SOCIOLOGICAL STUDIES AND PHILOSOPHICAL STUDIES: TWENTY YEARS OF CONTROVERSY[1]

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Pickering (1992, p. 1) speaks of the field now commonly called *sociology of scientific knowledge* (SSK) as "a new approach to thinking about science" that differentiated itself from contemporary philosophy and sociology of science in two ways: first, in that "scientific knowledge itself had to be understood as a social product"; and second, in that "SSK was determinedly empirical and naturalistic." [2] In this paper I intend to explore the nature of the divergences between SSK and philosophy of science. This exploration intends to illuminate the relationship between the two, a relationship that, from the beginning, has been marked by an open and bitter controversy which has ignited more fire than illumination of key points.

I will begin with a summary of the history of the relationship between the two. I intend to show that, during its development, there were significant changes in both fields; I will also display the internal development of each field, as well as—although to a lesser degree—the relationship itself. In the second part of the paper I will enumerate some of what seem to be the key points that differentiate philosophical from sociological studies. As we will see, the basic ideas of the sociological program appear, implicitly or explicitly, to be violating ideas that have traditionally governed the philosophical approach to science. This is precisely what would explain—and make reasonable—the strong resistance with which SSK has been met since the beginning. Yet the changes that have occurred meanwhile in the philosophy of science itself—especially those which have led to what is known as *naturalization*, have challenged the feasibility of some of those old basic ideas. It is thanks to these last changes, I will claim in the conclusion, that SSK might be able to find a "natural" place in the empirical and interdisciplinary study of science that philosophy itself has promoted under the name of naturalization .

THE HISTORY OF THE RELATIONSHIP [3]

The Decade of the '70s

Bloor—one of the founders of SSK—begins his (1991) with a question: "Can the sociology of knowledge investigate and explain the very content and nature of scientific knowledge?" The question has the merit of beginning at the very beginning; it has to do with the novelty—and, in this case, with the feasibility—of attempting to give sociology a role in epistemology—a field traditionally located in the philosophical province. Bloor presents his book as an argument to convince even hesitant, stubborn, critical, or skeptical people. But the interesting thing is that Bloor does not direct the question, nor address his argument, to philosophers but to sociologists.[4] These very sociologists had refrained from including the analysis of knowledge in their field of work.

But if Bloor did not address his argument to philosophers in the first place, their answer was as rapid as it was blunt: in absence of a plausible program of sociology of knowledge, and of empirical literature on which to base it, philosophers would reject the "sociological temptation" and refrain from searching for the roots of rational explanation of human beliefs in sociological soil. The "intellectual historian" ought to devote his attention to a "rational historiography of ideas" because of its greater success ratio compared to cognitive sociology.[5] This negative answer to Bloor's question—global rejection—is the initial philosophical position towards SSK. And this answer is the explicit target of Shapin's (1982) well known paper, where he responds in a similar blunt fashion to at least a part of the objection, the supposed lack of empirical literature. But, at the same time, paraphrasing in the title of his paper the title of Lakatos's (1971) influential paper, Shapin addresses himself to the very source of the philosophical reaction. The importance of Lakatos's paper resides, in the first place, in its decisive support for the demand by Kuhn of "a role for history" in science studies; this support will be, in turn, a decisive step in the process of the naturalization of philosophy. But the influence of Lakatos's paper in the later development of philosophy of science has another side; with the same firmness with which he opens the door to history, he closes it to sociology, transferring the radical dividing line between *descriptive and normative* to one between *internalism and externalism*. If Shapin, in turn, hopes to produce some impact by paraphrasing the title of Lakatos's paper, it is, so to speak, to reopen a door that Lakatos had closed. He hopes to attain for sociology what Lakatos had for history.

The '80s

This paper by Shapin marks the beginning of a decade of changes and novelties in both SSK and philosophy of science. Concerning SSK, at the beginning of the decade, well known anthologies [6] appeared that, as Giere (1988) claims, constitute "a sure sign of a movement's reaching maturity (or at least reaching *for* maturity").[7] Throughout the decade, several important books [8] appeared that show, in the words of Bloor (1991), that "the main proof of the *possibility* of the sociology of knowledge is its *actuality*."[9] All of these works, including the anthologies, show the broad diversity and divergences that had characterized this movement since its beginning regarding both the theoretical claims and analytical tools, and the empirical fields of exploration. Although this diversity makes it difficult to speak of SSK as a single movement or program, its protagonists have seen it as a positive sign, and have worked as if they were members of a movement whose convergences were more important than the divergences. [10]

Regarding philosophy of science—with its even more obvious diversification—the change that I want to emphasize here has to do with what is now call its *naturalization*.[11] Yet although naturalization is a clear bet for working empirically and interdisciplinarily in science studies—and, therefore, an arena where one could hope that there exists a place for sociology—philosophers' reaction against SSK remains the same as at the beginning. This is no longer expressed as an explicit rejection; it is, rather, a matter of completely ignoring SSK. In the philosophical literature of the '80s, there exist no references to SSK; [12] if anything, we hear only the distant sound of the controversy *rationality versus relativism*.

