INSIDERS & OUTSIDERS IN THE SOCIOLOGY OF SCIENCE; OR, HOW CAN WE FOSTER AGNOSTICISM?

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Volumes have been written evaluating the prospects for development of social scientific methodology as rigorous as that of the natural sciences. They ask, should social scientists attempt to experience the "lived world" of the people they are studying? Or, in the pursuit of objectivity, should social scientists attempt to distance themselves as much as possible from their subjects? Can sociologists confidently expect the eventual appearance of a Newton of the social world? Or should they ignore the canon of the hard sciences and formulate new forms of measurement, deficient by the standards of natural scientific inquiry but suited to the distinctive characteristics of the social world? However interesting these

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questions may be from the standpoint of most social scientists, they are peculiarly inappropriate for those sociologists who are concerned to explain the conduct of science itself.

All methodological arguments in the social sciences are based on one tacit assumption: that scientific activity is distinct from all other forms of activity.[1] All methodological disputes can be viewed as arguments about the place that social scientific method occupies on the objective-subjective continuum.[2] Most of the debates concern the *direction* the continuum social research should take. But whether the conclusion is that more objectivity or more subjectivity is needed in social scientific inquiry, the existence of the continuum itself is never questioned.

For the student of science, the legitimacy of this continuum is questionable because the nature of the hard sciences is itself problematic.[3] Hence, the methodological literature is rendered useless. How can the sociologist of science be expected to approach his research with "scientific objectivity" when the result of his research is the demonstration that scientific "facts" are quintessentially social phenomena? Why advise him to consider the "meaning" that social action has for participants when he knows that this "meaning" is inextricable from the scientific results produced in the laboratory he is studying?[4] Why oppose the rationality of scientific inquiry to the emotion-laden activity of the "lived world" when he knows that there is no such thing as "pure" rationality? All of these methodological injunctions are premised on the belief in pure and exact knowledge. What is left when this premise is itself taken as the object of study?

What methodological alternatives can one offer?[5] To get an idea of the peculiar problems sociologists of science have when they formulate methodology for their own field, consider the following example. Everyone agrees that a scientist has to be somehow both "inside" and "outside" the object under study. To combine some degree of "insiderness" and "outsiderness" can indeed be taken as the most general methodological injunction. Even this does not hold for the sociologist of science, however. If you say to a biologist, "You cannot study a frog because you are not a frog," you will be laughed at. Similarly, the sociologist can defend himself against the businessman who says that the sociologist cannot study business because he has not participated in corporate life; the businessman's objections are explained away as defenses of his vested interests. Only when science is the object of study is the merit of the outsider's position denied. If I say to a group of physicists that (a) I do not need to be a physicist in order to study physics, (b) I ought not to be a physicist in order to study physics, (c) I should not have to believe in the rationality of the natural sciences in order to account for them in my own terms, and (d) I should not use any tool from any science even in my own analysis of physics—no doubt I would immediately be thrown into an

asylum. This is curious, in contrast to what is expected of a sociologist of religion, for example. No one denies that the sociologist of religion can be both an agnostic and a good sociologist, but a sociologist of science is not permitted to be an agnostic.

When such a fundamental inversion of the usual methodological principles occurs, we have evidently neared the reference point for all these principles. All the methodological advice points toward what one may metaphorically style the magnetic pole of Exact Science. Regardless of whether one sails to the North, East, South, or West, once one reaches the magnetic pole, all compasses go wild. This is what happens in the sociology of science, common sense no longer holds. Those who want to travel toward this pole need to find another way of orienting themselves. The solution to the problem seems to be that one can study science by being *somehow outside* science. In the first part of this article I will examine what it might mean to be outside. In the second part I will show that it is impossible to be outside science because this position requires science to have an inside. In the final part I will consider the constraints thus imposed on the sociology of science.[6]

THROUGH THE LOOKING GLASS

All methodologies must derive from this axiom: no account qualifies as an explanation *if it simply restates the account it is supposed to explain.* The difference between the two accounts is the qualification which permits us to see one account as an explanation of the other.[7] There are, of course, other important methodological principles, but this is the most important one for the purposes of this discussion. How have social scientists studying science dealt with it? Have the sciences been *explained* at all? In order to answer these questions, I will consider seriatim a variety of actual solutions to the problem of studying science, leading the reader on a seven-stage journey through the looking glass of science. We are going to be led in a direction opposite to the one that prevailing methodologies usually indicate.

1. All analyses start with the assumption that scientists are "mere practitioners" in their fields and are entirely unconscious of the meaning of their activity. Such analysis of scientific practice usually focuses not on ordinary scientists but on Great Scientists. These Great Scientists are, as leaders of their fields (not infrequently recipients of the Nobel Prize), the essential "insiders". They are so high in the scientific hierarchy that they interpret the mysteries of science not only to laymen but also to their fellow scientists. Library bookshelves are full of the thoughts, reflections, memories and opinions of these Great Scientists.[8] But though this group

is certainly the most vocal of those explaining science to the public at large, it is rare to meet a social scientist who takes its opinions at face value; it is easy to show that the Great Scientists' explanation of scientific discovery is mere tautology, mere celebration of victories in scientific battles. Hence, a second group comes into the picture: the social scientists who treat the Great Scientists as "informants" whose "explanations" have to be carefully checked by other methods coming from other sciences.

