

# GROUP RATIONALITY IN SCIENTIFIC RESEARCH

Husain Sarkar

CAMBRIDGE

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## 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.

Husain Sarkar is professor of philosophy at Louisiana State University. A recipient of LSU's Distinguished Faculty Research Award, he is the author of *Descartes' Cogito: Saved from the Great Shipwreck*.



# Group Rationality in Scientific Research

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For

*My Mother and Father*

*Alas, I am so unforgivably late:*

وَصَيَّنَّا الْإِنْسَانَ بِوَالِدَيْهِ إِحْسَانًا  
..... بِحَقِّي إِذَا بَلَغَ أَشُدَّهُ وَبِكَلِّمِ الْوَالِدَيْنِ  
سَنَةً ۖ قَالَ رَبِّ أَوْزِعْنِي أَنْ أَشْكُرَ نِعْمَتَكَ الَّتِي  
أَنْعَمْتَ عَلَيَّ وَعَلَىٰ وَالِدَيَّ

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*



## The Overview

Charles Robert Darwin (1809–1882) published *Origin of Species*<sup>1</sup> on November 24, 1859; by the day's end, all the printed copies – all 1,500 of them – had sold out.<sup>2</sup> The book was read avidly even by the laity – 500 copies went to Mudie's Circulating Library – and the revolution it initiated was off and running.<sup>3</sup>

<sup>1</sup> The full title of the book as it appeared in the first edition was *On the Origin of Species By Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life*. The details of the references are given in the bibliography.

<sup>2</sup> Even that is not exactly true. The number of copies printed was 1,250, although Murray, Darwin's publisher, took orders for 1,500 copies. When Murray asked Darwin to send in the corrections post haste for a second edition, Darwin was most pleased. It is worth quoting what Darwin's wife, Emma Darwin, wrote to their son, William: "It is a wonderful thing the whole edition selling off at once & Mudie taking 500 copies. Your father says he shall never think small beer of himself again & that candidly he does think it very well written." Nine days later, on December 3, 1859, Mudie's Circulating Library advertised that *Origin of Species* was available to be borrowed. For a detailed account of this, and of the whirlwind that followed in the wake of Darwin's book, see Janet Browne's splendid biography, *Charles Darwin: The Power of Place*, especially Chapters 3 and 4.

<sup>3</sup> One would commit an egregious sin of omission if one did not mention the self-effacing, not-so-well-connected (in fact, ostracized by the community of his peers) co-discoverer of evolution, Alfred Russel Wallace. For a fascinating account of Wallace's own independent discovery of the theory of evolution, based on his researches in the Amazon (from 1848 to 1852) and the Malay Archipelago (1854 to 1862), and a judicious treatment of the evidence on the issue of priority, see Michael Shermer, *In Darwin's Shadow: The Life and Science of Alfred Russel Wallace*, especially Chapters 2 through 5.

The cardinal tenet of *Origin of Species* was that there is evolution in the biological world that can be explained by the principle of natural selection. Darwin had argued that biological species evolve (were not separately created) through competition for scarce resources, and that the winner in said competition is defined by differential reproduction (one who is able to leave behind more offspring than others). Among the key Darwinian ideas, in part inspired by Thomas Robert Malthus's 1798 *An Essay on the Principle of Population*,<sup>4</sup> was this: Nature is marked by ruthless, incessant competition for survival; to describe this idea, Darwin used phrases (that were to resonate long after) like "the universal struggle for life," "the struggle for existence," "battle within battle," "the great battle of life," "the war of nature," and "the great and complex battle of life."<sup>5</sup> The organisms locked in this struggle are not merely competitors, but enemies.<sup>6</sup> Crudely put, it is a zero-sum game gone haywire: Either you outlive your competition or you perish.

