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British Discussion of the Explanation of Science: The Sociological strong Program versus Philosophical Rationalism

The aim of this paper is to analyze both positions mentioned in its title, to repent the explanatory ideas elaborated by proponents of these approaches, reveal their presuppositions, discuss them critically and then offer a draft of a third position which--I hope--would be free from some weaknesses of both the points of view analyzed.[1]

1. Assumptions and conclusions of rationalism and the strong programme

That all knowledge is theoretical and conjectural is an idea accepted now by rationalists as well as by sociologists. Both sides regard particular bodies of scientific knowledge (theories, research programmes, systems of beliefs, etc.), and scientific activities, are determined by, and relativized to some factors. David Bloor, one of the founders of the sociological strong program, claims that "all knowledge is relative to the local situation of the thinkers who produce it" (Bloor 1980, 142). That claim seems to be accepted by both positions I analyzed, though the sense each gives to the phrase "local situation" is different, as will be shown.

The conjectural nature of all scientific knowledge means that it is imperfect, which amounts to this: any particular system of knowledge can be abandoned, rejected, improved--that is, changed. The fact that knowledge is changeable and thus has its own history is admitted by rationalists and sociologists alike. Both parties assume that the changeability of knowledge is connected with or caused by the fact that scientific knowledge is manufactured by real human beings that are not ideal knowing subjects. Rationalists and sociologists make change of science their main target and offer an explanation of it, the details of which will follow, In a sense their conceptions have a common point of departure: the fact that theories are underdetermined by data, in the sense that a certain set of data can be explained or predicted by different theories. If arguments from empirical data are not sufficient for deciding which theory is cognitively valid, or more valid than rival theories, and should be accepted, there arises the problem "about the character of the extra-empirical conditions that constrain theory-choice" (Hesse 1982, 326). This is a question common to both approaches. The attempt to answer it leads either to the rational models of theory-choice, or to the sociological conceptions of the factors influencing scientific practice.

Neither rationalists nor sociologists think that historically differentiated scientific knowledge is produced by a kind of a singular "pure self", although some rationalists explicitly do not bother about the subject of cognition. If science is not build up by a "pure self" it must be regarded as the product of many real subjects. This means that science is a social enterprise. The proponent of the rationalist philosophy of science claims: "The choice of theories is a social matter carried on in a dialectical fashion" (Newton-Smith 1981, 225), and the sociologist describes science as "a routinely-established, securely-financed activity, an activity carried out by trained and qualified professionals" (Barnes 1985, 8). For rationalists, the social nature of science boils down to the fact that scientific knowledge is established by rational discussion and criticism whose hidden structure is logic, being "both a theory of proof or justification and a theory of criticism" (Jarvie 1976, 329); to the sociologists it means that science is constituted by social institutions. For both sides, to take into account the social aspect of science means considering scientific theories in the context of scientists' acts of committing and of establishing or destroying consensus. The fact that "all scientists do manage to come to reasonable consensus about theories in spite of underdetermination"

(Hesse 1982, 326) requires explanation. This can be constructed in two different ways, by a rational or by a sociological model of scientific negotiation.

Having assumed the historical and social nature of science, philosophers and sociologists focus their attention on three main problems: (1) what determines scientists; (2) how and why science changes; and (3) how scientific consensus is possible. The constituents of rationalist and sociological answers to these questions can be sketched as follows.

1.1. The rational model of science

Rationalists believe that scientists are determined by the principle of scientific rationality. According to Karl Popper, this leads us to interpret an action "as an attempt to solve a problem" (Popper 1975, 179). This rational principle which he applied to a situational analysis of scientists' activity, may be characterized as a realist and methodologically substantive rule. This is a realist principle because it posits truth, "in the sense of correspondence to the facts or to reality", to be the goal of science. It is a methodologically substantive rule since it specifies the cognitive procedures which form the method of rational criticisms and simultaneously the method of producing theories as well as the method of the growth of objective knowledge. This means that any past, contemporary or future scientist who produces a piece of scientific knowledge and contributes to its growth is determined by the goal and method of science specified by the Popperian methodology, as well as by the existing body of scientific knowledge, "linguistically formulated theories", conjectures, problems, discussions, etc.; that is, by the existing content of the third world (Popper 1975, 73-74).

Thus according to Popper himself, his methodology is simultaneously a rational theory of the growth of scientific knowledge. However followers of Popper, namely, Imre Lakatos and William Newton-Smith, argue that there is no necessary or automatic transition from the Popperian model of scientific method to the idea of increasing *verisimilitude* which is the basis of his conception of the growth of objective knowledge. It was demonstrated by David Miller and Paul Tichy that "no two false theories are comparable in regard to verisimilitude" (Newton-Smith 1981, 58). This means that eliminating false theories does not guarantee an increase in verisimilitude. It turns out that the Popperian concept of rationality, which is appropriate for considering the local situation of a scientist who produces a (more) true explanatory theory, is not applicable to the local situation of a scientist participating in the development of science since this participation is regarded by the post-Popperian rationalists to require an act of choice between rival theories. Undoubtedly, the act of producing and empirically justifying a scientific theory is different from the act of choosing from (two) rival theories the better one, though some components, e.g., observations, are involved in both cases. While the distinction between the situation of producing and the situation of choosing a theory is introduced, it allows us to notice and understand the inner gap in the Popperian philosophy; his methodology is a conception of subjective activity (though the agent is not a real scientist but an ideal subject "generated" by the methodological rules) whereas his conception of the growth of knowledge is "epistemology without a knowing subject". Thus it is not astonishing that--in the opinion of post-Popperians-- Popper "has failed to deliver a rational model of science" because to follow the falsificationist methodology does not guarantee that successive theories have an ever-increasing degree of verisimilitude (Newton-Smith 1981, 75).[2]

The gap between the methodology and the theory of the growth of knowledge also has another consequence which is convergent with Popperian methodological individualism. Popper does not notice the social aspect of scientific production. When one considers the elaboration of scientific theories, the only rival theory--according to

Popper--is a psychological theory, but this should be eliminated since the psychological process which leads up to a subjective state of understanding "must be analyzed in terms of the third-world objects in which it is anchored" (Popper 1975, 164). When the supra-individuality process of the development of science is investigated, the realm in which it takes place, that is, the third world, is conceptualized by Popper as an intended or unintended by-product of inventions, books, arguments; it is an objective, autonomous realm governed by its own logic so its explanation does not need any reference to sociological analyses.

The shift in the approach to rationality, from consideration of the rationality of theory-construction and theory-evaluation to a search for the rationality of theory-choice, is visible in the Lakatosian conception--together with some problem and troubles that follow his modification.

Lakatos says that the term "'normative' no longer means rules for arriving at solutions, but merely directions for the appraisal of solutions already there" (Lakatos 1976, 103) and considers rationality with respect to theory-appraisal and choice. According to him, it is rational to follow the negative heuristic of a research programme and to maintain its hard core "as long as the corroborated empirical content of the protecting belt of auxiliary hypotheses increases", and rational too to abandon hard core "when the programme ceases to anticipate novel facts" as well as to stick to "a degenerating programme until it is overtaken by a rival and even after" because it is "perfectly rational to play a risky game" (Lakatos 1976, 4-9, 117) What is irrational is "to deceive oneself about the risk" or to deny the "poor public record" of a programme that one sticks to (Lakatos 1976, 117).

