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WHAT PHILOSOPHY OF BIOLOGY IS NOT*

Periodically through the history of biology, biologists have tried to do a little philosophy and occasionally a philosopher has turned his attention to biology. In the past decade or so a body of literature has arisen which might legitimately be called 'philosophy of biology'. The purpose of this paper will be to review the contributions made to this literature by philosophers during the past 10 or 15 years. Earlier work will be discussed only if it has proven especially influential. The contributions made by biologists to the philosophy of biology will be touched on only briefly, both because the biological literature is too vast to permit anything like a fair summary in the confines of a short paper and because the strengths and weaknesses of this literature tend to be quite different from the efforts of philosophers.[1] In this paper, though it is sure to result in acrimony, I have not refrained from criticism. There is too great a discrepancy between what philosophers produce under the guise of philosophy of biology and what philosophy of biology could be or, in my opinion, should be to pass over without comment.

One striking feature of the remarks made by philosophers about biology is how frequently they are misinformed. For example, Mario Bunge in a paper on the weight of simplicity in the construction and assaying of scientific theories asks the following question:

> What gave Darwin's theory of evolution through natural selection the victory over its various rivals, notably creationism and Lamarckism? Darwin's theory was in part logically faulty (remember the vicious circle of the "survival of the fittest"); it contained several false or at least unproven assertions ("Each variation is good for the individual", "Acquired characters, if favorable, are inherited", "Sexual selection operates universally"); it had not been tested by observation, let alone by experiment on living species under controlled conditions (the development of antibiotic-resistant strains of bacteria, industrial melanism in butterflies, and a few other processes supporting the theory, were observed one century after *The Origin of Species* appeared); its explanatory power was clearly smaller than that of its rivals (irrefutable theories have the maximum *post factum* explanatory power); it had no inductive basis but was, on the contrary, a bold invention containing high-level unobservables. And, if these sins were not enough to condemn the theory, Darwin's system was far more complex than any of its rivals. ...[2]

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Critics of evolutionary theory seem evenly divided on the question of whether the survival of the fittest is false or tautological. In spite of the tiresome regularity with which this claim is made, it has little foundation. Leading evolutionists from Darwin to G. G. Simpson and Ernst Mayr have provided excellent explanations of why this principle is neither tautological nor viciously circular.[3] Fitness in evolutionary theory is a relative notion. Certain organisms in a given environment are fitter than others. A higher percentage of those organisms which are nearer the 'fittest' end of the scale tend to survive than those at the other end. This scale in turn is ordered at least in part independently of the actual survival of these individuals. Of course, the claim that the fittest tend to survive *can* be made viciously circular if fitness were determined only by means of actual survival or into a tautology by defining 'fitness' exclusively in terms of actual survival, but biologists do neither. (This issue will be discussed in more detail later in conjunction with Anthony Flew's attempt to reconstruct Darwin's theory.)

Bunge contends that Darwin held several false or at least unproven assertions. Regardless of whether they are false or unproven, Darwin held none of them. For instance, Darwin maintained from the first that the variations which result in rudimentary or atrophied organs are not good for the individual. Such organs were imperfect and useless.[4] Darwin did believe that "use and disuse seem to have produced some effect" but that there "is not sufficient evidence to induce us to believe that mutilations are ever inherited".[5] Nor did Darwin believe that sexual selection operated universally. He limited sexual selection just to animals with separate sexes, and among them the struggle between the males for possession of the females occurred only in most cases. Further, not all differences between males and females of the same species were due to sexual selection.[6]

Bunge says that industrial melanism in butterflies was observed a century after *The Origin of Species* appeared, when actually two long monographs were published by Tutt in 1890 (only 30 years after the *Origin*), in which he argued that melanism and melanochroism in British *Lepidoptera* were due to a combination of moisture and smoke under the action of natural selection.[7] And, although it might seem an insignificant point to a philosopher, almost all of the observations were on moths, not butterflies.

Finally, Bunge asserts that the theory of evolution through natural selection had no inductive basis. One wonders what Bunge can mean by 'inductive basis' if Darwin's 30 years' labor in attempting to support his theory and all the data marshalled in the *Origin* provided no inductive basis. Only the first and last of the points raised by Bunge are especially important to the philosophy of science, but his obvious disinterest in evolutionary theory as a biological theory is discouraging. It is certain that he would not treat quantum theory in such a cavalier fashion. The differences between mesons and pions are important. The differences between moths and butterflies apparently are not.

A second example of what can happen when a philosopher does not have sufficient understanding of the views which he criticizes is provided by Peter Caws when he terms the interbreeding criterion of the biological definition of species 'artificial'. Although he recognizes that the reason for the similarity of the members of a class of living things is that they have a common ancestry, he adds:

> Even in this case, the borderlines between species have at times been very difficult to draw, and some rather artificial criteria (such as the ability to produce offspring together) have been used for judging whether or not a pair of individuals belong to the same species.[8]

In the first place, ability to produce offspring together is not a criterion for judging whether or not a pair of individuals belong to the same species. Occasional hybridization between individuals of different species can and does occur. This does not mean that these individuals belong to the same species, nor does it invalidate the biological species concept. 'Interbreeding' as it appears in the biological definition of species applies to populations, not individuals, and is used to decide which classes or organisms (which taxa) are to count as species, not which individuals belong to the same taxon. Membership in a taxon is determined by phenotypic characters, mainly morphological. Whether or not that taxon is to be considered a member of the species category is determined by various evolutionary relationships, in particular interbreeding habits. (The taxon-category distinction which underlies the confusion in which Caws finds himself will be discussed later.)

Perhaps because of his failure to understand the role of the ability to produce fertile offspring in the biological definition of species, Caws terms this criterion 'artificial'. Numerous objections have been raised to the biological definition, most frequently that it is too difficult to decide when the criterion of potential interbreeding is fulfilled. According to Mayr, two populations are potentially interbreeding whenever they are prevented from breeding only by geographical isolation. If any isolating mechanism is operative, they are separate species.[9] But even the most persistent critics admit that the mechanism of interbreeding is central to the evolutionary development of sexually reproducing organisms. If the ability to produce fertile offspring is an artificial criterion in deciding what is to count as evolutionary units, one wonders what a natural criterion would be like.[10]

Numerous philosophers have taken an interest in evolutionary theory because of the consequences which they see in it for man. A book entitled *Evolution and Philosophy* by Andrew G. van Melsen is typical.[11] Van Melsen's main thesis is that natural science deals only with the 'outside' of reality, but there is also an 'inside' to reality which the natural sciences cannot touch. This 'inside' is manifest in man, who has direct access to the 'self', the primordial datum. Anything a physical scientist might tell us about man "is peripheral to this primordial datum". The existence of this 'self' is responsible for one of the gaps in the scale of nature, but there are others. Just as there is an abyss between man and (other) animals, there is an abyss between the living and the non-living. Some authors of this bent also mention a hiatus between plants and animals, but for some reason Van Melsen plays down this gap. The increasing complexity of externally observable structure is paralleled by a "growing interiority culminating in man's self-consciousness". It is this 'interiority' that necessitates the recognition of these different levels of being.

What can possibly be said in reply to claims such as these? Of course, human beings are conscious and self-conscious. A biologist can be a mechanist-reductionist-neo-Darwinian without being a simple-minded behaviorist. Such biologists can also be aware of the epistemological distinctions which give rise to these and similar utterances. For example, G. G. Simpson says that "our human universes, the ones in which we really have our beings, depend at least as much on our inner perceptions as on the external, physical facts".[12] But the philosophers who emphasize the distinction between the internal and the external world in connection with evolutionary theory do so in order to assert that the existence of the inner world somehow counts against the sufficiency of evolutionary theory. It is difficult if not impossible to discern why man's consciousness and self-consciousness provide greater problems for evolutionary theory than any other so-called 'emergent' characters. Until these authors make their claims a good deal clearer, a biologist would be hard put to decide whether they conflict with, are extraneous to or are consonant with modern evolutionary theory.