In January 1880, a few months before his death and twenty-one years after the publication of *Origin of Species*, the distinguished Russian ichthyologist Karl F. Kessler (1815–1881), rector of St. Petersburg University, chair of its Department of Zoology, and the first president of the St. Petersburg Society of Naturalists, delivered an address before a congress of Russian naturalists. His reverence for Darwin notwithstanding, he moved many a Russian biologist – but had little or no impact on the naturalists of Western Europe – with his concluding claim that "I obviously do not deny the struggle for existence, but I maintain that the progressive development of the animal kingdom, and especially of

<sup>4</sup> Full title: *An Essay on the Principle of Population as it affects the Future Improvement of Society, with Remarks on the Speculations of M. Godwin, M. Condorcet, and other Writers*. Malthus's core claim was that the growth rate of human population is geometric, whereas the growth rate of the food supply that is needed to sustain that population is only arithmetic. The latter would consequently seriously curb the former. Of his own doctrine of the struggle for existence, Darwin wrote, "It is the doctrine of Malthus applied with manifold force to the whole animal and vegetable kingdoms" (*Origin of Species*, 63). For Malthus's influence on Wallace, see Shermer, *In Darwin's Shadow*, especially 112–15.

<sup>5</sup> Darwin, *Origin of Species*, 62, 68, 73, 76, 79, and 80. Chapter 3, "The Struggle for Existence," in particular, details Darwin's views on this subject using a large array of examples.

<sup>6</sup> For example, Darwin, *Origin of Species*, 67, 69, 78, and 85.

mankind, is favoured much more by mutual support than by mutual struggle.”<sup>7</sup> Among those convinced was Petr Alekseevich Kropotkin.

Kropotkin, in company with his naturalist friend I. S. Poliakov, explored the animal world in Siberia, especially the Vitim, Amur, and Usuri regions. Having read the *Origin of Species*,<sup>8</sup> they bore the book’s claims vividly in mind as they journeyed off to do their fieldwork to test Darwin’s theory. But, Kropotkin reports, they were unable to find that keen competition among animals of the same species that Darwin’s magnificent work had led them to expect.<sup>9</sup> Instead, they found, as Kessler had taught, that while not everything in Nature was in harmony, it was remarkable how much of it was. There was an extraordinary number of examples of intraspecies as well as interspecies harmony, coordination, altruism, cooperation, and mutual aid and support.<sup>10</sup> And so, in his book *Mutual Aid*, Kropotkin wrote thus:

But it may be remarked at once that Huxley’s view of nature had as little claim to be taken as a scientific deduction as the opposite view of Rousseau, who saw in nature but love, peace, and harmony destroyed by the accession of man. In fact, the first walk in the forest, the first observation upon any animal society . . . cannot but set the naturalist thinking about the part taken by social life in the life of animals. . . . Rousseau had committed the error of excluding the beak-and-claw fight from his thoughts; and Huxley committed the opposite error; but neither Rousseau’s optimism nor Huxley’s pessimism can be accepted as an impartial interpretation of nature.<sup>11</sup>

<sup>7</sup> Quoted by Petr Kropotkin, *Mutual Aid*, 8.

<sup>8</sup> The Russian translation appeared in 1864 and was quickly sold out.

<sup>9</sup> Indeed, Darwin had claimed that competition between species of the same genera was far more intense than that between species of different genera; Darwin, *Origin of Species*, 76.

<sup>10</sup> The whole-heartedly anti-Malthusian Russian response to Darwin’s theory of evolution and natural selection is neatly delineated by Daniel Philip Todes. For a synoptic view, see his paper “Darwin’s Malthusian Metaphor and Russian Evolutionary Thought, 1859–1917”; and for a fuller account, see his book *Darwin without Malthus: The Struggle for Existence in Russian Evolutionary Thought*.

<sup>11</sup> Kropotkin, *Mutual Aid*, 9. Richard Dawkins would have fiercely denied this, dismissing it as “bad poetic science.” Dawkins’s position is that animals are neither essentially altruistic nor selfish. It is the genes that are selfish, and they, in the company of other genes, harness whole organisms in their service. Thus, whether organisms are selfish or not depends on whether this would have a salutary effect on the survival of genes; see Dawkins, *Unweaving the Rainbow*, especially Chapter 9, “The Selfish Cooperator,” 212–14, 224.

