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

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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

organizers of the symposium. It had become apparent, however, to those interested in the control of disease that the behavioral mechanisms by which parasites reach and enter their hosts are often the most amenable to intervention. This is reflected in the fact that most nematocides affect the nervous system. The articles reflect the state of knowledge of animal behavior in general, a field in its infancy, and the current interest in the subject. In addition, they serve as a starting point for dealing with the neglected problems related to parasite behavior.

The papers cover subjects as diverse as the structure of sensory organs of trematode miracidia, monogeneans, and hematophagous insects; host-finding behavior of the tsetse fly; the behavior of specific groups of parasites; biological clocks; and human behavior in relation to the acquisition of parasites. As one begins to observe species of increased neuronal complexity, behavioral differences between individuals become more apparent. The problems these differences create for humans in relationship to their parasites are discussed in a review by G. S. Nelson that is not only comprehensive but highly entertaining.

Holmes and Bethel analyze the transmission of parasites from the intermediate host to the definitive host in terms of the predator-prey relationships. They suggest that parasites have evolved mechanisms by which they make intermediate host organisms more vulnerable to being consumed by the definitive host. Specific behavioral changes such as those exemplified in formicine ants when infected by metacercariae of the liver fluke suggest their use as a probe to investigate underlying neuronal function. Most of the cercariae ingested by the ant penetrate into its abdomen, where they encyst until the ant is eaten by the ungulate definitive host while grazing. However, a few of these worms, which appear to be specifically differentiated in that they are not infective for the ungulate and follow a different behavioral pattern from the abdominal forms, migrate to the subesophageal ganglion of the ant. The infected ants then display markedly different behavior from their uninfected counterparts, which retire to their dark anthills while the infected ants travel up and attach to blades of grass at the times of day appropriate to increase the probability of being ingested by a foraging ungulate. Experimentally, the behavioral change has been related to an opposite response to temperature or light, or both, on the part of the intermediate host specifically induced by the parasite. The *Polymorphus-Gammarus*-mallard system is also analyzed in detail. The tactic response of *Gammarus* to light is changed from negative to positive by *Polymorphus*, as a result of which *Gammarus* can be picked up off the surface of the water by the mallard, which thus becomes infected with *Polymorphus*.

The evolutionary processes leading to the meshing of behavior in the specific host-parasite relationships are illustrated by B. O. L. Duke in studies of the life cycle of the nematode *Loa loa*. Simian and human parasites can be hybridized under experimental conditions but are kept apart in the wild by two separate host-vector complexes.

The volume lacks information on the genetic basis of transmission, which is available at least on the snail intermediate hosts of schistosomes in the work of C. Richards and others. There is also little on parasites that have sedentary hosts such as plants and the special behaviors they must develop.

Many of the parasitic organisms have evolved such specific behavioral patterns that they offer interesting material for the basic study of structure and function underlying behavior and the relationship of behavior and development.

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Embryonic Field of Ecology

Insect/Plant Relationships. Proceedings of a symposium, London, Sept. 1971. H. F. VAN EMDEN, Ed. Halsted (Wiley), New York, 1973. viii, 216 pp., illus. + plates. $19.75. Symposia of the Royal Entomological Society of London, No. 6.

In the introduction to this volume Southwood establishes a framework for considering the evolution, nature, and mechanisms of the insect-plant relationship. Rothschild in "Secondary plant substances and warning colouration in insects" observes that out of four recent reviews of the subject, covering about 720 papers, only 0.5 percent of the citations occur in all four reviews and only 10 percent occur in as many as two. Van Emden and Way conclude that "a comprehensive review of the literature is not being attempted especially as much of it represents supplementary information rather than added illumination." Wood comments that "a variety of effects will undoubtedly be described in an increasing number of papers and the level of frustration in attempting to understand an already confused literature will continue to increase."

These four comments reflect what the symposium overall makes quite clear. The embryonic field devoted to animal-plant interactions is cripplingly cluttered with data and attitudes from a multitude of reductionist, descriptive, and nonevolutionary studies by the past three generations of physiologists, biochemists, agriculturalists, entomologists, behaviorists, and others. We must cease examining animal-plant interactions in the traditions of these fields, which were established for other purposes, and focus on the questions that derive their relevance directly from a wish to understand what animals do to plants and vice versa. This symposium volume with the references it includes is a good place for the uninitiated to begin such a focusing. In this small space I can offer only a few precautionary comments, and hope that the reader will take them firmly to mind in reading the entire volume.

Southwood offers the conclusion that "the foliage of seed plants is . . . often only marginally adequate nutritionally" for herbivorous insects. Such a conclusion avoids the questions of why there was selection for the structural parts of plants to be indigestible, why we would expect natural selection to produce insects with a physiology such that plants were more than marginally adequate nutritionally, and why nutrient-rich seeds are no more available to most insects than is foliage except where the seeds lack antimetabolites.

No fewer than ten of the authors indirectly warn that "the animal diet is subject to all the vicissitudes of changing chemical composition and hormone levels that result from the plant's interaction with the environment" (Osborne). For example, the larvae of *Lymantria dispar* "selectively fed upon leaves of alder which had been normally exposed to light, and avoided leaves of the same tree which had been kept for some time in the dark" (Schoonhoven). Equally, the symposium is dotted with examples of how the reaction of the plant to the insect is circumstance-dependent. "The boundaries drawn be-