As is frequently the case with philosophers who wish to insulate the 'self' from all inroads by science, Van Melsen finds evolutionary theory below par as a scientific theory. Even today Darwin's explanation of evolution lacks a solid foundation. "The theory of evolution in general is, as we have seen, essentially based on 'belief' rather than anything else." But he also discusses the evolutionary views of G. G. Simpson and Teilhard de Chardin as if they were on an equal footing scientifically. If the synthetic theory of evolution is based on belief, what must Teilhard's theory be based on? What kind of proof is necessary before a scientific theory can be accepted? Is there no difference between the evidence adduced in support of modern evolutionary theory and Teilhard's views? The difference between the two theories is not so much in their conclusions but in the evidence and arguments adduced by their authors to support them. Teilhard seems to have no conception of what a scientific theory is, what evidence is, or how evidence can be brought to bear on a scientific problem. And finally we are reminded that "the Aristotelian view remains valid even in the context of an evolutionary world view".

The literature of this genre is unfortunately extensive. It also follows a surprisingly rigid script. Characteristically it is argued that evolutionary theory is not 'proved'. After all, it is 'only a theory'. Scientists are chastized for being dogmatic and taking too much on faith rather than on the evidence. Rather than keeping an open mind on the matter, they plot to stifle all contrary opinions. The utterances of Pius XII, on the other hand, are in conformity with the best scientific tradition. Teilhard de Chardin is inevitably interjected into the discussion, either to show how his views are superior to those of the run-of-the-mill evolutionist or in order to tar all evolutionists with him for irresponsible speculation. W. R. Thompson (1966) takes the latter tack, lamenting "the infatuation of Catholic intellectuals for the Teilhardian pseudoscience".[13] But he seems to overdo it a bit when he goes so far as to accuse Teilhard of perpetrating the Piltdown hoax. Medawar (1967) concludes more cautiously albeit more condescendingly that Teilhard "had about him that innocence which makes it easy to understand why the forger of the Piltdown skull should have chosen [him] to be the discoverer of its canine teeth".[14] Also characteristic of this body of literature is the view that, in the last analysis, Aristotle was right. Aristotle's concept of Nature provides a badly needed philosophic dimension to biology. DNA exists. Hence, Aristotle was right.

One feature of efforts such as those of Van Melsen's to discover what consequences evolutionary theory has for man is that evolutionary theory as a scientific theory plays no role whatsoever in his exposition. All he would have needed to know in order to develop his thesis is that man evolved from other animals and that living creatures developed from nonliving matter. All the intricacy of evolutionary relationships, the difficulties with various mechanisms, the recalcitrant data, the wealth of supporting evidence are passed over. Whatever philosophy of biology might be, this is not it.[15]

One exception among those philosophers who are primarily interested in evolutionary theory because of its possible consequences for the nature of man is T. A. Goudge.[16] In his book *The Ascent of Life* he goes to the trouble of providing a philosophically oriented analysis of modern evolutionary theory. Since his views on evolutionary theory are not especially controversial and depart in no important respect from those of the biologists he cites in his preface (Carter, Dobzhansky, Haldane, Huxley, Mayr, Muller, Simpson, and Wright), one might wonder what point there could be in a philosopher going over the same ground. This perplexity can be dispelled quickly be reading the book. Goudge's whole approach to the subject is different from that of a scientist. He does not organize his exposition according to various kinds of empirical phenomena (e.g., kinds of species, isolating mechanism, hybridization, populations, genetic recombination, etc.) but according to the logically important differences to be found among the phenomena (e.g., the historical aspects of reconstructing particular phylogenetic sequences, the peculiar nature of historical explanations, the causal aspects of evolution and the systematic explanations made possible by certain evolutionary laws and law-like statements). Only after such an analysis does he turn his attention to the implications of evolutionary theory for man.

Goudge is very cautious in his assessment of the place of man in evolution, but the importance he puts on the question is indicated by the title of his book. He is interested in the ascent of life and whether man is at its forefront. Goudge argues that in spite of retrogressive periods, more diverse kinds of organisms are alive today than ever before and a higher percentage of more recent organisms are biologically more efficient than earlier organisms. As far as man is concerned, he is extremely flexible in his adaptiveness. He is *a* dominant type and *the* dominant primate. He is also "almost certainly the youngest species of mammal now on earth" and as such "there is a sense in which he is quite literally the highest species".[17] As cautious as these claims are, Goudge goes too far. There is no evidence to indicate that man is the youngest species of mammal and, even if he were, he would hardly thereby become the highest species. Perhaps the highest species of mammal, but hardly the highest species period. Man is very efficient, flexible in his adaptiveness, and so on. So are cockroaches. Man is unique. So are cockroaches. Only when Goudge leaves the realm of such strictly biological features of man does the uniqueness claim for man begin to carry some weight. Only man has developed culture and has been able thereafter to pass on information by more direct means than can other organisms. Hence, new possibilities and new difficulties have opened up for the future development of man.

Enough has been said, I think, to show how unsuccessful contemporary philosophers have been in extracting the consequences of biology for philosophy. What of the other side of the coin? Have philosophical analyses of biology provided any insights into biological phenomena, any clarity which biologists themselves have been unable to provide, a deeper understanding of biological theories? When philosophers have turned their attention to biology they have tended to limit themselves to a few issues – vitalism, teleology, reductionism, and related topics. One thing is obvious from this list. Philosophers have not been motivated in their choice of topics by any concern with issues currently of interest to biologists.

From the point of view of contemporary biology, both vitalism and teleology are stone cold dead. No better proof can be found than that offered by recent attempts to argue to the contrary.[18] In support of vitalism the observation is made that living creatures are not just matter but structured matter and that the world exhibits finality because regularities exist. The major problem with this defense of vitalism and teleology is that no materialist or mechanist ever held any differently. Even though much of the heat generated by these controversies was due to misleading formulations, it is hard to believe that the disagreements were entirely verbal. There is substance to these disagreements, and central to all of them is the role of 'principles' in science. For example, J. H. Randall in his attempt to rehabilitate Aristotle emphasizes that Aristotle's formal and final causes are principles of understanding.[19] They don't enter causally into the world of things. They don't *do* anything. Living creatures do what they do because of their organization and can be understood only in terms of their organization. All it takes for a world to exhibit finality is for there to be regularities and for the understanding of these regularities be increased by stipulating a stage in these sequences which can be thought of as 'final'. On this attenuated view only a totally chaotic world could be non-teleological and un-Aristotelian. To be sure, Aristotle's formal and final causes are principles of understanding, but in too many instances Aristotle explicitly has them doing things in the empirical world. Randall himself finds it difficult not to slip on occasion and have these principles doing things; for instance, he has *nous nousing nous*.

