

ON MARTIN EGER'S "A TALE OF TWO CONTROVERSIES"

by Abner Shimony

Abstract. Criticisms are presented against Eger's challenge to the demarcation between the natural sciences and ethics. Arguments are given both against his endorsement of the "new" philosophy of science and against his rejection of the fact-value dichotomy. However, his educational recommendations are reinforced rather than weakened by these criticisms.

Keywords: creation/evolution; fact; moral education; "New" philosophy of science; normative fact; value.

Martin Eger's essay is extraordinarily rich in penetrating philosophical comments and in educational good sense. Nevertheless, I believe that there are serious errors in his fundamental philosophical theses, and much of this commentary will be devoted to exhibiting them. I shall then try to show that for the most part his educational recommendations are reinforced rather than weakened by my theoretical criticisms.

"NEW" AND "OLD" PHILOSOPHY OF SCIENCE

Eger challenges the demarcation of the natural sciences from the study of morals by questioning that they are different cognitively. The demarcation was clear and strict, he says, as long as an old, essentially positivist conception of the natural sciences was maintained and as long as the fact-value dichotomy was accepted. The "new philosophy of science," however, has profoundly criticized the old conception, and in spite of some reservations Eger on the whole assents to these criticisms. Likewise, he assents to arguments against the fact-value dichotomy, and indeed at one point he provides a very interesting argument of his own, which may be original while he makes no claim to this effect: "True, there is no consensus among *theoretical* experts on rules or

Abner Shimony is professor of philosophy and physics at Boston University, Boston, Massachusetts 02215.

[*Zygon*, vol. 23, no. 3 (September 1988).]

© 1988 by the Joint Publication Board of *Zygon*. ISSN 0591-2385

principles, nor on frontier issues. . . . But in regard to exemplars—history's moral heroes—the situation is quite different. When it comes to these *practical* experts, a widely acceptable list can indeed be drawn up. And imitating exemplars, as Kuhn has shown, is at once surer and more flexible than acquaintance with rules" (Eger 1988, 315).

Eger undervalues the "old" philosophy of science partly because he uses this term to refer to only a part of a diverse and complicated collection of methodological and epistemological doctrines. Positivism—which maintains that the total content of a scientific theory lies in its implications for human experience—was indeed very influential during the first half of the twentieth century. During this period there were also influential realists, who followed the tradition of Galileo Galilei, Isaac Newton, and John Locke of attempting to infer from experience the properties of an objective world which has an existence independent of human beings. (For example, Bertrand Russell in 1914 was a positivist, but by 1927 Russell had been converted to realism.) Eger also attributes to the old philosophy of science the use of algorithms of inference, but I have no idea to whom he may be referring. The formulations of scientific methodology in the best of the old philosophers of science (my personal favorites being Charles S. Peirce and Harold Jeffreys) are complex and sophisticated syntheses of diverse intellectual elements: hypothetico-deductive reasoning, probability theory, decision theoretical arguments, appeals to evolutionary biology to justify human skill in hypothesis formation, and appeals to the history of science for *a posteriori* refinements of method (see Shimony 1970). It should not be surprising that an adequate scientific methodology is complicated, if one considers the ambitiousness of the scientific enterprise: namely, to obtain good approximations to the objective truth about the universe at large, on the basis of experience which is very limited in space and time and constrained by the peculiarities of human faculties for gathering and processing information. It is true both that "Facts are stubborn things" and that "Nature loves to hide," and an adequate methodology must do justice to each of these divergent dicta.

I do not wish to deny that the new philosophy of science has made some real contributions. It has emphasized the indispensability of the history of science for a rich philosophy of science. Some of the innovators (notably Michael Polanyi and Thomas Kuhn) have pointed out that skilled scientific practitioners typically have much more "tacit knowledge" of their craft than they are able to articulate in explicit rules of scientific method. Some (notably Norwood Hanson) have drawn upon empirical psychology in order to carry out epistemological analyses of observation and other mental processes. For the most part,

however, the great value of these insights has been debased by drawing from them relativistic and subjectivistic epistemological conclusions. The following are some of the important criticisms that should be made of the new philosophy of science.