The '90s

At the beginning of the present decade, one can appreciate a few significant changes. In the field of SSK there is a change from the initial defensive attitude to the security that comes from the fruitful works of the preceding decade. Thus Bloor (1991), for example, says: "The cumulative effect of these [works] has been to alter the terms of the debate. It has tipped it in favor of the strong program"; however, he admits immediately: "This is despite inevitable and healthy differences of opinion as well as many unsolved problems." [13] Barnes (1990), for his part, claims that the sociological approach to knowledge has inspired, and is confirmed by, an important number of empirical works. The remaining resistance and misunderstanding, he says, is due to a "long-established notion of evaluation"; yet, he adds, "it is as the plausibility of this individualistic rationalist account of evaluation in science has declined that the alternative sociological conception has come to be taken seriously."[14]

Other important news in the sociological field has to do with the nature and range of internal divergences. As I have said before, since the beginning there has been an explicit acknowledgment of the existence of such divergences—although also an explicit agreement about the priority of convergences over divergences. It is the consensus around this agreement that has changed recently. What are these divergences? For Barnes (1990) the main internal divergence in SSK resides among those—like himself in the Strong Program—who look for a general explanatory theory, and those who, on the other hand, claim that sociological analysis must consist of—and limit itself to—a descriptive task. [15] For Woolgar, instead, there exists a general agreement among sociologists in their interest in the sociological analysis of the technical *content* of knowledge; the main difference consists of spelling out "what exactly such ‘contents’ might be and how they should best be studied." A sociohistorical explanatory argument based on *interests,* he says, is "undermined by incongruent causal and realist assumptions." [16]

Latour's case is somewhat different. He began working independently of SSK, joined it later, but now seems to be moving away from it. He—or what is now called the Paris School—sees SSK as undermined by traditional conceptual dichotomies such as subject/object, Nature/Society—precisely the ones which the current authority of natural scientists relies on. [17]

Pickering's case is also different; beginning as a militant protagonist of SSK, this author now complains that SSK—as its very name suggests—promotes a view of science studies that gives priority to its conceptual product, i.e., knowledge. In so doing, he says, it has given a weak, idealized, and reductive view of science, that, for this very reason, turns out to be useless "to catch up the richness of the doing of science," namely: "the dense work of building instruments, planning, running, and interpreting experiments, elaborating theory, negotiating with laboratory managements, journals, grant-giving agencies, and so on." Pickering, then, now challenges that the analysis of science-as-knowledge of traditional SSK could be useful for the understanding of science-as-practice; this last, he claims—since it requires a new set of principles, analytical tools, and work agenda—makes a separation from the originary root necessary. [18]

What about the impact of the '80s works of SSK on philosophy of science? Indeed, it has not been very great. Yet it is quite interesting to note in recent philosophical literature increasing references to SSK, even if only to mark distance. Far from the initial global disqualification and/or ignorance, some philosophers have made SSK in general—or some of its subprograms in particular—the main target of their arguments. So, for example, Earman (1992) gives it the status of "the most elusive and insidious opponent." In the same book, Boyd claims that, "The conception of science as a matter of social construction is worthy of serious elaboration and criticism." Niiniluoto (1991), for his part, sees SSK as a branch of naturalism, different from the philosophical one in that for the former "scientific beliefs have no special relationship with reason, truth or reality"; he acknowledges the interest of SSK's case studies, but considers them insufficient to justify its "radical conclusions." Kitcher's (1991) view seems more neutral, where he merely distinguishes a propositional model of knowledge—in which it appears as a set of well founded true beliefs, and knowledge as a property of individuals—from the view of knowledge as a social realization. Another reference of this last author, showing the current impact of SSK on the philosophical community, is from his (1993). Here, in the context of acknowledging that there could be, in science studies, other interesting issues besides the cognitive one, this author says: "Philosophers have ignored the social context of science; the point, however, is to change it." Although Kitcher's sentences clearly imply his rejection of SSK—whose concern is precisely with the cognitive issue in which it does not separate knowledge from its social context—it is very significant that a reference to social context as an issue that concerns philosophers even appears. Never—before the existence of SSK—had philosophers acknowledged that this *also* could be a philosophical issue.[19]

SOME BASIC IDEAS IN THE PROGRAM OF SSK

My strategy to cast some light on the controversy between philosophers and sociologists consists of reconstructing some of the basic ideas stressed by SSK, with the aim of comparing both metascientific programs: the sociological and the philosophical. I intend to show that a major point of confrontation, in the first place, has to do, not with what they say about science, but rather with what they say about the proper way to study science: the aim of science studies; what to prioritize to reach this goal; what path to follow, what to avoid; what its analytical tools are and what its working agenda is.

Sociological Naturalization

As Giere (1988) acknowledges, SSK presents itself from the beginning, not only implicitly but militantly, as a *naturalistic* program.[20] So Bloor (1991) claims: "The sociologist is concerned with knowledge, including scientific knowledge, purely as a natural phenomenon." His concern must be, then, the identification of the regularities and general principles that govern the field of cognitive phenomena, and, consequently with his naturalistic commitment, he will use the same causal language used in the research of other fields of natural phenomena. [21] This is, precisely, the content of the first principle of Bloor's program: the principle of *causality*.[22] Since causal accounts of knowledge have achieved philosophical status, thanks to the naturalization of epistemology, the interesting point here is to identify what differentiates sociological from philosophical naturalization. Thanks to the latter, epistemology "without a knowing subject" became the epistemology whose philosophical thesis relies on the sciences whose subject matter is precisely the cognitive subject, namely, cognitive psychology and evolutionary biology.

Now, with SSK, sociology wants to enter the scene. It would incorporate the study of the "social" dimension of knowledge. Yet the problem of understanding what, precisely, its contribution would be resides in the ambiguity of the term "social."[23] A first clarification comes from regarding it as referring to the fact that the unity of knowledge making—i.e., its genesis, evaluation, transmission and change—is not the work of the isolated individual, but a collective activity. *Science* would be, then, an enterprise—and *knowing* an activity—collective by nature. So Bloor (1991) asks: "How much of man's knowledge, and how much of his science is built up by the individual relying simply on the interaction of the world with his animal capacities?" In his view: "Probably very little." [24]

But this view is not now among the most prevalent. Indeed, the most widely accepted tradition concerning epistemological matters is what Bloor calls "individualistic empiricism," and Barnes calls "individualistic rationalism." In the conclusion of Shapin's (1982) well-known paper, he claims that historians of ideas routinely study knowledge as a base of cultural change, and they acknowledge that cultural heritage is socially transmitted; yet, he complains, "Many historians of ideas still treat contributions to culture as if they were generated *in vacuo* by atomistic individuals, and some of them continue to view the source of cultural material of science as a matter of moral concerns." [25] Collins's work relies on case studies in which the collective making of scientific consensus is the key point. As in the case of so-called laboratory studies, the key point of the collective making of scientific facts and literature is given such importance as to be the first of the six principles that summarize the approach (Latour, 1987).

