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Argumentation Theory and the Recent Philosophy of Science

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ARGUMENTATION THEORY AND THE PHILOSOPHY OF SCIENCE SINCE KUHN: THE RATIONALITY OF THEORETICAL ADVANCE

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Abstract:
The thesis of my paper is that argumentation theory provides a promising heuristic framework for addressing issues raised by the rationality debates in the philosophy of science, in particular the issues connected with scientific controversies over the appraisal and choice of competing theories. The first part of the paper grounds this thesis historically. In criticizing the logical empiricists, Thomas Kuhn set the stage for the subsequent opposition between a normative, anti-sociological philosophy of science and a descriptive, anti-philosophical sociology of knowledge. But he also hinted at the main lines of a multi-dimensional theory of argumentation which might frame a wide range of current investigations into scientific reasoning. In the second part of the paper I focus on the central normative aspect of this framework, dialectical argumentation, and clarify the key challenge, the underdetermination of theories by evidence. In the third part, I attempt to get beyond the dichotomy informing the rationality debates by reformulating the problem of theory choice within a broader context of scientific rationality.

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The dynamics and rationality of scientific progress continues to endure as a crucial issue in the philosophy of science. One of the central questions is this: What makes it rational for scientists to choose one theory over its competitors? More broadly, what justifies the claim that science is progressing, that our theories today are superior to those of previous generations? In the first half of the twentieth century, the logical empiricists attempted to answer such questions by reconstructing a normative "logic of justification" that primarily relied on the tools of formal logic, whether deductive, inductive, or probabilistic. This approach was in serious difficulty even before Thomas Kuhn's *Structure of Scientific Revolutions* (1970a) appeared in 1962.1 A watershed in the study of science, Kuhn's book opened the topic of scientific rationality to a host of sociological, historiographic, and rhetorical analyses which then competed alongside the philosophy of science. Unfortunately, cooperation among these various disciplinary perspectives has not always been the order of the day. One of the more conspicuous rifts opened up between philosophical attempts to rescue norms of rationality and progress, and more descriptive sociological and historical approaches, which often proclaimed a principled epistemological relativism. Consequently, the rationality debates sparked by the "sociology of scientific knowledge" (SSK) have pitted not just different theories against one another but entire disciplines as well.2

In this paper I assume that a less dichotomous, more cooperative atmosphere is desirable, and I propose that argumentation theory can foster this. Specifically, argumentation theory can provide a heuristic framework within which a number of disciplinary approaches, both normative and descriptive, could investigate and debate issues involved in the rationality of scientific advance. In the first part of the paper I motivate this thesis historically and

sketch the framework I have in mind. After examining, in the second part, the problematic relation between dialectic and sociology, in the third part of the paper I sketch a model that promises to break out of the dichotomies driving the rationality debates and to give full scope to argumentation theory. This model allows me to reformulate the central question for the study of scientific change.

I

The key opposition in the rationality debates pits a rationalist philosophy of science against a relativist sociology of science. Although the boundaries between these two camps are often fuzzy, the term "rationalism," as I use it here, entails a commitment to the claim that contemporary scientific theories, when properly developed, are superior *as science*, whether because they are better justified, more true, understand more of nature, solve more problems-the exact rationale depends on the conception of science. Rationalism is thus committed to a normative approach, the elucidation of standards of good science. Examples are Popper (1992), Merton (1973), Lakatos (1970) and Laudan (1977). Relativism has many faces in SSK, but in the present context the pertinent version is, ironically, philosophical: the rejection, in principle, of claims to scientific progress in the long haul. This rejection often stems from skepticism regarding the ability of science to arrive at generally valid knowledge, or truth about nature. Not surprisingly, relativists prefer a descriptive approach over a normative one (cf. Pera 1994, p. 138).3

Because the difficulties on both sides have been so often rehearsed in the opposite camp,4 in this paper I focus on how theorists might get beyond the dichotomies driving the rationality debates and achieve a greater interdisciplinary cooperation. In this first part of my paper, I return to a key source of the debates to locate the potential not only for future rifts but for cooperation too.