It seems that the Lakatosian rationality connected with the appraisal of existing programmes is a mixture of a critical attitude and a sort of intellectual honesty. This means that when qualifying someone's action as rational Lakatos does not determine what he did but only how he did what he did. Only the adoption of such a meta-criterion of rationality allows the assertion that scientist who sticks to a degenerating programme and does not deceive himself (or others) is rational in the same sense and degree as a scientist who abandons a degenerating programme in favor of another which--I think we should add--he perceives as a progressive programme. However, Lakatos fails to use a purely instrumental concept of rationality, for he determines directly the sets of rational and irrational scientific actions or events. Although he includes into the set of rational events some of those which had been regarded as irrational from previous methodological point of view, this does not mean that he regards all scientific events as rational.[3]

It is not surprising that having adopted such a meta-criterion of rationality Lakatos claims that "any rational reconstruction of history needs to be supplemented by an empirical (socio-psychological) 'external history'" (Lakatos 1976, 102). Although the internal history of science that consists in rational reconstruction "is self-sufficient for the presentation of the history of disembodied science, including degenerating problemshifts" (Lakatos 1976, 117), the empirical external history is necessary since "human beings are not completely rational animals; and even when they act rationally they may have a false theory of their own rational actions" (Lakatos 1976, 114). One may expect that the complete explanation of any scientific event needs both rational reconstruction and psycho-sociological, historical narrative. However, the Lakatosian claim about the necessity of taking into account psycho-sociological circumstances is ere rhetoric; internal and external histories cannot be combined into a coherent explanation. The internal history is not a historical narrative, it is a more or less radically improved version of historical facts; and even if we agree with Lakatos that this does not mean "fabricating artificial historical examples", an empirical account of the psycho-sociological situation in which real scientists produce knowledge can explain none of the components of the events interpreted by internal factors. To justify this

opinion let mi consider Lakatos's own example, namely, the case of the Bohrian programme. Lakatos claims that "the historian, describing with hindsight the Bohrian programme, should include electron spin in it" though "Bohr, in 1913, may not have even thought of the possibility of electron spin" (Lakatos 1976, 119).[4] And Lakatos himself reconstructs Bohr's programme in this direction. The hero (or--rather--an object) of Lakatosian reconstruction is a theoretical construct, "Bohr"--that is, somebody who did think about electron spin. Of course, such a construct of an "ideal" scientist is not forbidden or philosophically impossible; but it is not clear in what sense one can say that an idealized reconstruction could be supplemented by external history. It is possible that there were psychological factors which, besides the theoretical, explained why the real Niels Bohr was not able in 1913 to take into account the possibility of electron spin. But these factors can neither be regarded as influencing "Bohr", because this ideal scientist was able to take into account the spin; nor an they be used to explain why the real Bohr did not think like an idealized "Bohr", for if they determine anything at all, they determine the real Niels Bohr's state of mind and not the fact that it differed from the state of mind of an ideal physicist.

Moreover, the Lakatosian claim about the empirical external history is misleading. What he really has in mind is a psychologistic philosophy of science that is a psychology of an ideal mind and offers "a psychotherapy for turning one's mind into an ideal one" (Lakatos 1976, 92-93). it seems that Lakatos's well known phrase about the relation between the philosophy and the history of science should be completed by the statement that the philosophy of science without the internal (i. e., rationalized) history of science is empty. But now the formula seems to be a little tautological, and the superficiality of the Lakatosian way of connecting philosophy with the history of science becomes easily visible.

I think that there are convincing reasons for saying that Lakatos failed to combine the rational and non-rational factors determining scientists, and he also failed to connect the rational account of scientists' activity with the idea of scientific progress. His idea of a kind of rationality that forbids only the instant acceptance of a better theory may rationally explain the succession of theories, but it is not sufficient fro explaining the progressive nature of this succession.

According to Newton-Smith the lakatosian failure is caused by his attempt to "articulate a methodology and employ it to determine which have been the progressive moves in the history of science"; whereas a methodological model and the idea of scientific progress should be arranged the other way round, since the primary interest of the rationalist approach is to explain progress in science, and this "is best understood as an improvement in the verisimilitude" of scientific theories. That there has been progress in science should--according to Newton-Smith--be established first, and "without the use of methodological principles". Then the vindication of a methodology is possible (Newton-Smith 1981, 97, 208). However, this does not mean that one universal methodological model may be elaborated. A temperate rationalist--which Newton-Smith declares himself to be--will "work with an evolving series of models in giving rational accounts of scientific activity" (Newton-Smith 1981, 97, 271) because the fundamental concept of rationality he adopts is instrumental rationality. To use such a concept of rationality is to depict the person as following the dictates of reason, that is, to show "that on the basis of the goals and beliefs of the person concerned the action was the means he believed to be most likely to achieve his goal" (Newton-Smith 1981, 241). The goals of scientists may be different; some are scientifically respectable, and these are specified by particular methodologies, others are non-scientific, but this does not imply the impossibility of their rational interpretation. In both cases the rational account of scientists' activity can be supplemented by sociological or psychological explanation. This means that actions that realize scientific goals, that--in general--are truth-related can be externally explained, and Newton-Smith gives such a complete external explanation when

saying that "we have a general interest in following the dictates of reason because it is an evolutionarily successful strategy" (Newton-Smith 1981, 243-247, 271-272).

From the point of view of Newton-Smith's model of instrumental rationality, different procedures can lead to the achievement of the goals held by scientists qua scientists; and--in fact--in the history of science different methodological principles for comparing theories, and different ideas of the good-making features of theories, have been operative. Nevertheless, the plurality of scientific procedures and rules does not exclude--in the opinion of Newton-Smith--the existence of a common core in those procedures. This core is the strategy for comparing theories in order to select the best available. Its essential element is the scientists' faculty "of making decisions without being able to provide an explicit justification at the time" (Newton-Smith 1981, 223-232, 234). To reconstruct rationally scientific procedures it is necessary to examine carefully "the history of science to see what principles have actually been operative"; and, in order to explain why there has been progress in science, it is necessary to show how the procedures and principles have been operative in bringing progress which means that it is necessary to attribute some degree of truth to the beliefs of past scientists about the goals and methods of science (Newton-Smith 1981, 97, 260-261).

However, plurality in the goals of scientists does not mean--for Newton-Smith--that the goal of science changes: "the goal of science is to achieve an understanding of the physical world through the production of explanations having predictive power". This, though, cannot be reached instantly but only gradually, and for this reason scientists "aim at theories of ever-increasing verisimilitude". Such a conception of the goals of science alone can render the progress of science intelligible and understandable (Newton-Smith 1981, 222, 268).