First, the history of science need not be used as a surrogate for scientific methodology, as Kuhn maintains, with each historical epoch providing its paradigms which cannot be judged from a neutral standpoint. Instead, the history of science may be studied in order to provide *a posteriori* elements in scientific method, for there is no reason to believe that the human intellect is endowed *a priori* with all the methodical tools it needs for investigating the natural world. Experience is needed not only to learn substantive truths about nature, but also to learn how to learn (see Shimony 1976).

Second, the great virtue of the tacit knowledge of skilled investigators is not its tacitness but its knowledge. If methodologists eventually are able to articulate what these investigators know tacitly (as good athletic and musical coaches are able to do in their respective domains), then nothing is lost thereby and something is positively gained.

Third, the deployment of data from empirical psychology, especially from Gestalt psychology, in order to show that observations are "theory laden," provides *prima facie* evidence against the possibility of objective empirical assessments of competing scientific hypotheses. Yet a careful study of empirical psychology—such as the experiments on "cognitive dissonance" by Jerome Bruner and Leo Postman—reverses this judgment, and shows that human beings are capable of switching between integrative and analytic strategies of perception, and the latter is strikingly liberated from theory-ladenness (see Shimony 1978).

Finally, the occurrence of scientific revolutions is an insufficient reason to deny the meaningfulness of the concept of objective truth and to recommend that it be replaced by a concept of historically relativized truth, as Kuhn recommends (1970, 171-73). The most that one can legitimately infer is the unlikelihood that human beings can ever *achieve* the goal of objective knowledge of the universe. However, even this concession to relativism is excessive, for it fails to pay due attention to the detailed history of those scientific revolutions which have occurred since the seventeenth century. Typically in these revolutions the displaced theory is a good approximation to the displacing theory, with regard to empirical predictions and in some respects with regard to conceptual structure. In the terminology of Niels Bohr, there is a "correspondence principle" governing the relation between the old and the new theory. Consequently, the appropriate moral to be drawn from the occurrence of scientific revolutions is not relativism but a doctrine of successive approximations to the truth.

In summary I find a pervasive slovenliness of reasoning in the new philosophy of science. Its advocates have failed to use its excellent insights constructively by exploiting them for the refinement of scientific methodology and epistemology. This constructive enterprise requires hard work, which is evaded by their relativism and subjectivism, and of these Eger is too tolerant.

THE FACT-VALUE DICHOTOMY

The relation between facts and values is very subtle, and I make no pretense to professional expertise about it. However, I wish to present a few considerations which should make one resist any simple conflation of the natural sciences and ethics and to distrust the claim that rational criteria are the same in both domains.

At one level there is a universally admitted dichotomy: that what a person does and what a person ought to do in a given situation are not generally the same. What the person does is a fact, and perhaps what he or she ought to do is also a fact, but if so it is a fact of a different kind. The fact-value dichotomy would then be converted into a dichotomy within the domain of facts. But what, then, is the ontological status of the second type of fact, that is, of normative facts?

A possible answer is that desires are factual, and desires may or may not be achieved. Might not the ontological status of a normative fact be that of a desire? No, this suggestion is not sufficient for two reasons. Most people who are not morally nihilistic would make moral judgments not only about achievements but about desires themselves: that some desires are better than others. Secondly, in the same situation two different persons may have different desires as to what the actor should do. (The difference of opinion may depend upon whether the person judging is the same as the actor, but this is not the only crucial factor, for two people may disagree about what is desirable were their respective situations in the action to be exchanged.) Consequently, the identification of normative facts with desires would deprive normative facts of an objective or interpersonal ontological status.

It may be suggested that the ontological status of the normative fact is that of an authoritative prescription, with different versions of this point of view recognizing different authorities: God, society, the evolutionary history of the human race, and so on. I am skeptical that any of these appeals to authority can account for the motive force of a normative fact unless they endow the authority with a power of enforcement, to punish infractions and reward obedience. If the authority is so endowed, then its prescription is really a hypothetical imperative rather than a categorical imperative, for the motive force is effectively the desire of the subject to avoid pain or achieve gratification. In brief,

despite the above-mentioned shortcomings of the identification of the normative fact with desire, it is unrealistic to neglect the role of desire in the analysis of value.