2. How does this group implement our central methodological axiom? First, these historians and sociologists want to understand scientific activities as much as possible, and have through a variety of means mastered the technical details of the fields they study, thereby transforming themselves from "outsiders" into "insiders."[9] But they do not lose their "outsider" status altogether, for they approach science from the perspectives of history and sociology, and they publish the results of their work not in scientific but in sociological or historical journals. Their account of science is thus in conformity with our central axiom and might count as an explanation. But these students of science succeed so well in achieving "insider" status that they lose their "outsider" perspective, failing to account even in the most basic of their own terms for the explanations made by their informants.[10] Certainly, they make a show of defiance toward Great Scientists, for they qualify the testimony given in individual accounts. But they do not question the *collective* assumptions of the scientific community. We might compare them to a hypothetical student of witchcraft practices, who returns from the field expecting both a Ph.D. and recognition from fellow sorcerers. No matter how hard working these students of science are, they cannot transcend their contradictory loyalties. In fact, they replace our central methodological axiom with another: science is its own justification, and only science can explain itself.[11]

3. The third group of students of science meets our standard of retaining their "outsider" status by a peculiar strategem—complete ignorance of science. One does not need to be a physicist to count the number of Ph.D.'s in physics from the earliest days of the field to the present. But this group *mimics* "insider" status by adopting what they believe to be the methods of the natural sciences, emphasizing the quantitative measures of scientific activity.[12] One subset of this group is the sociologists of science who work in the Mertonian tradition; they share the quantitative bias of this group as a whole but approach science with an agnostic attitude, treating it as if it were any other sociological phenomenon. The Mertonians have developed concepts to study science which did not originate in science itself, analyzing science in such terms as "invisible colleges," "cocitation clusters," "stratification patterns," "norms of science" and the like. Unfortunately, Mertonian methods have not pro-

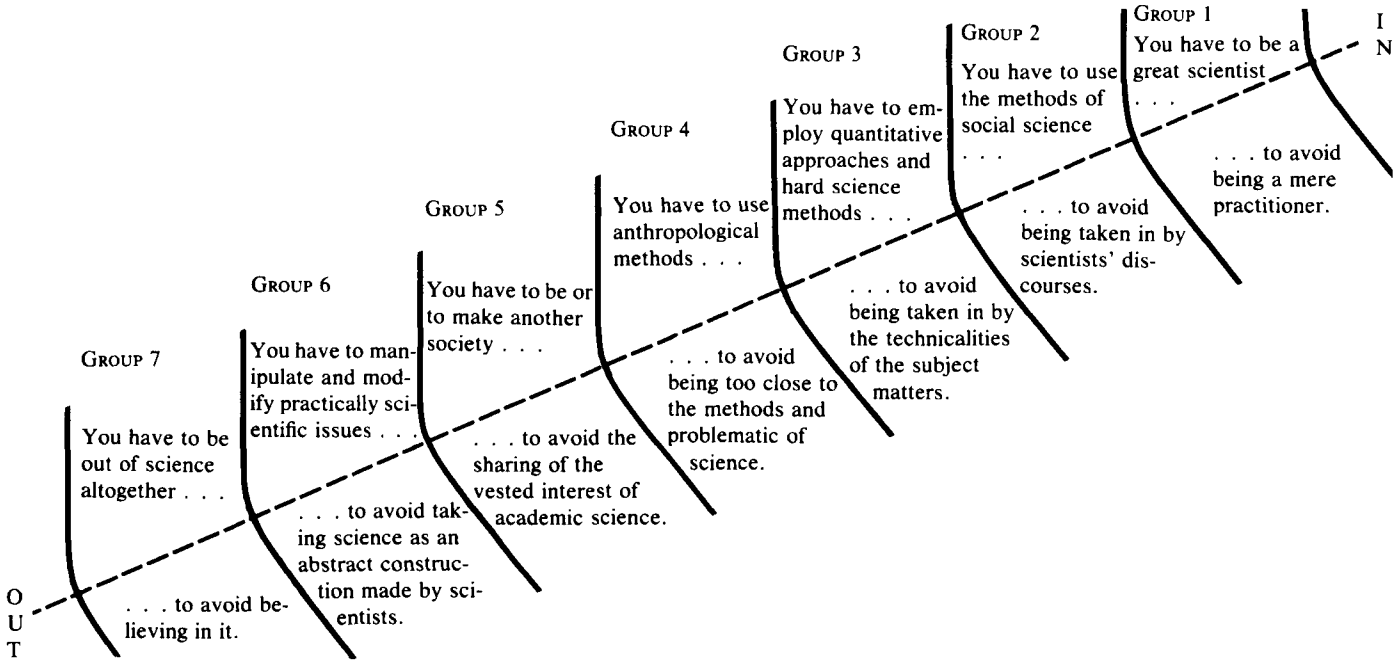
duced a true "outsiders'" analysis of science, for those who employ them suffer from complete identification with the ethos of science. At no point is this identification taken as the problem to be studied. Another problem with the Mertonian approach is that it leads to the study of *scientists* rather than science. A second subset of this group merely repeats the technical details of the field they are studying, making only minor modifications in framing. Finally, a third subset leaves the ground to scientists entirely, and repeats the scientific establishment strategy in its own attempts to establish itself.

