SOME REMARKS ON THE RELATIONS BETWEEN SCIENCE AND VALUES

Diderik Batens

"Wer die tatsächlichen sozialen Verhältnisse analysiert und ihre ideologische Maskerade durch seine Kritik destruiert, wer die sozialen Wirkungszusammenhänge aufdeckt und dabei die tatsächliche Rolle bestimmter Faktoren und ihre ideologische Verkleidung aufzeigt, der hat damit unter Umständen Aussagen von grösseren politischer Relevanz gemacht, als wenn er sich "normativ" geäussert hätte ..."
Hans Albert [1]

"Now the time is ripe that the main call should be : Scientists of all countries, unite ! Create a forum which can be looked upon by mankind with trust, and which is able and willing to give advice in all the most vital questions with objectivity and from the highest ethical level."
Hugo Boyko [2]

"Die Idee wissenschaftlicher Wahrheit ist nicht abzuspalten von der einer wahren Gesellschaft."
Theodor W. Adorno [3]

"Technological rationality has become political rationality."
Herbert Marcuse [4]

".. l'idéologie le plus dangereuse et la plus puissante aujourd'hui est le scientisme ..."
The editors of Survivre [5]

"... in einer Zeit, da schon die Spatzen die Forderung nach 'Politisierung' der Hochschule von den Dächern pfeifen ..."
Wolfgang Stegmüller [6]

1. *Some attacks on science.*

During the nineteenth century and during the first half of our century, the most current objections against science stated that scientific statements were, although true, unimportant in some or

other sense. Cristian churches, e.g., preached that Christ and his
Message were The Truth, whereas science, as well as technology for
that matter, was relevant only to man's earthly life. All kinds of
romantics, as well as people impressed by Eastern thoughts and
practices, advocated the unimportance of scientific knowledge as
compared to some or other kind of feeling or some or other kind of
intuitive experience. These arguments were not directed against the
validity of scientific statements with respect to their supposedly
specific domains, nor were they directed against the usefulness of
science. Their point was that science, and especially natural science,
was less important than other things, such as religion or "culture",
and this in virtue of an explicit or implicit anthropology.

To this attack corresponded a sociological reality : the existence
of certain groups, the factual validity of certain values and norms e.g.
with respect to the choice of a high school curriculum, etc.

In the late sixties Western Europe and Northern America
experienced the student revolts. In Europe–I am less informed about
America–these revolts were directed in the first place against the
university structures and, given their dependence on the structure of
society, against the latter as well. Attacks against the way scientific
research and the teaching of science were actually performed at the
university came along quite naturally. Mainly two kinds of arguments
were proposed. First of all, the university's (finantial and other)
relations with industry and with military institutions (national
armies, NATO, etc.) were seen as an indication that actual research
was not aimed at the progress of scientific knowledge itself, lest at
the interests of the people. The second argument had to do with the
lack of meta-level reflection with respect to the choice of subjects for
research and for teaching. It went more or less as follows. As far as
these subjects are not directly related to industrial or military
interests, they are choosen on the basis of the accidental interests of
some professor and justified in a pretty circular way (a subject is
supposed to be important because of the importance of the teacher
or because there is a publication about it; the teacher is supposed to
be important because the members of his own group, his "reference
group", appreciate him by virtue of his being a member of the group
or by virtue of his doing the things that other members of the group
are doing; and a paper is published because it is appreciated by the
members of the aforementioned group). As a consequence of all this,
actual scientific practice misses any guarantee on either scientific or
social relevance.

In some universities the attack on the actual scientific practice
turned into an attack on science, or at least on scientism, i.e. the set

of ideological components connected with science in our society. Several relevant texts of the period were collected by Alain Jaubert and Jean-Marc Lévy-Leblond[7]. To some extent this attack goes back on thoughts of the Frankfurter Schule and Herbert Marcuse. The most extreme statement was that present-day scientific practice was actually supporting a conservative political and socio-economical ideology[8]. Notice that this statement is not inconsistent with the belief that science has played an important role earlier in the fight against conservative and inhuman ideologies (that do not exist any more in their earlier forms).

A neatly distinct attack on science came from people who got aware of our present ecological situation. I mean the ecological situation in its broadest sense here, including overpopulation, the energetic resourses left, ect. Recently this awareness was provoqued on a large scale by the two "Reports for the Club of Rome Project". It meant, for lots of people, the end of the myth of progress through science. It gave birth to another vogue of pessimism and lead, in its most stupefied extremes, to measuring people's publications in terms of the number of trees that had to be cut down for making the printing of the publications possible.