The replacement, which Newton-Smith introduces, of a methodologically substantive concept of scientific rationality by an instrumental concept is, undoubtedly, a step toward taking seriously the psycho-sociological aspect of science because it takes seriously the beliefs and goals of real subjects of scientific operations. The description of the knowing subject becomes, in a sense, less epistemological; the very possibility that scientists have non-cognitive goals is admitted and such scientists are not disqualified as irrational; the intuitive and thereby unpredictable judgments involved in scientific choice are taken into account; and scientific activity is seen as a dialogue proceeding within and between scientific communities.[5] Moreover, this replacement leads to a clearer separation of the historiosophy of science from its methodology which--according to Newton-Smith--is in a sense empirical and historical. The approach offered by Newton-smith, which admits that (scientific)) Reason manifests itself empirically and that its manifestations are historically dynamic, seems to be a far more promising rationalist proposition than those of Popper and Lakatos. However, when the epistemological claims about the goal of science and its method are concerned, Newton-Smith follows his predecessors, merely tempering their radicalism. In fact, he vacillitates between an empirical description of science as a factual historical process and a philosophical understanding which could overstep historical narrative and self-consciousness of scientists in order to avoid deception produced by appearances. He does not query the whole rationalist conceptual framework and clarifications he offers may be questioned.[6] The most important doubt concerns the concept of rationality. By introducing the concept of instrumental rationality, and reversing the relationship between methodology and the idea of progress in science Newton-Smith does not, in fact, contribute in any degree to the elucidation of scientific rationality: he relates it to the progress of science the aim of which is to increase verisimilitude, but he does not demonstrate that a global progress toward truth is produced by singular instances of elaborating relatively more true theories and only by these instances. In consequence, either he must accept that any theoretical change is scientifically rational or he should give criteria for deciding which

changes contribute to a global coming nearer to truth. It seems that scientific rationality remains itself a vague idea; and perhaps Jarvie is right, when he says that this "may explain why it is so much discussed" (Jarvie 1976, 311).

All those weaknesses of rational models are not sufficient for the general claim that no satisfactory rational explanation of science can be given. However, before discussing the extent to which rationalist ideas might be utilized, the opposite, that is, the sociological, approach should be considered.

1.2. A sociological model of science

In accordance with my preliminary remarks I restrict my comments to the sociological model of science elaborated by the Edinburgh school of the sociology of knowledge.

Its proponents define their own approach as scientific, sociological, and naturalistic. Their approach belongs to science, a modern form of knowledge, which is "causal, theoretical, value-neutral, often reductionist, to an extent empiricist, and ultimately materialistic like common sense" (Bloor 1980, 141). This approach is sociological in the sense that knowledge--from its point of view--is "whatever men take to be knowledge" (Bloor 1980, 2).[7] It is a naturalist approach because "it tries to understand the convictions and the concepts of different cultures as empirical phenomena" (Barnes 1982, 5), as do the natural sciences. In particular, the sociology of knowledge should be causal, that is, "concerned with the conditions which bring about beliefs or states of knowledge", and impartial, symmetrical, and reflexive in the sense that it explains all beliefs in the same way, true and false, rational and irrational, its own beliefs included (Bloor 1980, 4-5). These principles constitute the strong programme of the sociology of knowledge. The symmetry and reflexivity requirements express its methodological relativism; this is a form of general relativism which David Bloor argues to be the preferred alternative to absolutism (Bloor 1980, 142).

According to the first principle of the strong programme, knowledge should be treated causally, and relative to "the local situation of the thinkers" which includes: "the ideas and conjectures that they are capable of producing; the problems that bother them; the interplay of assumptions and criticism in their milieu; their purposes and aims; the experiences they have and the standards and meanings they apply" (Bloor 1980, 143-143). It seems that every rationalist could accept that scientists are influenced by all this. And though, in fact, the sociological model shares some ideas with conventionalism (Popperian included), the sociologists' way of understanding some elements of the scientists' situation differs from the rational one. Barry Barnes, who is a little more aware than Bloor of the differences between sociological and rational approaches, emphasizes the anti-individualist orientation of the sociological account. Science does not proceed "as the sum of the independent actions of individual scientists", they "are bound together into an organized and effective system of knowledge production" (Barnes 1985, 39). What makes science a collective activity is, first, "a prolonged and intensive training which gives them [<recruits> to science - B. T.] a strong tendency to think and act alike", second, the process of making public their research results, and third, the process of the diffusion of these results through the scientific community, their evaluation and incorporation into existing knowledge. Every individual scientist participating in the whole game is subject to different conventional rules of "standardized procedures" and "routine ways of operating"; his language is an impersonal language; the taxonomic systems, schemes of nomenclature, and the units as well as scales of measurements he uses are common conventions; his results are "based upon the knowledge of all" (Barnes 1985, 40-43). The entire process of producing and evaluating scientific knowledge is a social process, which means, among other things, that scientists who participate in scientific institutions are motivated to act by a reward system, the "currency" of which is recognition. That science uses currency "different from that employed in the main body

of society", that scientists are recognized rather than paid, is connected with the special role played by science in modern societies. Science now bears cognitive authority; this is a necessary condition if scientists are "to serve as specialists in a society with high division of intellectual labour" (Barnes 1985, 43-47, 83).[8]

Science founded on trust and authority is to a certain degree an autonomous whole, and "scientists themselves try to insulate their esoteric culture from the <outside> (Barnes 1982, 117); but this does not mean that science is independent from its social setting. Quite the contrary. Inferences and evaluation in science are related to macro-sociological factors, to socio-political goals and interests (Barnes 1982, 114-115). Unfortunately, what Barnes has to say about this relativization is that knowledge is "developed and modified in response to practical contingencies", that it "is related to activity which consists precisely in men attempting to manipulate, predict and control the real world in which they exist" (Barnes 1979, 2, 10), and--finally--that sociologists "still lack a precise and detailed account of the relationship between goals and interests on the one hand, and concepts and beliefs on the other" (Barnes 1982, 103).

Nevertheless, Barnes and Bloor are convinced that one task of sociology is to explain "variations in institutionalized patterns of knowledge" which cannot be explained by the non-social causes that physics, genetics, physiology, or psychology illuminate, since all these causes, from inanimate reality to physiological or psychological processes, influence individual skills, perceptions, and experience. All those influences must be taken for granted by sociology because joint operation of psychological and sociological factors "only makes sense if we think of knowledge as a working relationship with our material surroundings" (Bloor 1982, 271). Having accepted the empiricist claims that "some responses [i. e., perceptions - B. T.] to our material environment are common and constant" and that there is "a stratum of biologically stable sensory capacities" (Bloor 1980, 26), any overall account of knowledge must search in history and the social environment for causes of the variation of knowledge since reality as an object of experience and psycho-biological factors influencing experience are common so they are not promising candidates "to field as an explanation of that variation" (Barnes, Bloor 1982, 32-34).[9]

Sociologists, having adopted methodological relativism, change the rationalist relation between truth and cognitive acts such as accepting beliefs. Investigating how people make use of existing beliefs sociologists can observe that people sort out beliefs, accept some and reject others, according to preferences and standards that are conventional and locally determined. truth and falsity, rationality and irrationality, reasonableness and its lack, plausibility and implausibility, etc. are context-bound and cannot be "formulated in absolute or context-independent terms"; for relativists, "there is no sense attached to the idea that some standards or beliefs are really rational as distinct from merely locally accepted as such". This anti-absolutist orientation leads sociologists to the claim that the validity of beliefs cannot be separated from their credibility (a locally conditioned matter) since there are no context-independent reasons for the validity of any belief (Barnes, Bloor 1982, 27-30). And though "the acceptance of a theory by a special group does not make it true", the acceptance "makes it the knowledge of a group", and "the basis for their understanding and their adaptation of the world" (Bloor 1980, 38). As far as "understanding", i. e., the use of knowledge, is considered one can speak about the discriminatory or rhetorical functions of truth and falsity; but when the adaptive role of knowledge is taken into account, truth becomes one of the fundamental concepts of our "ultimate schema with which we think" (Bloor 1980, 35-36).