It would, of course, be a very deflationist approach to reduce the meaning of "social" to "collective making." Yet it is an interesting starting point for the understanding of what the meaning and specific contribution of sociological naturalization would be. For if it is true that cognitive activity is constitutively and interestingly social in the above mentioned sense—then SSK would focus on something largely neglected in epistemology. So SSK would complement naturalized epistemology by claiming that the investigation of the nature of scientific knowledge can be reduced neither to that of the cognitive capacities of individuals nor to their evolutionary history. It turns out that humans would *also* be social beings, *also* have a cultural history, which would *also* determine the nature of their behavior—including their cognitive behavior. In this way, SSK would open the agenda of science studies to a series of new issues neglected until now. Now if *science* is an enterprise and *knowing* an activity collective by nature, it would be reasonable to integrate sociology within the interdisciplinary field that is now the study of science promoted by philosophy itself—as, in fact happened first with history of science, and later with psychology and biology. But why did this not happen? Why has there been no affirmative answer to SSK's call for "a naturalist understanding of knowledge in which sociology plays a role?" [26] The answer to this question leads to the first of a series of distinctions and dichotomies that have marked our philosophical culture about science; these, in turn, could be an important part of the reason behind the misunderstanding and resistance towards SSK.

Science Study: Internal versus External Factors

Bloor's (1991) defense of a naturalized study of knowledge implies the search for a general causal theory about cognitive phenomena. Such a theory has to explain *all* cognitive beliefs; that is, *all* of them have to appear as part of the same causal process, and all have to be the effect of the same kinds of causes. But, Bloor says, the problem with this demand is that the set of cognitive beliefs has been divided, traditionally, into a positive and a negative side, namely, the correct, true, rational beliefs, on the one hand, and the incorrect, false, irrational beliefs on the other. Moreover, and here is the point, for each of these sides one has a different kind of explanation. While the beliefs on the positive side—members of an autonomous intellectual realm—are explained by the exclusive action of *internal* factors, typically scientific rules and facts, those on the negative side are explained, instead, by referring to *external* factors—typically psychological and sociological. [27] In this way, differences in the set of beliefs give way to the dichotomization of their causes, and this turns out to be an obstacle to the construction of a single causal explanatory theory of knowledge. Bloor's principle of *symmetry* intends, precisely, to block this dichotomization: if social factors have causal efficacy, they have it for *all* beliefs alike. That is, the field of sociological investigation should not be limited to the pathology of beliefs. This is indeed the way Bloor introduces and presents arguments for his principle. The possibility of a positive answer to Bloor's question about the propriety of sociological incursion into epistemological matters is, thus, blocked by this radical distinction between internal and external factors and their causal efficacy. The rejection of this distinction is, indeed, the negative content of the principle of symmetry, whose positive side would be the demanding of *a role for sociology* in science studies.

One could say that to read the symmetry principle in this way is to give it a weak interpretation—even weaker than Bloor himself wanted. Indeed, the meaning of "strong"—as in Strong Program—has been understood as having to do with a reductionist oversociologization of knowledge's explanation; namely, in knowledge causation social factors would have priority—if not exclusivity. There would be no causal role for reality to play. But this last reading cannot be supported because of what the very text that introduces the principle really says. And Bloor himself explicitly rejects it, replying that the term "strong" in Strong Program was introduced to differentiate it from the standard, weak, approach, namely, the view according to which the only causal efficacy of social factors is in the distortion of beliefs.[28]

This misunderstanding about the use of the term "strong" could be a reason for the persistent rejection of a demand for a role for sociology in science studies. This rejection continues today, in spite of the fact that sociology's integration could appear as a further step in the transformation of philosophy which gave rise to its naturalization. In effect, philosophy was the first of the science studies in this century as a normative foundationalist discipline. This meant, at the time, that there had to be a sharp demarcation between the task of philosophers, on the one hand,

and that of historians, pscychologists, and sociologists, on the other. The first transformation came as a consequence of Kuhn's demand for a role for history and Lakatos's proposal to deal with a metascientific investigation program as if it were an historiographical one. With this change, the *normative versus descriptive* distinction became the *internal versus external* distinction. And this in turn meant, as I have said, that Lakatos opened the door to history with the same firmness that he closed it to psychology and sociology. The later transformation occurs with the naturalization of epistemology: the demand—which originates with Quine although it has a very different meaning today—to deal with knowledge as a natural phenomenon. This opens the door to science studies to psychology and, almost simultaneously, to biology. Science studies became, thus, truly interdisciplinary. But the traditional thesis about the autonomy of science—the core of the internalist view—continues to block the possibility of "a naturalist understanding of knowledge in which sociology plays a role."

Science Study: The Dichotomy of Contexts

After clarifying the difference made by adding sociological to philosophical naturalization, one finds another dichotomy that has marked philosophy of science since its beginning, namely, the dichotomy between the context of *discovery* and the context of *justification*. Indeed, philosophy began to see the scientific enterprise as divided into two very different kinds of activities: that of discovering and that of evaluating scientific results. Only this latter was thought to be philosophically relevant; the processes that *really* give rise to scientific results appeared as irrelevant to the justification of these results. This distinction turned out to function, as a matter of fact, as a veto against the investigation of real scientific processes. With *philosophical* naturalization there were changes: the investigation of real, historical, actual episodes of science are now thought to be not only relevant, but almost necessary conditions of what is considered to be good metascientific practice. Yet this philosophical change has had little effect on the dichotomy of contexts, because investigations—now empirical—of change and evaluation continue to be the major philosophical topic.

