Group Rationality in Scientific Research

Under what conditions is a group of scientists rational? How would rational scientists collectively agree to make their group more effective? What sorts of negotiations would occur among them and under what conditions? What effect would their final agreement have on science and society? These questions have been central to the philosophy of science for the last two decades. In this book, Husain Sarkar proposes answers to them by building on classical solutions – the skeptical view, two versions of the subjectivist view, the objectivist view, and the view of Hilary Putnam. Although he finds none of these solutions completely adequate, Sarkar retrieves what is of value from them, and also expropriates the arguments of John Rawls and Amartya Sen, in order to weave a richer, deeper, and more adequate theory of group rationality.

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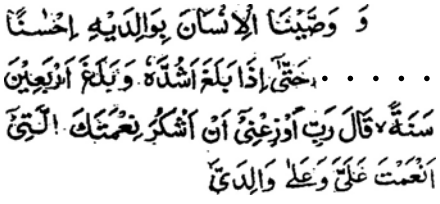
For

My Mother and Father

Alas, I am so *unforgivably late:*



We have enjoined man to show kindness to his parents. . . . When he grows to manhood and attains his fortieth year, let him say: 'Inspire me, Lord, to give thanks for the favors You have bestowed on me and on my parents.'

The Koran, *Chapter 46, "The Sand Dunes," Verse 15*

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Preface

The problem of group rationality can be fairly simply stated: Under what conditions is a group of scientists rational? There are a few scattered answers, some not so well known, some not so well reasoned. Our task is to examine some of these theories of group rationality to show why that problem is a marvelous puzzle, why that puzzle is yet unsolved, and why it needs solving. Hitherto, philosophers of science have dealt with the following problems: Under what conditions is a theory scientific? (Popper: when the theory is falsifiable.) Under what conditions is a theory making scientific progress? (Lakatos: when the theory is a successful research program.) Under what conditions is a decision to accept a scientific theory rational? (Bayes: when the prior probability of the scientific theory multiplied by the likelihood of the theory, the total divided by the probability of the evidence, is high.) Finally, under what conditions is a scientist generally rational; in other words, what is individual rationality? There is a paucity of theories on this; perhaps attempts at solving the problem of group rationality will stimulate interest in this area by making it evident that this last question lies intriguingly beneath the topsoil.

In 1983, the problem of group rationality was a new problem. By now, more than twenty years later, several philosophers have been engaged in the task of solving it; but in solving it, they have sometimes assumed that it is a problem of a different stripe. The problem of group rationality, I shall argue in Chapters 2 and 3, is a unique problem. In the first chapter, as in the last, the work of John Rawls plays a preeminent

role, both when I appropriate his views to show how in some places the problem of group rationality is immensely enriched if we heed his work, as well as when I show why in other places we might do well to explore on our own.

In Chapter 2, I shall argue that it is *not* a problem to be solved by the strategy of using evolutionary mechanisms; it is *not* akin to the problems treated in game theory (with its fruitful and fascinating analysis and extension of the Prisoner's Dilemma, iterated or otherwise); and it is *not* an adapted version of the problem of social justice. (How applications of such approaches in the domain of group rationality can lead to unmitigated disaster is illustrated in Chapter 5.) I have also drawn upon the ingenious, marvelously inventive work of Amartya Kumar Sen in Chapter 3; that merest sketch should indicate to the reader the rich possibilities that Sen's approach in welfare economics and social choice theory, duly adopted, promises to the field of group rationality. But I shall also show in this chapter that the problem of group rationality is *not* simply another version of a problem in welfare economics.

Game theory, evolutionary dynamics, and welfare economics may eventually throw a good deal of light on the problem of group rationality; for now, however, we must perforce use the less formal, more substantive, traditional philosophical method – not a less likely route to harvesting rich results. Indeed, unless concepts custom designed for a theory of group rationality are available – as are concepts unique to the domain of justice – game theory, welfare economics and social choice theory, and evolutionary dynamics will not know *what* they are supposed to explain in the first place.

