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Philosophy of Science and Sociology of Knowledge: Cognition and Knowledge Universalization'

A. The Problem-Situation

1. Facing recent developments in the reflection on science sometimes referred to as "the sociological turn"[2] the philosopher of science is tempted to ask the question: *What is the epistemological significance of the aetiology of knowledge in general?*[3]

By aetiology of knowledge I mean all kinds of investigations concerning the impact of the circumstances of cognition upon its content. There can be no doubt that sociology of knowledge and history of science (but not only history and sociology) belong to that wide field of investigations so fashionable today.

The traditional answer to the question concerning the epistemological significance of the aetiology of knowledge given by rationalist philosophers — not only by logical empiricists or by Popperians but also, for example, by Husserlians — was decidedly negative: If Pythagoras tried to find the foundations of being in mathematical relations, if the Darwinian theory of evolution was born by Malthusian inspirations and Malthus' ideas sprang from liberal ideology, if Lord Kelvin in his investigations on electromagnetic theory was motivated by the utilitarian values of Victorian England', if the controversy between Pasteur and Fouchet concerning spontaneous generation reflected the political controversies of the Louis Napoleon period in France[s], if the indeterminism of German physicists in the Weimar Republic was caused by the political and ideological atmosphere of that period in Germany', — all these determinations, even if well substantiated, should, according to those philosophers, have no impact upon epistemological evaluations, it is to say, on the acceptance or rejection of the theories and opinions in question. Psychoanalysts may claim that the theory of relativity was formulated by Einstein because of his familial complexes; the members of the Science for People group may denounce attempts to explain social phenomena by biological considerations as the expression of fascist or imperialist ideology[7], but physicists or biologists should not care much about such circumstances when they have to evaluate the content of those theories and opinions as true or false. Neither should the philosophers. There is a difference between science and politics. When a politician says something, we are immediately tempted to question his purposes or interests. As far as scientists' claims are concerned, however, we ask whether they are true or false, well substantiated or not — no matter what may have been the motives for advancing them.

2. At first glance this traditional point of view seems convincing. If the shoemaker drinks vodka it does not mean that his products will smell of alcohol.

However, when we think about this answer more deeply, we easily find what it presupposes: If one admits that the circumstances of cognition may have a locally selective or deforming impact, something that even a convinced rationalist would not deny, then in order to claim that these circumstances have no epistemological significance, one has to presuppose that knowledge distorted by these circumstances may be confronted with a non-distorted model. In other words, what must be assumed is the possibility of an epistemologically privileged situation, i. e. a situation in which we *know* that we have to deal with no distorting factors.

Putting the question in Cartesian terms: How can we know that the malicious demon is absent, that he does not delude us *hic et nunc,* if we know that he is present and deludes us sometimes? The Cartesian answer is well known: It is only the veracity of God that can protect us against the deceiving tricks of the demon.

Thus our first question concerning the epistemological significance of the aetiology of knowledge is *whether such an epistemologically privileged situation is possible? I* will discuss the problem below, in part B.

3. There can be no doubt that at any given time the scientific community accepts almost generally some criteria and values for appraising (accepting or refuting) scientific claims, though such criteria are difficult to codify and have always been disputed not only by philosophers but by scientists themselves. These criteria and values belong to a wider *background consensus* due to which scientific claims advanced in some local specific context are either universalized, i. e. accepted, even if not immediately, by scientists working in quite different cultural contexts, or discarded even by those who advanced and defended them previously. It is due to this consensus that the frequently occurring disputes and controversies at the frontier-areas of research are usually solved relatively quickly. Since

the universal character of scientific knowledge is one of its specific features I would claim that *the socio-historical analysis of a local cultural context in which some claims were advanced, cannot by itself explain why they were accepted elsewhere. What is needed in addition is the analysis of that background consensus.*

4. However, the quasi universal acceptance of scientific knowledge in different socio-cultural circumstances, which is one of the hard facts substantiated by its development, as well as its transmission in time, even if it were undisturbed, *does not prove* that the privileged epistemological situation in science is in fact possible. The answer depends on whether the *background consensus* is regarded as a "necessity of Reason" or as a historical fact.

If we believe that it is valid as a "necessity of Reason", that it cannot be other than it is, then, indeed, the content of the universally accepted knowledge cannot depend on the circumstances of its acceptance, whether cultural, historical or biological; in this case the aetiology of knowledge would have no epistemological significance. Such is the main rationalist thesis concerning the evolution of science.

If, to the contrary, we believe that the consensus is valid only due to some factual circumstances, if we do not treat it as a necessity of Reason, then the universalization of scientific knowledge is to be explained by factors codetermining this specific consensus. If the aetiology of knowledge could provide such an explanation, it would prove by the same token that genetic factors codetermine the content of knowledge *even when this knowledge is universally accepted.*

Let us remark, however, that in that case two possibilities should be distinguished: If the consensus in question is supposed to be valid in all historico-cultural circumstances, then its universal validity might be explained only by biological factors; I believe, that that is why today some scientists and philosophers are looking in biology for a *via media* between the Charybdis of epistemological absolutism and the Scylla of relativism. It is a path chosen not only by K. Lorenz, J. Monod, J. Piaget, N. Chomsky, or the sociobiologists, but also by Popper, no matter how important and deep the differences between their opinions are.