The source I refer to is Kuhn. At least in part, the rationality debates stem from Kuhn's influential *Structure of Scientific Revolutions* (1970a). Kuhn's historical investigations of important scientific advances convinced him that the logical empiricist account of the rationality of science was inadequate. But the account he put in its place is curiously Janus-faced. To borrow one of his own favourite metaphors, Kuhn's picture of science is a gestalt image-science appears both as a duck and a rabbit, as it were. Viewed from one perspective, Kuhn's gestalt foregrounds the unpredictable, irrationalist, and communal aspects of science-everything the logical empiricist had either repressed or sent off to the "psychologist of knowledge."5 In other words, "Kuhn's Duck" tells us that revolutionary science involves incommensurable claims, conversion experiences, and acts of faith (e.g., Kuhn, 1970a, pp. 148-51, 157-58). From another perspective-indeed one that Kuhn himself has come to privilege more and more-science remains a reasonable enterprise, committed to distinctly cognitive values. "Kuhn's Rabbit" tells us that scientists are ultimately persuaded by reasons, that such reasons include cognitive values, and that, its "losses" notwithstanding, science develops increasingly powerful problem-solving techniques (Kuhn, 1970a, pp. 158-59, 170, 184-86, 205-06; 1970b). In fact, given the analytic tools at Kuhn's disposal, this tension in his account is hardly surprising.

On the one hand, Kuhn knows the positivist-empiricist tradition, with its emphasis on formal-logical analyses of the relation between theory and observation, the privileging of justification over discovery, and the cumulative view of scientific growth by successive theory reduction. This tradition would account for progress in logical (or even mathematical) terms, as a formal relation between the available evidence and competing theories. This formal relation provides a kind of algorithm that should lead any competent expert to recognize the rational superiority of a given theory or hypothesis over its competitors. In this picture of science, the messy context of discovery largely recedes from view.

It is just this view of science that Kuhn's historical studies led him to reject. Logic plus evidence, in his view, can account for (some aspects of) "normal science" but not for the most important theoretical developments in science, i.e., "revolutionary science." But if the revolutionary cases are precisely our exemplars of "good science," then in what sense are they rational? Having rejected the logical-empiricist story, Kuhn falls back on allusions to persuasion and good reasons (1970a, pp. 152ff); at the psychological and sociological levels, he adduces mechanisms of training and membership along with other social-psychological dynamics. Here one can already discern the dichotomy that informs much of the rationality debates: on the rationalist side, logic and nature; on the other side, persuasive reasons, rhetoric, and social psychology. However, Kuhn is hard pressed to say what makes reasons persuasive in revolutionary contexts. Because the one avenue that could save the rationality of science remains so ill-defined-good reasons that somehow persuade scientists over the course of time-the account slides into the paths of least resistance offered by a looser notion of rhetoric, including "more subjective and aesthetic considerations," and social authority (1970a, pp. 156; 155-59; 165-67).

With Kuhn's picture of science, then, we can distinguish three ways of thinking about scientific advance: logic, rhetoric, and sociology in a broad sense, as potentially including individual and social psychology. But on this map, logic-unlike rhetoric and sociology-dead ends at each scientific revolution. Hence, having blocked the path of logical empiricism, Kuhn gives the green light for the sociology and rhetoric of science. To a large extent, one can trace the current constellation of science studies back to Kuhn's rudimentary map. Moreover, it is no surprise that the rhetorical and sociological lines of research are marked by skeptical and relativist views of science, given the earlier logic-driven account of scientific reason and Kuhn's powerful critique of that definition.[6] Given the influence that Kuhn has had on both philosophers and sociologists of science, it should be no surprise that the subsequent rationality debates followed channels that pitted philosophers who wanted to rescue the claims of scientific reason against sociologists who wanted to deflate those claims.

