Pluralism and Rationality in the Social Sciences

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This article takes it for granted that science is intrinsically social and that competition is part and parcel of science. Four kinds of competition are distinguished and related to four kinds of rationalities: technological, normal scientific, political, and philosophical. It is argued that science as a whole is rational when there is interaction between the different (sub-) rationalities. Science needs not only different disciplines, but a methodological *division of labor.*

TWO CLASSICAL APPROACHES

In Anglo-American studies of science, there are two classical approaches to the rationality of science. They may be called the philosophical-methodological approach and the sociological-causal approach, respectively. For the methodological approach, rationality is everything; for the causal approach, rationality is nothing.

In the methodological approach it is taken for granted that there exists a definite scientific rationality and the philosopher's task is to specify and develop this rationality. Traditional positivist and Popperian philosophy of science exemplifies this approach. In the causal approach, on the other hand, one tries to make purely causal sociological studies of science. One works from the start with an *arationality* assumption, that is, an assumption to the effect that science can be explained without any talk about rationality and irrationality. The so-called strong program in the sociology of knowledge—Barry Barnes (1982), David Bloor (1976), and others—is, of course, the ideal typical example, but the same goes for Bruno Latour and Steve Woolgar's (1979) book *Laboratory Life: The Social Construction of Social Facts.*

Even if it turns out, as I think it does, that the sociological-causal approach increases our knowledge about how science is related to the rest of society, it is nonetheless true that this approach cannot tell us how to organize science in the future. Because its outlook is purely

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causal, it cannot, by definition, discuss and try to improve the rationality of research.

In the methodological approach, one wants to improve the rationality of science by improving each *individual* scientist's beliefs and actions. Usually, one puts forward what might be called a Crusonian methodology and rationality. This approach often contains a tacit three-tiered assumption which says that (1) a scientist can be rational all alone, (2) all scientists should conform to the same main methodological rules, and (3) science is rational when all scientists individually are rational.

Not only stern positivist methodologies conform to this kind of individualism. Paul Feyerabend's (1975) anarchist methodology is also a Crusonian methodology. In positivist thinking, all scientists ought to follow the same rules all the time. According to Feyerabend, anything goes for any scientist at any time.

The theories of collective choice have made it perfectly clear that collective or social rationality is not in general reducible to individual rationality. Notions like "prisoner's dilemma" and the "free rider" are well-known today. However, this irreducibility has not, as far as I know, been taken into account in discussions of scientific rationality.

Sociology of knowledge is usually social and causal, and traditional philosophy of science is usually individualistic and rationalistic. In this article, I try to look at science from a sociological perspective but nonetheless retain the aim of improving rationality; that is, I try to combine a social and a rationalistic approach. My main conclusion is that monomethodological Crusonian rationality should be replaced by multimethodological social rationality. I claim that it is rational to have a methodological division of labor in science, in particular in the social sciences.

In chapter 23 of his famous *The Open Society and Its Enemies*, Karl Popper (1945) stressed the social character of science, and he talked derogatorily about Crusonian science. Notwithstanding, his methodology is essentially individualistic. According to Popper, scientists cannot be completely rational all alone because they need criticism. Criticism is needed because science is fallible, and criticism is necessarily social. However, Popper's specified methodological rules are rules for all scientists individually all the time. Popper has not argued for a fundamental social division of methodological labor. In the three-tiered structure referred to earlier, he modifies the first assumption but subscribes to the other two.

MEANS-END RATIONALITY

The rationality of science has to be regarded as a kind of means-end rationality. In a realist view of science, the aim of science is truth finding, and rationality is then the means to obtain truth or truth-likeness. In an instrumentalist view of science, on the other hand, rationality is the means to obtain useful instruments. I shall in this essay assume that at least some parts of science are looking for truth-likeness, but scientific rationality has to be regarded as a means-end rationality independent of this assumption.

I want to emphasize the means-end character of scientific rationality because rationality often appears as something of intrinsic value. This false appearance is, I think, due to the fact that scientists represent one of the most value-imbued roles in modern society. They are not only persons doing research, they are socially significant moral characters. Therefore, it seems to be of intrinsic worth to incarnate rationality. But this rationality is nonetheless a means-end rationality.