If there is neither Truth nor rules of Reason to force scientists to accept some beliefs and reject others, scientific consensus must be regarded as a product of social institutions. In fact, sociologists emphasize the institution of scientific training which is like drill, "dogmatic and authoritarian", and which thus produces homogeneity in scientific communities (Barnes 1982, 16). Does this mean that sociologists do not notice changes taking place in science at all? Of course they notice it; moreover,

changeability--in their opinion--is much more sweeping that it is for rationalists: there are changes in scientific goals, methods, and standards as well as in meanings, problems, and statements. It is not true that scientists have always been interested in explanation, prediction, and the control of structures within the world; these are aims of modern science, conditioned by the functions which science fulfills in modern societies. Methodological rules and standards are changeable since they are conventions, actively interpreted and established, for bringing about effective research, for rendering research a really social game that is performed within the framework of interpersonal relationships and institutions and which has to realize dynamic intrinsic or extrinsic aims. The entire mechanism of criticism as well as the mechanism for stabilizing knowledge once achieved are inherent in the scientific system, though the final source of changes lies in the individual capacity to invent the concepts under which phenomena are arranged.[10]

This many-dimensional changeability of science renders its boundary a conventional one: "it surrounds a finite cluster of concrete instances of science without implying that there is an essence which they share; the instances are the accumulated outcome of a historical process of negotiation" (Barnes 1982, 93). Admitting and analysis the dynamic nature of science sociologists in fact refuse to consider progress in science; "all knowledge generation and cultural growth should be regarded as endlessly dynamic and suspectable to alteration just as is human activity itself, with every actual change or advance a matter of agreement and not necessity" (Barnes 1979, 24).

The central philosophical point of the sociological approach is its relativism. Rejecting belief in objective truth, reason, method, and teleological progress in science, sociologists claim that the rationalist conception is based on Faith rather than Reason (Barnes, Bloor 1982, 46-47), and in a sense they are right. Some of rationalist philosophical beliefs are located in the sphere of philosophical Faith, which should not be confused with the religious Faith since their functions are different and only the former has a cognitive role to play.[11] Sociologists are right too, that the relativist is "helpless against Faith" though he can fight Reason (Barnes 1979, 46). However, it seems that they do not notice two facts; first, that the kind of fight which takes place between relativists and absolutists, and between themselves and rationalists, is neither empirical falsification of the opponents' claims nor deductive proof of their falsity or incoherence.[12] What sociologists really do is to invalidate, discredit an opposite confession of Faith, and this seems more similar to the act of expressing dislike or finding something to be uncomfortable[13] than to an act of rejection which is justified empirically or logically.[14] Second, they do not see that invalidation always implies the installation of another confession of Faith, and that is why their own position is not free of some presuppositions which are valid for them, but not for rationalists. The most that the relativist can do is to assume that Faith as a basis of Reason is pluralistic, and that his own confession of Faith is merely one among others, neither objectively better nor worse.[15] Unless he does this, he would still be trying to show and argue that he is right and not his opponents.

Now it is clearing what sense relativism--in the opinion of sociologists--is the opposite of absolutism, The belief that there are rational principles which are absolute, i. e., historically universal and trans-cultural, and that they govern the (ideal) course of scientific choices and changes, is spurned. In other words, the rationalist idea of reason comprehensible to itself is replaced by the conviction that everything, reason included, can be recognized only indirectly--through its manifestations. Moreover, they very idea of the transcendental (in a sense similar to that of Kant) nature of reason is spurned and the phenomenalist approach introduced. This means not only that reason is recognizable through its empirical manifestations, which Newton-Smith, for instance, could accept, but--further--that it is reduced to those manifestations; there exists only empirical minds with their abilities and activities, the knowledge established by these minds which is objective in the sense that it is a common social property, and social institutions that form relationships between knowing subjects.

In spite of all these clarifications in defence of the Barnes-Bloor approach I do not claim its perfection. I am sympathetic to relativist and sociological ideas, but I am not sympathetic to the phenomenalist reduction of reason to social phenomena since this leaves no room for philosophy of science.[16] Moreover, I cannot agree that the merits of relativization have been considered by sociologists in a sufficiently clear and convincing way. It seems that they took for granted the scientific nature of their own investigation and did not problematize the entire existing conceptual framework.[17] What I mean by problematization is the necessity of re-thinking the concepts and problems that form both traditional frameworks--the rational and the sociological--if a new coherent explanatory structure is to be established for the describing and understanding science.

2. Philosophy of science that is social and historical

That science and scientific knowledge are social and historical is now almost considered trivial by philosophers as well as by sociologists, but the problem of what this means remains an open question despite all the efforts of sociologists.

2.1. The social nature of science

Let me recall that rationalists incorporate this idea into their conceptions through two operations: (1) replacing the substantive rationality by an instrumental one, and (2) taking into account the phenomenon of consensus among scientists. Those lead them to consider science as a dialogue. However, their conception of the discursive, dialectical nature of knowledge-production is far from a purely sociological account. philosophers still concentrate on the epistemological aspect of the dialogue: on the relationship between a knowing subject and an object of cognition, on shared conventions, on arguments used to criticize opposite points of view or for convincing opponents, one reasons that allow the achieving of consensus, etc. They still do not regard learning from authorities as an act of cognition (cf. Lukes 1985, 293-294), whereas sociologists see this as an indispensable component of the most fundamental ways of acquainting oneself with concepts and using them, even in "the esoteric practice of scientific thinking" (Bloor 1982, 270-273; Barnes 1982, 70-73). Rationalists contrast explanation in terms of cognitive goals and reasons with sociological explanation in terms of causes; even though the rational behavior, i. e., this form of human activity that can be reconstructed with the help of the rational model, seems to be a product of the history of humankind, and therefore should be regarded as social and historical. Next, although rationalists must reckon with the existence of scientific communities and institutions, they regard them as a background fro the activity of scientists that creates and fosters the growth of scientific knowledge; or else they consider scientific communities in a idealized way as individual agents.

Sociologists from the Edinburgh school go, for of course, much further in conceptualizing science as something social, though it seems that they are not consequent enough in their pursuit. They conceive of science, in splendid accordance with the philosophical conceptions, as "an ongoing activity" (Bloor 1980, 143; cf. also Barnes 1985, 8), and focus their attention on explaining the beliefs and behavior of scientists, their goals and interests, as well as norms, standards, and conventions to which they conform. From a more radical "macro-sociological" point of view such a conception can be desribed as an account constructed within social psychology that describes the process of knowledge-creation as the social construction of knowledge. [18] What I mean by a more radical sociological point of view is an approach according to which science is a social system that is a subsystem of certain historical societies. It has its own structure, an

activity which is not merely the sum of scientists' activities, and historical values (goals). Only on the basis of such an idea can the development of science be treated as a process which is not reducible to the growth of knowledge, and which has as its "bearer" science as a whole rather than individual scientists; scientific knowledge can be regarded as a global result of the productive activity of the system, and any body of knowledge can be treated as material for further manufacturing; changes in knowledge can be considered as produced by scientific mechanisms, and, thus, irreducible to the sum of scientists" individual theoretical choices. Moreover, such an account allows us to consider goals of scientists and effects, genuinely achieved by science, in the context of global social systems, the needs and expectations of which compel science to act in a certain way.

