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# TOWARD A SOCIAL CONCEPTION OF SCIENTIFIC RATIONALITY

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This paper examines the consequences of Feyerabend's thesis against the notion of scientific method. It is claimed that he has a strong case. Comparisons are made with other contemporary philosophers of science such as Kuhn and Lakatos. A result of the case against method is that science appears not to be a rational enterprise. This conclusion is resisted. Nevertheless, in order to show that the rationality of science is compatible with Feyerabend's thesis, it is necessary to switch from a conception that ascribes scientific rationality to the individual scientist to a conception in which rationality is ascribed only to the enterprise of science as a whole. Then, scientific rationality is a social, or perhaps structural, property and our science actually has it to a large extent.

† † †

The work of Kuhn (1970) and Feyerabend (1975 and 1978) has challenged anew the thesis that science is a rational enterprise. The reaction has been swift, but the arguments so far advanced against Kuhn and Feyerabend are not very compelling. On the other hand, what Feyerabend says is not incompatible with a rational picture of science. This paper develops such a picture.

First, it is necessary to understand the nature of the case presented by Kuhn and Feyerabend. In philosophy of science circles, scientific methodology and rationality go hand in hand. There seems to be a plausible case for such a connection. Science has seemed to be a most successful enterprise, and it is not unreasonable that people would want to know the basis for that success. It was thought that science succeeded where other human enterprises failed because science proceeded differently, because it had a method all its own. Determining the method acquired great importance, then, because by its rigorous application we could improve already existing science and extend science to new areas.

According to this scheme, following the method would guarantee the success of science, or at least make such success more likely—thus the connection between method and reason. To be rational in science, then, is identified with living up to the standards of scientific rationality, *i.e.,* to follow methodological rules such as, "reject hypotheses that are in conflict with well confirmed hypotheses," "reject hypotheses that are in conflict with the facts," "do not make *ad hoc* moves," and the like.

Of course, whether science could be shown to be rational has always been a favorite subject for skepticism. But Feyerabend's case is of a different sort. What he argued is that in paradigm cases of scientific success it seems that the relevant scientists did not follow the method. Furthermore, success did not come about in spite of the violation of the method (*e.g.,* by luck, shortcuts, *etc.*), but rather it required that very violation. Feyerabend's argument against method, and hence against rationality, is a *reductio*:

1. Success is the justification for adhering to scientific rationality.

2. But, rationality actually gets in the way of success.

3. Thus, rationality and success are incompatible.

Feyerabend need not have any particular stake on whether science is actually successful, and he does not have to believe in the sanctity of argument or in the evidential techniques that he uses in historical analyses. He simply plays the rationalist game, accepts the rationalist starting position for the sake of argument, and then shows that no methodological rule can be excluded from violation, that from the point of view of the rationalist, "anything goes." It seems then that anarchy has reigned in science, and that it ought to reign if science is to progress.

There have been so many misunderstandings on this score that the main points bear emphasizing. First, Feyerabend made no claim "to possess special knowledge about what is good and what is bad in the sciences." According to him:

> *Everyone can read the terms in his own way* and in accordance with the tradition to which he belongs. Thus for an empiricist "progress" will mean transition to a theory that provides direct empirical tests for most of its basic assumptions. . . . For others, "progress" may mean unification and harmony, perhaps even at the expense of empirical adequacy. . . . *And my thesis is that anarchism helps achieve progress in any one of the senses one cares to choose* (Feyerabend, 1975).

Second, it is a simple point of elementary logic that when giving a *reductio ad absurdum* one need not be committed to the truth of the assumptions one accepts for the sake of argument. Thus Feyerabend (1975) said,

> Always remember that the demonstrations and the rhetorics used do not express any "deep convictions" of mine. They merely show how easy it is to lead people by the nose in a rational way. An anarchist is like an undercover agent who plays the game of Reason in order to undercut the authority of Reason (Truth, Honesty, Justice, and so on).