My own (tentative) position is to relate norms to desires, but to do so in the wise manner suggested by Aristotle's *Ethics*, which points out some remarkable facts about the structure of desire. He notes that all people agree verbally as to what is desirable: namely, happiness (Aristotle 1.4), but they have many different ideas as to what constitutes happiness. People also have subordinate desires—for example, for wealth or honor or learning—and the satisfaction of these may or may not lead to happiness. Furthermore, because a human being is neither a beast nor a god but a social animal, his or her happiness is bound up with the happiness of others, in the family or in the state (or—by a nonaristotelian extrapolation—in the human race as a whole). It is a fact about human nature that character—and hence what is desired by an individual—is to some extent plastic, and is formed by habituation and education. It is also a fact that not all modes of forming character are equally conducive to that universally desired but vague end, happiness. Most of Aristotle's *Ethics*, after the preliminaries of the first book, is devoted to the investigation of the moral and intellectual virtues, which he regards to be the true way to happiness. Even if one has followed Aristotle this far, however, one may suspect that he was unavoidably ethnocentric, because of the limitations of his social experience. One may draw upon the mass of evidence accumulated by anthropologists to suggest that there is not one but a plurality of ways to human happiness (e.g., a contemplative life, a kinaesthetic life, a ritually organized life). One is not thereby committed to a thoroughgoing cultural relativism, for not all cultures are equally satisfying to their practitioners in their own eyes; as Edward Sapir (1924) pointed out, some among the great variety of cultures are "genuine" and some are "spurious."

It should be clear why I resist Eger's attempt to narrow the gulf between the natural sciences and ethics. With regard to the former, I have argued, albeit briefly, that there is a domain of entities independent of human experience which are endowed with definite properties, and a scientific proposition is objectively true if it correctly characterizes this domain. Whatever the difficulties may be for human beings to discover on the basis of their limited experience the objective truth, it is, so to speak, "there" to be found out. In ethics, however, the ontological status of the normative facts is much more problematic. I do not wish to say that they are merely matters of convention or subjective opinion, for it is not the case that "anything goes"; the constitution of the human psyche and the social character of human

beings set limits upon the range of life styles which will permit the achievement of happiness by the standards of the subjects themselves. Yet it is by no means clear that these constraints uniquely determine an optimum life style, and with it a unique set of norms.

PEDAGOGY

After these criticisms of Eger's philosophy of science and ethics I shall turn to his discussion of pedagogy, with which I largely agree. He is properly outraged about an authoritarian handling of the creation/evolution controversy combined with the slackness of the program of moral education in the public schools.

The passages cited by Eger against teaching creationism express anxiety about its corrosive effect upon the intellectual faculties of young students. I agree, of course, that intellectual faculties should be cultivated, and a primary way to do so is to perform experiments and demonstrations concerning phenomena which are simple enough to permit a fairly complete exercise in scientific methodology, without gaps and without the intrusion of authority. It must be quite confusing, and perhaps even demoralizing, to thoughtful students to be presented with fragmentary and authority-adulterated applications of the scientific method unless the instructor is candid about the lacunae in the reasoning. How is the student to know whether his or her vagueness of understanding is due to the incompleteness of the reasoning itself or to personal intellectual shortcomings? With this consideration in mind, how is a rational account of the theory of evolution to be presented to young students? Here is the most outstanding case in the history of science of a great theory which is confirmed globally, by an immense variety of taxonomic, zoogeographic, embryological, and paleontological evidence falling into place, rather than by the prediction of striking, unexpected phenomena. The long history of resistance to the theory of evolution (see Mayr 1982, 510-70)—because of genuine conceptual difficulties, not just because of stubborn dogmatism—shows how ill-suited this theory is for elementary instruction. What then would be the danger of a good, open debate by clever students about the creation/evolution issue? It is hard to imagine a creationist positively persuading a classmate who is not antecedently convinced to accept the Biblical account. The worst that is likely to happen, from the standpoint of an advocate of evolution, is that the class will forcefully feel the lacunae in the standard textbook presentations, and is this such a bad thing for the cultivation of intellectual faculties? I suspect (partly because of introspection) that people who object to permitting creationism to be discussed in the public schools fear that it somehow will be the opening wedge of a general anti-intellectual, authoritarian,

fundamentalist, and fascist seizure of political power. However, I believe that the real danger of such a catastrophe lies in racial and economic tensions, which will not be assuaged by the prohibition of a debate on creationism.