The former attack was ideological in nature. It furthermore presupposes a view on society that conflicts with our actual society, in its basic social and economic organisation. No wonder the attack was mainly supported by people whose ideology contained (sometimes very contaminated) Marxist views. The later attack fits with almost any ideology, and is up to be combined with whatever objections one has against science, against actual scientific practice, or against any field of applications of science or technology. This might help to explain the success of the latter attack. Another element of this explanation might be the fact that publications like the reports for the Club of Rome are presented in a scientific guise. From a social-psychological point of view, the fact that information is presented in a scientific guise provides it with a greater persuasive power, even for people who want to use this information to support a point of view which is, in some or other sense, anti-scientific.

I have preferred to begin my paper by describing some facts instead of by mentioning arguments as such. The reason for doing so is that I believe that the aforementioned facts were important in that they made people realise the weight of the connected arguments. This will become more clear later on. Of course I am not going to discuss the problem in connection with these attacks.

The more I read and thought about the subject, the more I got perplexed by its extreme complexity and by the amount of

confusing statements made in connection with it. As we all know, the term 'science' denotes quite a few different things : a set of theories, a set of theories in their historical development, an ideal set of theories (to be reached at the end of time), a methodology (either actual or ideal), etc. Analogously for 'technology'. Even worse for 'the industrial technological complex'. Etc. Each of these "sciences", "technologies", etc. may be linked in numerous ways to value judgements. Each of them might furthermore be judged good or bad. Some claim that science is value-free. Others claim that it is not and that it furthermore should be directed towards the good values. Still others claim that scientism should be fighted. And so on.

The trouble with such a complex subject is that a discussion, however valuable and serious, of a part of the problem is easily provoking a misleading interpretation, not in the least for the authors themselves (the feeling to prove a good point leads easily to a kind of euphoria which makes one forget connected problems). Part of the literature on the subject proves more the author's sincere motivation than it proves their discursive capacities or their courage to confront a strong opponent. In the aforementioned book edited by Jaubert and Lévy-Lebrun we find a treatment of the following myth on science :

> "Tout ce qui peut être exprimé de façon cohérente en termes quantitatifs, ou peut être répété sous des conditions de laboratoire, est objet de connaissance scientifique et, par lá même, valable et acceptable. ..."[9]

The muth is proven false by a bright *reductio ad absurdum :*

> "Une nouvelle science, la polémologie ou science de la guerre, a même été crée par des pacifistes bien intentionnés [sic !]. Donc la guerre est acceptable, étant un object d'investigations scientifiques."[9]

Of course there are more valuable papers on the matter. But even these suffer now and then from unworthy arguments. Although freshmen learn to identify an *argumentum ad hominem* in "introduction to logic" textbooks, Ernst Topitsch writes :

> "... jene Kulturkritiker, die über Druckerpresse, Radio und Fernsehen der Menschheit verkündigen, sie habe den äussersten Grad technischer Barbarei erreicht, und die oft die ersten sind, welche angesichts der jüngsten

bahnbrechenden sowjetischen Fortschritte ängstlich und
ungeduldig-vorwurfsvoll nach der Technik Amerikas
blicken."[10].

Even if we leave such arguments out of account, most papers on the
subject suffer from one-sidedness. Herbert Marcuse's *One
dimensional man*[11] points to negative consequences of science
and/or technology–and even from this point of view his analysis is
obscure and confusing–but fails completely to offer an analysis from
the point of view of epistemology and philosophy of science (even
worse, Marcuse proves not to be aware of the present state of the
philosophy of science).On the other hand, Wolfgang Stegmüller, Hans
Albert, and Ernst Topitsch[12] present interesting thoughts on the
matter, especially from an epistemological point of view, but fail to
pay enough attention to the sociological implications of the actual
organization of scientific and technological practice. Sometimes they
seem to be arguing without any reference to actual social reality, as
may be illustrated by the following statement by Hans Albert :

> "Man wird auch durch normative Formulierung des
> technologischen Systems Andersdenkende nicht von seiner
> Ausnutzung für andere Interessen abhalten können, *wenn
> sie die nötige Macht dazu haben.*"[13] (my italics)

The italicized passage sound almost cynical. Social reality as well as
the actual scientific practice are greatly disregarded in an older
(1946) booklet on science and society by Michael Polanyi.
Commenting on "sovereignty over the world of science" he writes :

> "Every time a scientist makes a decision in which he
> ultimately relies on his own conscience and personal
> beliefs, he shapes the substance of science or the order of
> scientific life as one of its sovereign rulers [...] inasmuch
> as a scientist is following the ideals of science according to
> his own conscience, the resultant decisions of scientific
> opinion are rightful."[14]