Missing in Kuhn's Janus-faced account is a well-marked middle path between logical compulsion and social psychology. There are, however, hints of such a path. If we examine what Kuhn says about persuasion and reasons in scientific discovery and theoretical innovation, we find elements that might be called "dialectical." Deep theoretical shifts in science cannot be justified, Kuhn tells, in formal-logical terms. But they do involve good reasons and values shared by scientists: empirical accuracy, predictive power, problem-solving power, consistency, plausibility, simplicity, and the like. Because these values are plural and open to interpretation, however, they cannot resolve all scientific dispute; differences in their interpretation and weighting may lead different scientists to assess the same evidence differently. Thus, in a revolutionary shift, scientists have good, albeit not logically compelling, reasons for choosing one theory over another (Kuhn, 1970a, pp. 185, 199-200). Although Kuhn does not, to my knowledge, thematize such persuasion as dialectical argumentation, the label would seem to fit, at least according to some definitions of dialectic, such as that proposed by Nicholas Rescher (1977). According to Rescher, dialectical argumentation is based on reasonable presumptions and judgments of relative plausibility. These judgments may appeal not only to empirical evidence but also to the authoritativeness of sources and "principles of inductive systematization," which include such ideals as simplicity and uniformity (Rescher, 1977, p. 41).

Could a theory of argumentation that took dialectic more seriously-yet without neglecting the rhetorical and socio-institutional contexts of argument-hold together the often mutually repulsive research programs unleashed by Kuhn? Before examining dialectic more closely, note how much an argumentation-theoretic framework already informs the study of science. I have already suggested its rudimentary outlines in Kuhn's own book. To recap, his account touches on a range of elements that have been treated by theories of argumentation going back to the ancient Greeks. Although he rejects *formal demonstrative logic* as incapable of fully capturing

scientific rationality, this aspect continues to play a role, especially in normal science. His references to good reasons and the "techniques of persuasive argumentation effective within ... special groups" of scientists (Kuhn, 1970a, p. 94) point to *dialectical* and *rhetorical* aspects of reasoning. Finally, his concern with the community of scientists, with its specialized training methods and gatekeeping, suggests a *socio-institutional* aspect of argumentation. I do not claim that Kuhn explicitly develops these different aspects, certainly not in the detail that argumentation theory would require. But his account of science invites such development-a multi-dimensional argumentation-theoretic approach.

Each of these aspects of scientific argumentation has at some point attracted the attention of a particular discipline. Philosophers in the spell of logical positivism once championed logic as the high road to scientific justification (e.g., Hempel, 1966; Scheffler, 1981); nowadays they also turn to dialectical models of various sorts (e.g., Brown, 1977; Kitcher, 1993; Pera, 1994). Sociologists and historians of science favour various sociological and institutional explanations (e.g., Shapin, 1982; Fuchs, 1992; Lynch, 1993). Still other theorists, drawing on literary, rhetorical, and communication theory, have been contributing to a growing literature on the rhetoric of science (Prelli, 1989; Gross, 1990; Myers, 1990; Campbell, 1986, 1990). Because each of these strands represents one dimension of argument, argumentation theory should be able to provide an overarching framework for the rationality debates that heretofore have been conducted somewhat at cross-purposes. Probably one of the most ambitious attempts to pull these various strands together in a theory of argumentation is Fuller (1993b).