The purported solutions to the problem of group rationality have entered a phase that might be called classical orthodoxy. This book is about some of those attempts to solve that problem. I have allowed myself to reconstruct solutions, weaving materials extracted from the works of philosophers who have collectively, and masterfully, defined the field of methodology. Thus, although Paul Feyerabend, Thomas Kuhn, Imre Lakatos, and Karl Popper have never spoken of the problem of group rationality – not in any direct way, at any rate – I have scavenged their works to reconstruct various solutions to the problem. This task is performed in Chapters 4 through 7. This results in the presentation of the skeptical view, two versions of the subjectivist view, and the objectivist view. In brief, the skeptical view is presented as offering

an epistemic challenge to anyone offering a theory of group rationality (on pain of accepting its own crazy view), while each successive view can be seen as claiming that it answers the skeptical challenge better, or more effectively, than do the preceding views. The arguments presented in the book do not move in a linear direction; there is some looping back, as when the skeptical view is recalled to plumb the depths of other views.

The penultimate chapter centers on aspects of the later philosophy of Hilary Putnam, maker and keeper of philosophical traditions. Putnam, as far as I can tell, has no interest in the problem of group rationality. But Putnam's iconoclasm is profitably used in this chapter – used, I say; not misused, I trust – to cast the notions of science, rationality, and relativism in a more revealing light, thanks in large measure also to Charles Sanders Peirce. What will emerge, I hope, is that the notion of individual rationality will appear utterly indispensable to solving the problem of group rationality. Moreover, if we are not careful with the notion of individual rationality, not only will the problem of group rationality remain unsolvable at the deepest level, we might also find ourselves sliding into relativism. I shall also show, vis-à-vis what I call the Williams problem, after Bernard Williams, that once we distinguish between a Social Utopia and a Scientific Utopia, we may no longer be able to claim that even an ideal democracy (Social Utopia) solves, in any significant way, the problem of group rationality; at most, democracy may be a necessary condition.

The final chapter records nine problems that lie at the heart of the investigation into the problem of group rationality; and, in so doing, it does not merely sketch these problems and the unique manner in which they are knotted together, it also signals a fascinating additional problem or two – none of them even touched upon in the rest of the book – namely, *why* do scientists owe allegiance to fellow scientists? And, given that they owe it, *what* will sustain that allegiance? Perhaps the answers to these questions ultimately lie in our speculation over a problem that lies considerably underground, namely, what is science *for*?

I have endeavored to show how deep the problem of group rationality is; why the classical theories fail to solve it; why new foundations are needed; what problems will need to be addressed in order to arrive at a more plausible solution; and, finally and above all, why the importance

of the problem of group rationality – let alone its beauty – overshadows the problems that have occupied us these past fifty years or so in philosophy of science, thereby showing these problems their rightful place in the scheme of methodology. Despite their depth and scope, as is evidenced by the reconstruction, I argue that on the whole the vices of these classical theories of group rationality exceed their many virtues; hence, one must also engage in the task of dismantling.

As I view things, there is room for one more vision of group rationality, a vision that for now is buried in footnotes, or tracked only in the implications, several of them far-flung, of what is said in these pages. That vision will have to wait its turn for full expression in the second half of this project. For now, our task is to calculate the value of what we have on our hands; and calculate we must. The stakes are high, if I am right. For not only is this task about an ideal scientific group, it is also about reasons generally (and how they are anchored) and about utopias (and why we owe them allegiance). It is about what a society stands to reap if a rational scientific group flourishes in its midst – or what it stands to fear if it does not. I leave all that for now (since the rest of the book is occupied with it) and turn to acknowledging the debts I have incurred.

Acknowledgments

From what follows, it would be fair and reasonable to surmise that my debts – I record them in chronological order – are several, considerable, and, in a case or two, unpayable.

Paul Feyerabend discussed with me an earlier paper of mine, "Against *Against Method*; or, Consolations for the Rationalist," which now plays its own small part as section IV of Chapter 4, which centers upon Feyerabend's skeptical views. Of course, he found in that paper much with which to quarrel (an a priori truth). When I was a mere graduate student and he was at the height of his fame, he corresponded with me (and this continued for several years thereafter), giving me advice mingled with encouragement. I was back then too much of a Popperian, and he too anti-Popperian, for either of us to heed what the other said (old loves don't entirely wither away), and yet he was kind enough to write letters on my behalf. From an utterly chance encounter in 1979 (en route to Ian Hacking's National Endowment for the Humanities Summer Seminar at Stanford University) on an uphill street in Berkeley – we stood on the sidewalk and talked for well over an hour – I caught a glimpse of the man's core, especially when he talked of his loss in Imre Lakatos, that I had not known from our correspondence. Undoubtedly, he would have returned, as was his custom, with a scathing argument – or, more likely, laughed – at what I have written here; but not even *he* would have doubted that it was written with much affection, gratitude, and respect.

