Book Reviews

A Populational Approach to Scientific Change

Human Understanding. Vol. 1, General Introduction and Part 1, The Collective Use and Evolution of Concepts. STEPHEN TOULMIN. Princeton University Press, Princeton, N.J., 1972. xiv, 520 pp. $13.50.

Stephen Toulmin's *Human Understanding* has two aims. One is to develop a general analysis of the processes by which historical populations evolve. What Toulmin terms the "populational approach" has already been applied successfully in biology to evolving species. He proposes to show that it can be applied just as successfully to any historical entity that evolves through the action of variation and selection, including conceptual populations, scientific disciplines, professions, societies, and languages. To some extent, it can be extended even to such unlikely areas as technology, literature, and the fine arts. In this first volume of a proposed trilogy Toulmin is chiefly concerned with science.

By using the populational approach, Toulmin hopes also to resolve the perennial dilemma posed by epistemological absolutism on the one hand and epistemological relativism on the other. Philosophers from the time of the ancient Greeks have reasoned that for the world to be knowable it has to be constituted of eternal, immutable objects. Knowledge of that which is changeable is not knowledge at all. Absolutists like Parmenides argued accordingly that change is an illusion, masking the static reality hidden beneath. Relativists like Heraclitus countered that eternal verities are the illusion. Everything is in constant flux and, hence, unknowable. The absolutists are so entranced by the timeless truths of logic and geometry that they find their guarantee for the rationality of human knowledge in similar equally unchanging principles mandatory on all rational thinkers. The relativists are so impressed by the diversity and variability in the empirical world that they deny any impartial standpoint for judging human knowledge. A belief is rational or irrational depending on the localized, temporary conceptual system in which it occurs.

Toulmin blames the failure of philosophers from Plato and Parmenides to Popper and Kuhn to solve the problem of conceptual change on their adherence to the "philosophical cult of systematicity" (pp. 83 and 481). The reason most philosophers have reacted to the problem as though it allowed only two alternatives is the "familiar assumption that rationality must be equated with logicality, and that different concepts and beliefs can be compared 'rationally,' only so long as they can both be referred to a single 'logical system' " (p. 52). Both sides view the "problem of rationality as requiring us to give final intellectual authority to one or another logical system: either an axiomatic system of propositions or a presuppositional system of concepts" (p. 81; see also pp. 169–71 and 185–89). Toulmin finds the recent logical positivists and logical empiricists especially guilty in that they have equated "scientific explanation" with "explanatory argument," preferably deductive (p. 370).

Toulmin is none too clear about the precise nature of this cult of systematicity, nor altogether fair in his discussions of the philosophers he treats. He repeatedly remarks that rationality is not a property of systems or a measure of a man's ability to recognize the validity of the axioms, formal entailments, and logical necessities of any theory of rationality, but an attribute of human activities or enterprises:

> A man demonstrates his rationality, not by a commitment to fixed ideas, stereotyped procedures, or immutable concepts, but by the manner in which, and the occasions on which, he changes those ideas, procedures, and concepts [p. x; see also pp. 44 and 133].

I am not sure that many of the philosophers he criticizes would disagree.

Toulmin's own account of the growth of knowledge is evolutionary, dealing with changing populations of concepts, disciplines, procedures, problems, and aims. In recent philosophy the notion of definitions as providing strict sets of essential characteristics has given way to that of cluster concepts, definitions in which the properties mentioned are rarely necessary and never equally important. Causes are no longer analyzed in terms of sufficient, let alone necessary and sufficient, conditions. Toulmin now proposes to treat the intellectual content of a scientific discipline in the same way, not as a tight and logical system but as a conceptual aggregate or population. I am in full agreement with Toulmin's desire to present a scientific theory of the evolution of science. I do not see the point of his contrast between concepts and systems, however. Both can be treated as static and unchanging on the one hand, or as evolving populations on the other. As Toulmin himself says,

> A procedural account of scientific explanation may at first seem as static and unhistorical, in its own way, as the logical and "systematic" accounts we have already criticized [p. 164].

Toulmin seems to equate "systematic" with "static." I fail to see why systems cannot be treated in the same way biologists treat species and philosophers treat definitions, causes, and concepts. In fact, the evolutionary development of science could be understood much better in terms of the evolution of populations of theories than in terms of the evolution of populations of concepts. On such an interpretation Mendelian genetics, for example, would be not a single, completely axiomatized (or even axiomatizable) theory but a family of partially formulated theories differing from each other in a variety of ways.