If a biological explanation were admissible, then the consensus could be treated neither as a transcendental "necessity of Reason", nor as a fact relativized by historical circumstances, but as the incarnation of the historically unchanging human biological nature. The analysis of this possibility would lead me, however, beyond my present subject.

If, on the other hand, the background consensus is supposed to change in time, which seems to me a more plausible position, then both socio-historical and biological explanations would be conceivable, and some kind of cultural or historical relativism could not be avoided in the explanation of the development of knowledge.

Such seems to me to be the problem situation when we ask the question concerning the epistemological significance of the aetiology of knowledge in most general philosophical terms.

5. Let me try to summarize:

(i) The aetiology of knowledge would have no epistemological significance if a privileged cognitive situation were possible.

(ii) The aetiology of knowledge would have epistemological significance if it explained not only why some claims were advanced, but also primarily why they were universally accepted. If it does not do that, it may provide penetrating explanations of local historical particularities in the process of development of sciences, but it has no epistemological significance. *The local factors cannot explain the universal acceptance of scientific knowledge.*

(iii) In order to explain the universal acceptance of scientific knowledge the aetiology of knowledge must investigate the factors due to which there exists a background consensus on the basis of which the scientific claims are accepted or rejected by scientific communities, and due to which the consensus possibly changes with time. Whether the explanation is to be provided only in biological terms or in historico-cultural as well as biological terms depends on the supposed historical stability of the consensus.

6. If contemporary sociology or social history of science had no other goals than to explain circumstantial particularities of cognition in different social settings, there would be no problem of its epistemological significance and of its relation to philosophy of science. Both disciplines would aim at answering quite different questions. The first — to explain local particularities of cognition, the second — to clarify the universal acceptance of some of its results in different socio-historical contexts. Let us remark that this difference is not the same as the well-known distinction between *the context of discovery* and of *justification;* when we ask why and how knowledge is universalized we are not obliged to exclude the question concerning the genesis of the criteria of its evaluation.

In fact, however, the proponents of the strong sociological program as well as the social constructivists believe that their local case-studies do have some epistemological consequences. Today we are no longer in the same situation as T. Kuhn was when he asked: "How could history of science fail to be a source of phenomena to which theories about knowledge may legitimately be asked to apply?"' The tables have been turned and now I feel obliged to ask: Can indeed social history and sociology of science replace philosophy of science in solving epistemological problems? If this were not what the sociological turn implies all my remarks would miss the point.

Having presented the problem situation as I see it, I turn to the first question: Is indeed a privileged epistemological situation possible in science?

B. The Problem of the Knowing Subject

7. At least since modern times the methodological criteria for the construction and evaluation of scientific claims were regarded by philosophers as valid *de jure,* no matter how they presented and substantiated them. On the basis of this assumption science was regarded as the incarnation of human rationality. The assumption has found its expression in the concept of the knowing subject.[9]

According to that conception it was assumed that the knowing subject, at least as far as scientific cognition is concerned, may not depend either on the inherited tradition, or on all the accidental circumstances in which cognitive activities take place. The knowing subject was supposed to be able to overcome his physical and historical particularity and to produce knowledge that had to be accepted at any time and place, and by any other rational knowing subject. Except for particular circumstances that might perhaps distort the results of his cognitive activities, but which may be neutralized by the intersubjective control of the obtained results, such a subject was treated as if he stood completely outside the world that he investigated, as if the results of his theoretical and experimental activities depended neither on his physical make-up, nor on the instruments he used, nor on his conceptual apparatus, nor on the historical situation in which he was living. We could say that the philosophers endowed the human knowing subject, at least potentially, with some attributes of a god. The Laplacian demon could serve as the model of such a subject.

It was just this conception of the knowing subject that constituted the commonly accepted basis for philosophical discussions concerning the method, which — if rigorously applied — would enable the potentially rational subject to be actually rational, to accept all those and only those claims that must be accepted by everybody in all places and at all times.

From Bacon and Descartes to Carnap and Popper[10] this concept of the autonomous knowing subject engendered different ideas of the scientific method that was supposed to be universally valid and to express the rational abilities of human nature. The fact that for such a long time almost all of the philosophical reflection on science was concerned predominantly with methodological problems, was mainly due to the conviction that the scientific method is the incarnation of human rationality and the tool for its realization.

At the same time, this concept of the knowing subject served as the philosophical justification for postulating the autonomy of science: of its intellectual autonomy with respect to philosophy, religion or political opinions, and of its institutional autonomy with respect to churches or the state, at least since the state was becoming more and more interested in the development of science and its applications to practical matters. Due to the postulated autonomy scientists could pretend to be impartial arbiters in all human conflicts which were supposed to be soluble by the scientific method of which they were the masters.

For a long time the epistemological point of view according to which the cognitive activity may be completely independent of the circumstances in which it takes place and the conception of science as of an autonomous social institution corresponded to the state of knowledge about man and to the actual social situation in which science was not linked to the economy or to politics by strong institutionalized bonds. This situation, regarded as corresponding to the very nature of cognitive activity, encouraged the treatment of science as if it were not a product of a definite and changeable culture that could be different from what it is, but as a fact of nature. It also led to the treatment of science only as a system of opinions — for example, religion or philosophy, though opposed to them in method.

