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Abstract

Since the mid-twentieth century a considerable part of the philosophy of science has been driven by the adoption of a naturalized point of view, that is, the purpose of characterizing science with the only help of empirical researches coming from the historiography of science, sociology, psychology and other scientific disciplines. Quine, Kuhn and several other authors attempted to elaborate epistemological doctrines free from any philosophical a priori argument. Nevertheless, we think that those attempts have failed. On many occasions the alleged resource to purely empirical research hides some philosophical postulations. In the Introduction we summarize the historical background of the sociology of scientific knowledge and analyze its main aspects, its claims and its failures. In particular, we point out that naturalized epistemologies are permanently in risk of falling either into a vicious argumentative circle or into a self refuting one. In section II we focus in the case of the Strong Programme of Sociology of Scientific Knowledge developed by the Edinburgh School. In section III we examine the troubles of the ethnographic analysis of science emblematically represented by Latour and Woolgar´s Laboratory Life: the construction of scientific facts.

Keywords: Philosophy of Science; Naturalized Epistemology; Sociology of Science, Strong Programme; Laboratory Life.

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The Sociology of Science: Historical and Philosophical Background

The philosophy of science acquired pre-eminence as one of the main branches of philosophy in the 19th century (although the theory of knowledge and in particular the reflections on what is meant by science go back to the roots of Western philosophy, and had splendid manifestations in the works of Plato and Aristotle). This pre-eminence was associated in the first place with the emergence of positivism in the works of Claude-Henri de Saint-Simon and Auguste Comte, and later in the ideas disseminated by logical positivism and logical empiricism. Around the middle of the 20th century, the influence of this movement lost strength in the face of a series of criticisms that opened the way to naturalized approaches to science.

Classical positivism was mainly motivated by social and political concerns. It has a prominent place in the philosophy of science because positivists were convinced that society had reached a stage in which science and technology were making society's progress possible. One of the most characteristic theses of classical positivism is the so-called law of the three stages. In Comte's terms, these three stages are the theological, the metaphysical, and the positive (Comte 1853). According to H. Scott Gordon, "the essentials of the law of the three stages are contained in Saint Simon's writings of 1813, four years before he met Comte" (Gordon 1991: 280). Both authors thought that during the early stages of social history, the pre-eminence of dominant groups prevented society from becoming aware of its reality. This goal, they thought, would only be achieved with the emergence of sociology, a new discipline inspired by physics but applied to the knowledge of society. The term "sociology" was introduced by Comte to name a perspective that he previously had called "social physics" (Comte 1853).

The works of Saint-Simon and Comte enhanced two aspects that were decisive for the problematics of the philosophy of science at different moments in the following century. On the one hand, the central role that they gave to scientific knowledge was inherited by the members of the Vienna Circle and their followers. On the other hand, their recognition of the influence that political and social factors exert in the construction of scientific knowledge would give rise to social approaches to science.

These two aspects—the role given to scientific knowledge and the recognition of the influence exerted by political and social factors—came together in Karl Marx's and Friedrich Engels' doctrine. The purpose of building a social science following the objectivity of the natural sciences and mathematics was embodied in Marx's efforts to fuse historical, sociological, and economic knowledge into a comprehensive doctrine. According to the Marxian conception, earlier social studies were in general strongly tinged by the interests of the ruling classes: "It is not the consciousness of man that determines their existence, but, on the contrary, their social existence determines their consciousness" (Marx 1904: 11-12). As for scientific objectivity, Marx and Engels had some hesitations (Scott Gordon 1991: 321; Bunge 1999: 148), but the idea that has prevailed is that mathematics and natural sciences escape ideological distortions, while only a

part of social research can elude them. According to their doctrine, the social science that is finally objective is scientific socialism, eminently embodied by Marx and Engels, which is based on a rational analysis of society and does not represent partial interests but the point of view of the proletariat, which becomes universal.

The ideas of the founders of positivism, especially in relation to the importance of sociology, decisively influenced the task undertaken by Emile Durkheim, who carried out well-known researches on various aspects of society, for example the correlations between different religious beliefs and suicide rates. Also of enormous value was his effort to formulate norms intended to make sociology a science comparable to the natural sciences. This undertaking was expressed essentially in his book *The Rules of Sociological Method* (1982). Convinced that causal relations actually govern reality, since "it is only the philosophers who have ever called into question the intelligibility of the causal relationship" (Durkheim 1982: 149), he recognized the presence of causal relations also in social phenomena, and considered that finding them was a task of sociology. The method he recommended is a comparative method; moreover he adopted the rules indicated by the English positivist John Stuart Mill, (Stuart Mill 1846) but with important caveats. It accepts the method of concomitant variations, while rejecting the methods based on concordance, difference, and residues.

