

*Sociobiology and the Preemption of Social Science.* By Alexander Rosenberg. Baltimore, Md.: Johns Hopkins University Press. 1980.

John Stuart Mill was puzzled by the failure of the social sciences to produce true and significant generalizations about human behavior. He attributed this problem of science to the complexity of social phenomena. By and large, scholars concerned with the conceptual foundations of the social sciences have found this explanation satisfactory, as have many practicing social scientists. In the meantime, there has been an exponential increase in the amount of data gathered by social scientists and a proliferation of alternative paradigms, models, and methods. The long-awaited principles about social facts have not materialized and show little promise of doing so. Both sociology and anthropology have been buffeted around by every wind of doctrine. Some philosophers of social science and some social scientists have been so disturbed by the failure to produce laws of human behavior that they have suggested that the methodological unity of science, presupposed by Mill and his followers, was the root of the problem. Some social scientists have gone so far as to dispense with an interest in explanation altogether and have called for understanding, empathy, thick description, and the like. On occasion, even metaphor, intuition, and revelation have been encouraged to wait in the wings or take a bow on stage right. And of course, some have stuck to their empiricist guns and come up with nothing.

Recently the entire debate about the quality, stature, and nature of social science has been considerably enlivened by the emergence of a new form of argumentation about the explanation of human behavior, namely, sociobiology. It has not gone unnoticed that laws of human behavior are rather scarce. Some biologists, especially those working within the context of evolutionary theory and mathematical ecology, have developed a discipline named sociobiology with the express purpose of including the explanation of human behavior within the larger context of the explanation of all forms of social behavior. What is being proposed is that the life sciences can include the social sciences, by subjecting the data of the latter to the methods, concepts, and principles of the former.

Philosophers of science are naturally interested in this emerging confrontation between biologists and social scientists, and anthologies about the sociobiology debate have already appeared. There has been a distinct tendency in this debate to raise many ideological questions, and arguments have even reached the pages of the popular press. What the debate has lacked is rigorous and sustained philosophical argument.

There is no better introduction to the philosophical and scientific issues involved, no more sustained argument proposing an explanation for the failure of the social sciences to produce laws of human behavior, and no more convincing evaluation of the prospects of the emerging discipline of sociobiology than Alexander Rosenberg's *Sociobiology and the Preemption of Social Science*.

In this work, Rosenberg develops an inductive argument to the best explanation to account for the lack of social-scientific progress. He also encourages social scientists to redirect their efforts by operating within the context of the life sciences. Rosenberg does not argue for the truth of empiricism; he assumes its truth and argues that those social scientists who are followers of Mill can remain empiricists by recognizing that social science is a *life science* and that they should expect to develop and employ theories of no less and no greater generality than biological theory does. He presents us with an explanation of a failure and a prescription for a success.

What then is the best explanation for such a failure? Rosenberg argues that empirically motivated social scientists have searched for the determinants of human behavior in the joint operation of the desires and beliefs of intentional agents. It is his claim that even those social scientists, such as Claude Lévi-Strauss, who have attempted to proceed without locating the determinants at the intentional level nevertheless appeal to it at certain crucial points.

We are thus presented with the following situation. We obviously are sometimes correct in identifying particular desires and beliefs as the causes of particular actions. However, no matter how hard we have tried, we have failed to find any laws that will sustain the assumption that we are sometimes correct in such causal identification. Rosenberg thinks that it is a methodological principle that causal claims must be sustained by laws. The one candidate for such a law (proposed by Paul Churchland) does not qualify, because it fails to designate a causally homogeneous class of events, states, and conditions. Such events, states, and conditions are not natural kinds. He does not deny that such "kinds" (desires, beliefs) may be used to express true, singular, causal judgments, but he insists that there is no law expressible in terms of these notions that can sustain such singular claims.

Naturally, the claim that beliefs, desires, and actions are not natural kinds is itself in need of independent substantiation. Rosenberg provides the proof by showing that the concept "species" as used by biologists is not a natural-kind concept but the name of a spatio-temporally restricted particular. *Homo sapiens*, in other words, is a spatially distributed object and does not appear in the laws proposed in the life sciences. What does appear in such scientific laws are qualitative predicates. What we are to conclude from this is that there could not be species-specific laws and, hence, laws of human behavior. Although human behavior can instantiate a law, it cannot confine one.

