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CAN BIOLOGY BE AN EXACT SCIENCE?

Both biologists and philosophers sometimes puzzle themselves about the status of the biological sciences.[1] They wonder why biology does not seem to have the mathematical precision and systematic unity of the physical and chemical sciences, and they ask whether perhaps in the future biology could be brought into such a precise and unified form. Partly for this reason, J. H. Woodger has tried to axiomatise genetics.[2] Such an attempt resembles, superficially at least, such an axiomatising of particle mechanics as has been carried out by McKinsey, Sugar and Suppes.[3] (Indeed Woodger is in a sense even more ambitious than McKinsey, Sugar and Suppes, as the latter writers base their axiomatisation on unformalised set theory, whereas in Woodger everything, including the relevant parts of set theory, is presented in a formalised way.) However, there has always been a very odd look about attempts to treat biology on the model of physics. They stem, I believe, from a profound misconception of the biological sciences (including psychology.[4]) This misconception is that biology is a science of the same general sort as physics, just as chemistry is. Of course chemistry is not as fundamental a science as physics, for ultimately we hope that there will be an explanation of all important chemical laws in terms of physics, as by the quantum theory of the chemical bond. Nevertheless though chemistry is not so fundamental a science as physics, I wish to point out a sense in which chemistry is, and biology is not, a science of the same general sort as physics. I shall try to show that the important analogy is not between

[1]) An earlier version of this paper was read to the Biological Discussion Group organised by Professor W. P. Rogers in Adelaide University. In this version I hope I have benefited from the subsequent discussion in which many interesting and stimulating points were made.

[2]) J. H. Woodger. *Axiomatic Method in Biology*, Cambridge University Press, 1937.

[3]) J. C. C. McKinsey, A. C. Sugar and P. Suppes, 'Axiomatic foundations of particle mechanics', *Journal of Rational Mechanics and Analysis*, Vol. 2, 1953, pp. 253–277.

[4]) For an illuminating analysis of psychological method, which is in general harmony with the present treatment of biology, see B. A. Farrell, 'On the limits of experimental psychology', *British Journal of Psychology*, Vol. 46, 1955, pp. 165–177.

biology and the physical sciences but between biology and some of the technologies, for example electronics. At once, however, I must forestall a possible misconception. In drawing an analogy between biology and electronics I do not intend to suggest that biology is an applied science. It is in the logical structure of their explanations that I wish to draw the analogy between biology and electronics, and so the analogy will hold, if it holds at all, even when we are considering the most practically useless parts of biology. I do not of course deny that much of biology is like physics in that it is pursued primarily for the sake of intellectual satisfaction, not on account of its applications. It is in quite a different respect that I shall find the analogy between biology and electronics.

Physics and chemistry have their *laws*, which are often called in honorific fashion 'laws of nature'. For example there are the laws of motion in classical mechanics, the laws of the electromagnetic field, the laws of thermodynamics, and the equations of quantum mechanics. In chemistry there are the innumerable laws expressed by chemical equations. There is one important feature of all these laws which it is most important to stress. These laws are universal in that it is supposed that they apply everywhere in space and time, and they can be expressed in perfectly general terms without making use of proper names or of tacit reference to proper names. Such laws I shall call 'laws in the strict sense'.

Biology, it seems to me, does not contain, as peculiar to itself, any laws in the strict sense. It does contain, of course, innumerable generalisations. Even Mendel's laws, it will turn out, are generalisations rather than laws in the strict sense. For the moment, however, let us consider a more homely looking example. Consider the proposition that albinotic mice always breed true.[1]) What are *mice?* They are a particular sort of terrestrial animal united by certain kinship relations. They are defined as mice by their place on the evolutionary tree. The word 'mouse', therefore carries implicit references to our particular planet, Earth. In other words, the definition of 'mouse' involves the proper name 'Earth'.

Provided we make use of this proper name, or of some equivalent device, such as uniquely referring definite descriptions, in the definition of 'mouse', then our law 'Albinotic mice always breed true' is in the logician's sense perfectly general. It can be put in the form

'(x)(x is an albinotic mouse $\supset$ x breeds true)'.

[1]) Cf, for example, H. Kalmus, *Genetics*, Pelican, England, 1948, p. 58.

The law applies as truly to a mouse on Mars or on some planet in the Andromeda nebula as it does to a mouse on earth. Indeed the proposition

$$\text{'}(x)(x \text{ is an albinotic mouse in Andromeda} \supset x \text{ breeds true})\text{'}$$

is trivially true, in so far as a mouse-like creature in Andromeda would not, by definition, be a mouse. (The sentential function 'x is an albinotic mouse in Andromeda' is true of nothing at all.)