It should now be clear to the reader how the game is played among historians and sociologists who study science. The *right* to explain can only be based on "inside" knowledge of a field, and such claims can be refuted only by demonstration that such knowledge is lacking. The result is a process of infinite regress, such as that summarized in Figure 1. Whatever position one takes, it can be negated by a subsequent claimant who bases his case on a greater degree of "outsider" status. We are now approaching the middle of the diagram, further from mere practitioners of science, but more in keeping with the central axiom that we must study science and not merely repeat it.[13]

4. A fourth group of investigators has defined its position in opposition to the previous groups. Their theoretical approach is derived from anthropological method. What do they hope to learn from anthropology? They can follow the central axiom in a new way because they attempt to realize the anthropological goal of simultaneous insiderness and outsiderness. The investigator has no desire to become a scientist, an insider, and accepts on face value not a word of his informants' claims. Whatever information the sociologist repeats must be translated into the terms of the sociologist's own explanatory scheme. All of these "taken for granted" acts become problematic: duplicating an experiment; writing a paper; drafting an article; accounting for a discovery; defining a test of an hypothesis; choosing to see something as a problem.[14] On the other hand, however, this fourth group of investigators wants to study science at much closer range than any of its predecessors. Its method is like that of an anthropologist studying an unknown tribe. It treats scientists as strangers, yet observes them in the midst of laboratory activity.[15] Viewed in these terms, all previous investigators have been simultaneously insufficiently and excessively involved with science; they neither account independently for scientific activity nor follow this activity closely.

Have we now reached the limit of the central axiom and found that science has as last been explained? There is one belief that we share with scientists—the possibility of studying science "scientifically"on which we base our disbelief in scientific prejudices. "If sociology," writes Bloor, "could not be applied in a thorough-going way to scientific knowledge it

Figure 1. A Seven-Stage Journey Through the Looking Glass of Science.



Note:

The different groups trying to define the nature of a scientific study of science are spread along an axis going from the maximum or insiderness-top right- to the maximum of outsiderness-bottom left. Each group claims to be more able to analyze science because it is more distant from it and account for its production with a more autonomous set of terms. The claim of each group is summarized above the axis, and the criticism of the other former groups is written below the axis. In the first three groups the main problem is to escape from the scientific discourse. In the three last ones, the main problem is to integrate it. The fourth group, anthropology of science, occupies a strategic position at the middle of the axis.

would mean that science could not scientifically know itself."[16] This fourth group, like other students of science, also imitates scientists, but, unlike them, it imitates scientists by adopting the critical, disrespectful and slightly iconoclastic aspect of science. The same movement that debunked religion is now leading to the demystification of science, *in the name of science*. It is not only the general program of science that we share, it is also the safety, pleasure, rewards, and prejudice of academic science. We produce a new subdiscipline—the anthropology of science—in order to achieve credibility and supercede other approaches.

5. Hence, in order to obey our central methodological axiom, we must find a form of investigation that is outside the realm of professional science. Many people, among them Marxists of some sort, claim to bring to their analysis of science the perspective of another society, even if that society is hypothetical rather than actual. No matter what the value of their work, these people are certainly methodologically sound. If you share materially the vested interests of the scientific establishment, and share intellectually the beliefs and ethos of science (no matter which ones), you lose the right to explain what science is all about; you can only *repeat* it, and add a science to the other sciences. The Marxist investigators[17] urge us to adopt the same method employed by science fiction writers; they formulate a "thought experiment" by urging us to imagine the form science would take in a society with an entirely different form of social organization.[18] Thus, we can become conscious of the degree to which science and technology are historically determined, "bourgeois" phenomena.

At this point on our regressive diagram, the problem is no longer to be outside science but to be sufficiently inside it to be able to say something about it. This new problem is as hard to overcome as the former one. The result is easy to imagine: sciences are criticized in a very general way, but nothing in particular is really said.[19] Discussion quickly enters the realm of abstraction. In the case of Marxist investigators, the problem is compounded by the claim that Marxism itself is a science! Marxists denounce academic and bourgeois science, but themselves have the most crude sort of "scientific" pretensions. These contradictions are not easily overcome. Hence, we must now consider a sixth sort of approach to the study of science.