So we have reached a dead end. The empiricist spirit was willing but the concepts were weak. In fact, they were still-born. Rosenberg insists that the exclusion of species-related notions from the vocabulary of scientific laws is not a philosopher's trick but a fundamental constraint on scientific theory that nomologically successful sciences like physics, chemistry, and biology have satisfied and that unsuccessful ones like sociology and anthropology have not. Behavioral psychology is a more dubious case, but he shows there are serious obstacles to the development of that discipline.

What we have, then, is not a problem with the *empirical commitments* of many social scientists. That has been evident since John Stuart Mill. The problem lies in the failure of such scientists to *grasp the constraints on scientific laws*. The new direction for social scientists to move, then, is to discover the kinds of laws, with the appropriate qualitative predicates, that are applicable to human behavior. From Rosenberg's argument it is clear that such laws can be found only at the level of either neurophysiology or population and evolu-

tionary biology. The chances of development, as some psychologists have proposed, at the level of neurophysiology are slim. Sociobiology, therefore, becomes the likely context for the redirection of social-scientific energies.

There is, of course, a great deal more in Rosenberg's essay than has been presented here that makes it such a worthwhile book to read. He even invents intriguing examples to show how sociobiology might not only "preempt" sociological "explanations" but, in fact, could produce surprising new facts about human behavior.

What is clear is that in the next few years, social scientists and sociobiologists will be wrestling with empirical issues and that sociobiology will be challenged to produce the kinds of explanations and predictions that Rosenberg thinks they are capable of. Philosophers, of course, will remain content to let these opposing sciences fight it out. But clearly, Rosenberg's essay is of considerable philosophical interest. The most important issue is whether Rosenberg is correct about the nature of scientific inquiry and the kinds of constraints upon scientific explanation. Rosenberg is quite clear in stating that he assumes and does not argue for the truth of empiricism. He acknowledges that the rationalist/empiricist debate will be around for a long time to come.

One minor problem I have with Rosenberg's case is not with the assumption of empiricism but with the *content* of that assumption. Clearly, as Rosenberg himself acknowledges, there are different versions of empiricism. Does the kind of empiricism one assumes make a difference to the kind of argument one develops about the status of the social sciences? For example, the empiricist distinction between the logic of discovery and the logic of evaluation has become increasingly clouded in philosophical debate. Far more attention is now being paid to the rationality of scientific discovery. Social scientists desperately wish to discover significant facts about human behavior. While the paucity of laws is obvious to any honest philosopher and scientist, there is no guarantee that moving down a level as a heuristic procedure will be more nomologically productive. Rosenberg is aware of this problem and thinks that our expectations may be too high. I would be the last to claim that reasons, beliefs, and desires are sufficient for explaining human behavior. But it is not at all clear that it is terribly efficient for social scientists to move down a level when they are dealing with economic, political, or religious systems. Furthermore, cognitive science seems to show some promise of dealing with the problem of mechanisms in a nonintentionalist vein.

Carl Hempel acknowledged the heuristic importance of the kind of functionalism employed in earlier versions of social science, although he saw no reason for expecting laws of human behavior to come from such inquiry. By acknowledging the highly idealized version of science characteristic of empiricism, and by focusing on discovery procedures, we might not so much argue for the preemption of social science as the reevaluation of the nature of all forms of scientific inquiry without having to conclude that we are forced into a rationalist mode.

Empiricism has an important normative quality. It emphasizes what is required of scientific inquiry, how it should proceed. Without such norms, philosophy degenerates into a purely descriptive enterprise. And there are better describers around; we call them scientists. These norms, however, need to be flexible enough to acknowledge a broad range of discovery procedures within the scientific domain. None of these latter comments, however, should

detract from the first-rate quality of *Sociobiology and the Preemption of Social Science*. Social scientists ignore it at their peril.

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