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to not having any objective criteria for supraspecific categories and to a conception of the principles of classification as little more than an art with canons of taste.

Crowson also criticizes Simpson for suggesting that all that is important in Hennig's book on phylogenetic systematics is subsumed in Simpson, and that disagreements between them reflect only Hennig's alleged ignorance of the thought of modern American systematists.. He points out by example that in Hennig's sense ". . . Simpson could not be described as an advocate of phylogenetic classification, any more than could Mayr . . ."

Nor should it be assumed, because of a shared concern with phylogenetic systematics, that all that is important in Crowson's work is subsumed in Hennig's. Crowson's book is sometimes deeply philosophical, always exploratory and conscious of alternative views and methods, filled with an impressive diversity of examples from zoology and botany, and of several purposes not the least of which is to encourage thoughtful dissent and reappraisal of the objectives and methods of systematics. In addition to the early philosophical chapters, there are major sections dealing with infraspecific, specific, and supraspecific categories, the classification of fossils, the nature of classificatory characters, biogeography, zoological and botanical nomenclature, and an epilogue on the future of systematics, to cite a few. In contrast, Hennig's Phylogenetic Systematics is almost single-minded in its attack on the crucial questions of phylogeny and classification. Hennig's arguments on cladistics are more rigorously and completely presented and organized, but, of course, his purposes were fewer.

Some of Crowson's views on the phylogenetic significance of fossils and geography are rather conservative. He suggests, for example: 1) that fossils provide the most direct evidence of phylogeny [for a critique of this idea see Schaeffer, Hecht, and Eldredge (1972)], that ancestral forms can be identified (and by implication subjected to a test of their alleged ancestral status), and that therefore absolute age of a taxon is knowable (although a fossil gives us only a minimum age of the group to which it belongs), and 2) that phylogenies may be deduced from the present day distributions of organisms (when, as seems to me, one must add the distributional data to a previously hypothesized phylogeny in order to understand a group's evolutionary history). Others may also be somewhat disappointed that such formless, if traditional, ideas have been incorporated into an otherwise so excellent treatise. Lest the moment of disappointment blur our vision and tempt us to reject the book's general theses, I can only observe that some of the most ardent opponents of phylogenetic systematics, Simpson, Mayr, and Darlington, have repeatedly advocated these same fallacies.

For those committed to the 'Aristotelian essence' as a means to understand phylogeny and classification, either by faith or the force of circumstances, Crowson's book will be a bore or an irritant. For the remainder of systematists, it will provide insight into the future of systematics as a science.

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Donn E. Rosen

The Philosophy of Biology. — Michael Ruse. 1973. Hutchinson University Library, London, 231 pp. £3.00.

> The real question is whether any straight philosophers of science ever say anything that leads their listeners to improve their practice as scientists.
> Michael Scriven (1969:189)

Traditionally, philosophy of science has been dominated by study of the physical sciences. For a decade or so an increasing number of philosophers have begun investigating biology and several approaches seem to have developed. Some philosophers at-tempt a comparison of biology with the physical sciences and thereby seek to demonstrate that biology is (or is not) similar to the physical sciences in logical structure or methodology. A much smaller number of workers seek some philosophical resolution of problems which are controversial within biology, problems which are perhaps uniquely biological. Not unexpectedly, the former group is composed of those workers with relatively less sophistication in the intricacies of biological problems, whereas the latter approach demands more than a cursory knowledge of the subject matter.

In this new book and in his many other writings Michael Ruse speaks primarily to philosophers rather than biologists but still with insight into the biological problems themselves. Biology stands to gain certain advantages and disadvantages from this approach. Certainly Ruse's writings will help convince more philosophers that biology can tell us something about the structure of science and that it is worthwhile of study on its own merits, irrespective of the degree to which biology and the physical sciences parallel each other. On the other hand, the question should be raised, as Scriven does in the quotation above, whether philosophers have by their tendency of speaking mainly to their own colleagues, materially influenced the outcome of controversies within biology. I will return to this point later in the review.