6. For many people, investigating the nature of science requires not academic research (even of a Marxist variety) but active intervention of some sort. From material modification we learn the fundamental processes of science. These activists are evident outsiders; their notion of criticism of any scientific activity—recombinant DNA research, creation of alternative technology, work with nuclear power—is political pressure. The fact that scientific work can be altered by political pressure consti-

tutes experimental proof that any ostensibly "purely" scientific or technical issue is fraught with social factors. What is soft? What is hard? What is logical? What is political? What is scientific? All these questions can only be solved *experimentally*. One such experiment involved the Cambridge City Council in the recombinant DNA controversy. It can be shown that each of the council members underwent an intellectual transformation as the result of the controversy, becoming aware, for example, that conflicting judgments about the necessary levels of safety precautions are functions of the diverse interests of the parties to such disputes. These activists are much more efficient than the anthropologists of science; they provide the only experimental basis for a science of science. They bring into the sacred ground of science a disreputable crowd of outsiders, overcoming each successive setback with a renewed invasion of mixed protestors—politicians, businessmen, lawyers, and even some respected scientists. In consequence, though they add nothing to the body of scholarly literature on the culture of science, they legally or politically modify the flow or outcome of a given scientific controversy.[20]

This last group of investigators demonstrates that the boundary between scientific outsiders and insiders is very vague, subject to the fluctuations of social debate. When this boundary becomes indeterminate, it is difficult to extend application of the central axiom. There is one further way to appreciate science from the outside: to imagine someone who could account for elementary concepts like "observing," "explaining," "studying," or "recording" in entirely new terms. We are so imbued with the notion of the scientific vision that we can scarcely conceive of a person who could look clearly *at* this vision. But we can imagine a hypothetical investigator capable of such skepticism, someone such as Ialo Barassowah, a hunter from the nation of Youme in the Ivory Coast. What if Ialo, not I, a European social scientist to whom scientific activity seems natural, spent two years in a biology laboratory?[21] Such a situation represents the maximum possible distance between the inside and outside of the natural sciences and then also represents the best possibility for really explaining science. If we cannot stage such a situation experimentally, we can approximate it through literature. The most famous character with whom we can identify is Ulrich in Musil's *The Man Without Qualities*. Ulrich has an intimate knowledge of several sciences (mathematics and engineering), but he strives to distance himself from them, so that he never *repeats* them.

This is the end of our seven-stage journey through the study of science. If we look at the progression that led us from the practitioners to the pure outsiders, we see that our reflexive approach to science has dictated an approach antithetical to the usual methodological prescription. Generally, one is enjoined to get *closer* to science. Here, objective treatment of

science requires one to draw away from it. In most sciences (natural and social), the problems of method are spread along a continuum linking the maximum of objectivity to the maximum of subjectivity. Whether a given scientist wants to go upward or downward along this continuum, he will not cut across the regressive line that forms when the same general principle is applied to the study of science. The two lines—one leading toward science, the other away from it—are obtained by applying the *same* principle, and do not reach the same point; at the bottom of the first line there is a state of pure unconscious subjectivity; at the end of the progression away from science, there is a maximum of awareness and objectivity. Our conclusion is paradoxical: if the main problem in many fields of science is how to get *in* (the data, the field, the "meaning"), in the study of science the main problem is how to get *out*; it seems as if there is no outer space where one can go to forge an explanation of it. The paradox is inescapable: if the reader takes science seriously, he has first to go to science. But once there he *has to* go away from it, in order to account for it. If, like most, he stops midway, this is the best proof that science is not taken seriously since it cannot explain itself.

"BEWARE OF PURITY, IT IS THE VITRIOL OF THE SOUL"[22]

The progression in the foregoing analysis was made possible by one assumption that I want now to question. Why do I want to question it? Because it still arises from science and then precludes any analysis of science if it is not challenged. The central axiom states that no account is taken as an explanation if it simply repeats what is supposed to be explained. I did not question this axiom since there was no alternative to it. I did not use it as it stands, however. I used it as an interpretation of this axiom which goes as follows: the first account and the second are not proposed or uttered *by the same person.* All along I supposed that there were mere practitioners and others (called scientists) explaining what the mere practitioners were doing. It is this hidden assumption that I want now to question. Ethnomethodologists have undertaken to demonstrate that there is no qualitative difference between observations made by a "competent member" and a sociologist. The same is true, I think of a scientist and a sociologist, as I will show with illustrations from my own work.

With Steve Woolgar[23] I showed that after a few years of work with biologists the sociologist can no longer distinguish between his insider and outsider roles. He cannot decide if the biologists are like him, or if whatever differences obtain between him and them are of degree or of kind. This occurs less because of the familiar process of assimilation of

the observer into the group he is observing than because the difference between "practice" and "explanation of this practice" is largely artificial. The sociologist explains what his informants are doing. But his informants do that as well. In fact, his informants do their own autosociology, and they do so *in order to go about their work*. When a project interests a given scientist, he must justify his interest. How does he do so? Contrary to our expectations, he does so with reference to social and personal factors. The following quotation from a conversation between two scientists illustrates this point.

> But they don't know their business. It may be that they see progesterone which has been known for years to be an analgesic. . . . Also there is a flag in all that. The English have discovered that, they push it. That's normal.