Let this serve as a prolegomena to the problem of group rationality – namely, under what conditions is a group of scientists rational? – with which we are occupied in this book. Then, first, one might insist that the problem of group rationality is a problem whose solution must be sought in evolutionary terms, either in purely Hobbesian or purely Rousseauian terms – even if Kropotkin was right in admonishing that the truth resides somewhere in the middle. To offer a Hobbesian solution is to start with the premise that each scientist is interested only in his own domain of science; that is, each scientist identifies his own welfare with the welfare of his scientific domain. One then proceeds to show how these purely self-interested scientists could collectively reason themselves into a group of cooperating scientists. This I take to be the approach a game theorist might adopt (although, as we shall see, not only game theorists do); it is the approach I eschew.

Second, the Rousseauian approach may be far more interesting than the Hobbesian one. Here's why. The puzzling, momentously significant thing in biology was the discovery not only of the fact of evolution and the principle of natural selection but also of the *fact* of cooperation, altruism, and mutual aid and support among animals. It was a fact that for a long time remained unexplained in Darwinian terms. I should like to argue that the problem of group rationality is not to find out, at this late stage, whether there is, or ought to be, cooperation among scientists or not, or even whether the group is better off cooperating or not. *That* is surely a given. It is rather to discover what shall be the *modus operandi* of that cooperation. If he was minded, Kropotkin would have argued, as Paul Feyerabend was to do nearly a century later, that scientists should let the democratic method govern not only their fundamental political structures but also their scientific practices. This, then, is what the Rousseauian approach will make starkly clear: Given that they ought to cooperate, *how* should scientists do so? – *that* is the cardinal problem of group rationality.

Third, it should enable us to focus more sharply on just *what* the problem of group rationality devolves around. Nearly all philosophers of science – in fact, I believe, all of them – take scientific theory to be the prime element in dealing with this problem. They then try to understand how the scientific group should be structured around a theory or theories. My approach is markedly different. I shall argue that in order to determine the solution to the problem of group rationality,

we ought to make method or methodology the cornerstone of our inquiry. This approach has other benefits, but it also has unintended, and sometimes surprising, consequences for the theories the group must pursue. For example, beginning with the methods that should structure a scientific society, we may be able to show, as an unintended consequence, that the society of scientists should proliferate theories. Consequently, this view of group rationality accommodates the earlier views that begin with theories rather than methods, yet the converse is not true; and hence, because of its depth, sweep, and generality – not to speak of its sheer elegance and beauty – this view of group rationality should have a much broader appeal.<sup>12</sup>

This chapter addresses three tasks. In Section I, it outlines the basic shape and substance of the main argument of the book. It does so by sketching the five reconstructed solutions to the problem of group rationality, revealing how the inadequacy of one solution leads to the next and how, in some instances, the virtues of one solution are preserved in subsequent solutions (with an occasional backward step). There is cross-fertilization, too: Later views are scanned in the light of earlier ones, revealing smudges. Another way of reading this book is to construe it as the unveiling of a budget of problems, separately marked and distinguished in this section, that a satisfactory theory of group rationality must solve. Section II then considers the question whether group rationality should be accorded priority over individual

<sup>12</sup> There is an important additional element that may introduce a wrinkle. “The virtue of Accuracy plays an important part in guiding and sustaining a collective division of epistemic labor. . . . and there is of course a genuinely historical story, a hugely complex one, of the cultural and eventually industrial sophistication of this idea into what is now called ‘science.’ One important feature of that process has been the way in which the understanding of nature itself affects what counts as an appropriate and effective division of labor.” Thus Bernard Williams in *Truth and Truthfulness: An Essay in Genealogy*, 141.