In agreement with predecessors such as Saint-Simon, Comte, and Marx, Durkheim distinguished between mature scientific knowledge and another type of beliefs which he called "mythological representations":

> What characterizes such mythological representations is the fact that they express a unanimous conception, and this is what gives them a force and authority which enables them to impose themselves without their being subject to verification or doubt [….] There is, and there always will be, room in social life for a form of truth which will perhaps be expressed in a very secular way, but will nevertheless have a mythological and religious basis. For a long time to come, there will be two tendencies in any society: a tendency towards *objective scientific truth* and a tendency towards subjectively perceived truth, towards *mythological truth*. This is also one of the great obstacles which obstruct the development of sociology. (Durkheim 1983: 90) [Our italics]

Durkheim's hypotheses about the social genesis of what he called "categories", such as *time*, *space*, and *causality*, have led some authors to consider that they do not designate objective properties of the material world because they are representations of the social world. Thus Scott Gordon considers that while on the one hand *The Rules of Sociological Method* seems to claim that scientific concepts derive from sensory perception, on the other hand categories, according to Durkheim, come from social factors such as the coercive power of parents, political authorities and collective consciousness, which produce the concept of causality (Scott Gordon 1991: 455). We believe,

however, that this apparent contradiction in Durkheim's thinking could be resolved through two considerations. One is that, as Kant establishes in the *Critique of Pure Reason*, in spite of the fact that all knowledge begins with experience, it does not exhaust itself in it. In the same way, even if it is true that certain concepts respond to social circumstances, this does not mean that their use in scientific knowledge subtracts objectivity from them. The second consideration, which is related to the previous one, is that the distinction between the context of discovery and the context of justification, even if it is controversial, could be applied in this case. Thus the recognition of the social origin of scientific concepts would be perfectly compatible with the recognition of the objective character of scientific beliefs, in particular those held in the natural sciences:

> Our lives would be based on positive scientific truths, which would be considered established, and the rest would be the domain of intellectual doubt. *I accept that this is so with regard to knowledge about the physical world, but it cannot be the case as far as the human and social world is concerned*. In these areas, science is still in a rudimentary state. Its methods of investigation are difficult, since direct experiment is impossible. Under such conditions it is not hard to understand why ideas expressing social matters in a really objective way are still rather rare. (Durkheim 1983: 90) [Our italics]

Our comments on Durkheim's position could also be extended to the case of Karl Mannheim. Bunge states that, unlike his predecessors Weber and Durkheim, Mannheim did not construct any detailed theory nor conducted specific research -although he did emphasize the social conditioning of ideas and the importance of the sociology of knowledge as a complement to the history and the psychology of knowledge- (Bunge 1991: 525-526). But Mannheim is nonetheless considered the father of the sociology of knowledge because he coined the very expression "sociology of knowledge", and also that of "style of thought". Bunge adds that Mannheim never argued that all sciences have a social content and identifies that refrainment as the reason why Mannheim was criticized by later sociologists of science. Mannheim did face the need to take into account the social and historical situation of cognizant subjects to understand the meanings of their beliefs.

> One of the two directions taken by epistemology emphasizes the prevalence of situational determination, maintaining that in the course of the progress of social knowledge this element is ineradicable, and that, therefore, even one's own point of view may always be expected to be peculiar to one's position. (Mannheim 1954: 269)

However, Mannheim explicitly stated that recognizing the influence of existential conditions does not imply embracing relativism and renouncing the postulate of objectivity. This author remarked the contrast between relativism

and his own position, which he called *relationism*. The relational approach enables the possibility of achieving a new form of objectivity resulting from articulating the different perspectives and thus discovering a common denominator. (Mannheim 1954: 70-71)

In any case, the necessity of this relationist perspective is relevant with respect to the social sciences, and Mannheim points out its differences with respect to the perspective best suited for the natural sciences:

(a) In the case of existentially-determined thought, the results of the thought process are partially determined by the nature of the thinking subject.
(b) In the natural sciences, thinking is carried on, in idea at least, by an abstract 'consciousness as such' in us, whereas in existentially-determined thought, it is –to use Dilthey's phrase– 'the whole man' who is thinking. (Mannheim, 2003: 129)

The American sociologist Robert Merton drove a major turn in the consideration of the relationships between social issues and scientific knowledge. In 1937, Merton reflected on the recent discipline that had received the name of Wissenssoziologie and pointed out that the word *Wissen* should be interpreted in a broad sense, that is to say not as referring to the physical sciences (except where explicitly indicated), but as referring to social ideas and thought. Merton titled his article *The Sociology of Knowledge* and also used this name to refer to Wissenssoziologie, in this way revealing some ambiguities with respect to that kind of studies. Bunge regards Merton as the true founding father of the sociology of science, both as a science and as a profession (Bunge 1999: 152).