In one single utterance, the statement in dispute is rephrased by the use of four "extrascientific" allusions: to the incompetence of one group of experimenters; to their ignorance; to their nationality; and to the norms of science—in this case the counter-norms. It is not possible here to discriminate between a "pure" scientific sentence, and an explanation of it provided by the sociologist. Insiders are constantly using "outside" concepts and tools to account for any fact in construction. The central axiom is applied, but the same person or group provides the explananda and the ad explanandum. In this process the sociologist is not really an outsider: he feels very much at home in a laboratory because he does the same thing.[24]

The pervasive presence of autosociology is striking even in a "harder" science. I showed elsewhere[25] that a peptide chemist wishing to modify the chemical nature of a given molecule could not rely on a logical process. He had to use many heterogeneous "accounts" of his former moves in order to decide the next modification. (The accounts included: colleagues' strategies; evaluation of the reliability of chemical factories; various interpretations of the same bit of chemical knowledge; chances and outcomes of the "black art" of chemistry; reception of previous articles; reactions of patent lawyers; availability of supplies, and so on.) There is no difference between the sociologist's reconstruction of the research process and the actual negotiation of this process by the scientist; the latter does not fit the stereotypes of either the relentlessly logical or entirely unselfconscious researcher. Indeed, the chemist had to practice autosociology, constantly reconstructing his prior activity, in order to go on with his research. He had to synthesize a mass of contradictory accounts of his and his colleagues' behavior—rationalizations, pure lies, systematic classifications, literary devices, and some logical rules. How, for example, is the chemist to assimilate a series of papers which may all

identify an identical change in a molecule but in each instance follow a different sequence of reasoning? The difference between the inside and outside of science disappears when one looks empirically at the research process: the inside of science is full of outside factors. So the central axiom of this paper is applied by the scientists themselves, not because they are more competent sociologists than the sociologist, but because the sociologist of science is often, as we say in French, "plus royalistes que le roi."[26]

What is clear in a hard field is even more evident in a soft science. Invited by a group of primatologists to observe their meeting, I was caught in a very strange situation. I had written a preliminary paper, "Observing Scientists Observing Baboons Observing. . . ."[27] This paper infuriated two primatologists who did not wish to be observed, and I was persuaded to sign a waiver that I would not "observe" the meeting. Interestingly, though, the conference members were also observing their colleagues intensely, doing just what I would have done. They were observing each other's reactions and taking notes on the attitudes and positions they held. Moreover, they were "explaining" these attitudes and positions by referring them to "social factors": nationality, gender, career history, ideology. The concept of "dominance" that a zoologist used was, for example, criticized by others on the basis that the zoologist was male, American, a student of De Vore (a major exponent of the theory of dominance hierarchies in primate studies), that he used field notes written ad libitum, and that he followed his baboons and observed them from the top of a Land Rover. An older woman, working in an anthropology department in England, saw the concept of dominance quite differently, but it was noted that she had filled in observation sheets, had a background in the humanities, had followed her baboons on foot, and supported the Labour Party. The primatologists discussed psychoanalysis, cultural history, microsociology and philosophy, not in order to make small talk but in order to define their work and to probe their preferences for theoretical approaches and data selection. The most ironic scene occurred when the gentleman who had tried to exclude me from the meeting asked all of us to fill in a sociological questionnaire in order to check if the positions we had taken could be related to our educational background. The amount of autosociology was so astounding that, without breaking my pledge to not observe, I could gather excellent sociological data on the field, simply by writing down what they said about themselves.

All these examples show the erroneous character of the hidden assumption. You cannot study science from the outside because this would be admitting that science has an inside. Conversely, it is quite easy to apply the central axiom in every field of science, because the distance required

by the axiom is always there: scientists themselves are at *some distance* from their own field so they can *explain* science in different terms. A field is a heterogeneous world that has to be observed from the outside only in the sociologist's or the textbook writer's imagination. To get into it and to apply the central axiom with the help of the many scientists engaged in the same work is not much of a problem. It is only by granting at the beginning that there are internal and external boundaries between scientists and nonscientists, scientific assertions and "indexical" assertions that the study of science is forestalled.[28]

The hidden assumption is not limited to science. It is a general prejudice, a belief in boundaries. On one side there are objects of study, on the other there are people studying these objects. This belief in boundaries is essential to the study project. But in order to study scientific practice we must take the *drawing of these boundaries* as the object of our study. I do not mean to suggest that there are no boundaries, no differences between hard and soft sciences, experts and laymen, scientific and unscientific procedures, laboratory rats and people studying them, ethnographers and tribesmen. I am saying simply that the construction of these dichotomous categories is not unproblematical, and that this must be recognized if science is to be really *explained.* My point is best illustrated with the hypothetical case of Ialo from the Ivory Coast. What could be the result of allowing this preliterate hunter to observe Western science for the first time? We cannot assume that science would be completely inaccessible, for that would be granting *too much* to science. No mind is altogether unscientific, and the difference between science seen from inside and outside is not that great. Scientists do too much autosociology to persuade us of the existence of an inside, and conversely, every outsider can get into science as easily as scientists can get out.