If Williams is right, then our very understanding of nature will affect what will count as an appropriate and effective division of labor; methodology seems to play *no* role in this. The science of Anaximander will proclaim one division of labor in cosmological science, the science of Hawking a different division. Yet to ask whether either division of labor is effective, or which one is more effective, is not to raise a query in cosmology; it is a normative question pertaining to group rationality. Let us, then, accommodate Williams’s point in this way. Science will tell us what disciplines and subdisciplines it will need. A theory of group rationality will tell us how the group is to be structured, or how the division of scientific labor is to be made, if the group is to function effectively and the disciplines and subdisciplines are to exhibit growth of knowledge.

rationality, or the other way around – neither notion being dispensable. Finally, section III details what I call the Williams problem. This is a problem of explaining the relationship between social structures and a solution to the problem of group rationality. Specifically, it asks what kind of ideal society (Social Utopia) would be needed to nurture the ideal scientific society (Scientific Utopia) envisioned in the solution. Conversely, the consequences, unintended or otherwise, of having a certain kind of Scientific Utopia for the shaping and form of a Social Utopia would make for an additional – hardly innocuous – way of evaluating a solution to the problem of group rationality.

## I. The Plan of the Book

Let me sketch the spine and structure of the book. Simply put, the problem of group rationality is this: Under what conditions is a group of scientists rational? It is astonishing what a marvelous variety of problems can lie behind that seemingly simple formulation of the problem. It is with these various formulations of the problem that we shall be concerned in Chapters 2 and 3. In significant part, Chapter 2 will try to demonstrate that the problem of group rationality is a unique problem, neither reducible to nor analogous to a problem in game theory, social choice, social justice, or another such approach. After the uniqueness claim has been established, the problem of group rationality will be defined at the end of Chapter 3, and it is this formulation of the problem that will be deployed in the rest of the book. In attempting to gauge the adequacy of various solutions to that problem, we shall try to retain the integrity of each solution by first formulating the problem from the perspective of each of those solutions, in order to see how that particular formulation fares against our own. Once or twice, we shall also examine the adequacy of a solution in light of a different formulation of the problem in order to divulge the complexity and richness of the task.

Epistemologists, following Descartes, typically begin by delineating the skeptical position. Our starting point shall be no different. One begins with that position – and there is no other position antecedent to skepticism more challenging – with the aim of showing that such a position is either answerable, untenable, or contradictory (or even unworthy of a reply). Thus, the first solution we shall consider is that of



the skeptic (Chapter 4); we shall see him offering both a negative and a positive solution. The negative solution will consist in arguing that a scientific group is rational provided the group is structured along the lines of *any* method that seems viable to *any* practicing scientist: no exceptions. This is famously captured in the slogan “Anything goes.” The aim is to produce what the skeptic desires, namely, a vast, conflicting set of scientific theories, metaphysical outlooks, and methods of doing science. This view of the skeptic will be illumined by a tale. Intriguing as the skeptic’s notion of a Democratic Council (wherein a lay person rules) may be, it is essentially an offshoot of his skepticism; thus, the skeptic’s claim, reminiscent of the Greek sophists, is that whatever this council decides is epistemically right.

The positive solution consists in the skeptic’s own favored method; of course, he makes no special plea on its behalf, claiming only that it is his preference. Let any scientist join in who has a similar preference; arguably, an enlightened laity will follow the skeptic’s plan. Not only shall we find both these solutions – negative and positive – untenable, we shall also try to show how the skeptic’s view is infected by a contradiction at its center (as if that should matter to the skeptic).<sup>13</sup>

Not less significantly, the skeptical position will present us with a range of problems that a theory of group rationality will need to solve. For example, should there be a single aim that informs a group of scientists? The skeptic argues that there should be a multiplicity of aims (without any, other than self-imposed, restrictions). Then, assuming that there is a multiplicity of aims, how shall we avoid the problem of fragmentation, namely, the problem of the group being splintered, each scientist going his own way, resulting in lost scientific labor? The skeptic would quarrel with our contention that scientific labor is lost, especially if scientists are engaged in doing exactly what they want and are “flourishing.” Third, should there be a well-defined structure for the group? From the vantage point of the skeptic, clearly not.