Third, *Anything Goes* is not really offered as methodological principle. It is rather a description of what things look like from the rationalist perspective after the force of Feyerabend's arguments is recognized. Feyerabend (1975) is clear on this matter:

> One might get the impression that I recommend a new methodology which replaces induction by counterinduction and uses a multiplicity of theories, metaphysical views, fairy-tales instead of the customary pair theory/observation. This impression would certainly be mistaken. My intention is not to replace one set of general rules by another such set: my intention is, rather, to convince the reader that *all methodologies, even the most obvious ones, have their limits.* The best way to show this is to demonstrate the limits and even the irrationality of some rules that she, or he, is likely to regard as basic.

Feyerabend's demonstrations consist of historical examinations and epistemological analysis of the relation between idea and action. For every "basic" methodological rule of empiricism he claimed that he could show that a "counterrule" may be preferred. He suggested, for example, a counterrule which "advises us to introduce and elaborate hypotheses which are inconsistent with well-established theories and/or well-established facts. It advises us to proceed counterinductively" (Feyerabend, 1975).

In favor of his counterrule, Feyerabend argued that the evidence to refute a theory is often unearthed *only* with the help of incompatible alternatives. As for facts, observations, and experimental results, they all contain theoretical assumptions, or assert them by the manner of their use. But, those assumptions which shape our view of the world are often not accessible to direct criticism, for usually we are not even aware of them. Prejudices, then, are found by *contrast,* not by analysis. ". . . *we need a dream world,*" Feyerabend said, "*in order to discover the features of the real world we think we inhabit* (and which may actually be just another dream-world)." But the invention of this dream world designed to clash with our well established views, experimental results, and so on, is a counterinductive step. "Counterinduction is therefore always reasonable and has always a chance of success" (Feyerabend, 1975).

The history of science can be used to illustrate Feyerabend's sort of epistemological analysis. Consider the following two examples. At the beginning of last century Prout (*see* Lakatos, 1970) suggested that atomic weights should be expressed by whole numbers (since they were all multiples of the atomic weight of hydrogen). Unfortunately there were clear exceptions. The atomic weight of chlorine, for instance, was 35.6. Such was the result obtained from samples of "pure" Cl by the best practitioners of the science. There was no observational error and there was nothing really at fault with the purification techniques. More and better measurements would not have led to the conclusion (which would be favored today) that Prout was correct in spite of what seemed to be a clear refutation of his hypothesis. What was required instead was the development of a very different view of nature: modern atomic theory, and especially the notion of isotopes. As it turned out, the "pure" samples of Cl contained two isotopes of the element (of course, that is still pure Cl, for it is not mixed with other elements). The measured weight was that of the mix of the two isotopes.

Another case, which Feyerabend (1975) discussed in great detail, is that of Galileo. Let us concentrate on the Tower Argument. If a stone is dropped from the top of a high tower, the stone will hit the ground at approximately the same distance from the base of the tower as it was from the top of the tower at the initial moment of descent. Anyone can see that the stone moves straight down. This motion was taken as a refutation of the view that the earth moved. If the earth moves, the tower should have moved a considerable distance by the time the stone hit the ground. But, the distance between stone and tower remains the same. Or else, the stone must have fallen diagonally, but this is plainly not so. Thus, the earth cannot be in motion. Once again, what was required to overcome arguments such as this, was a new way of interpreting the phenomena, a set of "natural interpretations" that already assumes that the earth moves. Galileo challenged the concept of motion as observed motion. Some motion goes unobserved, he said, because it is shared by the observer. In the case of the Tower Argument, the stone only *seems* to move straight down. The *real* motion of the stone is a combination of circular inertia—which is shared by the earth, the tower, the stone, and the observer—and a perpendicular motion toward the center of the earth. The stone did really move as it had appeared ridiculous to suppose. (Not only did Galileo apply the counterrule, he saved the day by making a further and *ad hoc* hypothesis: circular inertia.)