There are, in summary, four specific assumptions which I shall make in the ensuing discussion of scientific rationality:

1. Science is necessarily a social institution.
2. One of the goals of science is to give us a more truth-like view of nature, man, and society.
3. Science has a means-end rationality which in principle can be improved.
4. Social rationality is not reducible to individual rationality.

I will first distinguish between four different kinds of competition and then between three different kinds of rationality. After that, I relate competition structures and rationality structures to each other. Finally, I examine the particular case of rationality in the social sciences, during which a fourth kind of rationality will be discussed.

I. COMPETITION

According to the traditional picture of the scientist, the scientist is not fighting or competing but is just seeking the truth. When scientific criticism is stressed, it is seen rather as a kind of cooperation than as a fight or competition. Without making any speculations about the motivation of individual scientists, this is clearly a false picture. It is an obvious social fact that most of the criticism put forward within

scientific communities is not a criticism put forward in a reward-neutral social setting. There are monies, internal prestige, and public fame to collect. The Nobel prize is merely a very clear example of the common state of affairs.

Some sociologists of knowledge have stressed the fact that scientists often are competitive minds, but, in my view, the sociologists have not really put scientific competition in a sociological framework. This I will try to do now, with Georg Simmel's ([1908] 1955) sociological classic, *Der Streit* (*Conflict*), as my departure.

Simmel distinguishes carefully between competition and other forms of conflict. The defining characteristic of competition is that the adversaries fight about something that none of them has or owns for the moment. In other kinds of conflict, let me say conflicts for short, one fights about something that belongs to one of the adversaries, be it land, valuable things, or resources of other kinds. The difference can, according to Simmel, be exemplified with men's fight about women. If two men both try to get a woman who has no relationship to either of them, then they *compete* for her. But if one man tries to get another man's wife, then there is *conflict*.

Sports is normally competition because the prizes one fights for (future fame included) do not belong to any of the competitors before the contest. Market competition is competition because the firms and the sellers do not primarily try to get each other's money. They want the customer's money. So too in research, there is primarily competition, not conflict. The money and status awarded do not beforehand belong to the scientists fighting for it.

The conceptual distinction between competition and conflict is in itself neither a distinction between different ways of acting when fighting nor a distinction between different kinds of goals that the adversaries have in mind. It is a distinction between two different kinds of *relationships* between the goal of a fight and the adversaries. When the goal is something that can be owned, there is conflict if one of the adversaries owns it. If none of them owns it, there is competition. If the goal is something that cannot be owned, there can only be competition for it. Fame and status, for instance, can only be competed for because they are things that cannot be owned. They are contingent attitudes toward persons. Sometimes they are rapidly changing, sometimes long-lasting, but even when they are long-lasting, fame and status are not owned. The same goes for love. When Simmel exemplifies his distinction with women, a specific view of marriage is presupposed.

Countercompetition and Parallel Competition

To understand scientific competition, we have to distinguish between different kinds of competition. One distinction, made by Simmel but not baptized by him, is between what I call *countercompetition* and *parallel competition*. Examples of countercompetition in sports are football, wrestling, and chess; examples of parallel competition are running, weight lifting, and figure skating. In a sport with parallel competition, one can compete with oneself. One can compare one's performances in relation to the clocks, the weights, and the style points, respectively. In a running contest, everybody is (in a way) competing against the clock simultaneously and in *parallel*. The winner is the one who has the best time. In games of football, wrestling, and chess, on the other hand, the competitors are directly fighting *against* one another. It is possible to compete with oneself only in the secondary sense that one compares one's own performance *against another* with earlier performances *against others*. In sports with countercompetition, it is absolutely impossible to compete with oneself without, at the same time, competing with others. When and where it is forbidden in advertisements to say bad things about another firm's product, there is parallel competition; when such advertisement is allowed, there is countercompetition.