Science is pictured by Polanyi in such a way that the question as to
its value-ladenness is simply out of order. The "ultimate point to
which we can trace the roots of our conviction espressed in affirming
any particular scientific proposition as true" appears to be "the
sovereignty of a free public opinion" in "a community pledged to
seek the truth"[15]. The latter entities are not considered as "ideals",

but are directly associated with "the institutions which give shelter to free discussion in a free society". This expression is immediately followed by a text which leaves no doubt about the interpretation of the expression : "In Britain, for example, there are the Houses of Parliament; the courts of law; the Protestant churches; the press, theatre, and radio; the local governments, and the innumerable private committees governing all kinds of political, cultural, and humanitarian organizations. Being of democratic character, these institutions are themselves guided by a free public opinion."[16] Etc. We are confronted here with an "ideal picture of science and society" which is uncritically and erroneously identified with actual science and actual society. Given the peculiarities of the British election system (for the House of Commons, not to mention the House of Lords), and given the peculiar way in which Protestant churches and British radio stations are democratic, it is also clear that 'democratic' is used here in a pretty tautological way.

The problems to be dealt with in this paper are so complex that one should not hope to solve even a part of them, and that one cannot even hope to present a fair overview of the literature within a short article as this. All I shall try to do is to clarify the problems a little bit, and to indicate some points that should not be overlooked by one who tries to form himself an opinion on the matter.

2. *Science and technology*.

It is an idea of Popper's[17] that a scientific theory can be described as "forbidding" certain events, and that its content can be measured by measuring the amount of events it forbids. Following the aforementioned paper of Hans Albert[18] one may characterize a technological theory by the fact that it contains only statements concerning possible human actions and with respect to a given set of ends. In contradistinction to a scientific theory, a technological theory does not forbid the occurrence of certain events, but does forbid actions that bring about certain events. Among the latter events are not only those that are physically impossible on the corresponding scientific theory, but also events that either cannot be brought about by human actions—and this "cannot" denotes again a technological modality— as well as—and this is the important point to our present subject—events that conflict with the given ends.

As McMullin[19] states, the term 'science' is used to denote a set of propositions (descriptions of facts, hypotheses, scientific theories, etc.) as well as to denote, in other contexts, a set of activities of scientists together with the aforementioned set of propositions.

Analogously the term 'technology' is used to denote a set of propositions (including technological theories) as well as a set of activities (e.g. the research connected with the articulation of a reliable technological system) together with the aforementioned set of propositions. For clarity's sake I shall use the terms 'science' and 'technology' in such a way that the application of science, or of a science, in view of the realization of certain ends does not belong to science itself, and that the application of technology, or of a technology, in view of the realization of certain ends does not belong to technology itself. I am well aware of the fact that this convention might seem to be a little bit unusual, especially with respect to technology. It seems to me, however, that it is advisable to distinguish between a theory and its applications, one of the reasons being that this distinction enables one to separate the value-ladeness of an application of a theory from the value-ladenness of the theory itself.

Several people subscribe to the view that the distinction between a scientific theory and a technological theory does not make sense. Etienne Vermeersch did so in a discussion with me and will presumably also do so in his contribution to this issue. I must confess that I feel the distinction to be meaningful, but that I do not have a sound answer to each argument that shows that a theory which is intuitively speaking clearly scientific, falls in fact under the definition of a technological theory. This problem, however, is not central to this paper. It has also been argued by several authors that technology cannot be described adequately either as applied science or as "restricted" science. These authors point especially to the difference between the aims of scientific and technological systems[20]. I have not found the time to study these matters in detail or to find out the consequences of such a position for the value-ladenness of technology.

3. *Values and Science.*

In my opinion values and ends are intimately connected. To each set of values corresponds, in a given situation and for a given individual, a set of ends, and *vice versa*[21], although the ends, respectively the values, may very well be implicit. Also, values are involved whenever a preference ranking is introduced. This means that science (in the broad sense) is value-laden whenever propositions or activities that belong to science, are expressing or presupposing an end or a preference ranking. It seems to me that preferences are more basic than ends and that ends are more basic than values. More

especially, it seems to me that values are "theoretical" constructions that are constructed on the basis of a given set of preference rankings and ends–or better : values, whenever introduced, should be constructed on the basis of such a set. The question of what is basic to what is, however, inessential to what follows.

It is also important to distinguish between values, ends, and preference rankings that are present in the consciousness of individuals or groups, and those that are implicit within the actual behaviour of these individuals and groups, or within the institutions that exist in a society. The question whether or not science is value-laden can obviously not be restricted to the question whether these individuals or groups see their scientific activities as connected to some end. The former question can even less be reduced to the question whether these individuals or groups intend to pursue certain interests by means of their scientific activities. That science and technology are value-laden depends on relations between values, ends, and preferences on the one hand, and science and technology on the other hand. Because of what was said before I shall distinguish between three kinds of such relations : those that are conscious to individuals and groups who perform scientific and technological activities, those that are of a conceptual kind, and those that are sociological or psychological in nature. Let us start with conceptual relations. It goes without saying that none of the following enumerations is intended as exhaustive.