The views expressed by early materialists and mechanists were certainly overly crude, but to the extent that contemporary knowledge is applicable to the various stages of these controversies, the mechanist-materialists were right. The only remaining issue which is even vaguely related to these theses is the translation of teleological modes of expression into nonteleological language and the specification of the characteristics of those systems whose actions are frequently described teleologically. Both of these tasks are proving a good deal more intricate than one might at first expect.[20]

The status of reductionism (and related topics) is more equivocal. At a recent conference on the history and philosophy of biology sponsored by the Commission on Undergraduate Education in the Biological Sciences, the discussion from the floor repeatedly returned to the question of whether biology was in some sense reducible to chemistry and physics and what such a reduction entailed. Two things were made clear by these discussions – biologists and philosophers mean very different things by 'reduction' and are worried by very different aspects of the problem. There does seem to be at least one way in which the issues of interest to both biologists and philosophers can be joined. During the last 30 years, a process has been under way in genetics which fulfills all the requirements of what both scientists and philosophers have in mind when they speak of reduction. A biochemical explanation is being produced for phenomena which have been explained previously in terms of classical Mendelian genetics. Geneticists have not been 'reducing' Mendelian genetics to molecular genetics in the sense of providing translation rules. The relationship is too complex for that. For example, classical genetics speaks of things like "the gene for albinism". In molecular genetics there is no such thing as *the* or *a* gene for albinism. Any one of several changes in the genetic code (deletions, inversions, etc.) at numerous different loci can result in the failure to produce pigments. What is the molecular version of the Mendelian statement that in man the gene for albinism is epistatic to the gene which produces pigmentation? Do all phenomena which were previously explained in terms of recessive epistasis receive the same kind of explanation in molecular biology? Is the molecular explanation for dominant epistasis at all related to that for recessive epistasis? These phenomena are closely related in Mendelian genetics. Do they receive comparable explanations in molecular genetics? For any philosopher who is interested in the question of reduction, a detailed investigation of the inferential and definitional relations between these two theories would seem absolutely necessary, as careful an analysis as that which thermodynamics and statistical mechanics have received.[21]

Regardless of the outcome of the dispute over reductionism, there is certainly more to the philosophy of biology than whether or not biology can be reduced to chemistry and physics! During the last decade or so, biology has been forced briefly to the center of the stage in the philosophy of science because of the relevance of explanation and prediction in evolutionary studies to a remark made by Hempel and Oppenheim in their classic paper on the logic of explanation.[22] After setting out four conditions of adequacy for scientific explanation, they state that the same formal analysis applies to scientific prediction as well. Although almost every aspect of the Hempel-Oppenheim thesis has been subjected to extensive criticism, it is the symmetry thesis which has received the most sustained attack. The significance of biology for this controversy in the philosophy of science is that biologists frequently set forth what they take to be explanations of the evolutionary development of certain groups when they readily admit that they could not have made much in the way of any reasoned predictions in the matter. As gratified as biologists may be for the attention that one of their theories had at last received from first rate philosophers of science, the actual nature of evolutionary inferences was investigated none too intensively. What biologists actually did in producing certain putative explanations rapidly became peripheral to the issue of whether or not these formulations were truly explanations.[23] Was Hempel and Oppenheim's analysis purely description, explication or legislation? The introduction of evolutionary reconstructions into a controversy in the philosophy of science points up even more strongly the need for a careful and detailed study of the logical structure of modern evolutionary theory and its relation to historical data.

Periodically philosophers have discussed evolutionary theory and evolutionary reconstructions, but their treatments have left much to be desired. Abraham Kaplan considers evolutionary theory a concatenated theory in contrast to hierarchical theories like relativity theory, but he says little about the actual structure of concatenated theories.[24] A. G. N. Flew on the other hand contends that there is a deductive core to Darwin's argument in the *Origin*.[25] Several attempts have been made to specify precisely what this deductive core is. Flew cites Julian Huxley's formulation[26] but finds it 'curiously slapdash'. His own formulation is scarcely better. He says, for instance:

> Though the argument itself proceeds *a priori*, because the premises are empirical it can yield conclusions which are also empirical. That living organisms all tend to reproduce themselves at a geometrical ratio of increase; that the resources they need to sustain life are limited; and that while each usually reproduces after its kind sometimes there are variations which in their turn usually reproduce after their kind: all these propositions are nonetheless contingent and empirical for being manifestly and incontestably true. That there is a struggle for existence; and that through this struggle for existence natural selection occurs: both of these propositions equally are nonetheless contingent and empirical for the fact that it follows, necessarily as a matter of logic *a priori*, that wherever the first three hold the second two must hold also.[27]

Everything which Flew says is true, important and needs saying, with one exception. Neither of his conclusions follow deductively from the premises which he presents. All that can be deduced is that not all those organisms which are born will survive. Flew is aware that he has not presented a rigorous deduction. To do that "one would have to construct for all the crucial terms definitions to include explicitly every necessary assumption".[28] Though he himself does not attempt such a rigorous reconstruction, he believes that such an endeavor would be "an exercise which might prove instructive".[29] Anyone who undertakes this exercise will find that it *is* highly instructive and a good deal more than an exercise. The only step which Flew takes toward such definitions is to identify surviving to reproduce with being the fittest. He observes that if this identification is not made, the deductive argument which he has set out is no longer valid.

The question of whether certain basic principles of a scientific theory are analytically connected within that theory is neither new nor unique to evolutionary theory, but in order to connect fitness analytically with actual survival, a distinction fundamental to evolutionary theory and to science in general must be ignored. This distinction is the difference between what could happen, given the appropriate laws, and what actually does happen. For instance, given Newton's laws, a planet *must* revolve around a star in a conic section. Which of these possible paths the planet actually takes depends upon the particular make-up and history of that star system. Similarly, biologists want to retain the distinction between which organisms do in point of fact survive and those which have the greatest likelihood of surviving – and they define 'fitness' in terms of the latter notion. Every organism which could survive, given the appropriate laws, does not survive. 'Accidents' do happen and are frequently important in evolution, especially in small populations. The appropriate laws in this case are those of physiology, ecology, embryology, and so on.

It may well be true that in principle all macroscopic phenomena are governed by deterministic laws and that all these laws can be organized into a deductive hierarchy, but biologists do not have these laws. The laws which give substance to the claim that an organism which did not survive was nevertheless exceedingly fit are currently not deducible from evolutionary theory and are formally independent of it. Until the day that biologists can organize all of the relevant parts of biology into one grand deductive hierarchy and know all the relevant antecedent conditions for the evolutionary phenomena under study, not only are they entitled to retain the distinction between what organisms actually survive and those that are the fittest, they must.

Flew can be seen to vacillate on precisely this issue in his comments concerning the survival of the fittest. Sometimes he says that "actual or possible survival is to be construed as the sufficient condition of fitness to survive". Sometimes he says that "actual survival to reproduce is itself within Darwin's theory the sole and sufficient criterion for fitness to survive". Only from this latter assertion is he entitled to conclude that "it is as always the fittest who have survived, the fittest who do survive, and the fittest who will survive".[30]

When philosophers have turned their attention from evolutionary theory to evolutionary reconstructions, they have also found much to criticize. For example, is it true, as Woodger said many years ago, that phylogenetic explanation is "a historical explanation in the strict sense and one which could not be generalized because it would describe a unique series of changes characterizing an evolutionary succession" and that we are "in possession of no inductive generalization regarding the *modus operandi* of evolution of such high probability and generality as will justify us in asserting with any confidence what happened in an historical example"?[31] The evolution of *Hypohippus* was a unique event, but the occurrence of adaptive radiations, the invasion of new ecological niches, cases of convergent evolution and so on are not. Such phenomena are generalizable. Whether or not a particular species resulted from one or more of these processes is another story.[32]

It is impossible, however, in the space of a single paper to summarize all the various criticisms which have been made of evolutionary theory by philosophers. Rather I have chosen to discuss in detail the criticisms of a single philosopher, Marjorie Grene. I have chosen her work to discuss for the simple reason that no criticisms of evolutionary theory in the past decade or so have irritated biologists as much as those set out by Grene in her paper 'Two Evolutionary Theories'.[33] The reasons for this irritation throw some light, I think, on the shortcomings of philosophy of biology.

In her paper Grene contrasts the evolutionary theories of G. G. Simpson and O. H. Schindewolf.[34] Such an undertaking is perfectly legitimate and should have proved instructive, since Schindewolf's theory, unlike the musings of Teilhard, is a respectable scientific theory. Although the paper begins by setting out a fairly balanced account of the two theories, it gradually develops into a sustained attack on the orthodox neo-Darwinian views of Simpson. But the synthetic theory has been criticized before. Like all scientific theories it is under constant revision and re-examination, and in many instances the severest critics are found among those who consider themselves advocates of the theory. Why then did Grene's particular criticisms appear so offensive?