However, the difference between what can be called "minimal" and "maximal" sociological inputs is much more profound than is the difference between the sociological model of knowledge-construction and the model of the social organization of science. The crucial point lies in the fact that one cannot build up a sociology of knowledge, of science, or of anything else, without constructing sociology *tout court* as a theoretical context. Though sociology is divided into sub-disciplines it is not similar to physics with its inner partitions. Despite possible cases of incommensurability or incoherence, physics is much more integrated, theoretically and practically, than is sociology. Sociologists cannot relate their conceptions of certain social objects to commonly shared concepts, principles, or laws because there is nothing like these in their discipline. Thus the aim of any good sociological construction should be maximal: establishing a sociological theory should mean the creation of sociology. Otherwise the construct will be paralyzed by different kinds of theoretical weaknesses: a lack of coherence, conceptual gaps, ignored or unresolved questions, implausible solutions, far-fetched conclusions, undesirable consequences, etc. Although the thesis of Barnes and Bloor suffers from many of these deficiencies, I shall restrict myself to discussion of only two, which are directly connected with the problem posed at the beginning of this section. These are: the impossibility of reconciling the experiential and the conventional constituents of knowledge, and the inconsistency of sociological explanatory practice with the principle of causality.

Bloor often emphasizes that a psychological (dispositional) factor co-operates with a sociological (conventional) one in the course of producing knowledge. The epistemological reason for this emphasis is clear: Bloor wants to maintain realism, but it seems that he is faced with a choice between realism and his principle of symmetry that states. In other words, he beliefs that all knowledge is relative to non-social (material) reality and to the social context of thinkers who produce it.[19]

Barnes and Bloor adopt the network model of Mary Hesse, who wants to "steer a course between the extremes of metaphysical realism and relativism" (Hesse 1980, xiv). In this model the correspondence postulate links beliefs with objective reality through 'empirical situations in which an association is established between some aspects of the situation and a certain word' and some predicates are initially learned (Hesse, 1974, 11). Seeing that every situation is indefinitely complex, the very possibility of cognitive identification of one particular aspect out of the multiplicity of other aspects means that there are objective physical similarities and differences in situations. This leads to the similarity condition. It is an assumption that some universal terms must 'presuppose primitive causal recognitions of physical similarities' and this fact is 'a necessary *a priori* condition of the applicability of a language containing universal terms' (Hesse 1974, 12). In this way she introduces a non-social condition for 'understanding and use of terms in science' (Hesse 1974, 10). Due to this condition, science 'retains its empirical basis' (Hesse, 1980, xvi).[20] However, the concept of the primitive causal recognition of physical similarities which happens in empirical situations seems to be a philosophical 'metaphor' which veils the process of genuine language learning, the theory of which is, according to Hesse, a problem for psychology or linguistics rather than for philosophy (Hesse 1980, 66). It seems that only by halting in front of the gates of psychology and

linguistics can Hesse retain a 'pure', non-social experiential fundament of cognition. Barnes and Bloor, however, cannot stop at that point because they regard language learning as a social process. In both its components, that is, ostension and generalization, acquiring knowledge from authorities is involved. Language learning 'needs the continuing assistance of culturally competent adults' (Barnes, Bloor 1982, 37). What a child is doing during first language acquisition is coming to know singular reference objects as well as similarities and differences between them. Reference objects, their similarities and differences, are objects and properties that have already been accepted as proper references for given names in a certain society. Therefore, unlike Hesse, the sociologists, having adopted a social model of language learning (combined with Wittgensteinian idea of a resemblance class), cannot claim that objective, non-socialized, pure physical reality enters into the process of cognition. Even when ostension is taken into account, its object is already socialized. When a teacher gestures towards something he calls 'a bird', this thing, a bird, is already knowledge impregnated in the sense that it exists as the correlative of previous cognitive or practical acts and the knowledge connected with them. In other words, all elements of the empirical situation in which words are learned (including the cases of meeting 'natural' objects or objects unknown to somebody) are socialized previously or they become socialized in the course of a given situation. Moreover, according to Barnes's analysis of the interests underlying cognition, any application of concepts to the recognition of an object is determined by interests. In Barnes's conception, Yearley says (1982, 359, footnote 25), "interests had been invoked to explain why certain *actual* similarities between objects rather than other likenesses were deemed the most important in some particular society. Now interests are taken to be constitutive of those similarities".

It seems that Barnes and Bloor do not realize that preserving the idea of the objective, pre-existing world-order which influences our knowledge is self-destructive for their position.[21] The ontological and epistemological premises of realism[22] --in any, even the weakest form-- turn against the principle of symmetry and force its rejection. Insofar as the idea of reality that exists and is structured independently and prior to any human activity is taken for granted, *no cogent reason and convincing reasons can be delivered against the belief that it is non-social reality itself which determines content of knowledge and*, thus, *its value*, so the acceptance of a certain true proposition does not require any other explanation than the very assertion of its corresponding with reality. Our common-sense knowledge based on everyday experience, philosophy, and religion as well as science are so deeply pervaded by the concept of reality which resists our cognitive and practical efforts and, thereby, plays a decisive role for its success or failure, that any conception which tries to run counter to realism seems to be artificial, implausible, and unacceptable. Perhaps the only way of overcoming these prejudices (if they are prejudices) is *to decry, devaluate them and established a conception of social reality one aspect of which is socialized nature*. This reality is simultaneously social objectivity, an objective context for practical and cognitive activities of historical societies, and the product of those activities. From such a point of view, nature in itself, independent of humans, can be conceived only as a horizon of the social world. Any attempt at approaching this horizon ends in establishing a certain historical form of nature for us, socialized nature. From this standpoint, what appears to us as objective physical reality or as a pre-existing environment to which we must adopt for survival is but a historically local collective construct established on the basis of a certain theory and practice. Elaboration of such a view requires, of course, problematization of the most fundamental philosophical concepts, like the very notion of being (perhaps in the heideggerian manner). As leszek Kolakowski shows in his essay on Marx and the traditional definition of truth, a constructivist epistemology cannot be reconciled with a metaphysical claim that reality exists-in-itself before we have any contact with it (Kolakowski 1967). It seems that an ontology coherent with the constructivist theory of cognition should regard being (including our own being) as relative to our acts of meeting the objects of these acts. Those acts bring into existence objects which *are* empirically, theoretically, practically, etc. In such an ontology the

concept of existence-independent-of-our-activity would lack any sense.[23]