The case against a curriculum of moral education which emphasizes "the critical attitude" and "choice among alternatives" can be built solidly upon Aristotle's *Ethics*. A young person is not suited for lectures on ethics because of inexperience in the actions that occur in life (Aristotle 1.3). Furthermore, first principles in ethics (as opposed to those of the theoretical sciences, which are obtained by induction) are acquired by habituation (Aristotle 1.7), and inculcation of the moral virtues by habituation must precede the acquisition of the intellectual virtues by instruction (Aristotle 2.1). An important way in which the schools can contribute to this inculcation seems not to have been mentioned by Aristotle, namely, to capture the students' imaginations. For this purpose an exposure to the biographies of "history's moral heroes," in Eger's phrase, may be particularly efficacious.

At this point there is a major ideological conflict between Aristotle and the designers of the curriculum of moral education. Aristotle believes that ethics and politics are continuous, and that the state is responsible for the moral education of the child not just for the child's sake but for the good of the state as a whole. The designers of the curriculum of moral education, on the other hand, wish to develop the child's independence of judgment in order to be able to resist the authoritarian claims of the state.

How can one adjudicate this controversy? I would say, above all not *a priori*. Only on the basis of experience can one judge whether a person inculcated with moral virtues in childhood and only later exposed to ethical analysis is more self-confident, more judicious, more tolerant, and in general more rational than a person whose critical attitudes on moral matters is fostered in early childhood. Of course, the question is complex, and much depends upon the exact character of the early inculcation. Likewise, only on the basis of experience can it be decided whether the inculcation of a sense of responsibility to society is subversive of individual happiness. This question is also complex, and the answer depends crucially upon the mode of inculcation and upon the details of the relation between individuals and society. In appealing to empirical evidence, however, I do not mean to conflate these difficult ethical, political, and pedagogical questions with the problems of the natural sciences. Practical reason is not concerned with the aspects of the human mind which are genetically fixed, but rather with those which are plastic. Hence, the evidence which it must marshal has to be drawn from human history and from the experience of people who have struggled with the concrete problems of life.

REFERENCES

- Aristotle, *Nicomachean Ethics*. Many editions, including 1941. *The Basic Works of Aristotle*, ed. Richard McKeon, 927-1112. New York: Random House.
- Bruner, Jerome S. and Leo Postman. 1949. "On the Perception of Incongruity: a Paradigm." *Journal of Personality* 18:206-23.
- Eger, Martin. 1988. "A Tale of Two Controversies: Dissonance in the Theory and Practice of Rationality." *Zygon: Journal of Religion and Science* 23:291-325.
- Hanson, Norwood Russell. 1958. *Patterns of Discovery*. Cambridge: Cambridge Univ. Press.
- Jeffreys, Harold. 1948. *Theory of Probability*, 2d ed. Oxford: Oxford Univ. Press.
- Kuhn, Thomas. 1970. *The Structure of Scientific Revolutions*, 2d ed. Chicago: Univ. of Chicago Press.
- Mayr, Ernst. 1982. *The Growth of Biological Thought*. Cambridge, Mass.: Harvard Univ. Press.
- Peirce, Charles Sanders. 1932. *Collected Papers*, vol. 2, ed. Charles Hartshorne and Paul Weiss. Cambridge, Mass.: Harvard Univ. Press.
- Polanyi, Michael. 1958. *Personal Knowledge*. Chicago: Univ. of Chicago Press.
- Russell, Bertrand. 1914. *Our Knowledge of the External World*. London: Allen & Unwin.
- _____. 1927. *The Analysis of Matter*. London: Allen & Unwin.
- Sapir, Edward. 1924. "Culture, Genuine and Spurious." *American Journal of Sociology* 29:401-29.
- Shimony, Abner, 1970. "Scientific Inference." In *The Nature and Function of Scientific Theories*, ed. R. G. Colodny. Pittsburgh: Pittsburgh Univ. Press.
- _____. 1976. "Comments on Two Epistemological Theses of Thomas Kuhn." In *Essays in Memory of Imre Lakatos*, ed. R. S. Cohen, P. K. Feyerabend, and M. W. Wartofsky. Dordrecht: Reidel.
- _____. 1978. "Is Observation Theory-Laden? A Problem in Naturalistic Epistemology." In *Logic, Laws, and Life: Some Philosophical Complications*, ed. R. G. Colodny. Pittsburgh: Univ. of Pittsburgh Press.