Because the proposition

$$\text{'}(x)(x \text{ is an albinotic mouse} \supset x \text{ breeds true})\text{'}$$

involves implicitly a proper name (Earth) it is not a law of nature in the strict sense. The laws of physics and chemistry need no such word as 'terrestrial' for their elucidation. Could we not, however, define 'mouse' in a different way, without reference to a place or a particular evolutionary tree, but by specifying a necessary and sufficient set of properties? Let us see what happens if we adopt this course. Suppose there is a certain set of properties $A_1, A_2, \ldots A_n$ possessed by all mice and only by mice. Thus A_1 might be the property of being four-legged or of being very closely related to a four-legged animal. (This last clause is put in to take care of occasional freak mice with three or five legs.) No doubt we could find such a set of properties that all and only mice possessed.

The trouble is that we now have no reason to suppose our law to be true.

$$\text{'}(x)((A_1x \,.\, A_2x \ldots A_nx \,.\, \text{albinotic } x) \supset x \text{ breeds true})\text{'}$$

is very likely false. May be the universe is infinitely big, in which case, to make use of a remark of F. Hoyle's,[1]) somewhere in the depths of space there must be a cricket team to beat the Australians! (Indeed an infinite number of such teams.) In any case the universe is very big indeed, and on some planet belonging to a star of a remote galaxy there may well be a lot of animals with the properties $A_1, A_2, \ldots A_n$ and of being albinotic but *without* the property of breeding true. Or consider such general propositions of biology as those describing the process of cell division. Is it not very likely that in distant nebulae there are organisms whose cells divide according to rather different methods?

My conclusion so far is this: that if the laws of biology are made general

1) F. Hoyle, *The Nature of the Universe*, Basil Blackwell, Oxford, 1950, p. 95.

in the sense not only of being preceded by universal quantifiers but also in the sense of not containing any words whose elucidation requires proper names or similar devices, then such laws are very likely not universally true. The laws of physics, by contrast, seem to be truly universal. That is, they can be expressed in the form of universally quantified propositions which neither explicitly nor implicitly make use of proper names or refer to our own particular station in space and time. Why is there this difference? Part of the answer, at any rate, seems to be this. The physicist (and to a lesser degree the chemist) talks about entities which are relatively *simple* or else *homogeneous*. Thus classical particle mechanics deals with point masses, and rigid mechanics can proceed because, for example, it can be shown that a homogeneous sphere behaves gravitationally like a point mass of equivalent mass at its centre. In rigid mechanics we do not have to take account of the admittedly very complex minute structure of any actual rigid body. Again, the physical properties of the atom are explained because the theory of the atom can be reduced to that of still simpler particles such as electrons, protons and neutrons. It is important that such small, simple constituents are believed to be ubiquitous in the universe. In this respect electrons and protons are not like albinotic mice or even, say, haploid cells.

What about macroscopic laws such as the gas laws and the laws of thermodynamics? These occur in physics because of statistical averaging, and once more depend on homogeneity and the essential absence of fine structure. We can treat a gas as a homogeneous sort of thing in a way in which we cannot so regard a cell or a radar installation. There are, I would submit, no laws in the strict sense about organisms because these are vastly complicated and idiosyncratic structures. No one expects all motor cars of a certain make and year to behave exactly alike. Yet a motor car is a very simple structure compared with even a single living cell. Still less, therefore, should we expect to find laws (as opposed to generalisations) about organisms. Even if such generalisations should turn out to have few exceptions in our terrestrial experience, it would be rash in the extreme to suppose that they have universal validity in the cosmos. A generalisation of biology is thus even unlike such approximate laws of physics as Boyle's law and Newton's law of gravitation. Boyle's law is very nearly valid except when the pressure of a gas is high, and Newton's law is very nearly valid except in the region of some very massive body.

That is, we can specify the circumstances in which such a law breaks down, and provided these circumstances do not obtain we suppose that the law is applicable even in the furthest nebula. Consider, on the contrary, such an apparently fundamental law of biology as that of Mendelian segregation. Even on earth populations never segregate quite according to the Mendelian principle, for a multitude of reasons of which the phenomenon of crossing over is the chief. Even if we tried to protect our law, by adding clauses such as 'provided there is no crossing over', we should be pretty sure to be caught out by queer mechanisms of reproduction obtaining on other spheres.