A multi-dimensional framework is certainly not new to argumentation theorists. It has roots in Aristotle, and more recently has appeared as the distinction between product, process, and procedure (Wenzel, 1979, 1987; Brockriede, 1982; Habermas, 1984; Rehg, 1997). Here I want to explore its possibilities as a framework for interdisciplinary research on the topic of scientific change. This requires a certain flexibility, given the wide range of approaches to these various aspects of argumentation (Williams and Hazen, 1990; Eemeren et al, 1996). Moreover, it excludes the principled approaches that rule out certain conclusions-and disciplines-at the start. Excluded are both a philosophical rationalism that relegates sociology to the genesis of error (Laudan, 1977, pp. 202-03) as well as the principled, anti-philosophical relativism and anti-normativism one finds among certain sociologists (e.g., Bloor, 1991; Collins, 1985; Woolgar, 1988). Such approaches have perhaps made valuable contributions, but these can be assimilated into the proposed framework only if we extract them from their dogmatic shells. This point is particularly important for addressing the rationality debates. The debate is already settled for sociologists who assume a relativist answer at the start. A flexible, multi-dimensional theory of argumentation allows us instead to take an unprejudiced, case-by-case approach to the rationality problematic, open to a range of explanations for a given episode of theoretical development. In other words, I propose we leave it *an open question, to be settled by detailed argumentative case analysis,* whether a given theory was chosen "on the merits," or as a result of "extra-scientific factors" (cf. McMullin, 1987; Engelhardt and Caplan, 1987; also Rescher, 1977, pp. 117-18). This also allows us to use argumentation theory critically, as a normative means of testing actual arguments.

One cannot answer an open question of this sort, however, unless one knows what it means to say that a scientific controversy has been resolved and one of two competing theories chosen "on the merits," i.e., according to the better arguments. For this outcome to materialize, what must also be true of scientific controversy, reasoning, and consensus formation? The logical empiricists turned their attention away from actual controversy and focused instead on the retrospective justification of the superiority of the winning theory. Assuming this project has indeed failed, can a dialectical approach fill the gap? Can scientific dialectic open the door to a rationality of science that is not compellingly logical but still allows us to see how scientists can choose

one theory as rationally superior to another?

II

Before we can answer this question, we must face a challenge posed by dialectical rationality itself. Dialectic straddles the line dividing semantics and pragmatics, relations between sentences and relations between persons. In dialectic, arguments become social interactions, dialogues (cf. Goldman, 1994; van Eemeren et al, 1993; Walton, 1992). Consequently, the dialectical aspects of argument cannot be fully separated from the communal and socio-institutional aspects. This is hardly new, but it was Kuhn who brought out its implications most forcefully for the philosophy of science: the good reasons and shared values that fill out dialectical argumentation presuppose induction into a specific community: the training, the ways of seeing evidence and applying methods, the standards of good scientific reasoning, modes of defining problems-all of these are learned from a community of scientists, and such communities are multiple and shifting, even if they share some broad scientific commitments.

Thus, Kuhn's standards of good science are at once dialectical and "sociological":

> Some of the principles employed in my explanation are irreducibly sociological.... In particular, confronted with the problem of theory-choice, the structure of my response runs roughly as follows: take a *group* of the ablest available people with the most appropriate motivation; train them in some science and in the specialties relevant to the choice at hand; imbue them with the value system, the ideology, current in their discipline ... and, finally, *let them make the choice.* ... Whatever scientific progress may be, we must account for it by examining the nature of the scientific group, discovering what it values, what it tolerates, and what it disdains (Kuhn, 1970b, pp. 238-39).

But is not this move just what opens the door to the principled relativism championed by SSK? If the standards of good argument are relative to a given community of scientists (and their reigning paradigm or conventions), then when two different communities pit their respective theories against one another, we have no neutral standards by which to make a rational choice between the two views. If we follow the line of thought leading from Kuhn to SSK, then the social character of dialectic threatens to swallow up the very meaning of "the better argument." This would not destroy rationality as such, but it would settle the rationality debates in favor of the conventionalists and sociological relativists (cf. Hesse, 1980, pp. 29-62). The critical use of argumentation theory envisioned above would then be seriously hamstrung.