Once the imposition of boundaries is brought into focus, the violence of the operation appears in full light. Was the reader conscious of the use I made of "small" words like "pure," "mere", "absolutely pure," "automatic," "irreflexive," "unconscious"? I imitated what occurs so often in science texts.[29] In the expressions "pure science," "purely logical" and so on, the crucial features are not the words "science" and "logical," but the small word "pure." I showed *per absurdum* where a pure study of science would lead (outside of anything we believe). I can now show why. It is the belief in purity that imposed the hidden interpretation of the central axiom: on one side of the line pure objects, on the other pure minds, with no contact between them. One has the duty to explain and the other must be explained. If we are to be agnostic toward science we must give up even this last belief, this religious respect for purity.[30] At the end of the first section of this paper I proposed a paradoxical strategy: if we take science seriously we must both move *toward* and *away* from it. Short

of this, science is a belief that cannot be accounted for. Now this position seems untenable, for if we take science seriously we cannot analyze it from the outside because it has no inside, no boundary that can be taken for granted. Short of that, science is a belief that cannot be explained.

A CHEAP AND IMPURE RESEARCH STRATEGY

We saw the peculiar methodological position of someone who intends to study science. We saw how the usual navigation instruments fail to tell us in which direction to go. And finally we decided that this confusion of all limits between inside and outside, science and nonscience, object and subject, is essential to our analysis and should not be eliminated. We now have to concoct a "provisional moral"—as Descartes would say—to get by in our study of science without losing our way and without finding our way back to the usual path (which as we now know leads us to believe or to repeat Science instead of explaining it). This provisional moral has three components: a stylistic one, an ethical one, and an economic one.

Continental semiotics and British ethnomethodology have approached the study of science from a linguistic or even a stylistic point of view.[31] In these approaches the difference between scientific styles appears as a difference in the literary *genre* that is used. The genre is made of a corpus of literary devices (or linguistic keys and frames) which can be empirically studied. All these devices can also be deconstructed by a careful use of other genres. It is no coincidence that many of the most fruitful insights into the workings of science have been made by people whose style is completely at variance with the usual scientific mode, people such as Paul Feyerabend or Michel Serres. Most studies of science are, however, measured by the degree to which they approximate scientific styles of scholarship, rather than by their *distance* from scientific style. I recommend, instead, that the student of science do some literary research, so as to become familiar with the stylistic tricks employed by scientists.[32] By drawing on these two sources (fiction and science) the social scientist will soon realize that there is in fact only one large literary genre: that of science fiction (the best part of which is *not* written by science fiction writers).

Sociologists of science are beginning to put together a picture of what a science is. It is made of three main elements: an inscription device of some sort (questionnaire, field notes, bioassay, mass spectrometer and so on), a body of scriptures and an agonistic field of some sort.[33] Through the use of inscription devices the scientist might be able to modify the status of an assertion inside the body of scriptures (its modality), if he is able to win in an agonistic encounter. The agonistic field is made of people like him who stop being interested in the use-value of this assertion, and

any other, and become exclusively interested in their exchange value; this value is defined in the market of the field only as far as it allows other scientists to accelerate their own cycle of production. Needless to say, this "capitalist economics of truth" as Michel Foucault says, bears no relation whatsoever to truth. Truth effect and reality construction are only the consequences of successful moves inside (or outside) the agonistic field to stabilize some controversies. Now, here is the ethical requirement. Knowing what a science is made of, we should not want to develop one. Instead of fighting for more chairs, instead of excluding scientists and laymen alike from the study of science by drawing boundaries, instead of enforcing stricter rules of access to the field, instead of creeping inside ministries and corporate rooms to advertise our trade and extend the domain of application of our market words and concepts—instead, in a word, of imitating the people we should study, we have to do everything to make clear that we do not want and do not intend to be scientists. The constraints that we put on agnosticism are inescapable. It would be unethical for a student of science to ask for the garment, status and role of a scientist.

A study of science is not economically feasible. It takes years to show that a scientific fact fabricated by a group of biologists has been socially constructed.[34] To study science and technology would require as many social scientists as there are scientists. Not only would this be absurdly expensive, it would only result in duplicating the science in a similar amount of science of science and so on ad infinitum (and ad nauseum). A cheaper strategy is possible if we act on the implications of the second section of this essay. Science has no well-defined inside. Scientists are themselves fighting to define the boundaries of their fields and to exclude or include social determinants. Disciplines, especially the younger, softer, more applied and more controversial ones, are heterogeneous and full of gaps and loopholes in which the sociologist can easily find his way. Many scientific issues are already under attack by many outsiders (see the discussion of the sixth group in the first section), and each social and political controversy in which science figures represents a cheap and convenient experiment from which the student of science can learn. The student of science must recognize that he has allies whom he has not exploited effectively, either because he despises them or because he admires them to excess. I recognize that an alliance between Great Scientists, well-connected social scientists and high-ranking administrators, would not be a very illuminating one, but all sorts of other alliances are imaginable, given that the territory which science occupies is so controversial. So long as a scientific discipline is still in the process of formation, it is possible for the student of science to apply the central axiom with little difficulty. Once a discipline is solidified, it becomes

increasingly costly and difficult to show that the discipline itself, and the facts on which it rests, must be viewed as socially constructed. Thus, the third injunction of my small provisional moral is that the student of science must enroll as many allies as possible, so as to decrease the cost of explaining a given scientific issue.[35]