An analogy may help to bring out the implausibility of the supposition that in biology there could be laws in the strict sense. Consider a certain make of radio set. Can we expect to find universal truths about its behaviour? Surely we can not. In general it may be true that if you turn the left hand knob you get a squeak from the loud speaker, and that if you turn the second knob from the right you get a howl. But of course in some sets this will not be so: a blocking condenser may have broken down or a connection may have become loose. It may even be that the wires to our two knobs may have been interchanged by mistake in the factory, so that it is the left hand knob that produces the howl while it is the one second from the right that produces the squeak. And if there are no universal truths about all radio sets of a certain make, still less are there universal truths about, say, *all superheterodynes*. Here we must except, of course, such universal truths as may be true by definition. Thus if we define 'superheterodyne' as 'set containing a frequency changer' then of course all superheterodynes contain frequency changers. But this does not enable us to settle the question of whether the particular piece of hardware before us, with 'superheterodyne' written on it, in fact contains a frequency changer. A mistake may have been made at the factory and it may be working on some quite other principle. (When I was a boy I once made what was meant to be a straight two-valve receiver but which by some queer quirk of construction, which I never tracked down, functioned on the super-regenerative principle.)

From a logical point of view biology is related to physics and chemistry in the way in which radio-engineering is related to the theory of electricity and magnetism, and so on. Biology is not related to physics and chemistry in the way in which one branch of the physical sciences is related to another. Just as the radio-engineer uses physics to explain why a circuit with a certain wiring diagram behaves as it does, so the biologist uses physics and chemistry to explain why organisms or parts of organisms (e.g., cell nuclei), with a certain natural history description, behave in the way they do. A very large, if not preponderant, part of biology consists in these natural history descriptions. We must not think of natural history as being merely about such things as lions and tigers, gum trees and bamboos. It is also about nucleoli, mitochondria and other such small entities. The description of these small entities is logically on a par with generalisations about tigers rather than with a physical law of nature. Descriptive biology consists in generalisations of natural history, not laws in the strict sense. So while, roughly speaking, radio-engineering is physics plus wiring diagrams, so biology is physics and chemistry plus natural history.

On this view biology is ultimately biochemistry and biophysics. Of course some biological explanations fall short of this ideal. We may explain a phenomenon as due to some hormone though we do not know the chemical nature of this hormone, or the ways in which it enters into chemical reactions, nor in detail how these chemical reactions tie up with the general functioning of the organism. Again a great deal was done with the notion of the gene before it was known that a gene is probably a DNA molecule. Even now practically nothing is known of the chemistry of such molecules. To pursue the analogy with radio-engineering, in a great deal of biology we have got down to a 'block diagram', but are far from knowledge of the detailed wiring diagram. Our explanations are therefore very partial and tentative.

If it is asked whether biology can be made an exact science, the answer is: 'No more and no less than technology'. If by an 'exact science' is meant a science with strict laws and unitary theories of its own, then the searcher after an exact science of biology is on a wild goose chase. We do not have laws and theories of electronics or chemical engineering, and engineers do not worry at the lack. They see that their subjects get scientific exactness from application of the sciences of physics and chemistry. No one wishes to axiomatise mechanical engineering. Why should Woodger wish to axiomatise genetics? There are no true laws of biology for the very same reason that there are no special 'laws of engineering'.

This conclusion is, I think, borne out by the different way in which mathematical statistics characteristically enters into biology from that in which it characteristically enters into physics. The imposing look of the mathematics tends to blind us to this important difference and so to lead us to suppose that biology has its unitary and special theories just as physics has. In biology we frequently have to decide whether an experimental result is significant. Let us take a crude example. Suppose we put some mineral into the soil and we find that we get bigger cabbages than before. Is this due to what is put into the soil? After all, the cabbages are going to vary in size, to some extent, anyway, and it may be merely by chance that we have got a bigger lot on this occasion. Statistics may enable us to calculate the probability of our big cabbages having turned up purely by chance, and if this probability is small we may become fairly confident that the new mineral helps to make bigger cabbages. In genetics such reasonings as to the likelihood or otherwise of accidental factors producing the experimental results can be very subtle and intricate. Mathematical statistics helps the biologist to get at the reality which is masked by chance variations. Let us call this the 'extra-theoretical' use of statistics.

The mathematical statistics which occurs in physics can be very subtle too. Consider Boltzmann's gas theory. Here, however, the statistical reasonings have a different function. They are not used here in order to estimate the significance of experimental results, to pull aside the curtain of chance variations. They are used in order to explain how a multitude of randomly varying microscopic events can average out so that we get definite macroscopic laws. Thus the use of statistics in gas theory (and other parts of physics, too) is different from its characteristic use in biology, and this ties up with my characterisation of biology as a science without laws of its own in the strict sense. Let us call this second type of use of mathematical statistics which we find in gas theory the 'intra-theoretical' use of statistics.