It seems obvious that as long as this conception of the knowing subject was accepted, it was impossible to concede that aetiology of knowledge might have any epistemological significance. In this framework there was neither place for a history of science that would go beyond a chronicle of scientific achievements and failures, nor for a sociology of scientific knowledge. And, as a matter of fact, sociology of science was born only when these opinions were undermined by the development of knowledge and by a new situation of science in the global social structure. The radical turn in the methods of history of science was due, I believe, to the same circumstances. However, as long as those opinions prevailed, it had to be believed that the circumstances in which knowledge is advanced may only have a distorting or, perhaps, selective but not a constitutive impact upon its content. In such a framework neither a strong sociological program nor the conception of the social construction of knowledge was possible.

8. It seems evident to me that the *conception of the knowing subject and of the scientific method which grants complete autonomy to the content of knowledge with respect to the circumstances in which it has been advanced and accepted, has been undermined by the very development of knowledge during the last hundred years.*

As a result of developments in the natural as well as in the social sciences the knowing subject cannot be and is no longer treated as a subject dwelling outside the world he is investigating. On the contrary, his cognitive possibilities have been more and more relativized with respect to that world and his relations to it. The autonomy of the knowing subject, his ability to achieve knowledge unmediated by his own natural and social constitution, is questioned by physics, biology, and neurophysiology, as well as by linguistics, cultural anthropology, sociology and history (history of science included), not to speak of philosophy. The great achievements of contemporary science — Einstein's theory of relativity, Heisenberg's principle of indeterminacy, the Gödel theorems — seem to show that the more we know about the world, about ourselves and about how we know, the more difficult it is to believe that our knowledge does not depend on our own biological make-up, on the functioning of the brain, on the language we use, on the culture we inherit, on the social situation in which we live.

Husserl was well aware of these philosophical consequences of the development of knowledge and of the danger of relativization of all the values of our culture that they involve. His whole intellectual effort was directed towards overcoming this danger by finding metaphysical foundations granting the universal validity of our knowledge and values no matter what the circumstances of our life are. It seems that he did not succeed in this global enterprise.

At the other end of the philosophical spectrum, the conception of a pure empirical basis on which all scientific knowledge is, or ought to be, based, the conception advocated by logical empiricism, was supposed — at least in the first period of its evolution — to accomplish the same task as the Husserlian conception of the transcendental ego. It was supposed to grant scientific knowledge independence from any and all circumstances under which it is achieved and accepted. This effort did not succeed either, though obviously for quite different reasons.

More recently the same philosophical aim has been pursued by Popper in his epistemology without the knowing subject[11] known also as the theory of "world three". Contrary to what he had said in the "Logic of

Scientific Discovery", Popper now agrees that knowledge advanced by the knowing subject can never be quite objective, free from all circumstantial and nonrational co-determinations. The concept of the rational method which, if applied, was to grant the objectivity of results of human cognitive activities, is now interpreted as some kind of impersonal mechanism according to which science develops in the Platonic world of ideas and problems; this mechanism is seen as an extension of natural selection. I would argue that as a result of the development of scientific knowledge, as well as the history of science, Popper faced the alternative: either to give up the conception of the historically permanent rationality of scientific knowledge based on the assumption of the autonomy of the knowing subject, or to get rid of the knowing subject altogether, and move into "world three" where, due to its impersonal character, no subjective or circumstantial, non-rational factors could have any impact upon the universalization of the knowledge produced in world two. The reason why he has chosen the second possibility seems evident with respect to the main tenets of his philosophy and the role he expected science to perform in our culture.

To summarize: If we do not believe in the Cartesian God who protects us against the tricks of the malicious demon, or in the transcendental reduction, or in a pure, epistemologically unquestionable empirical basis of scientific knowledge, or in an autonomous mechanism of the evolution of the world of ideas, *we cannot avoid the statement that aetiology of knowledge has epistemological significance.*

It seems rather unrealistic that one day we shall be able to look into a well so deep that we will not see our own face at its bottom. In other words, everything we know, we know as humans — no super-human point of view is possible: The content of our scientific knowledge is determined by the object under study as well as by some other factors the impact of which is constitutive and should not be disregarded by epistemology. A privileged epistemological situation does not exist, though obviously not all epistemological situations are equally good — not everything is possible, the object under study imposes its constraints and frustrates some human designs. The Kantian conception of a priori knowledge may well be essentially correct, providing the "a priori" is not transcendental but determined by genetic (biological, historical, socio-cultural) factors.

C. Does Science Exist at all?

9. Thus far, I suppose, there were no essential disagreements between the philosophical point of view I have presented, and the general assumptions of sociology of science, except perhaps for my opinion concerning the epistemological significance of investigations concerning the circumstances in which scientific claims are advanced.

One more remark is, however, needed.