The Philosophy of Biology deals mainly with genetics, evolutionary theory, and taxonomy and therefore is important reading for evolutionary biologists. The five chapters on genetics and evolution have a common theme running through them: that biology utilizes the hypothetico-deductive (covering-law) model of scientific explanation, and therefore is similar to the methodology of the physical sciences (or at least has the potential to be), and that this model is the best one available for constructing explanations. Ruse presents a strong case (Chapter 2) that biological entities are no different from those in the physical sciences; any classification one might choose (observable, unobservable; hypothetical, nonhypothetical; theoretical, nontheoretical) can apply to both. He also argues effectively that Mendel's laws are similar to "laws" in the physical sciences, although the former are statistical (but, he asserts, so are Boyle's laws), and that (Chapter 3) Mendelian genetics is axiomatic, i.e., it conforms to the covering-law model of explanation. From these assumptions about the nature of biological entities, laws, and the axiomatic structure of Mendelian genetics, Ruse then concludes (Chapter 4) that evolutionary theory is comparable (favorably) with theories in the physical sciences. He admits that evolutionary theory does not possess the "deductive completeness" of Newtonian mechanics, but nevertheless (p. 49) "one can say that evolutionists have the hypothetico-deductive model as an ideal in some sense—they are far from having it as a realized actuality." Although it is possible to agree with Ruse that evolutionary theory can be formulated in terms of a covering-law model, his method of argumentation might be considered unsatisfying (although not unconvincing). He contends that populations genetics can be axiomatized, that population genetics is the core of evolutionary theory, and that, therefore, evolutionary theory is essentially hypothetico-deductive. For Ruse, all evolutionary explanations must lead back to genetics; why he maintains this "reductionist" viewpoint merely to validate his claim that evolutionary theory is axiomatic is not clear nor necessary in my opinion. He cites, for example, Williams' (1970) axiomatic analysis of evolutionary theory in which she relies very little on genetics, but then Ruse dismisses this work as quickly as it is mentioned. It would appear that Williams' work is yet another argument for the axiomatization of evolutionary theory and as such would support Ruse's basic position *vis a vis* the physical sciences. Much of Ruse's argumentation does not distinguish between two aspects of explanation, first, whether the explanation is hypothetico-deductive in structure, and second, whether the explanation is sufficiently complete or not: "I am not here arguing that (logically) necessarily evolutionists must use Mendel's laws. . . . What I am arguing is that if evolutionists would explain they must use laws of genetics, and for those who accept the synthetic theory then these laws will . . . start with Mendel's laws" (p. 56). Ruse leaves one with the impression that all evolutionary explanations should include a fairly detailed genetical component if those explanations are to be hypothetico-deductive in structure and/or sufficiently complete. Ruse argues in Chapter 6 that the covering-law model is better suited for evolutionary explanations than are several others that have been proposed by philosophers (but curiously he leaves out a discussion of inductive inference; see below). Ruse is probably correct, but often the issue seems to be the "goodness" or "peer-acceptability" of the explanation and not the details of its logical structure. Ruse argues for the *explanans* (thing explaining) having sufficient conditions to allow deductive inference of the *explanandum* (thing being explained). Since other methods of explanation do not have this strong link (as they probably would not by definition), they are considered less powerful than the hypothetico-deductive model. Maybe so, but what are "sufficient conditions"? It has been argued (Lewontin, 1969, and elsewhere) that evolution can be "explained" by (1) phenotypic variation, (2) a correlation between parents and offspring, and (3) a differential survival of phenotypes in remote generations. Evolution must follow from these assumptions, and genetics *per se* plays little or no role. Perhaps, then, we could have evolutionary explanations that are devoid of any detailed genetics but which are covering-law in form and sufficiently complete to satisfy most workers (this does not imply detailed genetics is not desirable). Most evolutionary explanations today pay lip-service to population genetics in the form of discussions of "gene flow," "isolating mechanisms," "genetic revolutions," and so forth, but this is somewhat gratuitous because of our profound ignorance of the actual genetic structure of natural populations (Gould and Johnston, 1972).