Grene's attack on the synthetic theory is divided into two parts. First she tries to establish that, as far as the evidence is concerned, the two theories are about on a par. She claims that Simpson and Schindewolf "disagree seldom, if at all, about the 'facts'". The two theories merely "provide alternative frameworks for understanding the data". Although for some details one point of view is preferable, for other details the other point of view is preferable. Thus, "it seems to be purely a matter of choice which we prefer. ... Perhaps what we need, then, is a more inclusive theory, which will assimilate adequately both sides of the ambiguity".[35]

After establishing to her own satisfaction the equality of the two theories as far as the evidence is concerned, she proceeds to argue that Schindewolf's theory is more adequate than that of Simpson on epistemological grounds. Simpson claims not to make use of types in an epistemologically significant sense; that is, he may occasionally refer to types, but he claims that in no instance does he suppose that natural kinds can or must be defined in terms of necessary and sufficient conditions.[36] The reason that Schindewolf's theory is more adequate than Simpson's is that he admits types and makes them a part of his theory, whereas Simpson makes use of them surreptitiously, though they are incompatible with his theory. Grene does not claim that "Schindewolf's type-theory is adequately explanatory. Only that it is not self-contradictory and so is at least a possible starting-point for *asking* philosophical *questions* – not for giving philosophical answers, as my critics suggested I mean to do."[37]

What is to be said of Grene's argument? In the first place, almost all biologists disagree with her assessment of the evidence. Taking the two theories on a whole, the vast majority of the evidence supports the synthetic theory, and it is in just those cases where Schindewolf departs most radically from the synthetic theory that the evidence is most decidedly against him.[38] For example, Grene cites Schindewolf's belief that basically new types or patterns of organization have a 'sudden origin'. There were no feathered creatures. Then there were. The fossil evidence happens not to be decisive on this issue, but given what we know of genetics and physiology, it is unlikely that such macro-phenotypic changes resulting from mutation could occur in the space of a single generation and the results be viable – even once, let alone in the origin of every new type. It does not help in the least to say that the gross phenotypic changes were due to micromutations early in development. The magnitude of the change in the genetic make-up is not at issue but the magnitude of the resulting phenotypic change. According to Schindewolf, one basic plan of organization must be changed into another in the space of a single generation. Most biologists find this unlikely.[39]

Thus, with respect to the first part of Grene's argument, most biologists think that Grene is factually in error, and it was these factual errors which elicited much of the negative response from biologists.[40] Grene, however, is not primarily interested in these factual disputes but in her second point that Schindewolf's theory is somehow epistemologically more adequate than Simpson's theory. To be specific, Schindewolf freely admits certain concepts which Simpson pretends to do without, but introduces surreptitiously. One of these concepts is the concept of 'type'. Traditionally the notion of type entails that membership in a natural kind is determined by one set of characters which are severally necessary and jointly sufficient. If Grene intends to be referring to the essentialist notion of type, then she could be making either of two claims – either the weak claim that Simpson thinks he is not using an epistemologically significant concept of type when actually he does or the strong claim that not only does he use such a type concept, but also he must.

On the first count, Grene is probably right. Even though Simpson, Mayr, and other evolutionists have repeatedly emphasized that evolutionary theory is incompatible with the essentialist type concept, this point of view is quite difficult to maintain in the midst of complex lines of reasoning.[41] Simpson may well have slipped on occasion. The strong claim, however, is another matter. Essentialism is a philosophic position of long-standing, but if Grene is to resurrect it, she owes the reader some explanation of why this particular notion is a necessary element in any adequate epistemology. In the article in question, she provides none. In a later paper, she returns to this theme but this time she equates the concept of type with 'gestalt-idea'. But it is one of the key features of a gestalt that one or more of the elements can be replaced or altered without affecting the gestalt, in direct opposition to the traditional notion of type.[42]

It seems to be this confusion over exactly what Grene intended to be arguing in her two evolutionary theories paper that occasioned much of the controversy. A later paper in which she criticized Fisher's mathematical version of evolutionary theory did not meet with any strong objections, since in this paper Grene explicitly states what it is she is attempting to establish and provides careful arguments in support of her thesis.[43] In the original paper, biologists read her to be claiming that a false scientific theory could be preferable to a highly confirmed theory because of some preemptive epistemological considerations. They failed to see what these epistemological considerations could be or how they could outweigh considerations of empirical truth. To give Grene a run for her money, however, one might also suggest still another, more subtle possibility. The leading proponents of the synthetic theory of evolution are also anti-reductionists. It would be easy to understand why an essentialist would be anti-reductionist, but given the synthetic theory of evolution, it is difficult to see why these biologists are equally opposed to the possibility of reduction.[44]

From what has been said thus far, the reader might infer that philosophers have very little to contribute to biology. On the contrary, there are many aspects of the scientific endeavor to which philosophers could contribute. My complaint is that by and large they have not. A classic example of how ineffectual philosophers have been in communication with biologists is provided by the taxon-category distinction. J. H. Woodger spent most of his life doing what he called "turning the Boole-Frege searchlight upon statements in biology". The differences between individuals, classes, and classes of classes is one instance in which such an effort could have resulted in considerable benefit for biology. The logical tools were available. The time was right. Just when Woodger was beginning his work in mathematical logic, biologists were beginning to subject the species concept to intensive re-examination.[45] All it takes to appreciate how greatly the work of these biologists would have benefited from having more than an intuitive grasp of the differences between defining the name of a particular species (a class) and defining the name of the species category itself (a class of classes) is to read the literature of the period. But Woodger did provide just such an analysis in his book *The Axiomatic Method in Biology*.[46] Unfortunately, Woodger's account went unnoticed.

Finally, 10 years later, when biologists did begin to point out the significant differences between what they called taxa and categories, the increase in clarity was substantial. It would be pleasant to be able to say that philosophers played a large role in the recognition and dissemination of this important logical distinction, but such does not seem to be the case. Biologists had to work out the appropriate distinctions and terminology for themselves. At this same time philosophers were producing parallel accounts.[47] I have asked several of the biologists involved whether they found these accounts helpful. They replied in the negative. What was the reason for this failure in communication? Two factors combined to keep the work of these philosophers from having any significant impact on biology. In setting forth these factors, I do not mean to imply that all parties were equally guilty in every respect or that some of the blame cannot be laid at the feet of biologists. I really am not so much interested in fixing blame as in discovering the causes for this failure in communication.

First of all, philosophers tend to exhibit what can only be described as disdain for the issues and distinctions which biologists find important. For example, in his 1950 paper John R. Gregg argues that species are classes, not individuals, and that the relation between an individual organism and the species to which it belongs is membership, not the part-whole relation.[48] These issues were raised because two biologists had advanced independently the notion that species are as much concrete, spatiotemporal things as are individual organisms. On this score I think Gregg is right, but in his arguments Gregg seems almost willfully blind to the reasons these biologists might have had for making such an assertion. The point that they were trying to make was that species are not *just* sets, just collections of isolated individuals like the class of all things smaller than a breadbox. The members of a species are interrelated in numerous biologically significant ways, among which is spatiotemporal proximity. The ontological questions of whether a class can be identified with its members, whether the class of all cells that compose an organism is identical to the class of all molecules that compose that organism, or for that matter, whether the whole universe can be viewed as an organism are irrelevant to the issues raised by these biologists. Gregg says that this problem "is a pseudo-taxonomic one which is resolved by reference to the semantic structure of language, and upon which no purely biological evidence (geographical distribution, interbreeding relations, etc.) has the slightest bearing whatsoever".[49] If so, then Gregg has misidentified the problem.

