Husain Sarkar
Group Rationality in Scientific Research.
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Ever since the publication of Thomas Kuhn's *Structure of Scientific Revolution* philosophers of science have been much concerned with the problem of the rationality of science in terms of its practitioners. Interestingly enough, the problem of rationality in science has been of minimal concern for scientists themselves. This is not say, however, that scientists have not been concerned with the question of the validity of scientific claims. It is rather that modern scientists have been so convinced of the relative superiority of their research methods that questions concerning the rationality of their research protocols are hardly ever raised.

But yet the overarching epistemological questions concerning the products of scientific research must still be answered if the general comprehensive questions concerning the ultimate meaning of the scientific enterprise is to be understood. It is in this context that Husain Sarkar's *Group Rationality in Scientific Research* is to be understood. Sarkar's basic question is a simple one: 'Under what conditions is a group of scientists rational?' (ix). His answer is a varied one which takes us through myriad approaches to his question.

Sarkar begins by informing us that the group rationality of scientists is not an evolutionary problem, nor is it a game theoretical one. This means that his analysis of the problem is to be approached from a variety of standpoints, because there has been no single structure of research that successful scientific theories have followed over the years. Matters have simply evolved. Compare, for example, the research programs of a Newton, Einstein, Hubble, and the phalanxes of researchers working in their various laboratories funded from private or governmental sources. The truth is that successful research programs over the years have followed diverse paths, both evolutionary and game theoretical, but all maximally dependent on the creativity and behavioral psychology—quirky, eccentric, bland, ultra-competitive, etc.—of their lead scientists.

In any case, Sarkar's goal is first to formulate the problem of group rationality, then to critically evaluate attempts to solve it. These he lists as the skeptical position, which carries both negative and positive components; the subjectivist solution with its two versions; the objectivist position; and lastly Putnam's 'obligation to be reasonable' position as the basis for group rationality on the part of all scientists (241). Sarkar finds little comfort in the set of approaches he lists and discusses, so his final decision amounts to the following: 'Ultimately, I decided to present the cardinal problems of group rationality—nine in all—as a substantial body, not merely a filigree, and to color it as a distinctive point of view' (243). These nine problems could be summarized as answering

a first question concerning 'what … the structure of a society of scientists—where structure is defined in terms of method—[ought to be] at a given time, or over a period of time, that would enable that society to reach its goals better than it would under any other structure' (244). A second question entails exploring the conditions under which a group of scientists would want to offer their allegiances to such a structure. To answer this question Sarkar invokes Rawls's idea of an 'original position' from which would be derived a 'structure defined in terms of methods—that will give the society of scientists a higher probability of reaching the truth than any other structure' (257).

At this stage Sarkar again poses the question of whether Putnam's approach to the problem via the idea of a generalized individual rationality could be the answer. Such a rationality would spring from Putnam's assumption that 'we have an *underived,* a *primitive* obligation of some kind to be reasonable' (240).

Sakar's topic and ideas are useful if only because of the need to keep posing questions about the practical and theoretical role of science in modern life. However, the questions he poses are misplaced, given that the problematic of science in terms of group rationality is not to be parsed within research programs themselves but between them. Research programs tend to sort themselves out in strictly evolutionary fashion based on contingencies such as the creativities and personalities of lead research scientists and the teams of assistants and kinds of funding that they attract.

As Kuhn pointed out in his *Structure of Scientific Revolutions* established paradigms that have been successful hardly tolerate critics within their ranks, thereby establishing a priori the basis for a group rationality. Sarkar's idea of a 'Council of Scientists' that would 'arrive at a covenant about how to structure their society' would hardly ever apply, because successful research programs develop simply from the research prowess of lead scientists who almost single-handedly assemble their own teams.

In this regard, the dynamics of actual scientific research is clearly stated by prominent physicist Lee Smolin, who in his *The Trouble with Physics* (2007) effectively argues for a Kuhnian sociology of scientific research. Accordingly, the norms of research are more or less established contingently, according to the prevailing sociology of scientific research. Smolin worked for a while in string theory but abandoned that line of research when it did not bear fruit. One major problem with string theory is that it relies maximally on theoretical analysis. The adherents to this research program have nevertheless remained committed and tend to be opposed to alternative theories. As Smolin puts it: 'There is good evidence that the progress of string theory itself has been slowed by a sociology that restricts the set of questions investigated and excludes the kind of imaginative and independent-minded scientists that progress requires' (Smolin 2007, p. 268). No doubt, it is just this approach to scientific research that fits in with the questions posed by Sarkar.

Sarkar's book is useful in that it brings to the fore questions concerning the possible ways in which scientific communities ought to be structured. The problem, though, is to answer the question of how much attention research scientists pay to issues concerning the organization of research communities. Apart from questions of scientific fraud, scientific research programs usually find their own styles and research moorings. Under such considerations human psychology rather than norms of universal rationality tend to hold sway. Given the relative success of the scientific enterprise it would seem that particular rather universal rationalities have been selected for. For example, the Crick-Watson research program had its own style, which was evidently at variance with the equally successful one of, say, Richard Feynman.

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