Merton considered that the investigation of the influence exerted by social factors on the emergence and acceptance of scientific theories is extremely relevant, but held that the study of these problems does not amount to questioning the validity of knowledge. At the same time, his sociology of science pays particular attention to the social world of scientists, i.e. the organization and functioning of institutions (at various levels) that are the contexts in which scientists perform their professional tasks (Merton 1937).

Clearly, the conceptions we have mentioned about the relationship between scientific knowledge and the social circumstances of its production correspond to an initial stage of the modern philosophy of science animated by positivist ideas, both in its original incarnation and in neo-positivism. The protagonism of these ideas began to weaken in the mid-20th century as a result of the growth of critical attitudes whose main spokesmen were Willard van Orman Quine, Ludwig Wittgenstein, Thomas Kuhn and, a little later, Paul Feyerabend.

Very often, Quine is presented as a sort of philosophical parricide, since he went from being in close contact with the logical positivists—and with Rudolf Carnap in particular, after the dissolution of the Vienna Circle—to subject the main theses of this movement to severe questionings. In his classic article *Two Dogmas of Empiricism,* Quine doubted that a distinction could be made between

analytic and synthetic statements. Inspired by Duhem, he also argued for a holistic conception of empirical testing. Years later, he presented his *naturalized epistemology*, which explicitly manifests his rejection of the use of any first philosophy for grounding scientific knowledge, and concludes that "[...] epistemology, or something like it, simply falls into place as a chapter of psychology and hence of Natural science. It studies a natural phenomenon, viz., a physical human subject" (Quine 1969: 82).

It could be said that throughout the rest of his life Quine devoted his efforts to try to reconcile the old empiricist creed, namely, that *all possible knowledge is based on the experience of the senses*, with the richness of the network of our beliefs.

However, Quine's proposal that philosophy should be replaced by the natural sciences to explain the transit from sensitive stimuli to scientific beliefs has given rise to certain questions. In the first place, it is quite evident that naturalized epistemology gives rise to the accusation of incurring a vicious circle, for to the extent that epistemology must validate a form of knowledge, natural science would validate itself. Faced with this foreseeable objection, Quine replied:

> This interplay [the reciprocal inclusion between natural science and epistemology] is reminiscent again of the old threat of circularity, but it is all right now that we have stopped dreaming of deducing science from sense data. We are after an understanding of science as an institution or process in the world, and we do not intend that understanding to be any better than the sciences which is its object. (Quine 1969: 83-84)

In our view, this argument by Quine seems to be fallacious or at least unconvincing, because the alleged failure of foundationalism is not enough to justify a vicious circle. Nor is it entirely true that epistemology can be dispensed with in a more classical sense. In fact, to put previous empiricist theses in difficulty, Quine used concepts and arguments that are not strictly scientific but traditionally philosophical. Thus, his arguments in favor of an ontology committed to the existence of theoretical and abstract entities (such as atoms and sets, respectively) are outside science in a strict sense. These are philosophical convictions. Proof of this is that other philosophers, notably Bas van Fraassen, deny that the acceptance of a scientific theory entails anything more than a belief in its empirical adequacy (van Fraassen 1980).

In the mid-20th century, the philosophy of science, until then under a heavy influence of the agenda dictated by logical empiricism, was also questioned by an incipient current that suggested another form of naturalization of epistemology. This current is less based on knowledge arising from psychology and more on examining the history of science. Kuhn's *Structure of Scientific Revolutions*, which called for "a role for history", tried to show that the role of observation and reasoning in the development of scientific beliefs was much less important than most philosophers of science then claimed. Kuhn was foremost leader and convener of this position, but it was Feyerabend who

took the critique of classical epistemology to the extreme by proclaiming that there was nothing that could be called *the scientific method*, except for the maxim "anything goes" (Feyerabend 1975). From this latter perspective, science has no better cognitive credentials than any other set of beliefs. By reducing the importance of observation and logic in the process of acquiring, maintaining, or replacing scientific theories and instead emphasizing the role of persuasion as well as the similarity between scientific change and religious conversion, Kuhn opened the door for some social scientists to try to reduce the emergence and development of scientific ideas to a set of social factors. The relativistic attitude expressed in Wittgenstein's late ideas and adopted by Norwood Russell Hanson and Stephen Toulmin also intervened in this process.