In both of these cases, as well as in many other crucial episodes in the history of science, the inductive rules of the so-called scientific method would have favored the received view. The moral is that work in alternative and even bizarre views should not be discouraged [that what Feyerabend (1975) called the "principle of proliferation" cannot be ruled out].

The principle of proliferation is perhaps the most important point where Kuhn and Feyerabend lock heads. They both would have agreed that there is no method, but Kuhn viewed the history of science as a series of periods ("normal science") in which the scientific community is totally committed to a single point of view (what he called a "paradigm"). During revolutionary periods, paradigms are overthrown and replaced by new ones which enjoy the same total commitment from the scientific community. Revolutions are brought about only by crises, and these in turn are caused by recalcitrant and significant anomalies (Kuhn, 1970). But as Feyerabend (1970) pointed out, no anomaly is more significant than one explained within an alternative view. Feyerabend realized, however, that Kuhn had a point when he insisted that a view needs time and commitment before its worth can be realized. This means that practitioners may decide to continue work on a view that may have had some of its predictions falsified, that they may simply choose to put the falsifying instances on the back burner. This Feyerabend (1970) called the "principle of tenacity." Such a principle is most reasonable, for, as Kuhn (1970) argued, all theories are always besieged by anomalies, but anomalies do not become counterevidence until they are assimilated within a competing theory (or paradigm).

In Feyerabend's (1975) account there is intense competition among alternative views. But, even those that come worse off in the struggle need not be abandoned. They may still make a comeback (as the atomism and the heliocentric view of the cosmos did), and even if they do not they may still perform a valuable service to science. Thus, progress in science permits (indeed requires) sticking to a view in the face of conflicting evidence as well as proliferation of views. Science should then be an interplay of the principles of tenacity and proliferation. Unfortunately, from the point of view of the rationalist such an account of science rules out nothing—if it is correct then *anything goes.* But if there is no scientific method, there is no scientific rationality either.

Some rationalists have offered a different approach to methodology. Lakatos (1970), for example, tried to allow for the principles of tenacity and proliferation, while still insisting that some activities are more proper (*i.e.*, rational) than others. The unit of evaluation is a series of theories (a research program) which changes under the heat generated by competing theories (principle of proliferation). Such changes constitute a progressive problem-shift if they lead to an increase in content. If the changes do not anticipate new facts but instead serve only to assimilate the new discoveries of the competition (thus being *ad hoc*), the program is said to be degenerating. A program, however, may degenerate for a long time and still make a comeback (principle of tenacity). As promising as this approach may sound, it has several problems. The main one, in the context of the present discussion, is that there is no time limit, nor can there be, that would force abandonment of a degenerating research program. But then nothing can be ruled out. In that case Lakatos' and Feyerabend's approaches will not be all that different after all, no matter how many rationalist garments Lakatos may wear. Thus, it seems that no rescue awaits rationality down this avenue of thought either.

Must it be concluded, then, that Feyerabend was correct, that science cannot be both rational and successful? As often happens in philosophical controversies, both sides share some crucial assumptions. In this particular controversy it is assumed that scientific rationality depends on the individual scientist's adherence to method. The issue of rationality is thus resolved, as Feyerabend did, by looking at the behavior of scientists in the light of certain theoretical (*e.g.*, epistemological) considerations. But is such a manner of resolution correct and is such an assumption warranted? The answer to both questions is no.

Why should the question of the rationality of *science* be considered equivalent to that of the rationality of individual *scientists*? After all, the question whether a particular basketball team is good is not settled by determining whether the individual members of the team are good players. To do so would be to commit the fallacy of composition. The quality of the team is determined instead by whether the players exhibit certain relations to one another on the court. It is, in a sense, a social property. It may, perhaps, also be called an organizational or structural property. Now, science may not seem to be an organic whole, unlike a basketball team. Nonetheless science is a communal enterprise, not only for those who work as members of research teams, but also for those who would be described as lone investigators. Even the latter form part of a community that shares certain ideas and goals, though perhaps only in an overlapping manner, and who depend on others for the generation and judgment of most, if not all, of the new ideas that become part of their intellectual environment. It is in that communally generated environment that even individual genius must endure or perish (of course, survival of some ideas may lead to the transformation of the intellectual environment). In any event, the enterprise of science is very complex and, as a result, requires division of labor—no scientist can do it all alone. In this division of labor and in the relation to the aims of science, a new conception of scientific rationality may be glimpsed: a conception that treats rationality as a social or structural property of the scientific enterprise.