The second factor which has contributed to the failure of communication between these philosophers and biologists is their method of doing philosophy of science – the formal reconstruction of biological statements in the notation of mathematical logic. There is a continuum with respect to the degree to which these 'formalists' utilize mathematical logic. At one end such notions are used just for clarity and consolidation in the presentation of definitions. At the other end are those works in which the entire presentation is set-theoretical.

This method of doing philosophy of biology has two drawbacks. The obvious one is that few biologists are familiar with the notation. One reason why Woodger's work has had so little impact on biology is that biologists cannot read most of his later work. But isn't this the fault of biologists? Isn't it up to them to learn set-theory or symbolic logic so that they can reap the benefits of this large body of literature? The only answer that I can honestly give to these questions is: No. Formalists such as Woodger and Gregg have made some biologically significant points in their work, but few that could not have been made just as clearly without extensive use of these notations. Perhaps the discovery of certain logical distinctions was aided by the use of these techniques, but the results need not have been communicated in these same terms. The taxon-category distinction is a case in point. Too often the applications of mathematical logic to problems in biology give the impression that more or less commonplace ideas have been expressed in tiresome exactitude when they could have been conveyed more easily and more directly in a few sentences of plain English.

The second drawback of the formalist method is that more often than not the method becomes the message. The substantive problem in biology which occasioned the formalization is forgotten as special, technical problems arise in the formalism. The numerous papers which have been written to solve what has been called Gregg's paradox provide an excellent case in point. In his 1954 monograph, Gregg set himself the task of providing a set-theoretical reconstruction of the Linnaean hierarchy within the confines of extensional logic. (Woodger in 1952 had attempted a similar reconstruction.[50]) The problem is that biologists make use of intensional notions in constructing their classifications. In fact, the distribution of the characters of the organisms being classified is a primary consideration in constructing biological classifications. Some biologists argue that it is the only consideration. As might be expected, Gregg's purely extensional reconstruction gave rise to paradoxes. Thereafter, a long series of papers appeared in which various devices were used to eliminate these paradoxes (or antinomies) without introducing the notion of intensions.[51] But why? Even if one or more of these devices work, what relevance will this reconstruction have for biology? Taxonomists will continue to make their decisions on the basis of the distribution of characters among their specimens. The need for reconstructing the Linnean hierarchy in intensional or modal logic will remain. All the effort expended in attempting to reconstruct the Linnaean hierarchy within the confines of extensional logic may be first-rate logic. It has little claim to being philosophy of biology.

A second instance in which philosophers could have been of some service to biologists is afforded by the question – what is a character? This question has plagued genetics since the days of the one gene – one character hypothesis. Closely associated with this problem is the question of homology – when are two instances of a character to be considered instances of the same character and in what sense same? After the advent of evolutionary theory the answer has been that two characters are homologous if they are similar because of origin from a common ancestor. A decision as to whether or not two characters are homologous in an evolutionary sense requires recourse to all available evidence and to numerous scientific theories, including evolutionary theory. Because of the intricacy of the inferences involved in such decisions and the frequent paucity of evidence, some biologists have suggested that a different, more basic notion of homology – operational homology – should be substituted for the notion of evolutionary homology.

At first, operational homologies were supposed to be something observed directly with no recourse to inferences or scientific theories. Gradually biologists have come to see that such units were quite ephemeral and of little scientific use and have expanded the concept to permit inferences on the basis of certain scientific theories, in fact, any scientific theory except evolutionary theory. When evolutionary interpretations are put on operational homologies, the result is evolutionary homologies. As the reader may have detected, the efforts of biologists to clarify the ideas of character and homology have been excursions into pure epistemology. Within the scope of a decade, they have relived the history of phenomenalism, operationism, and logical atomism. There is really no need for biologists to remake all the old mistakes and to explore every blind alley.[52]

Another place at which philosophers could have helped clarify the issues in a dispute between biologists is the disagreement over the problem of whether higher categories evolved first and then later diversified into lower categories or whether species evolved first and then only later higher categories. Goldschmidt and Schindewolf incline toward the first view. Simpson and Mayr subscribe to the latter view.[53] The major difficulty with this controversy is the logical crudity with which it is frequently expressed. Both Marjorie Grene and T. A. Goudge discuss this controversy at some length.[54] Though they themselves do not make the fairly straightforward logical error at the bottom of the confusion, neither does anything to clarify the situation. The confusion lies primarily in the modes of expression of Goldschmidt and Schindewolf. On neither view can higher taxa (the term now in use) evolve first and lower taxa such as species evolve later. Perhaps *Archaeopteryx* evolved in one fell swoop. Perhaps its basic organization is so novel that it must be recognized as a new higher taxon regardless of any future developments. Even so, a new higher taxon has in no way evolved *before* a new lower taxon. Given the principles of classification agreed upon by both sides, it is logically impossible for a new phylum or family to evolve without a new species evolving *simultaneously*.[55]

The two sides are in disagreement, but this is not it. Their disagreement concerns one matter of fact and one matter of taxonomic strategy. The factual disagreement over the existence of large, abrupt changes in phenotypic make-up has been discussed previously. With respect to their differences on taxonomic strategy, Goldschmidt and Schindewolf want to classify entirely on the basis of overall similarity and phenotypic gaps, regardless of the number and diversity of taxa which eventually exhibit this type of organization. Novelty alone guarantees a taxon a high category assignment. Simpson and Mayr classify on a multiplicity of principles. To maintain some kind of balance in a classification while still retaining a systematic relationship to phylogeny, principles of vertical classification must be tempered with those of horizontal classification. Thus a taxon with numerous included taxa is likely to be classified at a higher category level than its degree of divergence alone might warrant.

Finally, no review of the philosophy of biology could possibly omit the two recent works devoted entirely to problems in the philosophy of biology – the *Foundations of Biology* by Felix Mainx and Morton Beckner's

The Biological Way of Thought. As might be expected from the fact that his monograph is a contribution to the *International Encyclopedia of Unified Science*, Mainx emphasizes the verifiability criterion of meaningfulness and the unity of science. The errors and conceptual dangers which he most frequently points out in biological works are attempts to pass off tautologies and metaphysical claims as empirically meaningful statements, and a tendency among biologists for conceptual realism. Both of these tendencies are worth bringing to the attention of biologists, but unfortunately for Mainx's treatment, he fails to reflect the increased sophistication of the positivist position which had occurred since its inception. For instance he sees tautologies everywhere because he accepts a rather facile notion of the relationship between operations used to test the applicability of a term and the definition of that term. He says in one place, for example:

> If in the statement "The positive phototactic reaction of a *Euglena* is proportional to its light-requirement" the concept "light-requirement" is only testably defined by means of the establishment of the behavior under the stimulus of light, this is a tautologous statement of the above kind.[56]

Behavior under the stimulus of light is certainly neither logically nor physically the only way of 'testably defining' the concept 'light-requirement'. Hence, the statement is not tautologous. The same is true for most of the examples which Mainx gives. Mainx would have done well to have read Carl G. Hempel's earlier contribution to the same series on the foundations of concept formation in empirical science.[57] A careful reading of this earlier monograph might also have suggested to him that the basic distinction which pervades his book serves only to frustrate his efforts to provide an adequate explication of the foundations of biology. This distinction is between order-analytic statements, which express the coexistence of characters, and causal-analytic statements, which express a succession of states in time. As time-honored as this distinction is, it just will not do as a characterization of the relationship between concept formation and theory construction in science. For example, Mainx recognizes three different viewpoints in biology – the morphological, the physiological and the genetical. Although he warns the reader that these three viewpoints overlap somewhat, he makes it sound as if a morphologist could analyze an organism into organs and tissues independently of any knowledge of physiology, genetics or evolutionary descent. Further, a taxonomist supposedly can then erect a classification by means of the co-variation of these characters, and this classification is equally neutral as far as scientific theories are concerned. Only *afterwards* can such theoretical considerations as the functions these structures perform or their evolutionary derivation be brought into play. This is the impression his exposition gives although he says that it must be "remembered that even in these elementary descriptive statements the beginning of hypothesis construction must be recognized".[58]