Facing criticism and rejecting exaggerated and sometimes absurd uses of his ideas (especially that of paradigm), Kuhn weakened the scope of the relativism emerging from his work and ended up punctuating two or three ideas that is convenient to underline. He acknowledged that his interpretation of the history of science was misleading from the outset, because it was motivated by philosophical convictions (Kuhn 1992; Gaeta 1996). He also disavowed the possibility that attempts such as the one carried out by the Strong Programme were a consequence implied by his own ideas. Finally, he said that he respected the authority of science and that it had never been his intention to question it. However, many social scientists felt stimulated to develop historical, sociological, anthropological, and ethnographic studies, among others, supposedly protected by the doctrine latent in *The Structure of Scientific Revolutions*. In our interpretation, this type of studies implys, in some cases, an equivocal way—even for their own authors—of exchanging roles between epistemology and sociology of science, just as Kuhn became aware that his perception of the history of science had been contaminated by philosophical presuppositions. In this regard we shall consider in the next section the case of the Edinburgh School, and, in the subsequent section, the ethnographic research carried out by Steve Woolgar and Bruno Latour.

The Sociology of Scientific Knowledge

Barry Barnes and David Bloor, the best-known representatives of a refreshing trend inspired by the ideas of Wittgenstein and Kuhn, prompted what became known as the "Strong Programme" (SP). Although its natural location seems to be the sociology of science, they preferred to re-baptize the discipline under the name "sociology of scientific knowledge" (SSK) to differentiate themselves from Merton and his school. The expression "Strong Programme" indicates that, unlike Marx, Durkheim, and Mannheim, they do submit mathematics and natural sciences to sociological analysis (Kukla 2000: 7).

Before proceeding to analyse the main ideas of SP it could be useful to make some remarks about the relationship between the concepts of belief, scientific knowledge, and sociology. Traditionally, to say that a person S knows p (where p stands for "It is raining", "The Earth is round", or whatever)

meant that (a) p is true, (b) S believes p, (c) S is justified in believing p. Currently, the situation is different. Everybody accepts condition (b) because it makes no sense to attribute the knowledge of p to S unless S believes p implicitly or explicitly. Things are different about conditions (a) and (c). Most philosophers think that it is possible that we could never be sure that even the most prestigious recent theories are true. As a consequence, they content themselves with sufficient justification of beliefs. In the literature of philosophy of science, then, it is very common to talk about scientific knowledge in the sense of justified beliefs about scientific matters.[1] Authors disagree nevertheless about what should be the conditions for a scientific belief to be justified. Inductivists stress the confirmation value of fulfilled predictions, while falsationists assess a scientific theory as corroborated when it has overcome severe empirical tests. An alternative view about justification of beliefs is the one developed by Martin Kusch under the name of communitarian epistemology (Kusch 2002). He claims that the main source of support for our empirical beliefs is social institutions. The basic assumptions that animate this position are, first, the idea that knowledge is a social status. It is claimed, for example, that "knowledge" is a social kind term like "marriage" (Kusch 2002: 165). Secondly, the subject of beliefs is social rather than singular, which gives rise to communitarian beliefs. For a belief to be justified, it is necessary that it becomes a communitarian belief. These are justified within a social epistemic community formed by epistemic subjects who interact in a certain context.

"Science" and "knowledge" are words that can refer to both scientists' activities or the results of those activities. So, the important thing is not the name but the focus of our interest. Bunge claims that the sociology of science is one of the branches of the sociology of knowledge.

Some authors who serve in the SP army interpreted Kuhn's ideas as an invitation to replace philosophy of science with sociology of scientific knowledge. Among its most distinctive features, the main purpose of this programme was to advance a descriptive rather than a normative perspective or a rational reconstruction of scientific activity, paying special attention to the aspects belonging to the context of justification, which had previously been left out of sociological examination (Zammito 2004: 140). Secondly, and very consistently, SP followed what was called the "principle of symmetry", intending to provide a sociological causal explanation of any scientific belief, no matter whether that belief was true or false, rational or irrational. The model that inspired these proposals, together with the Kuhnian historicist tilt, was the task usually carried out by anthropologists when examining the customs and beliefs of primitive societies. Thus, philosophical arguments gave way to historical and sociological studies with the objective of determining how social circumstances made scientists harbour certain beliefs about their objects of study. Eventually, these objects were seen as social constructs.

[1] For instance, see the use of the term in Popper's *Objective Knowledge*, in Lakatos-Musgrave *Criticism and the Growth of Knowledge* and in Ayer's book *The Foundations of Empirical Knowledge*.

