

means exhaust the insults. This is unfortunate as their attitude turns a useful book, with valuable contributions from a number of writers, into a polemic.

PAUL POJMAN, TOWSON UNIVERSITY

Miriam Solomon, *Social Empiricism*. Cambridge MA: The MIT Press (2001), xi + 175 pp., \$32.00/£21.95 (cloth).

Until recently, philosophers tended to neglect the social dimension of the enterprise of knowing. Though traditional epistemology and naturalism differ widely in their analyses of knowledge, they both treated knowledge in an individualist framework. In this setting, it was no accident that SSK (sociology of scientific knowledge) became a major competitor to mainstream epistemology. By making social factors prominent in their approaches to scientific knowledge, the advocates of SSK filled a gap that had been unjustly neglected. Yet, the proposed filling was not to everyone's satisfaction, and justly so. For most of the advocates of SSK equated the social with the non-cognitive and simply neglected, or eliminated, epistemic factors from the characterisation of knowledge. In addition, the normative component of epistemology gave way to a purely descriptive enterprise.

Social Empiricism is an attempt to inject social sensitivity into naturalism, while retaining its normative component and avoiding the excesses of SSK (e.g., their denial of truth as an aim of science). As Solomon explains, her position is *empiricist* "because of the emphasis on the empirical success" (120). And it is *social* because of its claim that "scientific rationality is socially emergent and not dependent on such conditions as individual clear thinking, rational decision making or reasonable inferences" (12). The structure of the book is as follows. First, (chapters 2 and 3), Solomon outlines her views on empirical success and realism. Second, (chapters 4–6), she introduces the notion of a "decision vector" and explains how dissent and consensus emerge in science. Finally (chapters 7 and 8), she presents social empiricism and compares it with other "standpoint epistemologies" (e.g., feminism). Solomon offers a number of case-studies, but they are scattered across the book (some of them being continued in several chapters). I suspect that a better organisation of these studies would have helped Solomon to make her points more forcefully.

In chapter 2, Solomon argues that there are many kinds of empirical success in science. Here is her list: "observational, predictive, retrodictive, experimental, some explanatory and some technological" (31). From this, she draws the further conclusion that this point supports "the thesis of the

disunity of science" (31). I'm afraid it doesn't. Unless it is shown that different sciences are *typically* associated with different kinds of success, the (platitudinous view of the) multiplicity of types of success does not lead in any way to the (much more controversial view of the) disunity of science. Taking a stance in the realism debate, Solomon defends (in chapter 3) what she calls "whig realism". This is the view that "when empirical success needs explanation (. . .) it is due to there being *some truth* in the theories" (39). We are then told that although the "statements of the theory are false", there is a true "implication of the theory at the theoretical level (. . .) that may or may not be explicitly derived during the historical period in which the theory is accepted" (39). Now, what is worth stressing is that this position verges on incoherence. The problem arises from Solomon's unwillingness to even try to identify the theoretical components of the theory that are likely to be true (or approximately so), given the theory's empirical success. In fact, this unwillingness is accompanied by a critique (36–8) of recent attempts, such as Kitcher's, to achieve this identification. So, whig realism asserts *both* that for each and every theoretical assertion P of an empirically successful theory we *don't know* whether it is true and that we do know that *some Ps are true*. Even if this joint assertion is formally consistent, its first part removes any basis for accepting the second. Couldn't the empirical success itself be the very basis for the claim that there is some truth in the theory? Not really, for empirical success (without further constraints on its character) is very easy to come by. I think that the positive content of whig realism is just the view that observable phenomena are more likely than not to have unobservable causes. But this is too thin to warrant a realism worth fighting for.

The novelty of *Social Empiricism* is in its attempt to explain dissent. Consensus is defined as "a special case of dissent (where the amount of dissent approaches zero)" (12). The framework in which scientific decision-making is analysed is couched in terms of "decision vectors" (53), a neutral expression which is meant to capture all factors that may influence the outcome of a decision, and which include empirical vectors, such as "salience" and "availability" of data and "novel predictions" (57), as well as non-empirical vectors. The latter, however, are not just the traditional theoretical virtues (simplicity, explanatory power etc.). They explicitly include broadly social factors, such as "ideology", "pride", "peer pressure", "deference to authority" etc. (57–8). We are then presented with a model of how these vectors can be added up in order to evaluate the outcome of theory-choice at the (social) level of the scientific community. Here, however, things get a bit complicated. For what we should be after is a "normative account", which "evaluates decision vectors for their conduciveness to scientific success" (63). What we seem to get instead is a *descriptive account* (based on a number of case-studies) of how the decision vectors

were distributed among several competing theories. To be sure, this descriptive account is informed by a couple of general methodological assumptions, viz., that empirical vectors should be distributed *equitably* (that is, in proportion to empirical success) and that non-empirical vectors should be distributed *equally* to the various theories under consideration (76–7). But it's not clear how these methodological assumptions get normative force. Without further argument, it is at least question-begging to argue that so disparate non-empirical vectors as *agreement with scripture* and *simplicity* should be distributed equally to theories.

Chapter 7 aims to show that social empiricism deals with cases of consensus, whether this is establishing a consensus on a theory or dissolving an existing consensus. It is certainly an advantage of Solomon's theory that both consensus and dissent are explained within the same framework. But there is a point that needs special mention. She argues that "it is appropriate to form consensus only in the extreme case that one theory has *all* the empirical successes" (119). However, there are cases in which there are Kuhn-losses: the new theory does not successfully explain something that a competing theory does (e.g., the common direction of the planets' rotation was explained by Descartes' theory but not by Newton's). Does that mean that it was normatively inappropriate for the scientists to form a consensus around Newton's theory? This negative judgement seems to be implied by social empiricism. But if so, this means trouble for social empiricism. In comparing social empiricism with feminist epistemology (chapter 8), Solomon rightly stresses the need for "epistemic fairness" alongside the feminist call for "political fairness" (148). But we shouldn't lose sight of *epistemic correctness* (no less than of political correctness).

I recommend *Social Empiricism* to all those interested in the prospects of social epistemology—which should be just about everyone. The book is brisk, though some hard issues are treated too quickly. And it is certainly insightful.

STATHIS PSILLOS, UNIVERSITY OF ATHENS