Currently, biologists are carrying on an extensive debate over just these issues.[59] Must the sequence of events in an actual scientific study be the same as those in Mainx's epistemological reconstruction? There are good reasons for maintaining that they cannot be. As Beckner has pointed out,[60] the morphological characteristics which are used to produce a supposedly neutral classification are not purely morphological characters. The definition of 'kidney', for example, necessarily presupposes knowledge of physiology. Perhaps a biologist might begin with visual clues, but what makes a structure a kidney is determined as much by its function as by its structure. A similar story can be told for the evolutionary derivation of a character, though the relevant evidence is more difficult to obtain. The gills of a fish and the gills of a crayfish are not the same character even though they perform the same function, both because of their structural differences and because of their differing phylogenetic histories. These various aspects of scientific terminology are interconnected in very complicated ways in the formation of concepts in science. One school of biologists is presently arguing that all these different features in biological terminology must be disentangled, especially any assumptions about evolutionary development. This task is, to say the least, ambitious. Reading Mainx's overly simplistic treatment will help these biologists appreciate the difficulties which they are likely to encounter very little. The story is quite different with respect to Beckner's book.

The attention which Beckner's explication of organismic biology continues to receive from both biologists and philosophers is well-deserved. (*The Biological Way of Thought* has recently been re-issued in paperback by the University of California Press.) What Beckner says is important and expressed clearly – with only one major and pervasive exception. The main purpose of Beckner's book is to show that biology is an autonomous discipline with concepts and laws peculiar to itself. In general he tries to show that organization, directiveness and historicity play a more crucial role in biology than in other sciences. Specifically, he attempts to show that three classes of concepts – polytypic, historical and functional concepts – are characteristically biological in one sense and fully unique in biological theory in another. Beckner's explication of polytypic concepts affords a rare instance in which a philosopher has actually contributed significantly to the development of a biologically important notion, that of polythetic definitions.[61] Biologists had been using the notion for some time and had discussed it with varying degrees of clarity, but Beckner also provided a general explication and a philosophical justification for it.

The one glaring fault in Beckner's presentation is his introduction of two technical terms, 'W-definition' and 'E-definition', which he claims will be helpful in his exposition. A biologist attempting to read Beckner's book is confronted in the second chapter by a series of semi-formal definitions. Most readers, if they go on at all, are tempted to skip this chapter and, as it turns out, with no great loss. His brief discussions of polytypic, historical and functional concepts are expanded in later chapters devoted exclusively to them, and the notions of W- and E-definitions play almost no role in the ensuing pages. To be sure, Beckner periodically uses these technical terms in his subsequent discussions but to little consequence, since in most cases a term cannot simply be W-defined or E-defined. Instead they can only be E-defined with "preestablished criteria of adequacy for any W-definition", and Beckner leaves the nature of these criteria completely unexplicated. At the risk of seeming totally opposed to the formalist method of doing philosophy of science, I must point out that once again the extensive logical machinery which Beckner introduces serves to hinder rather than aid his exposition. Formalization may be an excellent way of working out problems in the philosophy of science. It is not a very good way of communicating the results of these endeavors. However, in spite of this flaw in presentation, Beckner's book remains the single major contribution of a philosopher to biology in over a decade.

In conclusion, there are many things that philosophy of biology might be. A philosopher might uncover, explicate, and possibly solve problems in biological theory and methodology. He might even go on to communicate these results to other philosophers, to scientists, and especially to biologists. He might show what consequences biological phenomena and theories have for other sciences and for philosophy or to show what consequences other sciences and even philosophy have for biology. These are some of the things which philosophers of biology might do. With rare exception, they have not. What philosophy of biology is not? It must be admitted that thus far it is not very relevant to biology, nor biology to it.[62]

REFERENCES

* This paper will also appear in the *Journal of the History of Biology* **2** (1969), No. 1.

[1] Both restrictions in the scope of this paper have been ignored when special circumstances seemed to demand it. For example, Woodger began his career as a biologist and published most of his work prior to the time limits set for this paper, but he has produced a body of work in the philosophy of biology too important not to include.

[2] Mario Bunge, 'The Weight of Simplicity in the Construction and Assaying of Scientific Theories', *Philosophy of Science* **28** (1961) 120–149.

[3] G. G. Simpson, *The Meaning of Evolution*, Yale University Press, New Haven, 1949; G. G. Simpson, *Principles of Animal Taxonomy*, Columbia University Press, New York, 1961; G. G. Simpson, *This View of Life*, Harcourt, Brace & World, Inc., New York, 1963; Ernst Mayr, *Animal Species and Evolution*, Harvard University Press, Cambridge, Mass., 1963. In most instances the assertion that certain principles in the synthetic theory of evolution are tautologous stems from an extremely superficial understanding of evolutionary theory and an embarrassing uncertainty over the precise nature of tautologies. For example, Murray Eden, in *Mathematical Challenges to the Neo-Darwinian Interpretation of Evolution* (ed. by P. S. Moorhead and M. M. Kaplan), The Wistar Institute Press, Philadelphia, 1967, began with the assumption that the tautological nature of certain concepts in evolutionary theory was hardly controversial. Under the onslaught of several biologists present, Eden retreated to the position that perhaps 'tautology' was the wrong word. Rather such claims are supposedly vacuous. From here he retreated to the assertion that the basic principles of evolutionary theory did not form a theory, and finally he concluded that since one cannot provide a crucial experiment to check whether or not the synthetic theory is false, it is in some sense tautological or unfalsifiable. Karl Popper was frequently cited in this discussion as maintaining the last position. Anthony Flew, 'The Concept of Evolution: A Comment', *Philosophy* **41** (1966) annihilates similar allegations by A. R. Manser, 'The Concept of Evolution', *Philosophy* **40** (1965) 18–34, though he himself argues later that the principle of the survival of the fittest must be made into a tautology, Anthony Flew, *Evolutionary Ethics*, St. Martin's Press, New York, 1967. See also T. A. Goudge, *The Ascent of Life*, University of Toronto Press, Toronto, 1961.

[4] Charles Darwin, *On the Origin of Species: A Facsimile of the First Edition*, Harvard University Press, Cambridge, Mass., 1966, pp. 450–456; Charles Darwin, *The Descent of Man, And Selection in Relation to Sex*, 2 vols., Murray, London, 1871, p. 92.

[5] Darwin, *Origin*, 472, 135.

[6] Darwin, *Origin*, 468, 89–90.

[7] J. W. Tutt, 'Melanism and Melanochroism in British *Lepodoptera*', *Entomologists' Record* **1** (1890), **2** (1891).

[8] Peter Caws, *The Philosophy of Science*, D. Van Nostrand, Inc., Princeton, 1965, pp. 40–41.