In the fall semester of 1983, Princeton University elected me its Visiting Fellow. Frequent lone and leisurely walks on and around the campus produced lots of first and fruitful ideas; I hold the incredible fall season of that year as my witness. The Institute for Advanced Study at Princeton, in the person of Donald McCloskey, invited me that December to give a talk on the problem of group rationality. In 1984, the National Endowment for the Humanities offered me a Summer Seminar Award, which enabled me to go to Harvard University. The seminar was led by Everett Mendelsohn on the topic "The Social Context of Modern Science." I worked on the problem of group rationality on the side when I could, and there I first crafted a crude sketch of the objectivist view, now Chapter 7.

Nearly everything by way of a first draft – no part of it was subsequently left untouched – was written between the two Octobers of 1984 and 1987. After a three-month recess, I revised the manuscript for approximately six months. I made further revisions in the summer of 1990 and the spring of 1991. Louisiana State University's Council on Research granted me a summer research award in 1985 and again in 1992, without which much of my work would not have been accomplished. Then, inexplicably – but not unhappily – for nearly a decade I abandoned the project as I turned to study the history of philosophy.

But, even during this interval, together with a couple of papers on scientific realism, I published earlier versions of Chapters 5 and 6 on the two types of the subjectivist view of group rationality, and earlier still a small part (section IV) of Chapter 4 on the view of the skeptic. I am grateful to the editors and publishers of *Philosophical Topics* and *Studies in History and Philosophy of Science* for permission to print here, duly revised, material that was originally published in their journals. Once again, I owe thanks to my son, Casim Ali, for the several diagrams in this book and for providing an engineer's help with Chapters 2 and 5. I added, deleted, and rewrote large parts of the manuscript for a year and a half beginning in late October 2003. A sabbatical leave for fall 2004 was crucial for much of my labor on the book; I am quite indebted to my university. Finally, the first six months of 2005, as well as of 2006, were spent reworking and restructuring, and thus was this work finalized. My gratitude to Russell Hahn for serving as production editor and, once again, as copy editor: no book should be without so patient and skillful a copy editor.

Let me here interpose an acknowledgment that has nothing to do with the making of this book (except indirectly, as a consequence of a long-ago event), and yet it is an acknowledgment I perforce must make, with due deference and publicly. Nearly a quarter of a century ago (and the reader, I hope, will understand why I waited so long to make this public), three philosophers – Baruch A. Brody, Samuel Gorovitz, and Leonard Linsky – and a chemist, Sean McGlynn, then Boyd Professor and Vice Chancellor for Research at Louisiana State University, acted in concert, judiciously and vigorously, to ensure the survival of my academic life, and ensure it they did. I am sorely in need of instruction on how to repay this debt.

This book is one half of the project; the other half is to propose an alternative to the theories of group rationality evaluated herein. Late in 2002, I sent a draft of the entire project to a few philosophers who were willing to aid in my cause. These were Joseph Agassi, Alexander Reuger, and Catherine Wilson. I have shaped the contents of the book, and remeasured its tone, in light of their advice and admonitions. Of the anonymous referees for Cambridge University Press, I thank two: for showing me how to illumine what was dark and obscure in the book, to tighten what was loose, and to provide an aerial view of the labyrinth. In that last advice I heard Seamus Heaney's voice: "So that the figure of the universe / And 'not just single things' would meet his sight." What has resulted is not faultless, but it *is* less faulty. I therefore proportionately thank them (concealing, for honor's sake, the extent of that proportion from the public's eye). When I was racked by uncertainty, Beatrice Rehl, my editor, admonished me thus: "Write the book you want to write," and then stood sentry over it. Those words have sustained me through the final rewriting. Finally, and once again, I owe a debt to Catherine Wilson: a lasting, burgeoning debt.

May 19, 2006
Baton Rouge, Louisiana