Sociological naturalization reverses the order of relevance; the key metascientific problems arise around the processes of knowledge making, before the crystalization of the results. Indeed, for SSK, what is more interesting and relevant is to analyze what scientists *really do* when they *practice the activity* called science:

when they produce laboratory phenomena, as well as when they produce scientific literature; when they intervene in a controversy, as well as when they decide on closure; when they relate claims about nature with claims about society, as well as when they relate claims about society with claims about nature. SSK claims, thus, that the most interesting and relevant place to look in order to know the nature of science is to the real practice of science. Without it no scientific *results* would exist.

This reversal of the order of priority, from finished products to their production processes, is quite important because, according to SSK, once scientific results—laboratory facts, controversy closures, scientific papers—are delivered, they are all that we know—where "we" includes not only lay-people but most scientists. The processes, agents, and resources that brought them about become, in this way, *invisible*. Scientific results become *black boxes* that have a life of their own. Here is a reason for the fruitfulness of focusing on production processes. But there is another still more important reason. It has to do with the fact that, according to SSK, this invisibility is the reason why one has traditionally seen scientific results as emanating directly from nature, as having to do with nothing but nature, reality, the world—such as it is in itself, *independently* of human action, intention, interests, and desires. This is the prevalent view of scientific results, and it is so generally shared that it is taken for granted. It is precisely for this reason that this view must be critiqued by analyzing in detail each and every one of the factors, actions, and resources that make science possible in the first place. For these reasons, one should give priority to *science in action* rather than to the *results* of its actions.

Science Study: The Demarcation Criterion

Returning to the real meaning and implications of attempting to add a social dimension to the naturalistic study of scientific knowledge, one finds another dividing line that philosophers insist on keeping sharp and SSK insists on softening—namely, the line that separates science from the rest of culture. This is technically called the *demarcation criterion*. Bloor (1991) says that the main "cause of [sociologists'] hesitation to bring science within the scope of a thorough-going sociological scrutiny is . . . . the conviction that science is a special case . . . naturally [he adds] philosophers are only too eager to encourage this." [29] The reasons behind this hesitation are, of course, what sociologists must struggle against if they are to make a place for sociology in the naturalistic program.

As we have seen, one of the reasons is internalism, which the supposed autonomy of science relies on. Another has to do with an individualistic view. Referring to this last, Bloor argues: "Does not individual experience take place within a framework of assumptions, standards, purposes and meanings which are shared? Society furnishes these things and also provides the conditions whereby they can be sustained and reinforced. . . . The knowledge of a society . . . is their collective vision of Reality . . . not of a reality that any individual can experience or learn about for himself. . . . Knowledge then, is better equated with Culture than Experience."[30]

This approach, assimilating rather than demarcating, integrating rather than separating knowledge from the rest of culture, is a recurrent theme in SSK. So, for example, Shapin (1982) says: "In the sociological approach to knowledge people produce it against the background of their culture's inherited knowledge, their collectively situated purposes and the information they receive from natural reality." These purposes are collectively designed, he says, and can pertain to both the technical scientific culture, and society in general. "Typically [knowledge], its usage and meaning, will be embedded within a complex social network, such that possible connections always exist between consideration in all parts of the net." [31] And Barnes (1990) claims that knowledge is inherited, routinely transmitted from generation to generation; it is part of a cultural tradition, and as such is sustained by the authority surrounding custom and tradition. "Then, Barnes claims, "what counts as knowledge in most social contexts it is tempting to call ‘customarily accepted belief.’ It is sustained by consensus and authority much as custom is sustained. It is developed and modified collectively, much as custom is developed and modified. This we might call the standard sociological conception of knowledge." This "conventionalist collective," as Barnes calls it, view of knowledge comes face to face with the "individualistic rationalist" traditional view, according to which cognitive beliefs have a privileged epistemological status. Yet, he claims, if this status is not presupposed, it does not appear as evident. Each move in the game of science could be different, without offending either reason or experience; given that there are no compelling reasons, agreement about the correctness of the move must be consensually established. [32]

Likewise, Collins (1985) claims that SSK approaches science as just another cultural activity, and he adds a demystifying aspect to this claim. In its broadest sense, Collins claims, a culture relies on the ability of human beings to see the same things and to respond to them in similar ways. Without this uniformity, there is neither culture nor society. Science, too, he says, relies on achieving and maintaining uniformities, but its tricks come to be so routinized that they are taken for granted. It is for this reason that in studying science one needs to pay special attention to what happens before the routine acceptance of uniformities begins. [33] This could be done by looking at controversies before they are closed. Science, he says, works by producing agreement among experts. But agreements are the end result of controversies. After controversies close, one hears a single voice, that of the winning group. Yet, he says, "Allowing everyone to speak is as bad as allowing a single group alone to speak. It is as bad as having no-one speak at all." [34] It is from the prevalent presentist view of science, a view that relies on hearing only the winning voice, that science appears to be empowered by a special authority. "Making science a continuous part of the rest of our culture," Collins says, "should make us less intimidated." [35]