At this stage in the argument, I must register another reservation or two. As Toulmin himself points out,

> The suggestion that cultural and intellectual change should be accounted for in evolutionary terms has had a long and chequered history. . . . Up to now, however, the results of all such attempts have been so generally disappointing that anybody who proposes to revive this suggestion must demonstrate how he proposes to avoid the trap into which earlier forms of "evolutionism" fell [pp. 319–20].

The problem with such attempts, however, is less that they have fallen into traps than that they have been at best vague gestures, offering no more than pregnant suggestions and promising insights. No analysis of conceptual change to date, including that of Toulmin, approaches that provided for organic evolution by Darwin over a century ago.

Similar observations must be made with respect to Toulmin's switch from systems to populations. Too often in the past, recourse to clusters, families, populations, and such has proven to be a device for evading problems rather than for solving them. It is easy enough to say that "beauty" and "good" are cluster concepts. It is not so easy to list the elements in the clusters, assign weights to them, and show how they are related. References to populations in

biology are not vacuous because biologists have made considerable headway on each of the preceding counts. Progress has been much slower in other areas, primarily I suspect because they lack a selection theory of the scope and power of the synthetic theory of biological evolution. The question is how far Toulmin goes in providing such a theory for conceptual change.

Toulmin's evolutionary analysis of conceptual change contains several technical terms. The two most important are "population" and "discipline." Toulmin borrows the term "population" from such biological expressions as "local population," "Mendelian population," and "population genetics." According to Ernst Mayr (*1*), the biologist who has been most persuasive in his advocacy of "population thinking," a population is not simply a class as traditionally defined by philosophers in terms of essential characteristics. As a first approximation, one might say that populations are classes characterized by one or more sets of properties which co-vary only statistically. Every member of a particular population will be characterized by a sufficient number of these variably weighted properties. Any attempt to legislate a single set of essential properties which all members of a population must have results in unacceptable consequences for biological populations, such as the placing of males and females of the same species in separate species.

A more important feature of biological populations is that individual organisms do not just belong to their respective populations; they are integral parts of them. Biological populations exhibit a unity and continuity that is absent in most classes. Currently biologists disagree among themselves over the relative importance of the various mechanisms responsible for the cohesiveness of evolutionary units. One school, headed by Mayr, maintains that genetic isolating mechanisms are the only relevant factors. Hence, asexual species, though they evolve, do not form populations. Other biologists have continued to urge a minority opinion that other factors, such as exposure to common selection pressures, are also important, perhaps even more important, in the maintenance of evolutionary unity. In any case, biologists agree that on the Darwinian view individual organisms cannot evolve. Rather it is the selective perpetuation of organisms through successive generations that results in the formation of more comprehensive units which do. From the point of view of evolutionary theory, such evolutionary units as local populations and species are more like individuals than classes. And if the taxonomic principle of monophyly is accepted, they are individuals by definition (*2, 3*).

Toulmin attempts to extend Mayr's populational approach to as many historical entities as possible. A historical entity is any entity that maintains a coherence and continuity despite all the changes it may undergo (p. 356). Populations are special kinds of historical entities. They are historical entities which have resulted from the dual process of variation and selection (p. 337). Biological populations are those biological units which result from mutation and various forms of selection; conceptual populations are those conceptual units which result from conceptual innovation and the selective perpetuation of certain of these innovations (p. 134). As in the case of species, it is not the similarity among the constituent concepts that makes conceptual populations genuine populations but the factors governing their historical development (p. 85). The distinction is between the criteria that make a particular conceptual population the population it is and the criteria that make it a population.

Because this distinction has proven in the past to be elusive, let me expand on it, using a biological example. *Drosophila melanogaster* is *Drosophila melanogaster* and not some other species because it is characterized by a particular, gradually changing cluster of properties. It is a species because it exhibits the degree and kind of evolutionary unity and continuity that it does in reaction to the selection pressures to which it is exposed. Mendelian genetics is Mendelian genetics and not some other discipline because it is characterized by a particular, gradually changing cluster of concepts, problems, procedures, and goals. It is a population because it exhibits a certain degree and kind of evolutionary unity and continuity (*3*).