I have chosen to speak about the aetiology of knowledge in order to pose the problem in the most general terms. However, since aetiology of knowledge embraces different kinds of investigations concerning the impact of the circumstances of cognition upon the content of knowledge, we should remember that to affirm that the aetiology of knowledge has the epistemological significance does not imply a priori that *all* the codeterminations we have to look for are to be explained in terms of social structure or interests, at least directly. History of ideas, for example, is also a part of the aetiology of scientific knowledge. Thus, we should not exclude the possibility that the *impact of social factors may be mediated by ideas and values commonly accepted in the scientific community at a given time.* Such a conception might contribute to the explanation of the process of the universalization of knowledge produced in different socio-cultural contexts.

Two years ago at the colloquium on Alexandre Koyré held in Paris, Y. Elkana read an interesting paper presenting Koyré as a "sociologist of disembodied ideas". Personally Koyré was — to say the least — rather sceptical about the sociological approach to the evolution of knowledge, and it seems that he would not be happy with Elkana's description. In fact, however, what Koyré achieved in the history of science might indeed have led to sociological questions concerning the quasi-universal acceptance of "un cadre des idées dans lequel la science progresse [...] un cadre de principes fondamentaux, d'évidences axiomatiques qui habituellement ont été considérés comme appartenant au propre à la philosophie".[12]

Yehuda Elkana called that conceptual framework *the image of science.* In my book *Between Method and History*[13] I have called it the socially accepted *ideals of science.* And I tried to explain how these ideals constituting the historically changing background consensus in which scientific activity takes place can impose on it some commonly accepted values, methodological rules of theory construction and explanation, some criteria of rationality, and can, by the same token, explain the universalization of scientific knowledge. Accordingly, I would regard the actual history of science as a realization of a series of different, consecutive and competing, socially accepted ideals of knowledge.

Without investigating and explaining the existence of such a background consensus (which obviously may be somewhat differently articulated in different disciplines and even in the work of different scientists) sociology of knowledge cannot go beyond the study of specific case-studies of knowledge production. Moreover, in presenting these case-studies the sociologists of knowledge are often tempted to treat the content of knowledge as an *unmediated* result of the local circumstances in which cognition takes place.

I do not know whether the program I am suggesting would be judged as "strong enough" by the sociologists. But I believe that it is the only way to give account of the evident specificity of science in respect to other products of human intellectual activities when we do not accept the conception of a supra-historical rationality of human nature.

What differentiates such a program from the old, the so-called "rationalist tradition", is the thesis that the background consensus is not the incarnation of immanent human rationality, and that it is not historically stable. What differentiates this program from at least some contemporary developments in the sociology of science, is the opinion that *if the circumstances of cognition have any impact upon the content of knowledge, this impact is not immediate, but rather is mediated by the relatively stable set of values and ideas constituting the research tradition.* It is just on the grounds of these traditions, which provide from within the resources for creative renewal, that new scientific knowledge is universalized.

10. Thus, the first point of my disagreement with the current developments in the social history and sociology of knowledge is the fact that its proponents do not ask the question which seems to me fundamental, namely, *how knowledge achieved in specific circumstances is universalized?* Most of them concentrate on the detailed study of the impact of more or less local circumstances of cognition which cannot explain the universalization of scientific knowledge. And universalization is a specific feature of science when we compare it to all other systems of beliefs or opinions — for example, philosophy, religion, morals, arts, customs. The sociologists of knowledge, even when they speak about universalization, usually discuss it only in terms of repeatability of experiments by means of commonly used instruments, whereas the universal acceptance of theories, which is obviously a different matter, is ignored.[14]

11. I believe that there are two main reasons for this lack of interest in the investigation of the background consensus in the frames of which science is practiced in given time.

The first reason, it seems to me, is that most of the sociologists of science simply do not believe in the existence of such a common background consensus in science. This is the case not only for the extreme social constructivists like Latour and Woolgar who claim that external "reality cannot be seen to have any discernible effect on the results of investigation which are manufactured and whose solidity is only a social construction"[15]. It is also the case for more cautious authors who go beyond the "ethnomethodology of laboratory life" in Latour's and Woolgar's sense and argue for the need to investigate the broader context of the "political economy of practices"[16], or "ecology of practices". But even in that case, the question concerning the background consensus shared either by the disciplinary community of specialists or the scientific community as a whole is not raised.

According to this opinion, science is a set of differently oriented practices, but there is no science as a culturally determined whole, and there are no individual sciences. There is no physics, but only specialized fields of research. The extent of such specialization is decided "empirically", on the basis of the actual institutionalization of research — such as what university departments, research institutes, journals, and instruments are used, which research problems certain groups of scientists are involved in. There is no community of physicists; there are only communities of people involved in some common practices. For example, T. Lenoir writes:

"Physicists [...] do not appear as homogeneous group with a unified culture, but as subcommunities with different knowledge, constitutive interests and with different experimental traditions organized socially in terms of access to different resources and oriented around different repertories of techniques and apparatus".[17]

