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The Overview

Charles Robert Darwin (1809–1882) published *Origin of Species*[1] on November 24, 1859; by the day's end, all the printed copies – all 1,500 of them – had sold out.[2] 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.[3]

[1] 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.

[2] 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.

[3] 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*,[4] 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."[5] The organisms locked in this struggle are not merely competitors, but enemies.[6] 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

[4] 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.

[5] 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.

[6] For example, Darwin, *Origin of Species*, 67, 69, 78, and 85.

mankind, is favoured much more by mutual support than by mutual struggle."[7] 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*,[8] 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.[9] 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.[10] 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.[11]

[7] Quoted by Petr Kropotkin, *Mutual Aid*, 8.

[8] The Russian translation appeared in 1864 and was quickly sold out.

[9] 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.

[10] 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*.

[11] 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 Rousseauean 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 Rousseauean 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 Rousseauean 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.[12]

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

[12] 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).[13]

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.

[13] "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 Rousseauean 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 *non*epistemic 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