"Population" is a metascientific term to be explained both by general exposition and by giving examples. What are biological populations? Whatever the operative units of biological evolution are found to be as the biological theory of evolution develops. What are conceptual populations? Whatever the operative units of conceptual evolution are found to be as the theory of conceptual evolution develops. One important thing to notice is that populations are the operative units only in selection theories. If the notion of a conceptual population sounds a good deal less informative than its biological counterpart, it is because none of the current theories of conceptual development come close to the scope and power of the synthetic theory of biological evolution—and even it has a long way to go before it satisfies even its most ardent proponents. Biologists have still, for example, to integrate recent developments in genetics into the framework of evolutionary theory. One can hardly expect more of a theory of conceptual evolution.

A final point about populations: if we take the proponents of the populational approach at their word, then populations are spatiotemporal individuals and not spatiotemporally unrestricted classes. Traditional analyses of "scientific law" are then inapplicable to them, since on such analyses laws can make reference only to classes that are unrestricted in space and time. Hence, "All swans are white" could not count as a scientific law, even if it were true. I doubt that Toulmin would begrudge the loss. If there are to be any evolutionary laws concerning populations, they will have to refer to classes of such populations. For example, in biology no evolutionary law could refer to crows, opossums, or dogs; only to classes of such populations, like cosmopolitan, primitive, or domesticated species. And this is exactly what one finds in the biological literature (*3*). Thus, no one should be surprised by the inability of modern evolutionists to predict the future development of a particular species *qua* particular species on the basis of evolutionary theory. It must be referred to an appropriate reference class first. The analog in conceptual history is that no theory of conceptual development needs to be able to predict particular future discoveries, only patterns of events, such as periods of crisis always preceding scientific revolutions, an early suggestion of Thomas Kuhn (*4*), since withdrawn (*5*).

Toulmin does not seem to appreciate this distinction, because he argues that a biologist's inability to make reasonable predictions about the future emergence of novel organic species makes the theory of biological evolution in a significant sense "nonpredictive" (p. 330). The issue arises in the context of the alleged symmetry between scientific explanation and scientific prediction. On

the covering-law model of scientific explanation, on which the symmetry thesis is founded, all genuine scientific explanations can be reconstructed as arguments in which the event to be explained is inferred from a set of laws and statements of antecedent conditions. If the event is inferred after it has happened, one has an explanation of that event; if before, a prediction. Toulmin and others have argued that evolutionary theory provides an adequate basis for explaining past evolutionary events but not for predicting future ones. Hence, the symmetry thesis fails.

Some defenders of the symmetry thesis admit the apparent asymmetry of the inferences supported by evolutionary theory and conclude that evolutionary theory is not a genuine scientific theory. Others argue that no such asymmetry exists in the first place. Biologists can reconstruct phylogeny on the basis of records such as fossils and DNA sequences. Because there are no comparable records of the future, they cannot provide similar descriptions of the future. However, this asymmetry has nothing to do with the alleged asymmetry of *inferences* which can be made on the basis of evolutionary theory. I happen not to be an advocate of the symmetry thesis or the covering-law model, but I must agree with this second group of defenders. I can find no temporal asymmetry in the inferences permitted by evolutionary theory. Evolutionary theory does not permit reasonable inferences about the future development of particular organic species as unique individuals, but it does not permit such inferences about the past either. I must disagree with the first group. The inadequacy of evolutionary theory as a basis for inferring either the past or the future development of particular organic species as unique individuals does not count against it as a scientific theory, because evolutionary theory does support inferences about classes of species and patterns of evolution (*3*).

Toulmin's second most important technical term is "discipline," which he defines as follows:

> A collective human enterprise takes the form of a rationally developing "discipline," in those cases where men's shared commitment to a sufficiently agreed set of ideals leads to the development of an isolable and self-defining repertory of procedures; and where those procedures are open to further modification, so as to deal with problems arising from the incomplete fulfilment of those disciplinary ideals [p. 359].

Like biological species and conceptual populations, disciplines form populations and evolve. Toulmin introduces the notion of a "discipline" as a first step toward avoiding epistemological relativism. According to him, theoretical concepts can change rapidly and discontinuously. On occasion one scientific theory completely replaces another. An intratheoretic justification for such changes is clearly impossible. Thus, from the point of view of theoretical developments, scientific revolutions appear catastrophic and arational (*4*). Toulmin agrees that the more fundamental a theoretical change is the less it can be rationally judged by reference to principles internal to that theory, but this does not mean that such changes have no rational justification at all. Instead, they are justified by an appeal to more general disciplinary reasons which, though they also evolve, do so much more slowly than theoretical concepts and principles (pp. 5, 79, 154).