What the term *practices* means is often not easy to understand. Sometimes, as in the Marxist tradition, practices are evidently opposed to theoretical activities and are called "technical practices". (The question of whether such practices are free from theoretical components and whether the opposition is sound must be left open here.) Sometimes the term is used in the broader sense in which any human activity is a practice and the terms "theoretical practices" or "interpretative practices"[18] are introduced in the sense that Althusser, Foucault or Bourdieu used. But do these terms have another meaning than the terms "interpreting" and "theorizing"? If everything that man does is a practice, why should it be used in such an equivocal way? I suspect that it is being used as a persuasive device: When science is regarded as a set of different practices it is much more convincing to speak about its direct social determinations, since it is commonly believed that human practical actions are determined or motivated by some conscious or unconscious interests or social circumstances. As far as thinking is concerned (and particularly as far as systems of statements are at issue), this belief is not so universally accepted. Still, the term surely tends to blur the differences between different human activities. At the same time — due to its common meaning — it reinforces the idea that science is a way of doing, of producing something (a telegraph, a bomb, a laser, a drug) rather than of knowing something in an abstract way. I will return to that point in the last part of this text.

12. This conceptual disaggregation of science into practices is perhaps one of the side-effects of the Kuhnian program. It was precisely T. Kuhn, who by the stress he put on the study of narrow communities of specialists sharing the same paradigm, opened the way for the *conceptual* disaggregation of science not only into the sciences, but further into narrow specialties, and, as the final result, into the set of unconnected practices which have to be assembled.

Kuhn, for example, did not, like Koyré, speak about revolutions in science consisting in the change of the "cadre des idées" in which science progresses, but about revolutions in paradigm oriented narrow specialties. This is one of the main differences between his attitude toward the history of science and Koyré's attitude to which I referred earlier. In both frameworks we are tempted to ask some definite questions and disregard others. For example, in Kuhn's framework there is no room for the investigation of the wider background consensus than that of different disciplinary paradigms or of disconnected "language games". By the same token, there is no room for asking philosophical questions concerning the scientific enterprise as a whole. (Kuhn himself did not go as far as his continuators, but he certainly blazed the trail.) However, if there is no science, but only specialties or practices (whatever the last terms may mean), there can be no philosophy of science and genuine philosophical problems concerning the whole enterprise. Both are reduced to more or less local problems of the social construction of knowledge and the methodology of practices. As a consequence the question of the difference between science and other human intellectual and practical activities must be regarded as obsolete. The way for asking the famous question "What is so great about Science?" is open, even if it is not explicitly said that science is not better than Azande mythology.

13. Does this conceptual disaggregation of science correspond to reality? Are the terms *science* and *scientific community* today only names for a set of disconnected activities or different narrow communities of specialists having nothing in common with one another — neither common method,

nor tradition, nor criteria for evaluation of their results, nor aims and commonly shared values? Do they play no common cultural role — regardless how appraised — in human life? I do not think that the actual process of specialization in science has gone as far as this conceptual desintegration presupposes.

Surely, we no longer believe in conclusive criteria of demarcation, but the fact that we are not able to draw such a sharp demarcation line between different activities does not mean that there are no essential differences between them at all.

No doubt scientific activity no longer is (and maybe never was) *exclusively* a disinterested search for truth; it produces not only systems of statements but also utilities. This does not mean, however, that the disinterested search for truth is not a value in scientific activity, and that science is not a system of theoretical statements. Scientists usually have other ways of deciding what is true and what is not, rather than by negotiation, unless the term *negotiation* simply means *debate.* I agree with P. Galison when in polemics with extremists like Latour he says:

"Experimentation should not be parodied as if it were not more grounded in reason than negotiations over the price of a streetfair antique"[19]

The stabilization of prices on the market, even on the free. market, is not the best metaphor for the way in which the results of scientific activity are universalized. There is a difference between negotiation, bargaining and scientific discussion.

No doubt the contemporary scientific community is not the "république des savants" that the enlightenment philosophers dreamed of, but this does not mean that there is no community at all. There are some reasons why not all human activities are regarded as scientific; even the most radical sociologists of science do not choose the objects of their investigations arbitrarily: They study some specific practices but do not investigate others. It means that in choosing their case studies they share some idea of what science was and is: What is this idea? What is the image of science or the scientific ideal they share?

D. The Anti-Theoretical Turn

14. The second reason for the lack of interest in investigating the long-term background-consensus lies, I believe, in the radical opposition against the so-called theory-dominated approach to science, represented by the traditional history of ideas in contrast to social history. The point

is, if that consensus exists, it is obviously a consensus of *ideas and values* commonly shared and transmitted in the scientific community. The program of explaining the development of science by some commonly accepted ideas, *even if those ideas are supposed to be in the last account socially codetermined, and historically not permanent, is regarded as not strong enough.*[20]

This opposition to the theory-dominated approach has its origins both in a particular vision of science and in some epistemological presuppositions.

According to this vision, science — especially contemporary science — should be treated as a set of skills for producing and controlling phenomena, rather than as an abstract system of statements about the world expressed in the form of general laws and theories suitable for applications in different domains of human activities. When Ian Hacking says

"think about practice not theory"'

and states that

"engineering not theorizing is the best proof of scientific realism"[22],

when he repeats Marx's famous saying that

"the point is not to understand the world but to change it"B, (Marx never said that about science but about philosophy),

he seems to express just this ideal. This is the reason why his book was so welcomed by the social constructivists.