Another way of putting the antidemarcationist argument is Latour's (1987) way. He tells the dramatic story of a fictitious character, a supposed dissident scientist trying to make the case for his dissenting voice. He promises, with this story, to show "the heterogeneous components that make up science, including, the *social* ones." At a given moment in the story, Latour anticipates a possible objection from readers: "What do you mean 'social'? [the reader might ask] Where is captitalism, the proletarian classes, the battle of the sexes, the struggle for the emancipation of the races? Western culture, the strategies of wicked multinational corporations, the military establishment, the devious scientists? All these elements are social and this is what you did *not show* with all your texts, rhetorical tricks, and technicalities." Latour agrees that he has shown nothing of all this, and yet, he has shown, he says, something more important than these traditional "social" actors. He claims to have shown that, at the same time that scientific literature becomes more and more technical, the dissident, in turn, becomes more and more isolated. Due to the magnitude of resources in the hands of his opponent, the dissident ends up "isolated, besieged, and left without allies and supporters." If this "is not a social act," Latour says, "then nothing is." "The distinction between the technical literature and the rest is not a natural boundary; it is a border created by the disproportionate amount of linkages, resources and allies locally available. This literature is so hard to read and analyze not because it escapes from all normal social links, but because it is *more* social than so-called normal social ties." [36]

In this way, by focusing on the view of *science* and *knowing* as collective social activities as one of the chief features of sociological naturalization, we have passed from a more "neutral" issue—such as that the unity of knowledge making is collective and not individual—to a less neutral one, that this activity is similar to other collective cultural activities. For there is no collective activity without an order, according to SSK, an order established by convention by a community's members. This order, in turn, once established, is routinely followed, and, after a time, is taken for granted, and then becomes invisible, and then, finally, is kept in place by authority. It is true that in our conventional wisdom one is not used to speaking of science and its knowledge in terms of order, routine, tradition, and/or authority. Yet it is not any less true that the relevance and/or propriety of using these expressions in a naturalistic approach is an issue that does not have to be decided *a priori*.

This issue is crucial in deciding about the demarcation issue in science studies. Thus it is up to the sociologists to prove, theoretically and empirically, that there is no sharp distinction. And, we saw, that is exactly what they intend to do. Similarly, it is up to the philosophers to demonstrate their affirmative decision by establishing a *normative naturalized* theory of *rationality*. Judging by the space that rationality has received in the philosophical literature, it seems that the Popperian idea that *demarcation* is the core issue in philosophy of science stands as firm as it always has.

The Controversy, Rationality versus Relativism

This is a label for a battlefield on which philosophers and sociologists fight each other using their most sophisticated conceptual devices. [37] Philosophers appear entrenched in one of the fields, their arms made of elements of a traditional artillery which, it is supposed, will one day be the definitive weapon for defending the distinctive feature of science: a theory of rationality. Sociologists at the beginning seemed to be on the defensive in the face of something that did seem more like an accusation than a mere description. [38] Later, relativism became something to embrace rather than to avoid. Yet "relativism" is a term whose meaning has no level of precision higher than that of a "straw man" against or in favor of which a large part of the controversy is directed. Collins's work can be considered the turning point because he uses the term "relativism" in the title with which he presents his own SSK subprogram. [39] But there is something interesting with respect to his use of the term. For Collins presents relativism in the first place not as a *thesis about the nature* of science, but as a *strategy for the study of science*.[40] The interesting point is that the aim of this strategy in Collins's work is the same as that of other authors, with their other strategies. It is this that I am interested in highlighting first.

As we have seen, for Collins, science, as (or better, *more* than) any other collective activity, relies on an established order, whose principles—as is always the case with unchallenged, stable orders—are taken for granted and so become invisible. For this reason, one needs to make an extra effort to design a special strategy to uncover them. Collins calls his strategy to uncover the taken-for-granted principles which scientific culture relies on, precisely, *relativism*. His relativistic strategy, he says, "rests on the prescription: treat descriptive language as though it were about imaginary objects." But why precisely this prescription? Because if we want to know scientific culture, we should not take for granted what it does; this is what relativism intends, it "demands that the analysis of the way knowledge is established is not shackled at the outset by common sense judgments about what is and is not true." And the way scientists establish the truth is precisely by referring to descriptive sentences, i.e., sentences that are supposed to describe what the world is really like. If what we want is to study scientific culture itself, then we need the relativistic prescription, because "if cultures differ in their perceptions of the world, then their perceptions cannot be fully explained by reference to what the world is really like."[41] So taking a relativistic stance (his prescription) would lead us to begin by doubting what is generally taken for granted, namely, the nature and decisive role with which descriptive sentences are empowered.

Thus relativism, such as Collins presents it, is first a strategy for knowing knowledge making rather than a thesis about knowledge itself. This recommendation of distancing oneself from what everyone—especially scientists—thinks about science, as a fruitful strategy for uncovering taken-for-granted assumptions, is a topic in the SSK literature.

Bloor's principle of *impartiality* also demands that a naturalistic explanation of knowledge be independent of how scientists evaluate it; it has to explain both what they take to be true and what they take to be false, as well as what he takes to be rational and what he takes to be irrational. [42]

This is also the starting point of Latour and Woolgar's (1979) book. Here they recommend "maintaining analytic distance upon explanations of activity prevalent within the culture being observed." "In the case of scientific culture in particular there is a strong tendency for the objects of that culture [facts] to provide their own explanation. Rather than produce an account which explained scientists' activities in terms of the facts which they discovered, our interest was to determine how a fact came to acquire its character in the first place." [43]

And when Shapin and Schaffer (1985) present their impressive work on the experimentalist tradition in modern science, they claim: "Ordinarily, our scientific culture's beliefs and practices are referred to the unambiguous facts of nature or to universal and impersonal criteria of just how people do things (or do them when behaving 'rationally')." "To be a member of a culture," they add, "is to act considering some things as if they were self-evident, and this marks decisively the version of that culture that can give native members." Inversely, "The member who poses awkward questions about 'what everybody knows' in the shared culture runs a real risk of being dealt with as a troublemaker or an idiot. Indeed, there are few more reliable ways of being expelled from a culture than continuing to seriously query its taken-for-granted intellectual framework. Playing the stranger is therefore a difficult business; yet this is precisely what we need to do with respect to [scientific] culture. . . We wish to adopt a . . . . suspension of our taken-for-granted perception of experimental practice and its products. By playing the stranger we hope to move away from self-evidence." The advantage of the perspective of the stranger, the authors say finally, is that the stranger " *knows* that there are alternatives to [native] beliefs and practices." [44]