For example, "Mendelian genetics" cannot be defined in terms of its basic theoretical concepts and principles (for example, Mendel's principles of segregation, independent assortment, dominance, and the purity of the heterozygote), because these were modified extensively soon after their rediscovery at the turn of the century and have been further modified since. Yet Mendelian genetics has stayed Mendelian genetics. The continuity has been provided not by a continuity of concepts and principles, though they did play a part, but by a continuing commitment to a set of procedures, goals, and problems. The procedures were breeding experiments in which the transmission of phenotypic traits was followed from one generation to the next; the goal to discover Mendelian ratios and to explain them by postulating the requisite kind and number of Mendelian genes; the problem to make sense of the phenomena of heredity in the absence of any knowledge of the fine structure of the genetic material or how genes function to produce traits.

Thus, the content of a scientific discipline is defined not only in terms of its current repertory of concepts but also by its explanatory goals and procedures and by the accumulated experience of the scientists working in the discipline (p. 175). A new discipline evolves, not from the development of a novel theoretical concept or the acceptance of a new theory, but from the abandonment of an old research program and the adoption of a new one (p. 155). For example, molecular genetics is a separate discipline from Mendelian genetics, in part because it makes use of different theoretical concepts—biochemical pathways, enzymes, nucleic acids, and the like—but more importantly because its practitioners use different procedures, have different goals, and have accumulated a decidedly different set of experiences from those of Mendelian geneticists.

Toulmin emphasizes that complementing each scientific discipline is a scientific profession. Like disciplines, these professions also form populations—populations of scientists—and they too evolve by variation and the selective perpetuation of the variants. The study of the evolution of scientific professions is, of course, a task for the sociologist, not the biologist or the conceptual historian. However, the existence of these two complementary populations enables the boundaries of each to be drawn more precisely. The areas of vagueness do not necessarily coincide, and when they do not they can be used to cancel each other out. If a particular disciplinary boundary cannot be discerned on the basis of shared concepts, procedures, problems, and goals, check the corresponding professional boundary. And if a particular professional boundary cannot be discerned on the basis of degrees, positions, memberships in organizations, publications, and such, check the corresponding disciplinary boundary (*6*).

Toulmin also sets himself the task of formulating a theory of how scientific disciplines evolve, to show "on what occasions, and by what processes and procedures, . . . one basic set of collective concepts—in science or elsewhere—[comes] to displace another" (p. 121). I will have to content myself here with remarking that at best all Toulmin has done in this volume is to set out a research program (p. 504), which will stand or fall on how well he and his future co-workers carry it out.

The relevance of Toulmin's views on conceptual change to his goal of avoiding epistemological relativism is another matter. Toulmin seems to think that an evolutionary analysis of conceptual and disciplinary evolution will contribute to the resolution of this age-old dilemma. If it does, then it will be the first time that a genuinely philosophical problem was ever resolved by reference to facts or scientific theories, or at least this is what many philosophers would counter.

According to Toulmin, the absolutists view the acquisition of knowledge as a process in which Fixed Minds gain command of Fixed Nature by applying Fixed Principles (p. 21). Obviously much, if not all, of both nature and man's principles have undergone considerable change and variation through the years. Hence, on the face of it, the absolutists' position looks implausible. On Toulmin's view, variable minds gain command of largely variable nature by applying variable principles. He parts company with the relativists when it comes to the assumption that variability entails inexplicability. Conceptual change can be judged rational or irrational on the basis of disciplinary standards. But they too change. Periodically there are major redirections in disciplinary goals, procedures, and problems. Sometimes the principles of reason themselves come under attack. As Toulmin observes,

> A dispute over intellectual strategies is thus a dispute for which no established decision procedure exists [p. 236].

In such cases,

> Those strategies . . . must be viewed against the whole historical background of the developing rational enterprise concerned, and the men concerned will have to judge—by a critical and comparative analysis of previous experience in this field—what fresh strategy, or direction of advance, is most "promising" in this particular area of investigation [pp. 488–89].

Like Wittgenstein's reference to "whole ways of life" in his analysis of meaning (*7*) and Simpson's reference to "unitary evolutionary roles and tendencies" in his definition of the species category (*8*), Toulmin's recourse to the "experience which men have accumulated when dealing with the relevant aspects of human life—explanatory or judicial, medical or technological—in *all* cultures and historical periods" (p. 500) is infuriatingly vague. Perhaps this is all that can be said on the subject. Let us hope not, for if it is, Toulmin has taken us by a circuitous path right back to the problem of induction. If the accumulated experience of philosophers is any guide, reference to some version of the principle of induction raises at least as many questions as it answers.

