning attached to the term by such writers as Ernst Mach and Karl Pearson whose views will be examined in detail in Part I. The expression, 'immediate data of sense', is ambiguous, but, in the sense in which these authors understand it, most if not all our scientific theories as commonly understood would be metaphysical—a fact which is not always clearly appreciated by many who quote their works with approval. Mach and Pearson hold that such theories are not to be held as true of reality but should be regarded as mere devices for imposing order upon sense-experience. But their view is itself based on a theory of the nature of sense-experience which is not very commonly held at the present day by philosophers.

Let us look now at some opinions of philosophers themselves about the meaning of the term 'metaphysics'. They, presumably, will know what they are talking about. The well-known remark of William James that 'Metaphysics means nothing but an unusually obstinate effort to think clearly'[2] gives emphasis to one aspect by which metaphysics is distinguished from science. It does not imply that 'clear thinking' is not required in science. The emphasis is on the

[1] P. Geddes and J. A. Thomson, *Biology*. London, 1925, p. 10.
[2] *Principles of Psychology*, London, 1890, Vol. I, p. 145.

' unusually obstinate ' nature of the effort and refers to the fact already mentioned that metaphysics, in so far as it attempts to embrace the whole of experience cannot lighten its task by ignoring anything. Professor W. P. Montague writes that:

> ' Metaphysics is concerned with all questions of a general and fundamental character as to the nature of the real.'[1]

He then proceeds to distinguish two kinds of metaphysics—analytical and synthetic. He says that analytical metaphysics or Ontology is ' the study of the basic categories of the sciences '. This appears to be much the same as what Professor Whitehead means when he says:

> ' By " metaphysics " I mean the science which seeks to discover the general ideas which are indispensably relevant to the analysis of everything that happens.'[2]

Professor Montague applies the term synthetic metaphysics or Cosmology to ' the study of the generic conclusions of the sciences . . . which by the interrelating of these produces a unified picture of the world as a whole '. This last meaning of the term clearly marks off at least a possible science—a synthesis of knowledge professing to give us true information about the mutual relations of the various aspects of our experience. And it is in this sense that I have been using the term. The problem is to make clear the distinctions between this and certain other sciences which are related to and apt to be confused with it. The principal difficulty for the modern world arises from the fact that it is believed that a prior question has to be faced before we can attain to such a universal science—quite apart from the question whether we have enough data for such an attempt. And this prior question relates to the nature and trustworthiness of knowledge itself. We are reminded at every turn, as soon as we begin to reflect, that it is the world *as known* that we are dealing with, and that the interpretation of the content of knowledge cannot be wholly divorced from the inquiry into the nature of knowledge. Now this latter inquiry—or epistemology as it is called at the present day—is included by some within metaphysics and is included, I think, in the above definitions of metaphysics by Professor Whitehead, and of analytical metaphysics by Professor Montague. It is because I wish to

[1] *The Ways of Knowing*, London, 1925, p. 31.
[2] *Religion in the Making*, Cambridge, 1926, p. 48.

keep these two points of view separate as far as is possible that I am anxious to confine the term metaphysics to the sense I have already explained. It is extremely difficult to convey what is meant in a few words at the outset, and I must rely on the indulgence of the reader to permit me to do so in the course of this Introduction and in Part I. Meanwhile an example will perhaps make things a little clearer. We hear a great deal to-day about different kinds of 'space'. We hear of Euclidean and non-Euclidean space. People also speak of visual and tactual space, as well as of physical space. Now the question of the existence and nature of physical space I should suppose to be a metaphysical one, but the problem whether visual space is to be called space at all and what its relation to physical space may be I should call an epistemological problem. And any particular notion of space employed in physical science for descriptive purposes, independently of its metaphysical interpretation, such a notion I should say is employed methodologically, i.e. simply for the purpose of investigation, which can be carried out quite independently of the difficulties raised by the more 'unusually obstinate effort to think clearly' which a metaphysical theory of space would require.

6

So much, then, for what I intend to mean by the word metaphysics, and for what I understand to be its relation to the special sciences. Another term which is apt to arise in connexion with discussions about the logic of science is 'philosophy'. Like the terms 'science' and 'metaphysics' this term also appears to be used very loosely and ambiguously. Sometimes it seems to be used as a synonym for metaphysics, and sometimes as a wider term which includes metaphysics as a part. It is most frequently employed to include a group of sciences having certain common characteristics as regards subject-matter and method and excluding the natural sciences. What, then, is the distinction between the 'natural' and the 'philosophical' sciences? We have noted that in the natural sciences the minds which pursue them are brought into relation with their subject-matters through sense-perception, and the distinction might be based upon this. Professor Whitehead, for example, says: 'Nature is