In this way, relativism, as Collins explicitly presents it as the concrete way to approach science, follows the pattern generally recommended by SSK studies; namely, choose a path that will permit the uncovering of taken-for-granted assumptions. Why do they make this strategy a necessary starting point in the study of science? Perhaps the best answer to this question would be another question: why not? After all, our philosophical culture *takes for granted* that science is something very different *(demarcation)*, that it operates according to exclusive rules of its own *(internalism)*, rules, in turn, which are empowered by an epistemological virtue *(rationality)*. Although the technical terms come from recent philosophical developments, the assumptions behind them come from the conventional wisdom about science that we all share—lay person, scientist, and philosopher alike. For this reason, these assumptions represent too committed a starting point in an approach to science, especially if one intends to be a naturalist, one for whom what *really* happens must take priority over what is supposed to be the case.

When Gooding, Pinch, and Schaffer (1989) comment on the work of Franklin—a paradigmatic member of the *philosophical rationality* field—they say that for Franklin what counts is that there do exist rational strategies to evaluate scientific results. For SSK, instead, what counts is that those strategies are "culturally accepted practice"; but this does not mean that science is either an irrational enterprise or one in which anything goes. What the two sides disagree about is not what they say *about science,* but about how to *approach* the study of science. Contrasting Franklin's way and the SSK way, the former takes as *explanans* what the latter takes as *explanandum*.[45] There is thus a difference between philosophers (rationalistic) and sociologists (relativistic) and it is that the former use epistemological norms to *explain* knowledge reliability—this is all they think they have to explain—while the latter extend the field of what must be explained to include the epistemological norms.

It is important to point out that Franklin is working within a recently modified framework regarding methodological matters. For philosophy of science began with a foundationalist/normativist view, making demarcation a radical dividing line and cognitive phenomena part of the Platonic-Popperian "third world." Then came naturalization which treats cognitive phenomena as part of the natural world and bases its normative claims on the empirical investigation of real scientific practice—past and present. It is as a result of this kind of work that Franklin claims that there do exist epistemological criteria, and they do explain knowledge reliability. Yet he does not ask for an explanation of the criteria themselves.

Now enters sociological naturalization, which asks for such an explanation, and it gives one: its status is that of "culturally accepted practices." With this move, cognitive phenomena appear as part of the realm of social convention. And if this were true, demarcation would collapse. Here is where relativism stops being a mere strategy for knowing about knowledge, and becomes a substantive thesis about knowledge itself. It stops being a metamethodological strategy and becomes an epistemological thesis. And here, of course, is where the controversy about philosophical rationalism *versus* sociological relativism gets its focus. Yet the opposite of *rational* is neither *relativistic* nor *social*—terms the debate has turned around—but *irrational*.[46] Only if sociologists had called their own work—or science itself—irrational would philosophers be justified in using rationality as a weapon to fight against them.

Inversely, the opposite of *relativism* is not rationality but *absolutism;* and the absolute—be it norms, principles, or truth—may properly have a place in the Popperian third world, but it is hard to see how a naturalist could find it in the messy and ever-changing reality of which both the world we want to know and knowledge itself are a part. This difficulty could be the reason why relativism is not a problem that SSK imposed on philosophy from the outside. For, from Quine's ontological relativity to Putnam's conceptual relativity, and on through Kuhn's incommensurability, philosophy in the second half of this century, to a great extent, has been an attempt to banish the phantom of relativism. And perhaps these philosophers are the only ones who have remembered that one of the major tasks of philosophy has always been that of *uncovering taken-for-granted assumptions*.

The Controversy, Realism versus Constructivism

The rejection of the dichotomy of contexts and the reversal of priority between justification and the genesis of knowledge to which SSK gave rise has made of experimentation a chief issue in science studies. A move towards the experimental side of science has taken place recently within philosophy, too, with such influential works as those of Harré and Hacking, and also those of Franklin, Galison, and Nickles—all of them except Franklin sympathizers with SSK. This shift of attention has taken place in a climate of harsh criticism condemning the ironic fact that the traditional conceptualist view of science had neglected the very hallmark of science, experiments. Sociologists were also aware of the problems that philosophical theses about the "theory ladenness" of observation and the "underdetermination of theories by data" had caused for the earlier simplistic view about the nature and role of the empirical base of science.

Yet there is a difference with the sociological analysis of this topic; and it leads directly to the controversy, *realism versus constructivism*. For with so-called laboratory studies we have to deal with the problem, not that our *view* of facts depends on our previous expectations and beliefs, but that the very *existence* of facts depends on the processes that produce them in the laboratory. And this does not mean that nature or the world has no independent existence, nor that it has no role to play in knowledge. Yet it is *not this* reality which scientific knowledge relies on but the one that emerges in the laboratory. Laboratory work is indeed a *labor* that consists of *not* allowing reality to remain as it was when it arrived there. Whether mice, bacteria, or chemicals, scientists' work does not consist of passively observing them, but rather of manipulating them, and transforming them. And it does all of this with intricate instruments, apparatus, and resources which are also not taken from nature, but are themselves products of prior human activities.