To make matters worse, nothing that Toulmin has said thus far would dissuade a relativist. Perhaps reference to cross-cultural accumulated knowledge might have some bearing on the impartiality of our judgments, but not on their relativity. Judgments can be impartial and still relative. After all, knowledge has a way of continuing to accumulate. Though Toulmin argues that it is adherence to the cult of systematicity that has forced absolutists to opt for unchanging principles of reason mandatory on all rational thinkers, he himself still feels compelled to find *something* that remains unchanged—the needs and problems common to all humanity (pp. 491–503). Everything else about mankind has changed through the millennia, but not his needs and problems. Such a conclusion seems clearly incompatible with Toulmin's populational approach to historical entities.

Toulmin also succeeds in finding at least one principle which does not share in the variability of all other principles—his principle of rationality quoted earlier in this review.

> The burden of "rationality" then consists in the fundamental obligation to continue reappraising our strategies in the light of fresh experience [p. 503].

By changing his ideas in the face of fresh experience, a man increases his chances of survival and proves himself rational. I heartily agree, but not everyone during the history of mankind has subscribed to such a principle. It may not turn out to be universally adaptive in all environments. Future generations, God forbid, might come to subscribe to quite a different principle of rationality. On his evolutionary analysis, how could Toulmin argue that they were irrational to do so?

Let me conclude by suggesting a rather traditional solution to Toulmin's dilemma. I believe that Toulmin is correct when he reasons that change due to variation and selection can be understood in terms of evolutionary theories. From the fact that the units with which a theory is concerned evolve it does not follow that the theory itself evolves, however. From the fact that species evolve it does not follow that the synthetic theory of evolution evolves. Of course, on independent evidence, it is quite clear that theories about the evolution of biological species have evolved. A scientific theory of how scientific theories evolve would help us in turn to understand such changes. And I am afraid that at this point I must add "and so on." But as Toulmin argues, conceptual development can be "progressive and cumulative" (p. 384). At least, it has been in some of the physical sciences during the past hundred years or so. If this is true, then the growth of knowledge can be viewed as an unending process of gradual approximation, interrupted periodically by conceptual reorientations of varying degrees of pervasiveness.

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References and Notes

1. E. Mayr, in introduction to *On the Origin of Species: A Facsimile of the First Edition*, by Charles Darwin (Harvard Univ. Press, Cambridge, Mass., 1966).
2. M. Ghiselin, *The Triumph of the Darwinian Method* (Univ. of California Press, Berkeley, 1969).
3. D. Hull, *Philosophy of Biological Science* (Prentice-Hall, Englewood Cliffs, N.J., in press).
4. T. Kuhn, *The Structure of Scientific Revolutions* (Univ. of Chicago Press, Chicago, 1962).
5. ———, postscript to the second edition of *The Structure of Scientific Revolutions* (Univ. of Chicago Press, Chicago, 1970).
6. Kuhn (*5*) makes a similar move when he defines "scientific community" in terms of the possession of a shared paradigm and "paradigm" in terms of that which the members of a scientific community share. The procedure is not as circular as it might seem, because the respective decisions are made on independent grounds. It is possible to know that someone received a Ph.D. in genetics without knowing whether he believes that genes remain pure in the heterozygote, and vice versa.
7. L. Wittgenstein, *Tractatus Logico-Philosophicus* (Humanities Press, New York, 1961).
8. G. G. Simpson, *Principles of Animal Taxonomy* (Columbia Univ. Press, New York, 1961).

Host-Finding Strategies

Behavioural Aspects of Parasite Transmission. ELIZABETH U. CANNING and C. A. WRIGHT, Eds. Published for the Linnean Society by Academic Press, New York, 1972. xii, 219 pp., illus. $14.75. Zoological Journal of the Linnean Society, vol. 51, suppl. 1.

The central behavioral problem for a parasite is to meet its host or hosts in order to complete its life cycle. The problem has been solved by conventional patterns of responding to stimuli emanating from the host in parasites representing a variety of groups such as protozoa, worms, and insects. In addition, more exotic strategies have evolved. In some cases, the biological clocks of the organisms, particularly in the case of the protozoan blood parasites, have been adjusted by natural selection to the behavioral patterns and clocks of their primary and intermediate hosts. Specific modification of the behavior of an intermediate host is another example.

The 13 papers in this volume resulted from an interdisciplinary symposium sponsored by the Linnean Society. The subject had received little focused attention prior to being chosen by the