15. To my knowledge, no historian of science has expressed this vision of science so forcefully and explicitly as Norton Wise in his interesting study of William Thomson, especially in the essay "Mediating Machines".

"The theory-dominated approach", he says, "divorces our knowledge from what we do; it also divorces it from what we care about, from our purposes. It separates pure science, whose reference is *supposedly* to nature, from applied science, whose reference is to our purposes."' (Italics mine — S. A.)

He explains:

"When we conceive nature itself as the source and referent of our knowledge, we deny the essential relevance to our knowledge of any so called external factors"[25].

If I understand correctly, this means what science is about is not nature but our practices of producing and controlling phenomena, and if we regard nature as the source and referent of our knowledge, no sociology of science is possible. The last opinion would just amount to saying that only a sociology of practice and not of abstract knowledge is possible, and that methodology of science should spring from the "methodology of practices". If this opinion were right, the idea of asking about the socio-cultural determination of the background consensus would be nonsense.

I suppose that after what I have said above I will not be regarded as a defender of philosophical realism. It is, however, one thing to say that our knowledge about nature is mediated by different factors (biological, cultural, social) and that therefore we cannot represent nature "as it is", independent of our cognitive activities, that no epistemologically privileged cognitive situation is possible; it is quite another thing to state that theories *are not about nature at all,* but about practices of producing and controlling phenomena. Until now astronomers, cosmologists, geologists, anthropologists or linguists neither produce nor control phenomena; what, then, are their theories about? The claim that our knowledge refers to nature does not imply that our theories are exact images of the world.

On the other hand, though much scientific knowledge is, of course, produced for application

"the most highly valued knowledge is produced for the consumption and use of colleagues in the process of producing innovations themselves."[26]

This is one of the important reasons why the model of applied science, even if it were adequate (I shall come back to that point in a moment), cannot be used as a model of all science.

"Long lived theoretical entities which don't end up being manipulated, commonly turn out to have been wonderful mistakes"[27],

says Hacking. I do not know how long is long enough to make that judgment true, but it took more than two thousand years before we could manipulate atoms. When Lysenko denounced genetics, one of his arguments was that we cannot manipulate genes, that they are "metaphysical entities". Generally speaking, it is not the first time that taking the state of one discipline as a model of all science turns out to be a "wonderful mistake". *The ideal of intervening as opposed to that of representing is an ideal of a culture in which the possibility of manipulating things is regarded as the supreme value.* Accordingly, science is regarded as a means of production, and the process of cognition is treated as *production* or *manufacturing.* Even the language used by social constructivists is adequate to this vision of science.

I have no doubt that today this ideal shapes not only some trends in the contemporary reflection on science, but also an important part of the scientific enterprise itself. But this is not sufficient reason for accepting it. H. Poincaré said:

"La science a sa cuisine mais elle n'est pas qu'une *cuisine.*"[28]

I think he was right.

16. Thus the ideal of science we are invited to accept is that of Bacon rather than that of Descartes or Galileo, that of W. Thomson rather than that of Maxwell, not to mention Einstein. It is an image of applied science radically opposed to that of pure theoretical science, the ideal of intervening, not of representing. Theories are appreciated as far as they help practices and they are regarded almost as their emanations constructed in a definite local social context.

"The truth of science should be located in the stable assemblies of practices" not in theories[29],

says Norton Wise, and he explains:

"Bacon's forceful dichotomy between deductive and inductive strategies applies if we read inductions as the assembly of practices."[30]

I believe he is right — what the social constructivists do defend is a kind of inductivism.

However, we must ask whether this inductivist model is adequate at least for applied sciences.

The fact that Thomson's inductive-utilitarian methodology was defeated in competition with the Maxwellians is seen by Norton Wise as

"a social victory of the deductive-theoretical ideal of physics over the practical."[31]

According to my opinion this "social fact" (What fact is not social?) resulted from the development of scientific knowledge which since the second half of the 19th century made technological progress increasingly dependent upon the construction and application of abstract theories rather than on inductive improvements by technicians — even if they were theoretically-minded technicians. In other words, I believe that Thomson was defeated by the Maxwellians because the development of science, especially of physics, made his Baconian-inductivist ideal of science obsolete. (The question why W Thomson defended this ideal is quite another problem and the answer to *this* question given by N. Wise seems convincing.) This victory was an outcome of a long historical process which led to the development of the modern applied sciences which have been increasingly based on the application of abstract theories.

Therefore, I would say that the anti-theory dominated approach to history of science runs against the pattern of the development of science including that of applied sciences. Their evolution was due — as Koyré said32

— to the imposition on *techne* of the rules of exactness which were hitherto specific only to the *episteme*.

The inductivist approach has one more consequence in the making of the social history of science, namely, the belief that case-studies concerning the production of knowledge in specific local circumstances may provide knowledge not only about these particular cases, but also an adequate vision of science development in general. It seems, however, that I have said enough not to have to explain why this assumption seems to be an inductivist illusion.