In any sociological conception the problem of the nature of the connection between different components of the analyzed social whole must be considered. Barnes and Bloor claim that their own conception is based on the principle of causality. However, it can be shown that their own practice neither is nor should be causal explanation--at least, in the strict nomological sense, i. e., similar to that operating in the natural sciences.[24] Gad Freudenthal and Ilana Löwy (1988, 630) stress that "confirmable nomological theories are not within sight in the sociology of knowledge". Slezak notes that: "Establishing a causal connection requires more than merely characterizing in detail the social milieu which must have existed, and asserting its causal relevance" (1989, 565). Since genuine causal connections "must support counterfactual conditionals", the establishment of causal connections requires acknowledging that different social conditions might lead a scientist to formulate a different theory than the one he really formulated (Slezak 1989, 584-585). Yearley shows that studies in which the concept of social interests is utilized are not cases of causal explanation (1982, 117-118). Barnes also admits that "no laws or necessary connections are proposed to link knowledge and the social order, and that no abstract instructions are set out for the investigation and explanation of bodies of knowledge" (1979, 85). Hesse, having analyzed the causality principle involved in the sociology of knowledge, concludes that the strong program "does not imply social determinism, nor any particular privileged direction of causality from substructure to suprastructure, or indeed to converse. Nor is it implied that there are any universal laws of scientific and social development applicable generally across time. All that is implied is the possibility of finding some correlations amounting to historical explanations in particular cases, between types of scientific theory and particular social provenance" (Hesse 1980, 56-57). What she proposes to sociologists appears as a very weak form of causality and, in fact, she admits that it "has now become so weak as to be indistinguishable from something any rationalist or realist could accept in regard to the development of science" (Hesse 1980, 57). However, so ambiguous a concept of causality does not supply sociologists with any conception of explanation. They will have no methodological rules and standards for explanation and appraisal of the results of explanation if they accept that their task consists in: (1) separating different classes of objects, facts, events, processes, etc., and (2) showing that between these classes are relations of conditioning, inspiration, being responsible for, originating in, giving rise to, etc. All these terms, actually used by sociologists, refer to very different "factual cases"; and unless the clarify them they are unable to construct a coherent theory of social conditioning of cognition and knowledge. Moreover, the possibility of establishing such a theory is problematic. If sociologists assume that knowledge is what is considered to be body of knowledge--for instance a theory--in relation to the community of its creators, supporters or opponents, using their concept of a scientific theory. This means that in every case the concept of a theory is relativized to a particular and unique social context of the given body of knowledge. Any attempt to construct an abstract concept of a scientific theory applicable to all social contexts, to different disciplines or stages of historical development, means adapting a certain external perspective; for instance, the perspective of modern natural science or of a particular methodology. Insofar as this operation amounts to the possibility of generalization it runs counter to the sociologists' own interest and practice, which lie in the area of case studies. To a certain degree sociologists are aware of the whole problem. Barnes claims, perfectly right, that the boundary between the scientific and the non-scientific is generated by social processes, so "to understand where this boundary actually falls requires, not the formulation of any principle of demarcation, but rather the empirical study of those social processes whereby the boundary is made visible and sustained" (Barnes 1982, 93). If the concrete historical "instances of science" do not have a single property in common, do not have any essence, nothing can be said about science as such; only case studies of those instances, the resemblances or transitions between them, are possible.

The causality principle, and attachment to the naturalist approach, have other

negative consequence too. As i shall argue in the next section, the causal approach does not supply us with an explanation of the discovery of new data or of the creation of new ideas. The naturalist inclination makes the sociologists of the Edinburgh school insensitive to the advantages of a hermeneutic position which may offer a perfectly human interpretative perspective for the sociology of knowledge or of science. This is an approach much more coherent with all the principles of the strong programme (except the causal one) than is the naturalist's. I invoke it here as a possible alternative approach, the explanatory power of which deserves careful examination.

2.2 The historical nature of science

What was said about the nomological character of the explanation of scientific knowledge and of scientists' activity is closely connected with the problem of describing the development of science. The internalists' account is also in this respect nomological: they try to establish the model of scientific change as a model of the rational mechanism of developing science (toward truth), i.e., as a model of the (teleological) evolution of science. The crucial point, which raises the query whether such an approach can illuminate the real history of science, lies in the fact that it excludes even the possibility that the mechanism of scientific evolution changes during evolution because of some feedback loops. Even if internalists admit the changeability of the scientific method, they do not allow a change in the goal of science. In other words, a conviction that the history of science is not cumulative in respect to its mechanism, or dynamism, is presupposed as an *a priori* condition for the rationalist investigation of the growth of knowledge. One can argue that such an account is similar to that of the biological theory of evolution, and may be as successfully adopted as has been that model, which works pretty well as an explanatory device. However, this does not mean that the theory of biological evolution can be used to reconstruct natural history, that is, the history of natural evolution. It is difficult even to predict what might have happened to the theory of evolution if it were applied there. Exactly the same problem has been revealed by the rationalists' self-criticism. They argue that no plausible grounds were given to support the conviction that rational mechanisms of theory-production, theory-choice or theory-change, pointed out by (other) internalists, produced the cumulative growth of scientific knowledge, even when those mechanisms were reconstructions of research activity that were adequate to some extent. In other words, the fact that scientists follow the rational model for the production and evaluation of theories does not imply the necessity of any accumulation in the history of science, independently of whether it is to be the accumulation of rejected false theories as a result of which scientists approach nearer to the truth, the accumulation of approximately true theories, or the accumulation of theories more and more useful for prediction and manipulation of the world. it seems reasonable to say that if we want to consider science as an area of any such accumulation, progress toward truth, growth of knowledge, increased predictive power, etc. we must assume that there is a global dynamism of development, and introduce its model into our historiosophy of science. moreover, it should be emphasized that such a model ought to be introduced not only into those conceptions which presuppose the teleological nature of scientific development, but also into those which tend to explain the development in terms of improving and extending the existing body of knowledge, and which claim nothing about future aims of science.

In order to clarify the idea of a global developmental dynamism I want to remind of Kuhn's introduction of the concept of a paradigm. When it was introduced into philosophy of science, it allowed Kuhn not only to describe the context of scientists' activity, but also to explain the transition from an immature science (or scientific discipline) to a mature one. According to this explanation the transition happened to science, and not to scientists themselves; they were placed in a new science, even if they did not recognize this fact or were not willing to accept it. In a sense, the transition was

an objective by-product of the scientists' activity. It is such a type of explanation and mechanism that I have in mind when speaking about the global dynamism of scientific change. Though I am not going to elaborate here the details of that global mechanism, I want to consider briefly the crucial question it raises, namely: can such a global dynamism be reduced to a rational model, or series of models, of scientists' choices, procedures, decisions, etc.? In my opinion the answer is, and should be, in the negative for two reasons. First, if science is a social system, neither its structure nor its operation is reducible to the set of scientists and their activities. Thus, the mechanism of science's activity in a given time, or its development through time, must be ascribed to the entire system even though, indeed, the rational mechanisms that govern the activity of scientists are the components of the global mechanisms. Second, science that is a social system is not exclusively a rational system: it has a factual, "material" aspect and, for this reason, the global dynamism which governs its development cannot be reduced to any rational mechanism. Does this mean that the philosophy of science's development, the historiosophy of science, is impossible as a kind of descriptive enterprise[25], and that philosophers should restrict their attention to purely methodological analyses; whereas the development of science ought to remain the domain of socio-historical case studies? I do not think so. There is a possibility of maintaining a genuine descriptive and--in a sense--interpretative historiosophy of science that tends to overcome the level of data studied by the history, sociology, or psychology of science in order to discover what is in science essential, rational, *a priori* necessary, or ontological, in other words, what reveals itself only to philosophical cognition and allows, when discovered, to understand empirical, socio-historical particulars. This answer does not suggest that any socio-historical investigation is to be incorporated into such an interpretative historiosophy of science. There is room left for different scientific, i. e., empirical, studies of science, even though the necessity of reciprocal connections between them and philosophy is assumed. Moreover, the whole range of different philosophical positions is still admitted; from inductionist and rationalist ones which try to extract from the history of science its entire (rational) skeleton and define the "patterns" of resemblances that constitute science as a historical whole; to deductive and rationalist ones which try to establish almost the same, but *a priori*; to non-rationalist (in a sense) and relativist ones which can try to grasp science in flux through constant relativization of everything (themselves included). To the extent to which their position seems to prejudge little or nothing I regard it as the most promising stance for free, self-critical and creative philosophizing.