The problem here, as in many of the controversial points that pit sociologists against philosophers, is the use of quotations out of context. Laboratory studies in particular and SSK in general are supposed to claim a *metaphysical* thesis that would deny the existence of nature, the world, independent reality; and/or to claim an *epistemological* thesis that would deny that such reality plays a causal role in knowledge. Around these supposed claims the controversy *realism versus constructivism* is built. Boyd (1992), for example, says: "Realists and constructivists differ in that the former hold, while the latter deny, that the phenomena studied by scientists exist and have the properties they do independently of our adoption of theories, conceptual frameworks or paradigms." [47] But what in fact constructivists deny is that those phenomena exist and have the property they do, *not* independently of what scientists think, but of what scientists *do* with them in the laboratory.

The laboratory, thus, is not a place where scientist meets *nature,* and its power does not reside in allowing the conditions under which phenomena *naturally* occur to flow freely. Rather, "The power of a laboratory is measured by *the extreme conditions it is able to create:* huge accelerators of millions of electron volts; temperatures approaching absolute zero; arrays of radio-telescopes spanning kilometers; furnaces heating up to thousands of degrees; pressures exerted at thousands of atmospheres; animal quarters with thousands of rats or guinea pigs; gigantic number crunchers able to do thousands of operations per millisecond." [48] Speaking of laboratory studies, Giere (1988) acknowledges: "It is undeniable that these works of Latour and Woolgar and of Knorr-Cetina capture the texture of day-to-day research in a way that few other works, be they sociological, historical or philosophical, have ever done." "Still," he adds, "for anyone trained in the natural sciences or in an analytic philosophy of science, constructivism sounds wildly implausible."[49]

Yet Latour has an explanation for this sense of implausibility; it has at its cause a paradox of scientific activity. It has to do with the fact that, once scientific results leave the lab in the form of key pieces in scientific papers, they make no reference to the actions, agents, or resources that give birth to them in the first place. All the long, expensive, and complex processes from which the results emerge become invisible. It is a fact that the inclusion—or not—of references to places, people, or processes marks the status of a scientific sentence. By adding such references, the status of a sentence is undermined. By removing them, it becomes more reliable. *No* references at all, and it is *taken as* uncontroversial. It is well known that there are no scientific sentences uncontroversial in themselves, something that was highlighted by the old philosophical thesis of fallibility. It is also well known that the status of sentences can change, depending on several conditions. What sociological analysis now would show is that this variability takes the form of adding or removing references to agents, places, or processes. And this is the point; it is the lack of all reference to human activity in obtaining scientific results that makes them appear as if they were, indeed, really independent of all human action. It is in this way that the independent existence of scientific phenomena, a key thesis of the realist argument, turns out to be itself a phenomenon that results from removing all reference to the actions without which the very phenomena would not exist in the first place. So Latour (1987) claims: "[Scientific] statements are not borrowed, transformed or disputed by empty-handed lay people, but by scientists with whole laboratories *behind* them."[50]

Niiniluoto (1991) distinguishes philosophical naturalization (with authors like Lakatos, Laudan, or Giere), where science appears as the paradigm of human rationality, from sociological naturalization, where "scientific beliefs have no special relationship with reason, truth and reality." But, beginning with the word, "truth," I think it is reasonable to say that this has turned out to be a very elusive claim. As for "reason," I think it is reasonable to say that now—as had already happened in the '60s—it depends on how "reason" and "rationality" are defined. And now, as then, what seems to be challenged is *some* of the interpretations of such terms. Finally, with regard to "reality," I think that Niiniluoto is just plain wrong. Far from saying that scientists have no special relationship with reality, constructivists claim that scientists have a *very special* relationship with reality, but it is a relationship about whose nature we have no idea because its investigation was forbidden by the philosophical dichotomy of contexts.

Thus far we have spoken only of sociological constructivism. Yet constructivism is also an old philosophical thesis, at least in the sense of emphasizing the active role of the subject in the construction of knowledge. In this sense, it goes back at least to Kant. Of course, there must be important differences between SSK and Kant's view of knowledge. Yet Kant did claim that the "conditions of all possible experience are, at the same time, the conditions of possibility of the object of experience." [51] Perhaps the constructivism of laboratory studies could be read, pertinently and interestingly, as a *sociological/naturalistic* version of Kant's claim.

CONCLUSION

The controversial climate surrounding SSK has existed since the beginning because of both external rejection and/or resistance and internal divergences and ambiguities. In the course of this paper, I intended to make a reconstruction of some of what appear as key points of what SSK protagonists share (until divergences are explicitly invoked to make different points), as well as what separates them from their chief opponents, the philosophers. In this reconstruction, the key issue for SSK could be summed up under the slogan, *a role for sociology* in an interdisciplinary naturalized study of science. With this demand, sociologists would attempt to join scholars of other disciplines that have already been integrated, such as cognitive psychology, evolutionary biology, and the history of science. Why should sociological investigations not be able to complement other understandings of scientific knowledge? Indeed, it reveals that *science* and *knowing* are by nature collective activities. As such, science should not be differentiated from other collective human activities; it would be just another cultural activity. So the distinctiveness of science would reside not in a difference regarding its nature, but regarding its specific aim, the production of reliable knowledge. The search for the nature of this distinctive activity would rely on the empirical analysis of what it *really* is: what it produces, how it is produced, the resources needed for this, the places where this production occurs, and the contexts (proximate and remote) that affect both the activity and its results. It is from a precise empirical investigation of this kind that one ought, *a posteriori*, to decide what the nature and distinctive traits of science *really* are. It is, as a matter of fact, this kind of investigation that makes it necessary to overcome old distinctions and dichotomies.

Given the program with which philosophers first began to work in this century, the strong philosophical reaction against SSK could appear to be both expected and reasonable. For the basic theses of that program were foundationalist normativism, justificationism, demarcationism, internalism, rationalism, and realism at an observational or theoretical level (or both). Compared to this program, SSK's program appears as an irreconcilable alternative by rejecting dichotomies, dissolving distinctions, and reversing the order of priority. It seems almost designed to violate each one of these philosophical theses. Born as a naturalistic program, it must necessarily focus on its own subject matter, the social basis of knowledge making. With this starting point, it is committed to being first, anti-internalistic, and, then, antidichotomistic, antidemarcationistic, relativistic, and constructivist.