Although the development of SP by several authors led to discrepancies among different contributors, it is appropriate to recall some statements made by Barnes about the philosophical implications of this project (Zammito 2004: 134). Barnes noted that SP adopted a sceptical and relativistic methodology. That methodology could lead to argue that we cannot attribute more rationality or greater proximity to reality to one system of beliefs than to any other (Barnes 1974). Barnes recognizes that some researches made in the context of SP leave the feeling that knowledge of nature, being socially constructed or negotiated, has nothing to do with reality[2]. But he thinks that such conclusion is a by-product of some exaggerated enthusiasm for sociological analysis that is not shared by most sociologists, who accept that the real world plays an effective role in our knowledge. The world interacts with our knowledge and somehow restricts the beliefs that scientists may harbour about it. The impression that the real world played no role within the conception of SP was certainly what led Kuhn to disavow the claim that the supporters of SP could be considered Kuhnians.

Let's see how we would typecast the theses explicitly or implicitly included in SP, according to recently proposed criteria for characterizing the scope of scientific realism. André Kukla (1998**)** distinguishes three kinds of scientific realism: semantic, metaphysical, and epistemic. A semantic scientific realist is everyone who accepts—versus instrumentalism and reductionism—that theoretical claims are to be understood literally, so they do have truth values. Metaphysical scientific realism is the view that theoretical objects beyond the observable world do exist. Epistemic scientific realism is the view that we can know that certain theoretical entities exist. At the semantic level, and despite Barnes' claims above mentioned about the realist convictions of SP supporters, we think that SP does not adhere to any form of scientific realism. It is true that the principle of symmetry, which recommends that the same treatment should be given to both true and false beliefs, seems to imply that SP accepts the distinction between true and false statements. But the use of these terms does not mean that SP supporters are willing to concede that scientific hypotheses actually have truth values. What they mean is that we must offer the same type of explanation, that is, that we have to take into account the effect of social causes in the adoption of any belief, whether that belief is considered true or false.

The principle of symmetry becomes essential to the relevance of SP since it contrasts with a criterion shared by previous sociologies of science, namely, that adopting an obvious true belief does not require any further explanation apart from the fact that it is obviously true. If that is the case, what deserves a sociological explanation is the assent granted to unjustified beliefs. To show that this classic premise was wrong, Bloor tried to prove that even the conviction that "two times two equals four" must be explained in terms of social causes (Barnes, Bloor and Henry 1996: 182-183). Bloor argued that the apparent absurdity of demanding a sociological explanation for this belief

[2] We will consider the concept of social construction in the next section.

comes from assuming mathematical realism and believing that mathematical truths are valid by themselves, no matter whether anyone thinks about them. On the contrary, Bloor sees mathematics as an institution, and the strength with which mathematical results are imposed in our minds is something similar to a moral imposition internalized in human beings.

That is a rather forced interpretation of the dominant character of social causation, and its sole purpose is to extend social causation to the field of mathematics, a move facilitated by the problematic ontological status of abstract entities. But the universal recognition of mathematical operations, especially the most basic ones—which, in addition, contrasts with the cultural variability of moral standards—leads us to think that attributing the unanimous belief that "two times two equals four" to social causes is wrong. It is not even necessary to imagine that there are social causes shared by different cultures, for the coincident beliefs could be the result of inborn mental mechanisms, as rationalists argue. Or, if we prefer an option closer to empirical research, the assent to elementary mathematical truths could be explained as the result of the intellectual maturation of human beings, in the way suggested by Piaget's experiments. There is also a non-inborn alternative: attributing the recognition of the validity of arithmetic to the perception of the most common empirical regularities, as John Stuart Mill argued. Bloor's reasoning is doubtful. He argues that only mathematical realism can lead us to question whether elementary mathematical beliefs deserve a social explanation. He does not consider other alternatives like the ones that we just mentioned, although any of them could explain, in principle, the belief in mathematical truths without a commitment to mathematical realism. Moreover, even assuming that mathematical realism is the reason for the rejection of sociological explanations of mathematical beliefs, Bloor should embark himself on a philosophical discussion to show that mathematical realism is untenable, instead of taking it for granted. Furthermore, Bloor's argument itself is paradoxical, because he attempts to explain the belief that it is not necessary to give sociological explanations of mathematical beliefs by considering it caused by the fact that some people harbour a wrong doctrine about the truth of mathematical propositions. But we think that accepting any doctrine—in the present case, mathematical realism—is a propositional attitude, not a social cause.