E. Final Remarks

17. It seems to me that contemporary developments in sociology of science accept an oversocialized conception of man. Not only is everything in man regarded as social, but furthermore, only sociological explanations are seen as good explanations of all aspects of human life. I would say it is a new kind of reductionism — a sociological reductionism. It is why any other program of explaining science development is said not to be strong enough.

I agree with Andrew Lugg[33] when he says that the meaning of the term *social* is not exactly the same when it is said that man is a social animal and when it is postulated that all his activities and their products should be explained in sociological terms. Science is a social activity in the sense that it is a collective activity, but this does not mean that its content is an immediate expression of a local social structure or of particular group interests.

But even setting aside this equivocation, it seems to me that the fact that today we cannot defend the old conception of the cognitive subject does not mean that all cognition is *directly* determined by social factors. The oversocialized conception of man does not take into account some fundamental facts of human life: some human activities and abilities even if in the final analysis they prove to be socially induced, may reach such a high level of autonomy with respect to their "final" causes or sources, that their sociological explanation may be misleading. The sociological reductionism — like other kinds of reductionism — underestimates this fact. There is something important in human affairs that we underestimate when we explain them by their final social determinations. Not because these determinations are fictions, but because they are not strong enough to explain all their far-mediated results. It is not true that we are autonomous knowing subjects. But it is also not true that we cannot overcome our social determinations at all, in any sphere of intellectual activity. Thus, men are capable of disinterested action and of a disinterested search for truth. The explanation that says that a disinterested action is motivated by some hidden social interests is as good an explanation as saying that any disfunction in human behavior is functional. It seems to me to be a poor explanation. And if everything is determined by interests, the term looses all its cognitive importance. Its only function becomes the propagation of an assumed conception of human nature. But if we accepted this assumption, we would be authorized to ask the question: *What interests determine the attempt to explain everything in terms of interests?*

I refuse to ask, and even more to answer this question, since I do not accept the presupposition. I do not accept this presupposition either as a universally valid statement of fact about human nature, or as a universally valid methodological norm. In the first sense it seems to be false, in the second it is meant to explain everything. A rule that is meant to explain everything explains in fact nothing.

Let me be so naive as to believe that at least sometimes when we believe something and when we say what we believe, and act accordingly, we are motivated not by interests, but by the search for truth (even if we do not agree on its definition and criteria). Or, to put it differently: that our common interests are to find the truth. This belief, even if naive, seems to be a necessary precondition for defending our culture from deliberate manipulations that endanger its survival in the world we are living in.

Postscript

1. As usual in such discussions, none of us was happy with the way his point of view was understood and presented by his opponents. However, I would like to avoid polemics and rectifications in this short postscript. Our texts and the works to which we referred are available to the reader, and he will be able to decide by himself which arguments seem convincing, and which miss the point. The main controversy was explicitly stated and it would be senseless to repeat those arguments here. *As long as the problem of universalization of scientific knowledge will not be solved, the social constructivism cannot pretend to provide an explanation of the general pattern of development of scientific knowledge.*

2. There is, however, at least one point on which I feel I was not clear enough to avoid misinterpretation. I am referring to my final remarks

concerning the oversocialized conception of man, where I renounced sociological reductionism which treats all human actions, intellectual as well as practical, *as immediately* determined by social situations and interests. *The conception of the knowing subject according to which he is unable to resist social or cultural pressures at all, seems to me no less simplified than the conception of the fully rational, autonomous subject I spoke of in part* ***B*** *of my presentation.* In this sense a disinterested search for truth must be regarded as *possible* only if, for methodological reasons, we do not accept the argument saying that the resistance to some pressures means simply submission to other even stronger constraints. I have already said why this argument seems unacceptable to me.

3. Two arguments were advanced against this point of view: one, saying that there is no eminent "contradiction between the pursuit of interests and the pursuit of truth", and the other, stating that "the disinterested pursuit of truth is a dangerously naive myth — dangerous because it provides a standard argument today for researchers to work on whatever project they wish without responsibility for the interests that support it. [...] If we wish to avoid political manipulation, we would be far better off to examine with our eyes open the conditions of *interested* activity under which knowledge is regularly produced."[34]

4. As far as the first argument is concerned, I would answer that it would be sound if and only if it spoke about *universal* and not *particular* group interests as sociologists usually understand the term. If the "interests" are interests of particular groups, and the pursuit of truth is immediately determined by group interests, then in order to say that "there is no contradiction between the pursuit of interests and the pursuit of truth" we must accept the thesis that there exists a group whose interests are universal human interests — be it the working class as Marx claimed or the intellectuals as Mannheim claimed. It seems that none of us accepts this conception.

5. As far as the second argument is concerned, I think that if a disinterested cognition were not *possible,* we could not expect that somebody might renounce — against his interests — to participate in research which should be condemned for moral or political reasons. *Such a participation may be morally or politically condemned only if a disinterested action is in principle possible, if we are not utterly determined in all our actions by the social situations we are living in.*

What I said does not obviously mean that *all* scientific research is motivated by a disinterested search for truth. It only means that such a search for truth is possible, and that it is dangerous for our culture to deny this possibility. The oversocialized conception of man reinforces this danger.