The distinction between parallel competition and countercompetition, applied to science, partly coincides with the distinction between the hard and the soft sciences. Hard sciences are not primarily quantitative; the important thing is that they have well-specified and fixed methodological rules. In a hard science, researchers can compete with themselves. Without meeting any competing scientist they can decide whether or not they have made a good and accurate experiment or invented a simple and effective mathematical model. Here, the methodological norms function the way that the clocks do for runners and the way that the weights and style points do for the weight lifters and figure skaters, respectively. In the hard sciences, there is parallel competition.

In the soft sciences, for instance in history of literature, there may exist fixed methodological norms and parallel competition, but mostly the situation is somewhat different. The quality of an interpretation of a book can only be measured *against other interpretations* of the same book. Therefore, the researchers cannot rest content with their own interpretations of an author and his work. They have to also discuss interpretations made by others, preferably famous researchers. In

physics, normally, the one who makes an experiment does not need to discuss the experiments of others. In history of literature, there is normally countercompetition; in physics, there is normally parallel competition.

I do mean "normally," as I believe in at least something like Thomas Kuhn's (1970) famous distinction between revolutionary science and normal science. During normal science, physics contains well-specified and fixed methodological norms, and there is parallel competition. But in a competition between different paradigms, there are no specified fixed norms. The competition during a period of revolutionary science is, like the competition in literature interpretation, a countercompetition. Both normal and revolutionary science contain competition, albeit competition of different kinds.

Public-Oriented and Actor-Oriented Competition

Dance orchestras are competing. They are competing for the favor of the public. Avant-garde poets are also competing. However, they do not bother much about the public. They are almost exclusively competing for each other's favor. Their competition is a fight about the internal rank order in an informal esoteric society. In conformity with these two examples, I make a distinction between *public-oriented* and *actor-oriented competition*. In actor-oriented competition, the actors themselves are deciding about the prizes and rewards, but in public-oriented competition, the spectators make the corresponding decisions.

Actor-oriented competition is very similar to a conflict, that is, a fight about something that one of the adversaries owns, but it is really a kind of competition. Esteem and good reputation, which is what one mainly is fighting about here, is not something that can be owned. One cannot acquire esteem and good reputation in the same way that one can take money and property. Actually, I would say, it is *future* appreciation which one fights about, and that cannot belong to anyone before or during the fight.

Several activities contain and fuse both public-oriented and actor-oriented competition, but this fact does not cancel the conceptual distinction. The same goes for the fact that the one kind of competition may dominate over the other in a certain activity. Our conceptual distinction can, but need not, distinguish activities which are separated in space or time.

When the distinction between public-oriented and actor-oriented competition is applied to science, it tends to coincide with the distinc-

tion between applied and basic research. Ordinarily, applied research is defined as a systematic and methodical search for knowledge *with* a specific application in sight. Applied natural scientific research often results in products whose efficacy and usefulness can be evaluated by people who are not doing research. The same is true in medicine. Patients can, to a large extent, evaluate medical technology. This being so, competition within applied research is largely dominated by public-oriented competition.

Basic research is usually defined as a systematic and methodical search for knowledge *without* a specific application in sight. Basic research is pure truth seeking. The conditions for evaluating complicated truth seeking are different from those for evaluating the usefulness and effectiveness of a product. A layperson cannot, for instance, evaluate experiments and calculations made by physicists. Therefore, competition within basic research in physics is mostly actor-oriented competition. A crosswise matrix of the two distinctions is shown in Table 1.

So much for the moment about competition. Let us now for a while look at different kinds of rationality.

II. THREE KINDS OF RATIONALITY

Much has happened in philosophy and philosophy of science since the heyday of positivism and the positivist-Popper dispute. In particular, there are two developments within Anglo-American philosophy on which I want to focus: (1) the rise of a specific philosophy of technology and (2) the resurrection of metaphysics within analytic philosophy. For reasons of space, I have to be very brief about these two developments.