<sup>13</sup> “Suppose that we show that some X he holds or accepts or does commits him to behaving morally. He now must give up at least one of the following: (a) behaving immorally, (b) maintaining X, (c) being consistent about this matter in this respect. The immoral man tells us, ‘To tell you the truth, if I had to make the choice, I would give up being consistent.’” Robert Nozick, *Philosophical Explanations*, 408. What Nozick has a moral skeptic saying, we can have a skeptic in methodology saying (or something similar). See also, Chapter 3 of this volume, footnote 1.

These, then, are the cardinal problems that the skeptic's view leaves us to struggle with:

- What should be the scientific aim or aims of the group?
- How should the problem of fragmentation be dealt with?
- What should be the basic structure of a scientific group?

And so we are led to the second solution, the first version of the subjectivist view (Chapter 5). This solution shares one feature in common with the third solution, the second version of the subjectivist view, and with the fourth solution, the objectivist view (but, arguably, not with the fifth solution): namely, that a scientific group is rational provided the group is structured along the lines of a *single* method (that group structure should be defined in terms of method is explicit in none of the views). All these solutions, therefore, attempt to veer away from the skeptical view, and to that degree represent a small advance.

Now, a powerful dogma in methodology is the principle of proliferation. This principle – used as a yardstick against which to measure a theory of group rationality – states that there should be a proliferation of theories in a scientific society; a society nurturing a single theory, or an extremely limited number of theories, must provide a sharp defense for its practice. This first version of the subjectivist view claims that a society (the Rousseauian society of scientists) in which scientists aim to pursue truth and verisimilitude is unlikely to satisfy the principle of proliferation; but scientists interested in pomp, power, and circumstance (the Hobbesian society of scientists) will, inadvertently, satisfy it. This version of the subjectivist's view emphasizes subjective *nonepistemic* values of the scientists that, supposedly, will take the group to where it objectively should be, epistemically speaking. Thus, this solution must, at the very least, explain (let alone justify) the tie between the subjective nonepistemic values of the scientists and the objectively viable theories that the group produces as a consequence of holding those values.

Consequently, some of the new problems this version of the subjectivist's view presents are these:

- What are the subjective, nonepistemic values of scientists?
- How should these nonepistemic values be distributed in the group with a view to satisfying the principle of proliferation?

- Can a single-method model sustain the required distribution of nonepistemic values as well as the plurality of theories?

The third solution is the second version of the subjectivist view. While the first version emphasizes the traditional notions of truth and verisimilitude, the second version will have none of that. Instead, it talks in terms of maximizing efficiency in puzzle solving. Like the first version, it too highlights the social, political, and economic structure of the society in which scientists do their science; having transformed our image of science, it would appeal to the history of science as a judicious arbitrator of competing theories of group rationality. Yet the relation of the history of science to these competing theories is a bit ambivalent. This solution to the problem of group rationality emphasizes epistemic values, claims them to be defining of science; but it leaves it up to the scientists in the group to decide what weights should be assigned to these values. Without much argument, it assumes that somehow this way of assigning weights and distributing the epistemic risks will lead not only to the acceptance of a single theory or paradigm (its preferred way of structuring the group), but also to the growth of knowledge (understood as greater puzzle-solving efficiency).

In this version, we shall also introduce a new, significant distinction between the static problem of group rationality and the dynamic problem. The static problem of group rationality is to determine whether a group of scientists, at a given time, is structured rationally; the dynamic problem of group rationality is to determine whether a group of scientists has evolved over time to a rational structure or to a more rational structure. Even if no solution discussed in this book draws that distinction – and whether one assumes that methods or scientific theories lie at the core of defining the structure of a society of scientists – the distinction is mentioned here in order to register its utter importance.