[9] Mayr, *Animal Species*, 91.
[10] The biological literature on the species concept is overwhelmingly large. The best discussions of the pros and cons of the biological definition of 'species' can be found in Simpson, *Principles*; Ernst Mayr, *Systematics and the Origin of Species*, Columbia University Press, New York, 1942); Ernst Mayr, 'The Species Concept', *Evolution* **3** (1949) 371–372; Ernst Mayr (ed.), *The Species Problem*, American Association for the Advancement of Science Publication Number 50, Washington, 1957; Ernst Mayr, 'Isolation as an Evolutionary Factor', *Proceedings of the American Philosophical Society* **103** (1959) 165–230; Mayr, *Animal Species*. For two philosophers who take the biological aspects of the species problem seriously, see Morton Beckner, *The Biological Way of Thought*, Columbia University Press, New York, 1959, and Hugh Lehman, 'Are Biological Species Real?', *Philosophy of Science* **34** (1967) 157–167.
[11] Andrew G. van Melsen, *Evolution and Philosophy*, Duquesne University Press, Pittsburgh, 1965. See also Adolf Portman, *New Paths in Biology*, 1961 (English trans.: Harper & Row, New York, 1964) and Hans Jonas, *The Phenomenon of Life: Toward a Philosophical Biology*, Harper & Row, New York, 1966.
[12] G. G. Simpson, 'The World into Which Darwin Led Us', *Science* **131** (1960) 966–974; reprinted in Simpson, *This View*, 3–25.
[13] W. R. Thompson, 'The Status of Species', in *Philosophical Problems in Biology* (ed. by Vincent E. Smith), St. John's University Press, New York, 1966, pp. 67–126.
[14] P. B. Medawar, *The Art of the Soluble*, Methuen & Company Ltd., London, 1967.
[15] All of the above assertions can be found expressed variously in two volumes edited by Vincent E. Smith, *Philosophy of Biology*, St. John's University Press, New York, 1962, and *Philosophical Problems of Biology*, as well as in *Pacific Philosophy Forum* **6** (1968), No. 3, 1–99.
[16] Goudge, *Ascent*.
[17] Goudge, *Ascent*, 133, 134.
[18] See the previously cited number of *Pacific Philosophy Forum* (note 15), in which several philosophers argue that the vitalism-mechanism controversy cannot be dismissed as a dead issue. The major paper in this number by Hilde Hein is sufficiently innocuous, but in the others we are once again subjected to such inanities as "the human intellect violates the laws of the irreversibility of space and time", that "no less than the evolutionist's view, the approach of the creationist yields a true and satisfactory philosophy of life", and that the fossil remains of so-called missing links between man and other primates are actually the remains of abnormal individuals of the unaltered human species. Edward Manier had the unenviable task of replying to these papers.
[19] J. H. Randall, *Aristotle*, Columbia University Press, New York, 1960. Numerous other examples in addition to Randall could be given. For example, Michael Polanyi, 'Life's Irreducible Structure', *Science* **160** (1968) 1308–1312 recognizes various levels of organization and principles which apply to certain levels and not to others, but he also has these higher principles 'harnessing' the lower processes. See also Marjorie Grene, *The Knower and the Known*, Basic Books, Inc., New York, 1966. No biologist would deny the existence of levels of organization or of principles which apply to these levels, but how can these principles *do* things? What can William E. Carlo, 'Mechanism and Vitalism: A Reappraisal', *Pacific Philosophy Forum* **6** (1968) 57–68, mean when he says that "there is a principle of organization which takes the matter of a man, shapes it, molds it, and organizes it into a man"? See also P. R. Durbin, *Philoso-*

phy of Science: An Introduction, McGraw-Hill Book Company, New York, 1968, and V. E. Smith, *Science and Philosophy*, Bruce, Milwaukee, 1965.

[20] See Beckner, *Biological Way*; Hugh Lehman, 'Functional Explanation in Biology', *Philosophy of Science* **34** (1965) 1–19; J. V. Canfield, 'Teleological Explanation in Biology', *British Journal for the Philosophy of Science* **14** (1964) 285–295; J. V. Canfield (ed.), *Purpose in Nature*, Prentice-Hall, Inc., Englewood Cliffs, 1966.

[21] There is a large body of literature devoted to the problem of reduction. Recent discussions of reduction in biology can be found in Grene, *Knower*; R. T. Blackburn (ed.), *Interrelations: The Biological and Physical Sciences*, Scott, Foresman, Chicago, 1966; K. F. Schaffner, 'Approaches to Reduction', *Philosophy of Science* **34** (1967) 137–147; K. F. Schaffner, 'Antireductionism and Molecular Biology', *Science* **157** (1967) 644–647.

[22] Carl G. Hempel and Paul Oppenheim, 'The Logic of Explanation', *Philosophy of Science* **15** (1948) 135–175.

[23] Of all the papers devoted to the relevance of explanation in evolutionary theory to the symmetry thesis, Michael J. Scriven in his 'Explanation and Prediction in Evolutionary Theory', *Science* **130** (1959) 477–482, is the most concerned with evolutionary theory as a biological theory, rather than as a handy source for an example. See also Michael J. Scriven, 'Explanation, Prediction, and Laws', in *Minnesota Studies in the Philosophy of Science*, vol. III (ed. by H. Feigl and G. Maxwell), University of Minnesota Press, Minneapolis, 1962, pp. 477–482; Adolf Grünbaum, 'Temporally Asymmetric Principles, Parity between Explanation and Predicton, and Mechanism versus Teleology', in *Induction: Some Current Issues* (ed. by B. Baumrin), Wesleyan University Press, Middleton, 1963; Hugh Lehman, 'On the Form of Explanation in Evolutionary Biology', *Theoria* **32** (1966) 14–24.

[24] Abraham Kaplan, *The Conduct of Inquiry*, Chandler Publishing Company, San Francisco, 1964.

[25] Anthony Flew, 'The Structure of Darwinism', in M. L. Johnson, Michael Abercrombie & G. E. Fogg, *New Biology*, Penguin Books, Baltimore, 1959, pp. 25–44; Flew *Evolutionary Ethics*.

[26] Julian Huxley, *The Process of Evolution*, Chatto and Windus, London, 1953, p. 38.

[27] Flew, 'Structure', 28.

[28] Flew, 'Structure', 28.

[29] Flew, 'Structure', 29.

[30] Flew, *Evolutionary Ethics*, 14, 36, 14.

[31] J. H. Woodger, *Biological Principles*, 1929 (2nd ed.: Routledge & Kegan Paul Ltd., London, 1948), pp. 394, 402.

[32] For differing opinions on these views, see W. B. Gallie, 'Explanation in History and the Genetic Sciences', *Mind* **64** (1955) 161–167; R. P. Gould, 'The Place of Historical Statements in Biology', *British Journal for the Philosophy of Science* **8** (1957) 192–210; T. A. Goudge, 'Causal Explanations in Natural History', *British Journal for the Philosophy of Science* **9** (1958) 194–202; Beckner, *Biological Way*; M. J. S. Rudwick, 'The Inference of Function from Structure in Fossils', *British Journal for the Philosophy of Science* **15** (1964) 27–40.

[33] Marjorie Grene, 'Two Evolutionary Theories', *British Journal for the Philosophy of Science* **9** (1958) 110–127, 185–193.

[34] Simpson, *Meaning*; O. H. Schindewolf, *Grundfragen der Paläontologie*, Schweizerbart, Stuttgart, 1950.

[35] Grene, 'Two Evolutionary Theories', 185–186.

[36] G. G. Simpson, 'Types in Modern Taxonomy', *American Journal of Science* **238** (1940) 413–431.
[37] Marjorie Grene, 'Two Evolutionary Theories: A Reply', *British Journal for the Philosophy of Science* **14** (1963) 152–153.
[38] Of course, not *all* biologists agree with *all* aspects of the synthetic theory or that *all* the data are favorable to it. Almost any biologist will have some reservation about one or another of the tenets of the synthetic theory, and Grene cited several examples – H. Graham Cannon, Paul G. 'Espinasse, Ronald Good, Adolf Portman, W. R. Thompson, and C. H. Waddington. What Grene fails to mention is that these scientists disagree with *different* aspects of the synthetic theory and with each other. It is not the case that a unified, viable alternative to the current orthodox theory has been brought forth. Grene herself makes passing reference to looking at "phylogeny as an ontogeny writ large, at the history of groups as expressing a fundamental rhythm still, in its intimacy, unknown to us, but analogous to the rhythm of individual development". Instead of species becoming extinct by the various mechanisms of the synthetic theory, "this pattern of living – simply played itself out". Marjorie Grene, 'The Faith of Darwinism', *Encounter* **13** (1959) 52, 55, reprinted in Grene, *Knower*, 185–201. Biologists *have* investigated this possibility, most recently, Lawrence S. Dillon, 'The Life Cycles of the Species: An Extension of Current Concepts', *Systematic Zoology* **15** (1966) 112–126. The idea even occurred to Darwin, *Journal of Researches into the Natural History and Geology of the Countries Visited during the Voyage of H. M. S. Beagle Round the World*, 2nd ed., revised, Murray, London, 1845. Unorthodox views *do* get a hearing. Unfortunately, the weight of evidence has proved to be against this particular unorthodox view. The fact that most biologists, after listening to these dissident views, remain unconvinced leads Grene to label them dogmatic. If refusal to go against the weight of evidence is dogmatism, then these biologists are being dogmatic.
[39] Although Goldschmidt was a strong proponent of macroevolution by macromutation, he was aware that the two positions were independent of each other and said so explicitly:

"Continuing the line of argument derived in the foregoing chapters, we must find out further whether the development system is capable of being changed suddenly so that a new type may emerge without a slow accumulation of small steps, but as a consequence of what we called a systematic mutation.