The strength of philosophers' reactions would still be reasonable *today* if philosophy of science had remained the same as it was some decades ago. In such a case, the integration of sociological and philosophical studies would appear quite unrealizable, given that it would require radical changes in some of the studies. But many changes have taken place, recently and independently, within philosophy itself. There exists, for example, a general philosophical consensus around naturalization and the interdisciplinarity of science studies, and the hard edges of dichotomies have been softened, and demarcation lines have faded. So one could expect a new receptivity towards SSK. But this has not happened.

The issue demanding an integration of SSK within the naturalized study of science (within a naturalization that philosophy itself has promoted) is not a question of mere disciplinary boundaries, nor of defending interdisciplinarity. SSK makes interesting proposals both about how studies of science must be approached and about what its working agenda must include. Yet, regarding the possible impact of all this on the global image of science—an impact that sociologists have hurried to proclaim while philosophers have rejected it—we have a question that can only reasonably be answered *a posteriori*, never *a priori*. It is a matter of better elaborations of theoretical arguments and better development of empirical investigations. And these elaborations and developments demand an intellectual climate that one could hardly say has existed before today. With this paper, I hope to have collaborated in the creation of such a climate.

NOTES

1. Although I recognize that this paper is not directly related to philosophy of technology, its

contrast between philosophy of science and the sociology of scientific knowledge (SSK) exactly parallels similar contrasts between philosophy of technology and the social construction of technology (SCOT). See E. Aibar, in this issue of PHIL & TECH.

2. Bloor (1991), p. 1.

3. Taking into account that a considerable part of the obscurity in the controversy around SSK originated because of the difficulty of doing justice to its diversity, and at the same time the misfortune of quoting sentences out of context, my strategy will be to refer to some of the best known authors and works, and to add literal quotations whenever possible.

4. Bloor (1991), pp. 3-4. All the quotations from this book will be from its 1991 edition.

5. Laudan, L. (1977). This author is the precursor and most persistent philosophical advocate of the rationalist crusade against SSK, investing great ingenuity in it, as shown in his (1990).

6. Barnes and Shapin (1979); Knorr-Cetina, Krohn, and Whitley (1981); Barnes and Edge (1982); Collins, (1982); Knorr-Cetina and Mulkay (1983).

7. Giere (1988), p. 51.

8. Among the best known, besides Barnes's and Bloor's now classic books, are Brannigan (1981); Collins (1985); Knorr-Cetina (1981); Latour and Woolgar (1979, 1986); Latour (1987); Mackenzie (1981); Mulkay and Gilbert (1984); Pickering (1984); Pinch (1986); Shapin and Schaffer (1985).

9. Bloor (1991), p. ix.

10. It is necessary, of course, to soften this last statement. I will do that later, at the end of this section.

11. Among the most significant papers regarding naturalistic philosophy of science see Giere (1985) and (1988) chap. 1; Hooker (1986); and Kitcher (1992), but especially the book by Callebaut (1993).

12. There are some interesting exceptions, like Giere (1988).

13. Bloor (1991), p. x.

14. Barnes (1990), p. 62. Regrettably I do not have the space here to enumerate the reasons that, according to this author, lie behind the decline of the rationalistic conception.

15. This is the main point that separates the Strong Program from the rest of the subprograms in SSK.

16. Lynch and Woolgar (1990), p. 4.

17. See, for example, Latour and Callon (1992), pp. 343-369.

18. Pickering (1992), pp. 5-8.

19. Earman (1992), pp. ix-x; R. Boyd, in Earman (1992), pp. 133; Niiniluoto (1991); Kitcher (1991) and (1993) p. 391.

20. Giere (1988), p. 51.

21. Bloor (1991), pp. 5-8.

22. This principle is, we saw, the source of one of the internal divergences.

23. The works in SSK were not especially effective in dealing with the problem; moreover, on more than one occasion it has caused some protagonists to stop using the term. Such is the case in the edition of 1986 of Latour and Woolgar's well-known book and in Pickering (1992).

24. Bloor (1991), p. 15.

25. Shapin (1982), p. 196.

26. Bloor (1991), p. ix.

27. Bloor (1991), pp. 7-13.

28. Bloor (1991), pp. 164-265.
29. Bloor (1991), p. 4.
30. Bloor (1991), pp. 15-16.
31. Shapin (1982), pp. 196-197.
32. Barnes (1990), pp. 65-66.
33. Collins (1985) chap. 1; see especially pp. 5-6 and 18-19.
34. Collins (1993), p. 148.
35. In Pickering (1992), p. 18.
36. Latour (1987), p. 62.
37. Two by now classical readings on this controversy are Hollis and Lukes (1982), and Brown (1984).
38. See the suggestive things that Latour (1987, chap. 5) says about this.
39. See Knorr-Cetina and Mulkay (1983), paper 4.
40. This is specially clear in chap. 1 of Collins (1985).
41. Collins (1985), p. 16.
42. Bloor (1991), p. 7.
43. Latour and Woolgar (1986), p. 278.
44. Shapin and Schaffer (1985), pp. 5-6.
45. Gooding, Pinch, and Schaffer (1989), p. 23.
46. See the section, "Peopling the World with Irrational Minds," and chap. 5 in Latour (1987).
47. Boyd (1992), p. 131.
48. Latour (1987), p. 91 (my italics).
49. Giere (1988), p. 58.
50. Latour (1987), p. 91.
51. *Critique of Pure Reason*, A 111.

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