Finally, the second version of the subjectivist view employs the notion of “negotiation.” To explicate this notion, a device akin to the Rawlsian notion of the original position is utilized. Scientists are placed therein, where they can, without prejudice or hindrance, negotiate with one another over what the structure of their society of scientists should be. This not only highlights the problem of group rationality, but also brings to the forefront, in a novel way, the problem of how to characterize individual rationality and its connection with the problem

of group rationality. Thus, this version leads to some fresh, significant problems of its own making:

- What role should be accorded to the social, political, and economic structure of the society, as well as to the history of science, in developing a normative theory of group rationality?
- What makes a group of scientists statically rational, or rational at a given time?
- What makes a group of scientists dynamically rational, or rational over an interval of time?
- How might the notion of negotiation be conceived and connected to the reasons that individuals scientists offer in arriving at a plausible theory of group rationality?

The fourth solution is the objectivist view (Chapter 7). It is a marked improvement over the foregoing views in that, unlike the skeptic's view, it reckons the possibility of a genuine solution: a solution that, unlike on the subjectivist's view (both versions), can function as a criterion against which to measure subjective epistemic values themselves, and thus the rationality of the group (keeping this assumption sharply separate from a stronger one, namely, that the objectivist view *has* a solution that *is* objectively right; in my view, the solution isn't right). The objectivist view also prominently claims that a scientific group is rational provided the group is structured along the lines of a single best method. While, as we shall see, this is seriously inadequate – for one thing, it is too optimistic – it brings to the fore a powerful problem in the field of meta-methodology. This problem is best explained by analogy to the problem of demarcation.

The problem of demarcation is, “What distinguishes a scientific theory from a pseudo-scientific one?” The problem was deemed quite significant in philosophy of science, for reasons that are old and well known. Now, if the subjectivist views as well as the objectivist view of group rationality insist that a single method be used to structure the society of scientists, and given that there are a fair number of methods available, there are two nice problems to be dealt with – and not just for these views. The first is the new problem of demarcation: What distinguishes viable methods from those that are not? The second problem is: Of the viable methods, which is the best method? Since we want the group of scientists to be structured or grounded along the lines of the

best available method, the solution to the two problems is a *sine qua non* for any solution to the problem of group rationality. This, I trust, shows, in a rather intriguing way, how meta-methodology (the evaluation of methods) and methodology (a theory of group rationality, among other things) are inextricably bound.

The objectivist view, then, harvests the following problems:

- How should a viable method be demarcated from a nonviable one?
- How rational is a group of scientists structured along the lines of a single method?
- Given the deep underlying interlacing between method and meta-method, how shall we account for the growth of method (and not just for the growth of science)?

The penultimate chapter, Chapter 8, will examine the work of Hilary Putnam. Putnam's recent views attempt to demonstrate a remarkable parallel between the scientific image and the moral image, and in so doing Putnam delves deeply into what makes for individual rationality. Many moral images – constrained by the principle of equality and freedom – should adorn a society, says Putnam; might Putnam also claim that as many scientific images bedeck a society of scientists? What would serve as constraints on these images? The chapter will examine, too, the consequences that Putnam's view of individual rationality will have on group rationality; and it is here that Peirce's puzzle will play a central role. Suppose that an individual has to choose between two courses of action. If the probability of success of doing one action is higher than the probability of success of doing the other, why – Peirce's puzzle – should, the person do that action that has the higher probability of success? Any solution to the problem of rationality, which derives from the assumption that the ultimate ground for being reasonable is that one will arrive at truth in theory, and success in action, more frequently if one is reasonable, is untenable. So Putnam claims, and then offers a solution. The outcome of that inquiry will instruct us to hone and shape our theories of individual rationality differently, if we are not to succumb to relativism (a threatening prospect). One might adopt the Kantian view as an alternative; and this, I aver in a programmatic note, might enable us to reap benefits denied us on theories examined in this book.