If the social-thus sociological-character of dialectic is what generates relativism, I doubt one can escape the above difficulty by tightening up the concept of dialectic, i.e., by defining more explicit standards of dialectical rationality. Because such standards are not compelling algorithms, they *must* leave open some gap-perhaps the very gap that SSK has learned to exploit so well for relativist purposes. Consider two explicitly dialectical approaches to science, both developed in response to Kuhn. The first, that of Harold I. Brown (1977), emphasizes judgment. Brown locates dialectical standards, not in eternal standards of science, but in the common ground shared by the theories involved in a given controversy-the various conceptions, commitments, and

problems that the competing theories recognize (pp. 140-44). Because these standards are not algorithms, their application requires scientists to exercise their capacity for judgment. This means that, at some level, the practical wisdom of the trained scientist, and not logically compelling rules, "provide the locus of scientific rationality" (p. 149; also pp. 147-49). The reference to training and wisdom takes us back to the polis of scientists, and thus back to the actual consensus of the scientific community. In the end, Brown redefines "scientific knowledge in any era" as "what the scientists actually take as such" (p. 151), a position not so far from Kuhn's, nor so far from relativism (pp. 151-65).

Marcello Pera's interesting analysis of scientific dialectics provides a second example. According to Pera (1994), a theory T represents "progress" over its competitor if the proponents of T have scored an "honest victory" over the competing theory, i.e., if they have produced stronger arguments for T than the arguments for its competitor (p. 182). The relative strength of the supporting arguments for T depends, in turn, on the "dialectical situation" and the "substantive factors" that can be enlisted in support of T, i.e., the available facts, other accepted theories and assumptions, presumptions that assign burdens of proof, commonplaces of preference, and values such as simplicity, consistency, and so on (pp. 112-21). The process of argumentation is governed by procedural rules for conducting and adjudicating scientific debates (pp. 121-26). If a theory T can enlist more, or more important, substantive factors than its competitors, then it has a winning argumentative strategy. Pera pulls these various threads together in his "explication" of a winning dialectical argument:

> A dialectical strategy in favor of a scientific thesis T is winning for one side P against the other Q if, on the basis of the rules that govern scientific debates, P, starting with the premises granted by Q and with the substantive factors of scientific dialectics, forces Q either to assent to T, or to stay silent, or to withdraw from the debate (p. 121).

Note the interesting mix of social and argumentative factors in this definition. On the one hand, the "substantive factors of scientific dialectics" points to available arguments, such as the empirical data available to the community. On the other hand, a winning strategy-and thus, presumably, a stronger argument-is marked by a social change: one side finally shuts up.

The social process is what seems to be decisive for Pera: the *actual* debate, not the rules, decide which theory has the better arguments in its favor. It is, finally, the participants themselves who must uncover and weigh, in actual debate, the "substantive factors" at stake for them in the form of explicit commitments (p. 185). It could hardly be otherwise, given the inherently contestable character of many of these substantive factors and the canons for their appraisal, such as "pertinence" (see p. 118). Here too, then, we end up with an account of dialectic that opens the door to judgment, training, and sociology. That one side finally gives up its claims in a controversy will undoubtedly *involve,* if not be determined by, accompanying sociological factors.

However, the social character of dialectic leads to relativism only in conjunction with further assumptions. After all, C. S. Peirce considered argumentation to be social but did not draw relativist conclusions. Relativism follows only if one can identify a weak spot in the very content of scientific inference, such as a gap or circular relation between observation and theory. For Kuhn, the paradigm is what creates this gap. His relativism thus largely stems from his notion of paradigmatic incommensurability, which has been criticized by a number of theorists, including SSKers (Masterman, 1970; Bloor 1991, pp. 25-31; Kitcher 1993, pp. 97-105; Laudan 1996, pp. 6-14). Though I cannot detail them here, these criticisms are well-taken, in my judgment. Nonetheless, there is a gap, opened by a related, but broader, problem for the rationality of scientific advance, namely, the

underdetermination of theories by evidence. This problem-actually a set of problems-can also be overstated (Laudan, 1996, chaps. 2-3). As I use the term here, it does not refer so much to the idea that any number of different theories can be mapped onto the same data-as though theories were so easy to generate!-but that two or three different theories, each more or less equally imperfectly, explain the data. What then determines whether a given scientist will opt for one rather than the other? What leads the scientific community to go in one direction rather than the other? If sociological shifts in the makeup of the scientific community are decisive for theoretical developments, then in what sense can we say that current theories are rationally superior to their predecessors?