In principle, the situation described above could be different in the case of beliefs about the everyday world or the natural world, as frequently they consist of perceptible entities and facts, and therefore it seems reasonable to think that many of the beliefs that people have are explained precisely by the perception of the surrounding world (Grice 1988). On the contrary, it is not easy to explain how we establish mathematical truths because they are often associated with metaphysical components. Mathematical entities, objects of pure geometry, and classes are often considered as abstract entities. This kind of ontology produces distrust among those who prefer keeping away from metaphysics. Beliefs about what can be perceived directly, however, do not clash with that difficulty. We have seen that none but the most extremist among the SP supporters question the existence of an independent physical

world. This attitude is consistent with the basic conviction of common-sense realism, i.e. taking for granted the existence of a reality that exists by itself. However, that assumption is undoubtedly a metaphysical thesis. In any case, advocates of SP are willing to commit themselves not only to the existence of a physical world but also to the idea that reality exerts some restrictions on the beliefs harboured about it. Nevertheless, they do not believe that perceptual factors are sufficient to explain any belief:

> There is no need for a relativistic sociology of knowledge to take anything other than a completely open and matter-of-fact stance toward the role of sensory stimulation. The same applies to any other of the physical, genetic or psychological and non-social causes that must eventually find a place in an overall account of knowledge. The stimulation by material objects when the eye is turned in a given direction is indeed a causal factor in knowledge and its role is to be understood by seeing how this cause interacts with other causes. There is no question of denying the effect on belief of the facts (Barnes and Bloor 1982: 33).

These statements, insofar as they make some concessions to the causal theory of perception, seem to go beyond what Michael Devitt has dubbed "fig-leaf realism", i.e. the view that there is an independent world but we cannot have any knowledge about its features. One wonders whether the commitment to realism proclaimed by SP is consistent with the principle of symmetry. This is because referring to sensory stimulation and to non-social factors involved in genetic or psychological explanations seems to complicate the possibility of maintaining the requirement that the explanations of any belief should be based on the same type of causes: "The form of relativism that we shall defend is that all beliefs are on a par with respect to the causes of their credibility" (Barnes and Bloor 1982: 22). It is at least doubtful that this kind of realism actually acknowledges the role of external sensory stimulation. Hallucinations (even if they are collective) do depend on several causes other than "stimulation by material objects". But according to various epistemological doctrines, sensory stimulation plays a crucial role in the beliefs that have to do with the existence and the observable properties of physical phenomena, and it is difficult to imagine how social factors could have decisive influence on such beliefs. So stimulation by material objects has the most important role in many of our basic beliefs. For example, if a torrential rain is soaking someone, it is not easy to imagine how social conditions could change the belief that she is experiencing the effects of what in our language we call "rain"; in such cases, sensory stimulation seems absolutely determinant of the belief held by the subject. Of course, the certainty that it is raining may be accompanied by many other opinions related to it. If the person is a meteorologist, for instance, she will harbour certain beliefs about the causes of rain, while if she is someone immersed in a culture convinced that torrential rains are manifestations of a god's anger, the person will be inclined to give a religious interpretation.

It is obvious that the social circumstances in which both subjects in our example are situated have exerted some influence in their beliefs about the causes of the rain, although it is highly questionable whether such social circumstances have produced some effects on their thoughts that the rain is taking place, whatever could be the way of placing them in the sets of their respective beliefs. Nor would it be denied that scientific beliefs are far beyond the verification of a simple fact such as that it is raining. But at this point we could ask if there is any advantage in looking for the same kind of explanations for all beliefs, and we could also ask what could be the meaning of including social causes in order to explain any kind of beliefs. Of course, sociologists need not justify their interest in studying, with the resources of their discipline, any event in which human beings interact. But it seems that SP goes beyond the intention of extending sociological researches to a point not reached by the classical sociology of knowledge, i.e. mathematics and sciences. We think that the intention of the programme is also to overcome the Mertonian sociology of science and to question the supposed supremacy of scientific knowledge; it was precisely this issue that caused Kuhn's displeasure regarding this orientation. As we have just suggested, the Strong Programme aimed to replace philosophy of science with sociology of science, but at the price of transforming sociology in philosophy of science.