One final example should illuminate the extent of the problem. After the primatology meeting I discussed earlier I was appalled to see that the published reports omitted all the debates and the autosociology which had provided the dynamic behind the proceedings. The participants had chosen to imitate the hard sciences, formally ignoring all material problems and social controversies. Because the primatologists have adopted this strategy, the cost to the student of science who would persuade the reader that primatology is a social construction is very high. If, on the other hand, my primatologist friends had chosen to present a reflexive, soft and subtle understanding of baboon watching, it would be easy to show that my "explanation" of primatology is in fact made by many scientists themselves. But because the primatologists have chosen to present an image of their discipline which is in keeping with the stereotype people have of science, the student of science has a much harder time to expose the social production of facts. *They* made the barriers between science and non-science very high, although they were almost ready to accept that they were *not* different from non-scientists.

How then should future students of science proceed? Should the study of science be cheap, impure, heterodox, unscientific? Should it be linked with the constant struggle between scientists and nonscientists to forge some scientific assertions? Should it strive to abrogate the usual boundaries between science and nonscience? Should it repudiate scientific style and the strategy of the scientific establishment? If sociologists answer these questions in the affirmative they will be departing from their usual methodological ideals. Strange though it may seem, however, they will be following the only possible course to take if they wish to take science *seriously,* to reveal what it is made of and to be truly agonistic.

NOTES

1. So many counterexamples could be found that I do not want to consider them. As will become clear, I am not seeking parity with sophisticated methodological analysis in social science. Only the peculiar situation of the sociology of science justifies treating these issues so crudely.

2. The defence of subjectivity has been made by writers like Cicourel or Goffman. P. Winch's seminal *The Idea of a Social Science* (London: Routledge and Kegan Paul, 1958) is premised on the opposition of the social and natural science, and the subjective character of the former.

3. Bruno Latour and Steve Woolgar, *Laboratory Life* (London; Sage, 1979).

4. K. Knorr, "Producing and Reproducing Knowledge: Descriptive or Constructive?" *Social Science Information* 16 (1978): 669–96; B. Barnes and J. Law, "Whatever Should Be Done with Indexical Expressions?" *Theory and Society* 3(1976): 223–37.

5. One cannot speak of method without credentials in empirical work. This excursion in methodology is based upon: a one-year study of black engineers in the Ivory Coast; a two-year study of a biology laboratory in California; a one-year study of primatologists; and a current three-year work on Pasteur.

6. "Science" is an absurdly general term that has no strict meaning. I use it in this article because by using the word the belief in science is reinforced; an enchanted circle is drawn around any enterprise designated as "science". Since I intend to generate disenchantment, I am not bothered by the magical quality of this general term.

7. "Repetition" should be understood in a narrow sense. If I record with great care a myth that is told to me, I am not repeating it; rather I am putting it in a new framework (field study, sheaf of papers); this is enough to fulfill the requirement of the axiom even if I have no theory or grand explanation of this myth. Also this article does not deal with the further requirement that an explanation be good; it starts only from the minimum required for a study to take place.

8. Many names can be proposed to illustrate each step we are going to follow: Whitehead, Einstein, Medawar, Monod and Jacob are good examples.

9. The exemplary works are D. Edge and M. Mulkay, *Astronomy Transformed* (New York: Wiley Interscience, 1976); and G. Lemaine, ed., *Stratégies et Choix dans la Recherche* (The Hague: Mouton, 1977), at least in the narrow domain of sociology of science.

10. For a more complete discussion see Latour and Woolgar (1979) and the works cited in note 14.

11. It is in France that this new axiom has received its greatest extension. People like Bachelard, Canguilhem and Althusser have so worshipped science that no history—or at least no social history—of science can be developed. A general history of thought or of the institution of science is *added* to the grandiose unfolding of "scientificity." The idea of "explaining" science is made to seem blasphemous.

12. Quotology has mainly developed around the Science Citation Index invented by E. Garfield; the Indicators have been especially developed by the National Science Foundation and the work of J. J. Salomon at the OECD in Paris. The quantitative approach is best represented by D. de Solla Price. The semiquantitative approach is well represented by Merton, Hagstrom, Cole or Ben-David. For a bibliography see I. Spiegel-Rösing and D. de Solla Price eds., *Science, Technology and Society: A Cross-Disciplinary Perspective* (London: Sage, 1977).

13. D. Bloor, *Knowledge and Social Imagery* (London: Routledge and Kegan Paul, 1976).

14. See H. Collins, "The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of an Experiment in Physics," *Sociology* 9 (1975): 205–24; B. Latour and P. Fabbri, "Pouvoir et Devoir dans un Article de Science Exacte," *Actes de la Recherche en Science Sociale* 13 (1977: 81–95); K. Knorr, "From Scenes to Scripts" (forthcoming); Woolgar, S., "Writing an Intellectual History of Scientific Development, *Social Studies of Science* 6(1976): 395–422; Pinch, T., "Theoreticians and the Production of Experimental Anomalies: The Case of the Solar Neutrino", Callon, M., "De Problemes en Problemes: Itineraire d'un Laboratoire" in *Sociology of the Sciences: A Yearbook. The Research Process* K. Knorr, R. Krohn and R. Withley (eds.), 1980.