I have my doubts that most theory choice is underdetermined by the available evidence, at least after a certain point in an ongoing controversy (see Kitcher 1993). But there may be interesting cases where it is underdetermined. Moreover, if we concede this point for the moment, we may gain a richer concept of scientific rationality. Thus, in what follows I tentatively assume that, often enough, dialectical argumentation is both underdetermined and sociologically embedded. The challenge, then, is to show how a normative, nonrelativistic argumentation theory is still possible.

III

The foregoing considerations lend force to a question that we might formulate as follows: If dialectical standards are not algorithms and cannot be applied in scientific controversy independent of socio-institutional factors, then what are the consequences for the rationality of theory choice? More specifically, how can we define "resolution of a controversy on the merits" and distinguish such cases of resolution from those involving other forms of closure? In this last section of the paper I will not directly answer this question, but will only reformulate it in a manner suitable for interdisciplinary investigation framed by the multi-dimensional theory of argumentation sketched above.

One approach we might take would start with the kind of counterfactual idealizations that Habermas (1993) has used in the moral and political domains. As participants in scientific argument-and as virtual participants, analysts parsing the arguments of scientists-we cannot but assume that our firm conclusions reflect the force of the better arguments. That is, we would misunderstand the nature of scientific questions if we thought that correct answers should decisively depend on, say, our class interests, or on the scientist's ability to wield social power. These may well determine the ability to *pursue* a scientific question, but they do not determine what is true or epistemically justified.

A Habermasian approach to science (Habermas, 1988; McCarthy, 1988; Longino, 1990, pp. 66-82, 197-202; Bohman, 1993) would allow us to employ the multi-dimensional theory of argumentation without prejudice to the analysis of particular episodes of scientific theory choice: the model would be open both to the possibility that a choice was made on the merits, and to the possibility that social factors were determinative. But this approach would be unlikely to give sociologists of knowledge what they want, if "social factors" are brought in primarily to explain research directions or errors in judgment, and not the rational justification of judgment-so much was already offered by Robert K. Merton's sociology of science (Merton, 1973). Thus, a Habermasian approach might pose obstacles to cooperation with SSK.

This potential difficulty leads me to propose an alternative to Habermas. To prepare the way, we must revisit the relation between dialectical factors and sociological factors. When I first discussed this, I tended to view it as a zero-sum game. To explain how a theory is chosen rationally, "on the merits," I assumed-along with much of

SSK-that one must show how the choice is reasonable against some set of argumentative standards; I then further assumed that this project was threatened by sociological infiltration through gaps in scientific argument. If the dialectical is both evidentially underdetermined and sociological, then theory choice is likely to occur not on the merits but by group conventions, paradigms, interests, or some similar prerational determinant. But must we set up the rationality question in such either-or terms?

I suggest we can avoid this either-or if we situate scientific judgment in a broader context defined by the *choice of research directions.* Other authors have also attempted to redefine scientific rationality in broader contexts, but such moves have generally been accompanied by relativist or skeptical views of argumentation (Pickering, 1984a, 1984b, chap. 1; Knorr-Cetina, 1983; Latour, 1987; Fuller, 1988, 1993b; Rouse, 1996). What I shall call the research decision making (RDM) model does not depend on relativist or skeptical commitments. The RDM model views science as a largely *informal collective method of making decisions* about the lines of research to be pursued by the scientific community. Framing the question of scientific rationality this way has the advantage of allowing us to see *all* the dimensions of argument as potential contributors to the rationality of theory choice. Before explaining this, some broad remarks on the role of decision and judgment in science are in order.