A strong evidence in favour of the fact that SP is more a philosophy of science than a scientific inquiry is based on its explicit commitment to relativism. One might think that this confession of relativism functions as a methodological resource, as it occurs sometimes when scientists adopt a sceptical attitude in order to avoid the assumption of dogmatic beliefs. Nevertheless, relativism is a strong philosophical thesis. Kukla (2000) offers a definition of epistemic relativism as the thesis that there are no absolute guarantees for any rational belief. For an epistemic relativist, guarantees only make sense relatively to a culture, an individual, or a paradigm. It is important to note that Kukla establishes, against what Barnes, Bloor, and some of their critics have affirmed, that the principles that characterize the sociology of scientific knowledge do not imply epistemic relativism. In Kukla's eyes, this statement favours the discipline because he shares the view of many philosophers that relativism is incoherent. But even when SP does not involve epistemic relativism, Barnes and Bloor adopt relativism without reservations. Besides endorsing the principle of symmetry, they state that SP "dictates epistemic relativism" (Barnes and Bloor 1982: 22-3). They also assert: "For the relativist there is no sense attached to the idea that some standards or beliefs are really rational as distinct from being accepted as such" (Barnes and Bloor 1982: 27).

The most prominent advocates of SP grant metaphysic realism, but this concession is not enough to assure its compatibility with semantic realism, because of the great significance attributed to the symmetry principle and their explicit adhesion to relativism. The notion of truth they adopt is relativized and it seems to be dissolved in what is considered rational or true. So within SP it makes no sense to say, in accordance with semantic realism, that scientific

hypotheses can be true or false simply because of their relations to the world. And by the same token, we do not expect that for advocates of SP theoretical terms refer to real entities. Moreover, if this were the case, it would be possible that some hypotheses—at least in certain improbable circumstances—reached some approximation to the truth in a non-relativistic sense of the word.

These considerations are projected on the ontological level. Although, as we have observed, the kind of realism endorsed by SP seems to be more generous than that of the *fig-leaf* realist, it is doubtful that its allegedly stronger realism turns to be more defensible. This is because the role given to sensory stimulation vanishes in the context in which social causes play the main role. So, the outcome of combining social with non-social causes is contingent and unpredictable. SP is not willing to admit that the belief "It is raining" may be true simply because it is raining and that the belief would be assessed as true by anyone who perceives the phenomenon in normal circumstances. If social factors play some causal role in our beliefs, even if it is not exclusive, then they have to alter beliefs in some way. Otherwise, the requirement of taking them into account lacks any purpose. But if the truth of "It's raining" is not determined by sensory stimulation, if it is "on a par" with any other belief (e.g. "God is angry"), then the role of the external world and sensory stimulation is more nominal than real, as a commitment to it sounds in no way consistent with the general attitude of SP. In this respect, SP is not able to assume either an authentic ontological commitment to common-sense objects or to theoretical entities.

The above comments show that the symmetry principle and the semantic and ontological relativism subscribed by SP undermine the importance of recognizing the role of the external reality and its effects on our common sense as well as on our scientific beliefs, and so it falls into epistemic relativism.

In the next section we will examine a line of research related to SP and the issue of relativism.

Social Constructivism

An important branch of SP made a transition from the basis of a theoretical project to the attempt of applying its principles to empirical studies about how scientists carry out their researches. In this respect the work of Bruno Latour and Steve Woolgar (Latour and Woolgar 1979; 1986) reached celebrity. Along a period of two years they observed, as ethnographers usually do, the activities of a group of scientists who were attempting to isolate and identify a biological substance. The results of their reports and their corresponding interpretations—which in our opinion involve arguable philosophical statements—were published by Latour and Woolgar in *Laboratory Life: The Social Construction of Scientific Facts*. The subtitle recalls a far precedent book published in German by the Polish doctor Ludwik Fleck (Fleck 1979). It was translated into English, also in 1979, with the title of *Genesis and Development of a Scientific Fact*. The present interest in this book, which had not much dissemination

when it was first published, is mainly due to the fact that it exerted some early influence on Kuhn's ideas. Fleck describes a set of episodes related with the ways in which syphilis was considered throughout the centuries, a process that finally culminated in the discovery of the Wassermann reaction, a reliable procedure to make a correct diagnosis of the disease. The author makes an epistemological analysis of these episodes and characterizes a scientific fact in the following terms: "A fact is supposed to be distinguished from transient theories as something definite, permanent, and independent of any subjective interpretation by the scientist. It is that which the various scientific disciplines aim at" (Fleck 1979: xxvi).

But the expression "scientific fact" can get different meanings. One of them is the traditional one, here mentioned by Fleck. We may rebuild it as a scientific statement or a well-established belief whose definitive status contrasts with the transitory validity of a mere hypothesis. Another meaning, closer to that suggested by Fleck and the fallibilist epistemologists, is to understand a scientific fact as a statement or a well-established belief but without any indubitable or complete guarantee of definitive permanence. In this respect, some have claimed that the evolution of biological species is a fact, while the way in which it takes place remains in a more hypothetical level.