15. The first long-term laboratory study was performed by myself, but several studies have since been made by other social scientists. The field study of laboratories is not a panacea but it has a unique virtue—it allows us to check what scientists say they do by observing what they do.

16. Bloor, 1976: 40.

17. In England the most representative are grouped around the *Radical Science Journal* and the work of Bob Young. In Italy, it is the work of A. Cicotti. In France, the groups gathered around Impascience and J. M. Lévy-Leblond.

18. I use this word not to destroy the illusion that the actual experiments have taken place, but only because it is true that all sciences are part of science fiction. A nice example of this is provided in the book published by Science for the People: *China Science Walks on Two Legs* (New York: Locust Books, 1974), which is exactly like a good science-fiction novel by, say, LeGuin.

19. Except for Bernal, Marxist scholars have not been very interested in science. This is not only because they are not interested in particular issues, but because they are excluded from the places where science is done. Once again, ignorance of the working details of science has been most extreme (see Althusser, for instance).

20. It would be wrong to confine this label to militant groups such as Nader's Raiders or Science for the People. The most important group is the *administrators* of science. At the U.S. National Science Foundation or at the French Centre Nationale de la Recherche Scientifique the influence of high executives in directing financial support and scientific priorities enormously outweighs that of leftist groups. Their objectives are similar, however; they want to manipulate and interpret science without permitting scientists to do so.

21. For a more elegant, tentative formulation, see Lamarosse, "Le pygmé et la licorme "d'ascèse," *Art Press International,* (Summer 1979), special science issue. I do not mean all who are outside science, but the very rare case of people who are outside of science and looking in at a very intimate and detailed part of the production of knowledge.

22. This is what the captain tells Robinson Crusoe just before the shipwreck. I allude, of course, not to Defoe's crude character, but to M. Tournier's wonderful *Vendredi, ou les Limbes du Pacifique,* (Paris: Gallimard, 1967).

23. Latour and Woolgar (1979).

24. The easiest way to present the issue is to the following: either you have a science, then a science of science, then the third-degree review of the science of science, and so on *ad infinitum;* or you have the science of science as a subset of science, science itself taking only a subset of everyday practice. The consequences are clear: in the first approach, reflexivity and consciousness gain a right to exist; in the second, reflexivity and consciousness are only subsets of practices. These are the consequences that people want to avoid by using the hidden interpretation of the central axiom.

25. B. Latour, "Is It Possible to Reconstruct the Research Process: Sociology of a Brain Peptide," *Sociology of the Sciences: A Yearbook* (1980).

26. The belief in science might be the invention of epistemologists, philosophers, and now the sociologists of science—that is, people whose method is patently unscientific but who worship science and scientists. Many of these points are *obvious* to a scientist but quite absurd to a sociologist of science or an epistemologist.

27. B. Latour, "Baboon Field Studies: Myths and Models," Wenner Grenn Symposium, New York, June 1978. All the following references are to this meeting or to the papers (unpublished).

28. This is not the resuscitation of the prejudice of the first groups (Part 1); I do not say that only scientists can explain science because oausiders are incompetent; I say that many times it is the outsider's belief that makes science unexplainable and that "walking into a laboratory" is enough to demonstrate that there is *no* inside because scientists are as much outside as outsiders are.

29. I refer of course to Kant's use of the word "pure" but also to the less conspicuous antinomies—hard/soft, rigorous/fuzzy, strong/weak, perfect/imperfect—that are employed to differentiate science from nonscience.

30. See D. Bloor, "Polyhedra and the Abominations of Leviticus," *British Journal for the History of Science* 11(1978: 245–72), in which Mary Douglas's anthropological classifications are applied to mathematical disputes. The most stimulating insights are Nietzsche's aphorisms in *Die fröhliche Wissenschaft*, especially No. 344: "Why are we still so devout?"

31. A review of the semiotic literature can be found in A. J. Griemas, Semiotique et science sociale, Paris: (Seuil, 1976); the unpublished work of Francoise Bastide, (Paris, CNRS), is crucial for issues of scientific "genre."

32. Latour and Fabbri, op. cit.

33. The term "scriptures" is borrowed from Knorr (1978); for a description see Latour and Woolgar (1979).

34. Steve Woolgar spent four years reconstructing the discovery of pulsars; Michel Callon spent two years following the social negotiations surrounding the choice of the problems in fuel-cell research in the 1950s; Francoise Bastide has already worked three years on a handful of articles by Claudè Bernard. Even if there are, as Harry Collins argues, strategic points to study, the size of the task is beyond the reach of a few social scientists.

35. Their most useful allies must be intellectual hybrids—alienated, marginal men of research who make ideal double agents: anthropologists who have turned to science; physicians who have turned to history; militants who have turned to epistemology; consumerists turned to the social history of technology; engineers who have turned to the philosophy of science, and so on. With their help, we will find so many gaps in the boundaries of science that the distinction between science and nonscience will finally be obliterated.

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