Imagine an individual scientist who is faced with the challenge of mounting a research project (although most people working in science face this challenge only in a limited fashion, as part of a research program already determined by the larger research group to which they belong). If she wants to survive as a scientist, she must make a wise *choice,* and this in turn requires her to examine the existing lines of research for some promising leads. On the basis of established theories and evidence, she must form a testable hypothesis that, if confirmed, would constitute an interesting result. If this hypothesis is a partial test or extension of a broader theory that has competitors, then our scientist must make a *judgment,* on the basis of the evidence, about which of the competing theories is most promising-she must, to borrow from Thomas Nickles, make a "heuristic appraisal" under conditions of cognitive uncertainty (Nickles, 1989). But this judgment is connected with decisions about how to act, in particular the decision about what line of research she ought to pursue. Such decisions will no doubt reflect a certain "opportunism in context" that exploits available material and theoretical resources (Pickering, 1984b, chap. 1). Her ability to pursue further research also depends in turn on her ability to argue, in a rhetorically effective manner, that her project deserves funding-again a choice, this time made by a funding agency (Myers, 1990). Further down the line, she will have to convince editors and referees to make the decision to publish her work.

Now the individual scientist represents only a single node in a dense network of judgment and decision. As we step back from our single researcher and view the field as a whole, we find a grid of interlocking scientific communities and institutions situated in a broader social whole. Allowing for false starts, mavericks and rebels, and numerous tentative gropings, we can perhaps identify, at least for some topics, an "overall direction" that science takes in that society, such as the direction defined by high energy quantum physics, or cancer research. This direction is partly an aggregate result of the individual judgments and decisions about what research lines to pursue; partly it issues from deliberate policy decisions taken by influential scientific, funding, and governmental bodies (e.g., Wenk, 1995). The key point is that the direction of scientific research does not issue simply from a judgment based on scientific evidence. There are many possible lines of research, and we cannot pursue all of them; there are many cases where judgments made in light of existing evidence are highly uncertain and heuristic, yet decisions must be made, funding appropriated, and alternative possibilities closed off, perhaps temporarily, perhaps permanently. Research decisions thus have an impact on the future judgments that will be available for us *even to entertain.*

That judgment and decision are closely intertwined in an enterprise such as science, which depends so heavily on material resources, is hardly a surprising or novel observation. I simply want to recall the broader context we could easily lose sight of as we focus on the rationality of theory choice. The problem of theory choice and progress arises within this social and institutional context, which has a steering effect on the process of scientific reflection, judgment, and theoretical development. The RDM model thus reconceives the rationality question as a problem of research direction-it is not simply an intellectual question about which of two, more or less fully formed, theories is more likely to be correct. This kind of shift is not new; Steve Fuller (1988, 1993b), for example, has also argued that the proper level for addressing normative issues in the study of science lies at the political, or policy level. But Fuller's approach trades on a skeptical view of scientific argumentation-in particular, he is far too quick to accept a notion of incommensurability more extreme than Kuhn's (Fuller, 1988, pt. 2). If such skepticism is taken too far, then the rationality of science policy decisions is also undercut.

According to the RDM model, a rational science policy depends in part on the capacity to appraise competing scientific theories on the merits. This is true of policy-making in general: workplace restrictions on smoking, for example, partly depend for their rationality on the scientific evidence for links between secondary smoke and damage to health. Likewise, decisions about where to direct research efforts and monies must rely in part on an ability to appraise the scientific arguments for and against various project proposals. But such decisions also depend on much more. This complexity is what allows the RDM model to outflank the dichotomies infecting the rationality debates. Moreover, it allows the RDM model to get beyond the zero-sum game noted earlier, which pits dialectical reasoning against sociological determination.

Because the focus is on a *social decision informed by theory choice,* and not simply on the theoretical judgment in isolation from the broader social process, a more encompassing concept of rationality becomes appropriate. Like most personal decisions, social decisions are often made in conditions of cognitive uncertainty and under the influence of various interests. In such contexts, it is rational to rely on modes of reasoning that appeal to more than the evidence immediately relevant for the matter to be decided. For example, appeals to expert authority and reliance on opinion leaders may be a reasonable "shortcut" in highly complex domains; indeed, modern science significantly depends on interpersonal trust (Shapin, 1994; cf. also Kitcher, 1993, chap. 8). Other, more formal decision-making mechanisms, perhaps analogous to what we find in the political and legal realms, can also enhance the rationality of such choices. At least ideally, such mechanisms are considered to enhance the rationality of decision making in contexts defined by competing interpretations, plural values, and insufficient evidence. If political choices can be rationally made under such conditions, why not scientific research choices?