Of particular interest to readers of *Systematic Zoology* will be the two chapters on taxonomy, one on the "evolutionary approach" and the other on the "phenetic challenge." The school of "phylogenetic systematics" in the sense of Hennig is relegated to a footnote, and consequently much of the two chapters constitutes a rehash of the evolutionists versus pheneticists arguments of the 1960's. Ruse's discussion of systematics will probably be informative for other philosophers and those practicing systematists who consider it a bother to worry about theory but not for those who actively think and write about such matters. Ruse appears somewhat partial to evolutionary systematics. He justifies the practice of weighting because to do otherwise would deny the "virtues" of inductive logic: "Mayr is doing no more than any other scientist must do, that is, he is relying on indictive generalizations." To some numerical taxonomists and phylogenetic systematists this is a central problem that needs critical focus from someone trained in philosophy. Why is it that in previous chapters Ruse seems to advocate deductive inference but here is willing to accept inductive inference at a level far below that of initial premises? Some evolutionary systematists suggest support for their position in the deductivist philosophy of Sir Karl Popper (especially 1959) yet clearly the basic tenets of evolutionary systematics have never been axiomatized as demanded by Popperian philosophy. Indeed, to my knowledge only phylogenetic systematics has received this treatment (Farris, Kluge, and Eckardt, 1970), although it might be argued that the mathematical approaches used in some numerical taxonomy are of a similar deductive structure. It seems that philosophers might focus profitably on the comparative axiomatic structure of the different systematic theories.

Given that evolutionary systematics and numerical taxonomy are the only schools of systematic thought to be considered. Ruse does a fairly credible job in seeing their strengths and weaknesses. But like the adherents of these schools, Ruse often confuses the issues of phylogeny reconstruction and classification construction. He is equally critical of the immediate aims of both taxonomies but, because he has not considered alternative approaches to systematics, Ruse sometimes is overly tolerant of the "latitude and responsibility," the "art" if you will, of advocates for the two schools. He is thus forced to conclude that "as things stand at the moment neither taxonomy seems perfect; but both seem to be legitimate tools for the classifier of organisms."

I recommend Ruse's book to biologists. He writes well and provides an easy introduction into what some philosophers are thinking about biology. Many biologists probably will not see much relevance—I known of no biologists, for example, who are actually concerned whether their explanations contain laws or are or are not reducible to chemistry or physics (although they may use chemistry or physics) or whether their entities or "laws" are comparable to those of the physical sciences. Some philosophers do seem to understand the importance of using philosophical analysis to improve biological explanations (Scriven, 1969; Hull, 1969), but perhaps the situation will not improve until philosophers learn more biology or biologists more (or better) philosophy. Clearly there is room for dialogue.

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Mathematical Taxonomy.—N. Jardine and R. Sibson. 1971. John Wiley & Sons Inc., New York. xviii & 286 pp. $15.95.

This is one of the most important publications of recent years to appear in the area of taxonomic theory and methodology. It has much to offer that is illuminating, controversial and stimulating.

The book is divided into three Parts, entitled respectively "The Measurement of Dissimilarity," "Cluster Analysis," and "Mathematical and Biological Taxonomy." Part III seems particularly worthwhile and forms the major basis for this review. Parts I and II are composed predominantly of chapters that are quite technical, with a complex notation that will probably be understood only by readers with a firm background in mathematics in general and topology in particular. Unfortunately, these chapters will serve to "turn off" most biologists long before they reach the best part of the book. It is regrettable that the present format was selected; most of Parts I and II would have been much better as a separate book, or as an appendix.

Part III deals with the components and aims of biological taxonomy, automatic classification as a research tool, and the application of methods of automatic classification. Subjects discussed by the authors under these headings are in general presented with a clarity and insight that makes for rapid reading and assimilation. However, certain passages could have been much improved by rewriting. The authors at times rise to truly striking levels of opacity, even for a field not always noted for outstanding clarity of exposition. One can only marvel at passages such as, "These considerations will under most circumstances lead to a choice of minimum permissible isolation of clusters which are to