Understood in that way, my thesis cannot be used as an "argument for researchers to work on whatever project they wish without responsibility for the interests that support it". It would be a misuse since in order to use it in that way, it must be presupposed that the search for truth is the highest value and never should be subordinated to other values. If we do not accept this presupposition (and none of us, as it seems, does accept it), then we agree that in some social situations a disinterested search for truth in some matters should be postponed for better times.

Notes

1 The present text is the original version of the lecture I read at the Wissenschaftskolleg zu Berlin on June 14, 1988. Invited to publish it in the Institute's Jahrbuch, I added a short postscript taking into account some of the comments made during the two discussions in the Kolleg.

2 J. R. Brown (ed.), *Scientific Rationality, the Sociological Turn.* Dodrecht: D. Reidel, 1981.

3 I use the term introduced by L. Kolakowski in his essay: "Epistemologiczny sens etiologii wiedzy w: Czy diabel moze byc zbawiony," *Aneks,* London 1982, pp. 35-46.

4 N. Wise, "Mediating Machines", *Science in Context, 2* (1988) pp. 77-113; N. Wise and C. Smith, "Measurement, Work and Industry in Lord Kelvin's England", in: *Historical Studies in the Physical and Biological Sciences,* Cambridge: U. P.

5 J. Farley and Geison (1974), "Science, Politic and Spontaneous Generation in Nineteenth Century France. The Pasteur-Fouchet Debate", *Bulletin of the History of Medicine,* 48, pp. 161-198.

6 P. Forman, "Weimar Culture, Causality and Quantum Theory 1918-1927", in: R. McCormmach (ed.), *Historical Studies in the Physical Sciences,* N 3, Philadelphia.

7 See the letter to the *New York Review of Books,* 1975, 22, 18.

8 T. S. Kuhn, *The Structure of Scientific Revolutions,* Chicago: University of Chicago Press, 1970, (2nd edition), p. 9.

9 S. Amsterdamski, "Le concept du sujet cognitive et l'évolution de la science", *Fundamenta Scientiae, 6* (1985), pp. 313-325.

10 I mean Popper as we know him from the "Logic of Scientific Discovery", but not from the "Objective Knowledge".

11 K. R. Popper, "Epistemology without the Knowing Subject", in: *The Objective Knowledge,* Oxford: Clarendon Press, 1972.

12 A. Koyré, "De l'influence des conceptions philosophiques sur l'évolution des théories scientifiques", in: *Etudes d'histoire de la pensée philosophique,* Paris, 1961, p. 236.

13 S. Amsterdamski, *Miedzy historia a metoda, PIW,* 1983; *Tra storia e metodo,* Roma, 1987.

14 This remark does not concern P. Galison, who differentiates long-term constraints in experimental as well as in theoretical investigations. For example he writes: "Such presuppositions offer an analogue to Braudel's geographical time, for they are not attached to the goals of any single research group and frequently not even to a single scientific specialty. [...] such beliefs lasted for centuries. Sometimes, as Gerald Holton has argued, transcultural commitments may come in `thematic pairs' such as the belief that nature must be explained in terms of continuous or in terms of discrete matter." *(How Experiments End,* Chicago: University of Chicago Press, 1987, p. 247.)

But even Galison does not notice that the epistemological significance of his long-term constraints is not the same as the significance of middle and short term constraints.

15 B. Latour and S. Woolgar, *Laboratory Life, The Social Construction of Scientific Fact,* Beverly Hills: Sage, 1979; B. Latour, *Science in Action,* Stony Stratford: Open University Press, 1986.

16 T. Lenoir, Introduction to *Science in Context, op. cit.* (fn. 4); N. Wise, "Mediating Machines", *ibid.*

17 Lenoir, Introduction to *Science in Context, ibid., p.* 4.

18 *Ibid.*

19 Galison, *op. cit.,* (fn 14), p. 277.

20 For the criticism and defense of the intellectual history of science, see: A. Lugg, "Two Historical Strategies: Ideas and Social Conditions in the History of Science", in: *The Sociological Turn, op. cit.* (fn 2).

21 Hacking, *Representing and Intervening,* Cambridge: Cambridge University Press, 1983, p. 274.

22 *Ibid.*

23 *Ibid.*

24 N. Wise, "Mediating Machines", *op. cit.* (fn 4), p. 55.

25 *Ibid.*

26 R. Whitley, *Intellectual and Social Organization of the Sciences,* Oxford: Oxford University Press, 1984, p. 11-12.

27 Hacking, p. 275.

28 H. Poincaré, *Les sciences et les humanités,* Paris, 1908, p. 31.

29 N. Wise, "Mediating Machines", *op cit.* (fn 4), p. 52 and 54.

30. *Ibid, p.* 53.

31. *Ibid, p.* 53.

32 Cf. A. Koyré, "Du monde de l'à-peu-près à l'univers de la précision", *in: Etudes d'histoire de la pensée scientifique,* Paris, 1961, pp. 311-329.

33 A. Lugg, "Two Historiographical Traditions: Ideas and Social Conditions in the History of Science", in: *The Sociological Turn, op. cit.* (fn 2).

34 Both arguments were advanced by Norton Wise in his "Rebuttal" to my text which he read during the discussion.