The historiosophy of science, having assumed that it is a social and historical whole, should--in my opinion--focus its attention on the creative nature of science, since this is the crucial point of the whole debate on the development of science. Independently of the answer to the question of whether science progresses toward truth or not, the primary concept of the development of science presupposes that its evolution is creative: without creating new ideas or data science neither could proceed toward truth nor could improve itself. That the novelty of ideas and data amounts only to their difference from the existing ideas and data, means that they were previously unknown, and is independent of their cognitive value, i.e., their truth or falsity, degree of verisimilitude, explanatory or predictive power, etc.

If we make the idea of the creative nature of science the central point of the historiosophy of science we can readily see that neither rational models of theory-construction or theory-choice, nor sociological "causal" studies, can genuinely explain science. The model of rational criticism is useless for considering scientific creation since it places the context of discovery in the realm of psychology; the rational models of theory-choice assume that theories to be evaluated are given and do not bother about their origin; the sociological studies of social (non-cognitive) interests or--in general--of the social context of science can explain, eventually, how this or that new idea

appeared but are useless for answering the general question of why new ideas or data are brought into existence.[26] It seems that the explanation of the creative nature of science, and of human cognition is general, requires the assumption that human thinking is able to overcome itself, to overstep every stage of thought that have been reached before. This ability is not a psychological one: individual cognitive activity is determined by psych-sociological factors, especially by the existing body of knowledge and methodological conventions; and thus--in a sense--it is not creative, even though criticism and inventions produced by knowing subjects are means of realizing the global development of cognition. Individual cognitive activity, together with relationships between individuals and supra-individual structures and processes, are indispensable conditions of the viability of the entire change of cognition and knowledge.

The ability of human thinking to transcend itself is a global dynamism which renders the historical process of cognition a self-development, in the course of which contents and forms (e.g., methods) are changed and overstepped together; so for this reason it cannot be described in purely methodological terms, Moreover, the self-development of cognition is a very complex interplay between absolutist (dogmatic) and critical tendencies; it is composed of interactions between the creation and the evolution of ideas, their establishment and use for different cognitive and non-cognitive purposes; it is an interplay between individual and social structures and actions, between the tendency to stabilize achieved results and to modify them, between different historical forms of cognition, e.g., the philosophical and the scientific.[27] These are mere examples of the components of the global dynamism of the change of cognition. This dynamism comes into existence in different historical forms and their interactions. To consider all those elements, interactions, processes, etc. is a task for philosophy and historiosophy of science of science: whereas to show its scientific empirical manifestations is a task to be fulfilled by the history and sociology of science or knowledge.

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[1] Of course, neither philosophical rationalism nor the sociological approach to science is developed solely by British thinkers; so the restriction of this analysis to their British cases is determined by external--in the rationalist sense--factors. First of all, the scope of the parer does not allow me to present the whole "international" spectrum of positions I analyze. Second, I was able to work on this paper thanks to the Oxford Colleges Hospitality Scheme for Polish Scholars which provided me with the opportunity of being a Visiting Fellow at St. Hilda's College in June 1987. This allow me to gather materials, and to discuss the draft of this paper with Rom Harre, Steven Lukes, William Newton-Smith and Kathleen Wilkes. I want to thank them for their encouragements and helpful remarks. I am especially grateful to Kathleen Wilkes for her assistance with improving the text which I wrote in "a language pretending to be somewhat similar to English", to use Leszek Kolakowski's phrase from his *Religion.*

[2] The Popperian rule for evaluating theories which appeals to the degree of corroboration cannot be used

for choosing between theories because--as Popper admits--corroboration is "an evaluating *report of past performance*" (Popper 1975, 18); whereas in choosing among rival theories scientists want to "select on the basis of the available evidence the theory which will provide us with better explanatory and predictive power *in the future*" (Newton-Smith 1981, 60; italics - B. T.)

[3] He includes in the set of rational events: simultaneous discoveries, priority disputes (both were favorite themes of the Mertonian sociology of science), successful predictions of novel facts, cases of adherence to refute or inconsistent theories. He excludes from this set the cases of instant acceptance of experiments as negative crucial tests "in the absence of a better theory" (Lakatos 1976, 114-116). The instrumental nature of the Lakatosian concept of scientific rationality was noticed by Paul Feyerabend, who claims: "the methodology of research programmes provides standards that aid the sociologist in evaluating the historical situation in which he makes his decision, it does not contain rules that tell him what to do" (Feyerabend 1976, 121).

[4] The idealizational nature of Lakatosian reconstructions was analyzed by Noretta Koertge in her *Ration Reconstructions* (1976).

[5] It can be easily noted that this is a third step in the evolution of the modern philosophical concept of science. According to the Vienna Circle science was a system of sentences; then it became a system of procedures, thanks to Popper; now it is a system of negotiation.

[6] For instance his conception of verisimilitude is deficient (Olczyk 1989).

[7] Sociologists do not equate knowledge with true belief. Barry Barnes says: "'True' and 'false' are terms which are interesting only as they are used by a community itself, as it develops and maintains its own accepted patterns of concept application" (Barnes 1982, 31). The fact that sociologists do not identify knowledge with true belief was a source of many misunderstandings which appeared during their discussion with rationalists, who claimed that knowledge consisted of true beliefs. However, in fact rationalists have weakened this equation. When they started to discuss the growth of knowledge, and recognized that it proceeded through the replacements of less true theories by more true ones, they did admit that what is knowledge is not entirely true. Otherwise they should speak about the process of achieving knowledge and not about the growth of scientific knowledge.

[8] The fact that cognitive authority lies with science is a historical one; earlier the Church possessed this.

[9] Barnes elucidates this point by asserting that all cultures, as well as all knowledge, relate symmetrically to reality in the sense that true and false beliefs are causally related to reality as "responses to the causal inputs" received from reality (Barnes 1979, 25).

[10] Bloor emphasizes that stability in a system of knowledge"comes entirely from the collective decisions of its creators and users" who protect "our hard-won ability to cope with our environment". This means that stability and "protective strategies" can be explained "in terms of the technical advantages" that kept elements secure for scientists, though this is only a partial explanation. A further explanation must take into account that "certain laws are protected and rendered stable because of their assumed utility for purposes of justification, legitimation and social persuasion" (Bloor 1982, 280-283).

[11] However, the boundary between philosophical and religious Faith cannot be easy determine; the difference between them is of functional rather than substantial nature. If we adopt Leszek Kolakowski's concept of religion, according to which "religion is not a set of propositions, it is the realm of worship wherein understanding, knowledge, the feeling of participation in the ultimate reality (whether or not a personal god is meant) and moral commitment appear as a single act, whose subsequent segregation into separate classes of metaphysical, moral and other assertions might be useful but is bound to distort the sense of the original act of worship" (Kolakowski 1982, 175), we can speak about a philosophical myth and faith by analogy to a religious myth and faith, even though Kolakowski himself makes a distinction between the realms of religion and science. At least, some components of the religious myth are present in the realm of philosophy of science: there are some fundamental philosophical beliefs, in the case of which understanding, knowledge, and intellectual commitment "appear as a single act", and their context of justification cannot be separated from their context of discovery. I refer the concept of philosophical Faith to such primary axioms of the philosophical discourse.

[12] This means that in a sense critics of the sociological approach (for instance Steven Lukes in his *Comment on David Bloor* (1982)) are right to claim that sociologists do not show rationalists' suppositions to be false. But this cannot be done by sociologists.