Once we broaden the concept of rationality in this general way, it becomes possible to see how various levels of argumentation-the logical, the dialectical, the rhetorical, and the socio-institutional-might be relevant for an understanding of the rationality of scientific advance, now redefined as not just theoretical but social as well. Here I have focused mainly on making the dialectical and social levels more compatible, but the point can be extended for the other levels as well (Rehg, 1997). The idea is that each aspect of argumentation can help us make a more rational decision about which scientific direction to take. This idea also supplies the *normative* standpoint for examining episodes of scientific advance from an argumentation-theoretic perspective: the analyst or historian examines the dimensions of argument in all their complexity and thickness, asking at each level, how did this part of the argument inform the outcome, i.e., how did it contribute to, or detract from, the rationality of this particular episode of change in the direction of science?

I want to leave off with this question. My aim in this paper was simply to reformulate the rationality problematic

as it looks from the perspective of a multi-level argumentation theory. I cannot explore here all the further questions and difficulties that arise from the reformulation. One will see, perhaps, that we have gone some way toward a "social epistemology" (Fuller, 1988; Longino, 1990; Schmitt, 1994). I have wanted to steer clear of the overly relativist versions of social epistemology, however: the RDM model I propose takes the "force of the better argument" seriously, but it recognizes that the relevant dimensions of such arguments include both rhetoric and sociology. At the same time, if I have headed toward a social epistemology of some sort, I have left the definition of "social knowledge" almost entirely open. That would be just one of the further questions I must leave aside for now.

Notes

1. Examples of the logical-empiricist approach include Carnap (1963), Nagel (1979), Popper (1992), and Scheffler (1981). For the problems afflicting logical empiricism, see Suppe (1977) and Brown (1977). On the distinction between contexts of discovery and justification, see Nickles (1980).

2. For philosophical attempts to rescue the rationality of scientific progress in the wake of Kuhn, see Shapere (1969), Lakatos (1970), and Laudan (1977). I use the term SSK broadly, to cover any sociological account of the content of science; SSK thus includes the "Strong Program" represented by Barnes (1974), Shapin (1982), and Bloor (1991); the various ethnographic, discourse-analytic, and social-constructivist approaches taken by Latour and Woolgar (1986), Knorr-Cetina (1981, 1983), and Collins (1985); and the radical reflexivist turn of Woolgar (1988)-to name just a few sources in a vast literature. By "study of science" I refer to all the approaches to science: philosophical, sociological, rhetorical, feminist, etc.

3. As SSK proponents themselves admit, their relativism (formulated as the "symmetry principle") is a reaction against rationalism; see, e.g., Bloor (1991), Barnes and Bloor (1982), Collins (1983), and Mulkay (1979, chap. 1). Partly this reaction expresses a methodological move and thus remains agnostic on the philosophical issue; but SSK typically slides into a *philosophical* relativism, which consequently suffers under the very dichotomy that SSK set out to correct.

4. SSK accounts often start with critiques of philosophical claims on behalf of science; see, for example, Bloor (1991), Collins (1985), Woolgar (1988). For philosophical critiques of SSK, see Brown (1989), Bunge (1991), Bohman (1991, pp. 40-56, 204-11), Kitcher (1993, chaps. 5-6). Sociological critiques of different strands of SSK are available in Woolgar (1981), Pickering (1992), and Lynch (1993).

5. The gestalt image is applied to Kuhn by Pera (1994, pp. 8-10).

6. The influence of Kuhn on SSK is described by Barnes (1982); for a literary approach influenced by SSK, see Myers (1990). For the developments after Kuhn, see Fuller (1992; 1993a).

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