During the past 20 or 25 years, a specific philosophy of technology has matured. So, today, there seems to be a far-reaching consensus to the effect that technology has a rationality of its own, that is, a rationality distinct from that of science—science in the sense of basic research. The most striking difference is that in technology it is rational to work with false and obsolete theories. The ordinary engineer does not, for instance, need relativity theory. For him, Newtonian physics works well enough most of the time.

In what follows, I take the existence of a specific rationality of technology for granted. What it looks like in detail need not bother us here.

TABLE 1
Competition Matrix

	Public-oriented	Actor-oriented
Parallel competition	1	2
Countercompetition	3	4

When positivist philosophy reigned supreme, the so-called underlaborer conception of philosophy dominated analytic philosophy. According to this conception, science tells us what the world looks like and philosophy is only concerned with language. Philosophers, however, can clarify the concepts used in science. They are in this sense underlaborers. Because metaphysics is regarded as meaningless, it follows that only scientists can investigate the world.

But things have changed. Nowadays even analytic philosophers are doing metaphysics, for when writing about, for instance, natural kinds and the mind-body problem, they try to say something about the world. Obviously, such a view implies that the underlaborer conception of philosophy is false, but the analytic metaphysicians have not bothered much about this implication.

If one believes in the underlaborer conception of philosophy, then it is possible to identify rationality with scientific rationality. If one believes in metaphysics, however, then it seems necessary to believe in the existence of another kind of rationality, too. Metaphysicians do not work the way that ordinary scientists do with empirical data and mathematical models, but they do argue. The problem is what kind of rationality pattern the arguments follow.

Prepositivist metaphysicians subscribed, implicitly or explicitly, to what Peter Winch (1958) has called the master-scientist conception of philosophy (see chap. 1:3). This is the view that the philosopher, independently of empirical science, can, in outline, say what the world looks like. The philosopher is assumed to supply us with a priori knowledge. However, this is no longer a viable alternative, which means that both the underlaborer conception and the master-scientist conception of philosophy are gone. What else, we have to ask, is then left?

Now and then it is said that philosophy is an activity without method. Philosophy, the argument goes, can by definition question everything including itself, which means that philosophy can have no methodological Archimedian point of departure. This fact, however, does not imply that there is no rationality. Methodology and rationality need not be identified, nor need rational arguments be identified with conclusive and infallible reasoning. If we accept fallibility in both science and metaphysics, then rationality is fallible, too.

With this in mind, take a quick look at Kuhn's (1970) incommensurability thesis. Incommensurability implies untranslatability, but it does *not* imply incomparability. Just as a bilingual person can compare two different but untranslatable languages, it is possible also to become, at least more or less, biparadigmatic. And two biparadigmatic persons can in some sense compare the paradigms and argue with each other, even if the argumentation lacks a fixed point of reference.

I think the situation in a paradigm conflict is similar to, if not identical with, the kind of situation in which metaphysicians today find themselves. Kuhn himself, by the way, talks about a metaphysical component of a paradigm. However, when Kuhn's *The Structure of Scientific Revolutions* appeared, metaphysics was still regarded as more or less irrational. Therefore, conflicts between paradigms and the corresponding kind of argumentation could not be compared with any specifically philosophical kind of rationality. Philosophy was then confined to conceptual analysis, but paradigm conflicts were conflicts about the way the world is. Necessarily, paradigm conflicts came to look irrational. Kuhn himself talked about persuasion, and Feyerabend exclaimed that "anything goes." However, now that metaphysics is accepted as a proper rational undertaking, we can no longer draw the conclusion from the premise "anything can be questioned" that "anything goes," or that everything is a matter of pure fashion.

As in the case of the rationality of technology, it is not possible for me in the brief space of this tentative essay to even try to pin down the specific rationality of metaphysics. I am simply asking the reader to take the existence of this kind of rationality for granted, at least hypothetically, so as to follow my line of thought to the end and see where it leads.