The intention here is to sketch a social conception of scientific rationality. Both the need and the plausibility of such a sketch arise from certain recent developments in the philosophy of science, most notably Feyerabend's work. So it is natural that the highlights of this sketch are designed to take care of Feyerabend's strongest criticisms against rationality. The intent is to show, then, not that scientific rationality *must* be conceived in social terms, but rather that it need not be concluded that the sort of case that Feyerabend developed is incompatible with the claim that science is a rational enterprise.

If science is structured in such a way, if its division of labor is such that it leads to progress (as exemplified in Feyerabend's kind of cases), then science is rational—or at least, the rationality of science is compatible with Feyerabend's position.

Science is a communal enterprise which tries to gain knowledge about the world. Science tries to give pictures of the world that allow us to make sense of it, to know what to expect of it, and to know how to deal with it. Such a communal enterprise would be rational if it developed appropriate strategies to enhance its chances of carrying out its task. Science would be rational, then, if it were organized (structured) so as to make success more easily achieved.

Now, in a universe of immense variety it would be surprising if the first ideas we ever came up with would be fruitful to explain all there is. The same point can be made about methods. New ideas, new methods will enable us to deal with new areas of the universe, or else permit us to confront changing circumstances. On the other hand, many ideas have much to offer in dealing with the world if we only give ourselves the chance to develop them. Thus, the communal enterprise which aims to gain knowledge is well advised to organize itself so as to ensure, or at least encourage, the generation of alternatives (but what is this if not the principle of proliferation?) *and* to permit people to develop their ideas even in the face of damaging evidence (but what is this if not the principle of tenacity?).

The interplay of these two principles leads to a society of researchers in which people do what they like best, in which they develop those views of nature that for any reason have caught their fancy. At the same time, the quality of work improves when strong challenge points the way in the directions that could use improvement. Such a society is appropriately organized to face the surprises, to search for the secret treasures our diverse universe has in store for an intelligent species. *But what is this society, this communal enterprise, if not science as Feyerabend described it?*

Rationality may still be the key to progress, then, but not the so-called rationality of the individual scientist, but the rationality of the scientific enterprise as a whole. It does not matter then that individual scientists either stick to their initial view, no matter what; or that they are always looking for alternatives and never stick to one point of view; or that they hold on to the most successful view of their time and look down upon all others; or that they rave against the received view, violate method, and perhaps even become historical figures thanks to that. In other words, it does not matter whether the individual scientist is dogmatic or anarchistic, as long as science itself permits the operation of the principles of proliferation and tenacity.

The point can be illustrated by drawing an analogy to Feyerabend's own political philosophy. A free society is normally envisioned as the sort of society in which different individuals are able to express different opinions and, in general, pursue the life-styles that they favor. In this Feyerabend very much followed Mill (1956). Such a society, moreover, does not cease being free because some, or even many, of its citizens are not open-minded, or because they think poorly of their fellows' ideas and life-styles. The society is free because the individuals do not interfere with their fellows' pursuits, no matter what they think of them, either because there is a tendency to live and let live, or because they fear the arm of the law if they so interfere. Freedom is a structural property that some societies have, then, Feyerabend claimed. It functions like an iron railing on the entire society (Feyerabend, 1978).

Likewise, rationality functions like an iron railing. It is a structural property that some knowledge-gathering enterprises may have. Science, as described by Feyerabend, actually has it to a large extent. So, science, to a large extent, is rational.

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