Such an analysis may be carried out in complete independence from the detailed conceptions which we developed concerning the architecture of the germ plasm and its changes. It does not make any difference whether a single macroevolutionary step is caused by a major change within the chromosomal pattern, a systematic mutation, or by a special kind of gene mutation with generalized effect, if such is imaginable. The decisive point is the single change which affects the entire reaction system of the developing organism simultaneously, as opposed to a slow accumulation of small additive changes" (R. B. Goldschmidt, *The Material Basis of Evolution*, Yale University Press, New Haven, 1940, p. 251).
[40] Walter J. Bock and Gerd von Wahlert, 'Two Evolutionary Theories: A Discussion', *British Journal for the Philosophy of Science* **14** (1963) 140–146; Leigh Van Valen, 'On Evolutionary Theories', *British Journal for the Philosophy of Science* **14** (1963) 146–152; G. S. Carter, 'Two Evolutionary Theories, By M. Grene: A Further Discussion', *British Journal for the Philosophy of Science* **14** (1963) 345–348; Marjorie Grene, 'Two Evolutionary Theories: Reply to Dr. Carter', *British Journal for the Philosophy of Science* **14** (1963) 349–351. One philosopher who attempted a philosophically oriented

criticism, Edward Manier, 'The Theory of Evolution as Personal Knowledge', *Philosophy of Science* **32** (1965), 244–252, was met with a harsh rebuff, Marjorie Grene, 'Discussion: Mr. Manier's Theory of Evolution as Personal Knowledge: A Quasi-reply', *Philosophy of Science* **33** (1966) 163–164.

[41] Ernst Mayr, 'Agassiz, Darwin and Evolution', *Harvard Library Bulletin* **13** (1959) 165–194; Mayr, *Animal Species*; Simpson, *Principles*; Simpson, *This View of Life*.

[42] Marjorie Grene, 'Positionality in the Philosophy of Helmuth Plessner', *The Review of Metaphysics* **20** (1966) 258.

[43] Marjorie Grene, 'Statistics and Selection', *British Journal for the Philosophy of Science* **12** (1961) 25–42.

[44] This interpretation was suggested by Hugh Lehman and is born out by certain comments in Grene, *Knower*, 185.

[45] Theodosius Dobzhansky, *Genetics and the Origin of Species*, Columbia University Press, New York, 1937; Mayr, *Systematics*.

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

[47] John R. Gregg, 'Taxonomy, Language and Reality', *The American Naturalist* **84** (1950) 419–435; John R. Gregg, *The Language of Taxonomy*, Columbia University Press, New York, 1954; J. H. Woodger, *Biology and Language*, Cambridge University Press, Cambridge, 1952; Beckner, *Biological Way*.

[48] Gregg, 'Taxonomy'.

[49] Gregg, 'Taxonomy', 426.

[50] Gregg, *Language*; Woodger, *Biology and Language*.

[51] A. F. Parker-Rhodes, 'Review of Gregg, The Language of Taxonomy', *Philosophical Review* **66** (1957) 124–125; A. Sklar, 'On Category Overlapping in Taxonomy', in *Form and Strategy in Science* (ed. by J. R. Gregg and F. T. C. Harris), D. Reidel Publishing Company, Dordrecht, 1964, pp. 395–401; Leigh Van Valen, 'An Analysis of Some Taxonomic Concepts', in Gregg and Harris, *Form*, 402–415; C. J. Jardine, N. Jardine and R. Sibson, 'The Structure and Construction of Taxonomic Hierarchies', *Mathematical Biosciences* **1** (1967) 173–179; John R. Gregg, 'Finite Linnaean Structures', *Bulletin of Mathematical Biophysics* **29** (1967) 191–200.

[52] Robert R. Sokal and P. H. A. Sneath, *Principles of Numerical Taxonomy*, W. H. Freeman and Company, San Francisco, 1963; Donald H. Colless, 'An Examination of Certain Concepts in Phenetic Taxonomy', *Systematic Zoology* **16** (1967) 6–27; Donald H. Colless, 'The Phylogenetic Fallacy', *Systematic Zoology* **16** (1967) 289–295; N. Jardine, 'The Concept of Homology in Biology', *British Journal for the Philosophy of Science* **18** (1968) 125–139. Woodger had provided a similar analysis some years earlier, 'On Biological Transformations', in W. E. le Gros Clark and P. R. Medawar, *Essays in Growth and Form Presented to D'A. W. Thompson*, Clarendon Press, Oxford, 1945. Even though Woodger, *Biological Principles*, provided an excellent summary of the problems inherent in phenomenalism, a few biologists continue to argue that sense data or the like are the fundamental subject matter of science; e.g., J. S. L. Gilmour, in *The New Systematics* (ed. by Julian Huxley), Oxford University Press, Oxford, 1940, pp. 461–474; J. S. L. Gilmour and S. M. Walters, 'Philosophy and Classification', *Vistas in Botany* **4** (1964) 1–22.

[53] R. B. Goldschmidt, 'Evolution as Viewed by One Geneticist', *American Scholar* **40** (1952) 84–98; Goldschmidt, *Material Basis*; Schindewolf, *Grundfragen*; Simpson, *Meaning*; Mayr, *Animal Species*.

[54] Grene, 'Two Evolutionary Theories'; Goudge, *Ascent*.

[55] Amazingly, two authors who should know better are guilty of exactly the same confusion: Felix Mainx, *Foundations of Biology*, University of Chicago Press, Chicago, 1955; Lancelot Hogben, *Science in Authority*, W. W. Norton & Company, Inc., New York, 1963.
[56] Mainx, *Foundations*, 5.
[57] Carl G. Hempel, *Fundamentals of Concept Formation in Empirical Science*, University of Chicago Press, Chicago, 1952.
[58] Mainx, *Foundations*, 3.
[59] Hogben, *Science*; Sokal and Sneath, *Principles*; Ernst Mayr, 'Numerical Phenetics and Taxonomic Theory', *Systematic Zoology* **14** (1965) 73–97; Robert R. Sokal, J. H. Camin, F. J. Rohlf, and P. H. A. Sneath, 'Numerical Taxonomy: Some Points of View', *Systematic Zoology* **14** (1965) 237–243; Colless, 'Examination'; Colless, 'Fallacy'.
[60] Beckner, *Biological Way*, 110–131.
[61] See also, Woodger, *Biology and Language*; J. H. Woodger, 'Taxonomy and Evolution', *Nuova Critica* **3** (1961) 67–78; J. H. Woodger, 'Biology and the Axiomatic Method', *Annals of the New York Academy of Science* **96** (1962) 1093–1104; Douglas Gasking, 'Clusters', *Australasian Review of Psychology* **38** (1960) 1–36; Hogben, *Science*.
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