Here, then, are three crucial problems that Putnam's view will bring to the fore:

- What is a scientific image, and what scientific images may govern a society of scientists?
- What is individual rationality? Or, how shall Peirce's puzzle be solved?
- What is the role of individual rationality in a theory of group rationality?

Each view of group rationality half-hides – sometimes not that successfully – a view of Scientific Utopia that it would promote, and each portrays, by implication, a scientific dystopia.<sup>14</sup> Consequently, we shall examine the nature of that utopia that is envisioned by each theory of group rationality and ask, in a manner after Bernard Williams, “What kind of social and political structure will be needed to create and sustain that Scientific Utopia?” Williams was hyperalert to the possibility that a seemingly plausible moral theory (such as utilitarianism) could be implemented only in a rather gruesome or high-handed way. He took that to show the defect of a moral theory. Williams offers us, then, an additional way in which we can criticize a normative theory of group rationality: If a normative theory of group rationality implies a social and political structure that is unacceptable from a moral point of view – the Social Utopia it requires to flourish cannot possibly strike our fancy – then it is not a normative theory that can appeal to us. (Some may see it as an occasion to revise our vision of a Social Utopia.) By implication, one significant task is to show how a Scientific Utopia implied by a theory of group rationality can coexist easily with a Social Utopia; they might even be shown to be mutually reinforcing.<sup>15</sup>

<sup>14</sup> To keep to manageable limits, I say little about dystopia; but I trust that what is said about utopia will leave little to the imagination about what forms of dystopia will result, and why they will be deemed unacceptable.

<sup>15</sup> It might be feared that I am attempting to construct, perhaps even to legitimize, conditioning Scientific Utopia on Social Utopia. I hope it will be abundantly clear in what follows that I give primacy to the solution to the problem of group rationality. Once that solution is at hand, then and only then may we ask what kind of Social Utopia will be needed to sustain that rational society of scientists. Like Williams, however, I do not rule out a priori the possibility that the Social Utopia that will be needed would be so bizarre or frightening that we might wonder whether we have found the right solution – and, if we have, whether we are morally obliged to bring about a Social Utopia in which that solution can be implemented. I cannot imagine any such obligation.

Perhaps this is just another version of Putnam's idea of trying to produce a coherent picture out of scientific and moral images.

Finally, we might say that Williams has presented us with these cardinal problems:

- What Scientific Utopia is implied by a given theory of group rationality?
- Under what kind of Social Utopia could that Scientific Utopia sustain itself and thrive?
- Would that Social Utopia be morally acceptable?

Shaped and colored by the arguments of the book, the [final chapter](#) will present various problems – nine, to be exact – related to the cardinal problem of group rationality, and the intricacy of their connections to each other, from a distinctive point of view, the better to enable us to offer a richer, deeper, and a more unified theory of group rationality.

## II. Group to Individual, or Vice Versa?

The problem of group rationality – “Under what conditions is a group of scientists rational?” – might be offered under two different guises. We shall call them the *Group-to-Individual Problem* and the *Individual-to-Group Problem*, respectively. The Group-to-Individual Solution attempts to show how individual scientists collectively decide how their group should be organized and structured and how, having thus decided, each member acts in accordance with that covenant. The scientists decide what should be the ultimate goal (say, truth or verisimilitude) toward which their group should be aiming (and one of the original problems may well be how to settle on that ultimate group goal). Next, given the goal, they determine what should be the structure – defined by method(s), I shall assume (contrary to the prevailing assumption) – of their group, and how that method or those methods should be selected. Finally, each scientist decides how best to act in order to comply with the foregoing two determinations or decisions. In this Group-to-Individual Solution, the group is accorded priority over the individual scientists who compose the group. Indeed, the identity of the group or subgroup, defined in terms of method(s), may well remain tolerably constant while the individual scientists move in and out of