Let me call the kind of rationality typical of metaphysics and paradigm conflicts "philosophical rationality." What I have said about this rationality is meant to be compatible with the existence of normal scientific rationality. Actually, I think a lot of things said both by positivists and by Popperians, and now usually regarded as false, will

turn out to be fairly close to the truth when they are confined to merely one kind of rationality, namely, that of normal science. Being all-embracing is one thing; being delimited by both a rationality of technology and a rationality of metaphysics is quite another. In short, I am convinced that there exist at least three different and nonreducible kinds of rationality structures. One is proper for normal science, one for technology, and one for metaphysics and paradigm conflicts.

III. DIVISION OF METHODOLOGICAL LABOR AND COMPETITION

I will now try to relate what I have said about rationality with what was said earlier about competition. If we look at physics, it is easy to place different kinds of research (normal science, technology, and paradigm conflicts) in the four squares of Table 1. The very complicated experimental set-ups and the complicated mathematics used in physics make it impossible for a layperson to judge normal scientific work. It means that normal science is dominated by actor-oriented competition, and because in normal science the methodological norms are fixed, it is parallel competition. In other words, normal science in physics belongs to square 2. Applied research and accompanying technology can, as I have said, be judged through its products, which means that it is public-oriented competition. Like normal science, it has fixed methodological rules, which means that it is parallel competition. Technology and applied physics belongs to square 1.

Paradigm conflicts, finally, belong to square 4. They contain actor-oriented countercompetition. A paradigm conflict in physics is actor oriented because no one without training in physics can grasp the arguments. Think only of the conflict between relativity theory and Newtonian physics, not to say that of quantum physics and Newtonian physics. The form for such conflicts is that of countercompetition, as there is no concrete standard by means of which the conflict can be settled.

Instead of *the one* scientific rationality, we have now three different kinds of rationalities connected with three different kinds of competition. Another matrix corresponding to the one for competition is shown in Table 2. Although square 3 is empty in physics, it is not in other sciences, as will soon become clear.

TABLE 2
Rationality in Physics

1 Applied science: Technological rationality	2 Basic science: Normal scientific rationality
3 —	4 Paradigm conflict: Philosophical rationality

IV. THE SOCIAL SCIENCES

Let me now turn to the issue of the specific rationality of the social sciences. In this undertaking, I will use economics and sociology as ideal-typical examples.

Actor-oriented parallel competition with the kind of rationality typical of normal science (i.e., square 2) abounds in both economics and sociology. It fits both theoretical work with mathematical models and statistical methods as well as empirical research done in order to find out what our societies really look like.

Let us next look at square 1—or rather squares 1. Remember that we now have two matrices, one for competition structures (Table 1) and one for rationality structures (Table 2). We get two questions: Is there any research within economics and sociology which is public oriented parallel competition? Is there in the social sciences something similar to technology and applied natural science which has a rationality of its own? My answers in both cases are positive.

Ordinary technology and applied natural science aims at producing inventions. In my view, the creation of new forms of organization, new rules, and new laws should be seen as *social inventions*. Politicians and administrators appear as ideal-typical examples of social inventors. Let me for the sake of convenience talk about decision makers

and look upon the decision maker as the social counterpart to the inventor, notwithstanding the fact that, of course, most decisions are routine decisions.

Many decision makers, both in the public and the private sector, use social scientists. They employ them in order to get decision bases. The social scientists are paid to find out how, for instance, a specific institution functions and/or malfunctions in different respects. The decision makers then look at the results of these investigations before they make their decision. The function of the social scientist is that of a *decision consultant*. Being a decision consultant is the social scientific counterpart to the natural scientist who is doing applied research.

I think it is as necessary in the social sciences as in the natural to keep basic and applied research conceptually and methodologically distinct. Norms that are necessary in basic research may not even be desirable in applied research. Take for instance the norm of "organized skepticism." All results in basic science ought to be carefully scrutinized before they are published, but that takes time and is, therefore, in the world of action often impossible. Let me use a concrete example.

Either a government decides to change the rate of interest or it does not change it. Whatever it does, the action may have far-reaching consequences. There is, so to speak, no neutral span of time within which scientific skepticism can be allowed to work. Mostly, a quick decision and a correspondingly quick decision basis is needed. A decision consultant who, in such a situation, tries to apply all the norms adequate in basic research has simply not understood the undertaking and the norms proper to it.