Certain qualifications to the last paragraph now have to be made. I do not wish to deny that in physics, often enough, statistics is used in the sense of the theory of errors, that is in the extra-theoretical way. Contrariwise, in the theory of evolution we have studies of the spreading of genes in populations and here we find an intra- theoretical use of statistics. There is also an intra-theoretical use of statistics in ecology. Here there are two interesting lessons to be noticed. The theory of evolution and ecology are two branches of biology which are quite obviously 'historical' in nature. They are concerned with a particular and very important strand of terrestrial history. No doubt there are analogous histories on remote planets, but in the theory of evolution we are concerned with the hereditary relationships of those particular species we observe, and so we are not concerned with laws in the strict sense. (If we try to make laws of evolution in the strict sense we seem to reduce to tautologies. Thus suppose we say that even in Andromeda 'the fittest will survive' we say nothing, for 'fittest' has to be defined in terms of 'survival'.) Let us now turn to the extra-theoretical use of statistics in the physical sciences. It is perhaps not entirely without significance that if we think of the theory of errors as applied to the physical sciences, the first thing that is likely to come into our heads is astronomy. Now astronomy is very much a history rather than a science analogous to physics, though it of course makes use of sophisticated physical theories. It is concerned very largely with the explanations of the particular facts of the celestial universe. However this point must not be pressed too far, because we can think of the stars not as particular objects of hstorical interest but as huge laboratories for the testing of physical laws. Similarly the planets once acted as test bodies for the testing of the laws of dynamics. In such cases astronomy becomes a part of physics, rather than vice versa: that is we are interested in the particular facts because they test our theories. (In the other type of case we are interested in physical theories because they explain the particular historical facts about celestial bodies.) The distinction between the historical discipline of astronomy and the purely general study of physics would become even more blurred if physics should turn out to have a cosmological basis (as is to some extent the case with general relativity) in which case physics itself would turn out to be 'historical' in nature. But even then there would still be a vast difference between physics and what we are accustomed to regard as natural history.

I have tried to argue, then, that those parts of biology which use intra-theoretical statistics are those that are most obviously historical in nature, and so the comparison with, say, the kinetic theory of gases must be a superficial one. I have also tried to argue that extra-theoretical statistics occurs characteristically in those physical sciences that are most historical in nature. My conclusion is that the occurrence of sophisticated mathematics in biology does not refute my general diagnosis, which is that biology does not stand to physics as chemistry does, or as one branch of physics does to another. We should not expect to find 'biological theories' but rather the application of physics and chemistry to explaining the generalisations of natural history.

Someone may feel puzzled at this stage. It looks as though I have drawn a line through nature, and yet the difference between an atom, a molecule, a virus, a cell, and an animal seems only one of increasing complexity of physical structure. The answer is that the line I draw is a methodological one rather than an ontological one. The difference is that between using propositions of observational fact in order to test laws and using laws in order to explain propositions of observational fact. Thus in physics we may look to see if there is an electric charge inside a closed conductor, and by this test (with appropriate Duhemian qualifications) the inverse square law of attraction stands or falls. At least in the days when the experiment was a 'live' one, the physicist was more sure of his observations than of the inverse square law. In biology, on the contrary, we are more sure of the laws (the laws of physics and chemistry) than we are of the initial conditions, the 'wiring diagram'. Clearly there must be places in which we are equally unsure of the relevant laws and of the initial conditions, and here our distinction will become blurred.

Again, I must now slightly qualify my previous classification of chemistry with physics. Consider the chemist who studies the structure of a protein molecule by means of X-rays. Here he can be more sure of the basic chemical laws that determine the structure than he is of the structure itself. It is the structure, therefore, that he sets out to determine by means of his observations, and the study of protein molecules takes on much of the logical nature that I have been attributing to biology. Conversely, if viruses of a certain sort should be discovered to be nothing more than a certain specific macromolecule whose formula were known, then the theory of such viruses would be chemical in nature, and there would be chemical laws of their functioning. (Though they might be too complicated for us to ascertain. Moreover the macromolecules might be susceptible to being squashed, twisted or bent, in which case the laws of their functioning might not depend on the chemical formula alone.) There is not a sharp division in nature between the objects of physical type science and those of biological type science: the difference is one of methodology. In the former we are interested in laws, whereas in the latter we are interested in the natural history of structure (e.g. wiring diagrams) and in the explanation of why things with this natural history function as they do.

Though there is not a sharp division in nature between the objects of the physical sciences and those of the biological sciences, there is, of course, a non-sharp division, which is one of complexity of structure. The methodological division does reflect this non-sharp division in reality.[1])

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[1]) And so my views would appear to be very much in harmony with the general position of Joseph Needham in his book *Order and Life*, Cambridge University Press, 1936. On the difference between biological generalisations and physical laws see W. I. Matson's solution to *Analysis* Problem No. 12., *Analysis* Vol. 18, 1957–8, pp. 98–9.