[13] In fact, such qualification is often formulated when relativism with its consequences is considered.

[14] The idea of the invalidation of the opposite confession of Faith implies, among others, that conceptions in question cannot be joint together to produce a complementary account. In our case the sociological conception of knowledge cannot be combined with the rationalist philosophy of science into a coherent whole. At this point I do not agree with Richard Jennings who claims that "enormous and potentially fruitful cooperation" is possible between them (1984, 202-205, 206). Such a cooperation is not possible because for internalists only the discovery of the universal, context-independent criteria of

rationality renders beliefs or actions intelligible, since such a discovery allows their explanation in terms of necessity (of course, this does not mean that the explanation is not tentative, conjectural; it will be rejected or improved if the weaknesses of the explanation are pointed out); whereas for sociologists such an explanation cannot be offered: the absolute criteria of rationality do not exist and possible explanation is formulated in terms of factual regularities and not in terms of "disembodied" rationality.

[15] It is well worth adding at this point that such an idea is fairly coherent with the sociological principle of reflexivity. And though Barnes and Bloor did not use in practice the reflexivity principle, other sociologists did. For instance, some self-reflexive theses and explanations were formulated by Randall Collins (1983).

[16] Unless the idea of reason that is self-explaining is held, philosophy vanishes--reduced to empirical science or common sense based on experience which forms a framework for their critical evaluation. The peculiarity of philosophy lies in its self-criticism (self-evaluation) which is based on the idea of self-intelligible Reason. This idea can be elaborated in many different ways. One of them may be historical and societal in the sense which was created in the German philosophy by Hegel, in his conception of historicity, by Marx, Nietzsche, Husserl. From this standpoint the approach of Newton-Smith, who admits that Reason manifests itself empirically and that those manifestations are historically differentiated methodological positions, seems the most promising rationalist offer.

[17] In fact, sociologists have re-defined knowledge; they abandoned its identification with true belief and emphasized its being a basis for social (practical) activity. (Cf. Gellatly 1980) However, neither is this concept comprehensible, nor are others re-defined, nor is a new conceptual framework established. Hesse is right when claiming that "Bloor does not quite come to grips" with the rationalist objection from the self-refuting nature of the sociology of knowledge because he does not realize that "the concept of what it is to be 'true' or 'false' must be changed to enable us to assert consistently that the strong thesis is true" (Hesse 1980, 59, 42). For the concept of truth to be re-thought the entire traditional conception of cognition should be viewed critically.

[18] Such a tendency of the Edinburgh school may be conditioned by the fact that its conception has been formed in the constant polemic with rationalists, as well as by the fact that the main object of explanation is knowledge, not science.

[19] I am not suggesting that Bloor wants to maintain a sort of naive realism, which neglects, for instance, the theory-ladeness of observational predicates or the underdetermination of theories by data. His realism is expressed by the materialist thesis: the material (non-social) world influences our cognition and knowledge, it responds with regularity "when subject to the treatment given it" in scientific experiments (Bloor 180, 29-30). In any case I do not think Bloor would accept the following solution offered by Collins. Collins says that "the idea that facts are 'socially constructed' and 'contingent' is compatible with common sense realism if, by that term, we underspend not that facts mirror, mimic or otherwise reflect a one-and-only real world in a one-to-one way, but rather that facts earn their status by helping people solve problems ('truth equals success') and are sustained through social interactions (conventionally)" (Collins 1983, 196). Collins, in fact portraits a radical form of naive realism and displaces it by a kind of pragmatic and constructivist thesis which can scarcely be called "the thesis of realism" since the entire traditional content of the concept "realism" has just been abandoned by him and not only weakened or restricted.

[20] It should be added that considerations concerning the empirical nature of science are closely connected with Hesse's philosophical self-consciousness. She stresses the difference between the history and the philosophy of science and states that "philosophers must use some criterion of judgment derived from modern science and philosophy to construct 'ideal types' of science that are relatively independent of historical cases" since philosophers have their own problems to solve. Philosophers of science are "primarily interested in science as *knowledge*, and therefore in the kind of knowledge science claims, and in the conditions for its truth". In order to distinguish science from metaphysical cosmology the idea that science claims empirical knowledge should be presupposed (Hesse 1980, xvii-xix).

[21] That the use of Hesse's network model is illegitimate if Bloor wants to maintain relativism is emphasized by Joseph W. Smith (1984).

[22] That there is reality, independent of and prior to any human activity, and that there is a kind of cognitive act (e. g., sensual experience) that is a way of our meeting with this reality, in the course of which reality influences us (causally).

[23] It is worthy of notice that in Hesse's exposition, some versions of post-empiricist instrumentalism affirms a similar thesis: "Truth and existence claims are determined, not by the world, but by the postulates of theory: for our physics *there are* fundamental particles and fields, a space-time continuum, forces and persisting physical objects; for other cultures *there are* spirits, witches, telepathic communications, persons not uniquely and continuously space-time locatable, and so on and so on." (Hesse 1980, 175).

[24] When the possibility of the causal nomological explanation of science is considered, rationalists seem to be in a much better position than sociologists: they can, in fact, follow the nomological model of

explanation. Their approach is universalist in the sense that they aim at generalized claims about science; for them science contains sets of observations, theories, research operations, choices, acts of acceptance, etc. which are classes of abstractions and not classes of resemblances; and if they assume any kind of realism they believe that elements of those classes are ultimately related to the class of real (physical) systems, which are causes of the truth or falsity of scientific propositions and thus determine (causally) acts of their acceptance or rejection.

[25] Discriminating between descriptive and analytical philosophical approaches is--in a sense--real and useful, though a little bit superficial since every description presupposes the analysis of concepts used in it. Philosophical reflection on science can be pursued analytically, exactly as it has been done up to now, in the sense that it is an elucidation of abstract concepts that could be devices for describing science. Seen from such an angle, it is constant remaking and repairing of previously established ideas which have been undermined or demolished by criticism (supported, if necessary, by the evidence of historical examples). However, the analytical philosophers of science used to declare their claims to be descriptive statements. But it is not demonstrated that the difference of aims (analytical or descriptive) does not influence the results that ar to be obtained. This means that investigation aiming at description could result in a different model from analytical investigation.

[26] More precisely it should be said that sociological studies were only able to describe the social context in which a certain new idea appeared. It is not possible for sociologists to demonstrate that this context is "a cause" of the idea in the sense that it could not have appeared if this context had not been present. Furthermore, since the sociologists emphasize the impartiality of their explanations, they cannot count the social context as "a cause" of the constant creativity of cognition; social factors can brig about changes in knowledge and thus new ideas, as well as the stagnation of knowledge. It should be added that the sociology of science based on the ideas of a phenomenological sociology (developed, for instance, by Karin Knorr-Cetina (1981)) is able to consider the production of novelties, innovations in science due to the concept of an actor"s interpretation of the situation in which the actor is placed. However, this solution suffers from individualist bias and is unable to grasp "the systematic, trans-local and inter-subjective aspects of scientific knowledge" (Freudenthal 1984, 293).

[27] That the form of ability to overcome itself is different in the case of science than in the case of philosophy can be easy shown. Science, unlike philosophy, is not self-reflective, so philosophy does overstep itself in the course of its own critical speculative activity whereas empirical science requires sensual experience and technological engagement as factors forcing it to overcome past achievements.