The scientific interest of finding out how economic trends function and why there are booms and depressions may require both theory development and extensive amounts of empirical data. What kind of data and what quantity are needed should be determined only by the existing theories and the need for theory development. The goal is truth-likeness. In applied science, on the other hand, the kind and quantity of data needed have to be determined by other goals as well. Research aiming at decisions about the rate of interest need neither the same quantity nor the same accuracy of data as in basic research. A decision consultant who mistakenly thinks he or she is doing basic research instead of applied will surely overdo the empirical work and will become a slow and inefficient decision consultant. Actually, I think this is often the case. Many public investigations already seem to be obsolete at the time of publication.

To make a long story short, if this criticism of mine is right, then it is even more important in the social than in the natural sciences to distinguish between the kind of rationality appropriate for applied scientific research, on one hand, and the rationality appropriate for normal basic research, on the other. A philosophy of *social* technology is as badly needed as one of ordinary technology.

Like physics, both economics and sociology contain paradigm conflicts. In contrast to physics, both economics and sociology seem to have such conflicts most of the time, and in both, there is a more or less continuous debate on fundamental principles. In economics, I am thinking of those between neoclassical and Keynesian economics and between both of these and Marxian economics. Also, the discussion of Milton Friedman-like economics has had this foundational character. In sociology, we can think of the three-cornered struggle between Marxian, Durkheimian, and Weberian sociology.

Of course, as in all basic science, large parts of this foundational competition are actor oriented. The detailed arguments put forward can only be understood by people with long education and good scientific training. Paradigm conflicts in the social sciences then at least partly belong, like those in the natural sciences, to square 4.

When Kuhn was talking about paradigm conflicts and revolutionary science he was primarily thinking of conflicts such as that between Newtonian physics and relativity theory and between Daltoninan and pre-Daltoninan chemistry. In those cases, one single paradigm replaced another single paradigm. In my view, however, there is nothing in Kuhn's *concept* of paradigm which necessarily excludes a long period of multiparadigm state. Kuhn's own picture, where one paradigm is always succeeded by another, seems to be a contingent effect of the fact that he is discussing only the natural sciences in general and physics in particular.

Physics has for a long time now been insulated from political-ideological disputes, whereas the paradigms in the social sciences often and naturally get involved in such struggles. This is to me the difference which explains the multiparadigm character of the social sciences and the monoparadigm character of physics. Paradigms in economics and sociology usually overlap with political ideologies, which means that the corresponding paradigm conflicts often contain as much public-oriented competition as actor-oriented competition. People outside science have to evaluate the paradigms when they take a political stand.

In economics and sociology, paradigm conflicts are often equally actor-oriented and public-oriented competition, and it is countercompetition: We are at one and the same time in squares 3 and 4 of Table 1. This fact has to be taken into account in a discussion of the rationality of the social sciences.

With regard to means-end rationality, it is always irrational to try to do the impossible, and I think it is impossible to get rid of the public-oriented competition which exists around paradigms in economics and sociology. Therefore, when discussing the rationality of economics and sociology, we should not try to get rid of it but, instead, ask in what way we should handle it.

A very interesting question in this connection is whether there is also a kind of rationality which corresponds to square 3, that is, to public-oriented countercompetition. In that case, we would have a very neat conformity between kinds of competition and kinds of rationalities.

Perhaps I am looking for too much orderliness, but I do suspect there is an unexplored kind of rationality typical of public-oriented paradigm competition. Furthermore, I suspect that it is more or less identical to a kind of rationality typical of modern democratic politics. Such politics with its public debates is, in my opinion, not reducible to persuasion and advertisement. It contains among other things an element of popularization, that is, simplification, which should not be looked down on. In mathematics, a new and simpler proof of an old theorem is often regarded as a first-class intellectual achievement. In public political debates, something similar is found. A good ideologist can make accurate and adequate simplifications of his or her complex ideology, and that is a rational thing to do even apart from persuasive purposes. It makes it easier for *everyone* to evaluate the ideology at hand.