Neither of the two meanings mentioned above, however, fits the current ontological distinction. Presently we locate the (natural, social, psychological, etc.) facts at one level, and beliefs, statements, or propositions at a very different level. What needs to be justified in a greater or lesser degree, in accordance with the available evidence, is the belief that certain fact has taken place. Thus the process of biological evolution, if it did happen, may be a fact if and only if it really occurred and developed over millions of years independently of the fact that anyone could ever think that it happened. But the belief and the statement claiming the occurrence of the process are not facts. This way of distinguishing facts from beliefs, statements, or propositions goes in parallel with the distinction between an object and the corresponding concept. Many people have the concept of a unicorn, but it is nearly impossible for anyone to believe that the referred object exists.

These distinctions just formulated between an object and a concept or a fact and a belief are too elementary and we wouldn't have mentioned them had the possibility of thinking the problem from a realist point of view not been presupposed. This does not mean that one should adopt realism, but only that one should understand the realist thesis. However, some philosophers consider these distinctions problematic, as is the case of Nelson Goodman. In a similar way, social constructivists slide from the social construction of concepts, hypothesis, and beliefs towards the social construction of objects and facts. Latour and Woolgar, for example, claim that they have attempted to avoid using language that could seem to engage them with realism. Even if it is a long quotation, it is worth reproducing their words literally:

> We have attempted to avoid using terms which would change the nature of the issues under discussion. Thus, in emphasising the process whereby

> substances are constructed, we have tried to avoid descriptions of the bioassays which take as unproblematic relationships between signs and things signified. Despite the fact that our scientists held the belief that the inscriptions could be representations or indicators of some entity with an independent existence "out there," we have argued that such entities were constituted solely through the use of these inscriptions. In order to stress this point, we have eschewed the use of expressions such as "the substance was discovered by using a bioassay". To employ such expressions would be to convey the misleading impression that the presence of certain objects was a pregiven and that such objects merely awaited the timely revelation of their existence by scientists. By contrast, we do not conceive of scientists using various strategies as pulling back the curtain on pregiven, but hitherto concealed, truths**.** Rather, objects (in this case, substances) are constituted through the artful creativity of scientists. Interestingly, attempts to avoid the use of terminology which implies the preexistence of objects subsequently revealed by scientists has led us into certain stylistic difficulties. This, we suggest, is precisely because of the prevalence of a certain form of discourse in descriptions of scientific process. We have therefore found it extremely difficult to formulate descriptions of scientific activity which do not yield to the misleading impression that science is about discovery (rather than creativity and construction). It is not just that a change of emphasis is required; rather, the formulations which characterise historical descriptions of scientific practice require exorcism before the nature of this practice can be best understood. (Latour and Woolgar 1986: 129).

It seems apparent that the claims stated by Latour and Woolgar place them among the advocates of antirealism, but the case becomes a bit confusing because they say that they are realists and deny being relativists: "We do not wish to say that facts do not exist nor that there is no such thing as reality. In this simple sense our position is not relativist" (Latour and Woolgar 1979: 182). In spite of this justification, the preceding paragraph clearly shows the ontological position of these authors. The entity "socially created" in the laboratory by the social activities of scientists is, essentially, a result coming from their beliefs, arguments, inscriptions and, in particular, the publication of papers and their impact. If we are going to speak in conventional philosophical terms, we should say that the socially-created entity has only an *ideal* existence. Therefore, to say that it is in any case real is to go far from the current use of terms. Woolgar and Latour not only try to avoid committing with realism, they also commit to a type of metaphysical antirealism, because, as Kukla claims,

> According to Latour, Woolgar, Knorr-Cetina, Collins and Pickering *(inter alia),* it's not only scientific *beliefs* that are socially constructed – it's scientific *facts*. If the social history of science had been sufficiently different, we wouldn't, according to SSKists in general, have the beliefs that we do have about quarks. This is a thesis which is relatively easy to

> swallow. But Latour *et al.* go further. They claim that if social history had been appropriately different, *there wouldn't be any quarks*. (Kukla 2000: 9)

A fortiori, we can't attribute to those authors any affinity with semantic or epistemic realism. So, their strong scientific antirealism in no way could come from a neutral scientific research. It comes from metaphysical postulates, no matter whether they are justified or not. We think that investigations as the one undertaken by Latour at the laboratory can be of great interest to ethnography, sociology, anthropology, history of science, and possibly other disciplines. But we do not believe that we can derive from that sort of empirical researches, philosophical principles or arguments very relevant to the epistemological and metaphysical discussions. We do believe, in any case, that those supposedly neutral researches carry assumptions that seem to be empirical conclusions while they conceal their true origin as hidden philosophical premises.

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