Now, when philosophy of science is no longer hunting for infallible and fixed methodological rules which incarnate rationality, the time is ripe for a new look at what is rational in politics. As long as philosophers identified rationality with infallible standards, they were implicitly looking for parallel competition, but politics is surely countercompetition.

Assume, if only for a minute, that even my belief in a specific rationality for public-oriented paradigm competition is true. In that case, we get a matrix (Table 3) describing the rationalities in the social sciences.

The rationality of the social sciences, in particular that of economics and sociology, is a multimethodological rationality. This gives us two alternatives: the craft and the industrial alternative, respectively. Ei-

TABLE 3
Rationality in Social Science

1 Applied science: Technological rationality	2 Basic science: Normal scientific rationality
3 Paradigm conflict: Political rationality	4 Paradigm conflict: Philosophical rationality

ther we demand that each and every researcher should be equally skillful in the four areas distinguished or we opt for a methodological division of labor. When clearly stated in this way, the industrial alternative is obviously the most effective one, even in science. If there is no specialization, a lot of things will never be discovered. I am thinking of technological artifacts, empirical data, and conceptual systems. Because scientific rationality is a means-end rationality, this division of labor has to be regarded as an essential part of scientific rationality.

If we require that every researcher should be equally skillful in the four areas distinguished, we would have science but a highly ineffective science. To require that every researcher should conform to one specific kind of rationality would mean disaster. If everyone should stick to philosophical rationality, science would be overspeculative, and if everyone should stick to normal scientific rationality, science would be overconservative. Interaction between the rationalities is rational because new philosophical ideas can give rise to new empirical data and new empirical data can give rise to new philosophical ideas. Even interaction with technological rationality is rational, as new artifacts represent a new kind of empirical data.

When the described division of methodological labor has come into existence, some traditional requirements on scientists can be viewed in a new light. It does not matter too much, then, if some scientists doing normal science are stubborn and dogmatic. Neither does it matter much if some theoreticians working with paradigm conflicts

are highly speculative and totally insensitive to empirical findings. Nor does it matter if some scientists disregard truth and falsity altogether and merely think in terms of usefulness. It does not matter—and here comes the qualification once more—as long as there is an *interaction* between the different kinds of rationalities. When this is the case, there emerges a kind of nonpersonal and all-encompassing social rationality which turns the other rationalities into subrationalities.

At the beginning of this essay, I mentioned a three-tiered assumption which is part and parcel of Crusonian methodology. The kind of rationality that I have now tried to advocate can be summarized in three corresponding points:

1. A single scientist cannot be fully rational.
2. All scientists should not conform to the same main methodological rules.
3. Science as a whole is rational when there is an interaction between the different (sub-) rationalities.

The interaction discussed here exists today. In fact, it delineates the actual rationality of science. In that sense, my essay, like Minerva's famous owl, flies at dusk. However, such an interaction can function more or less well. As long as many scientists still believe that there is only one *truly* scientific method, the interaction tends to function less well. If, on the other hand, everyone realizes both the actual existence and the *rationality* of this existence of different rationalities, then, in all probability, this necessary interaction will function much better and subsequently lead to the overall improvement of scientific rationality.

REFERENCES

Barnes, B. 1982. *T. S. Kuhn and social science.* London: Macmillan.
Bloor, D. 1976. *Knowledge and social imagery.* London: Routledge & Kegan Paul.
Feyerabend, P. 1975. *Against method.* Atlantic Highlands, NJ: Humanities Press.
Kuhn, T. 1970. *The structure of scientific revolutions.* 2d ed. Chicago: University of Chicago Press.
Latour, B., and S. Woolgar. 1979. *Laboratory life.* London and Beverly Hills, CA: Sage.
Popper, K. 1945. *The open society and its enemies.* London: Routledge & Kegan Paul.
Simmel, G. [1908] 1955. *Conflict.* London: Free Press.
Winch, P. 1958. *The idea of a social science and its relation to philosophy.* London: Routledge & Kegan Paul.

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