(a) Each research project and each theory is actually directed towards a certain end : (the acquisition of) knowledge that enables one to solve a more or less determined set of problems. Each research project presupposes an at least implicit choice, viz. the choice of a definite attempt to solve a set of problems rather than another. The set of problems that is intended here must be considered as narrow as possible. The end which is conceptually linked to a research project or theory does not involve the solution of all problems to which the research project is relevant, but does involve the solution of these problems which can be solved directly by means of the information that the research project is centered on or that the theory supplies. So e.g. the end connected with an optic theory will involve, among other things, the calculation of the properties of lenses, but will not involve the construction of a telescope as such or the construction of a projector as such. I return on this distinction later on.

The choice of a subject or of a field of research has not to be separated from the choice of a research project in connection with the conceptual relations considered. Indeed, the values–respectively ends or preferences–that are involved conceptually in the choice of a

subject or of a field of research are completely determined by the values–respectively ends or preferences–involved in the choice of the research project, i.e. the choice of a definite attempt to solve a certain set of problems. (Compare, however, with the corresponding statements below concerning the other kinds of relations).

(b) A research project also involves the choice of a certain method–or a set of methods. A research project is one definite attempt among a set of possible attempts to solve a certain set of problems. The choice for a method is, at least conceptually, connected with a high preference for this method. Since methods are not ends in themselves, this preference must rely on efficiency considerations, *in casu* the factual statement that the method considered is optimal, at least among known alternatives, with respect to the end meant *sub* (a).

(c) A theory determines a more or less defined set of possible applications in action and in explanation–analogously for a research project. The set of problems that can be solved by means of a theory is obviously much wider than the set mentioned *sub* (a) : the latter forms a proper subset of the former. Which problems belong to the set meant *sub* (a) can be found out in principle by means of a theory of cause and effect and by means of a general theory of action and observation. If a given theory enables one to find out that, say, an arbitrary object a has both property A and property B, and if every act which leads to the ascertainment of Ba also leads to the ascertainment of Aa, and not *vice versa*, then the problem as to find out whether or not an object has property B does not belong to the problems meant *sub* (a). Analogously, if a theory contains information that enables one to transform a situation A into a situation B and if it enables one to transform a situation C into a situation D, and if every transformation of C into D results from a transformation of A into B, then the problem to transform a situation C into a situation D does not belong to the problems meant *sub* (a)[22].

The choice for the construction or elaboration of a theory (analogously for a research project) which is connected with a certain set of possible applications, apparently presupposes again a preference ranking among sets of problems.

(d) All that was said *sub* (a) and (c) may be restated with respect to problems situations that may possibly arise within the actual world, and also with respect to problem situations that, in view of the warrantly accepted knowledge about the actual world, may reasonably be expected to arise.

(e) With respect to empirical control scientific and technological

theories depend on the "facts" present in the actual state of affairs. The contingent properties of this state of affairs might very well be codetermined by human actions in which values, ends and preferences play an explicit or implicit role.

(f) As we have seen earlier, a technological system differs from the corresponding scientific system in that it contains only propositions that are relevant to possible human actions with respect to certain ends. These ends are connected with the corresponding sets of problems mentioned *sub* (a) and *sub* (c), without being necessarily reducible to one of them.

(g) Scientific and technological theories also force upon the people who use them a certain "world-view", a certain way of looking at the world. That this is so should need no further explanation after the works of Kuhn, Hanson, Feyerabend, and others[23]. It should be noticed, however, that this phenomenon is also important in the context of the value-ladenness of science. The fact that the world is approached by means of a given set of scientific theories narrows down the possible outlooks on the world. Not only man's action possibilities and explanation possibilities but also his most direct observation of and thinking about the world are influenced by scientific and technological theories.

(h) There is also a (deductive and inductive) relation between scientific and technological theories on the one hand and metaphysical theories and explicit value theories on the other hand. Just to mention one aspect of this connection : value statements presuppose and entail factual statements.

Let us now turn to some relations between values and science, which are sociological or psychological in nature.

(a) The fact that certain problems are tackled and the fact that they are tackled in a certain way are determined to a larger or smaller extent by value judgements that are implicitly or explicitly accepted by a society or by a social group. This holds for science as well as for technology. This relation is not only dependent on the finantial support of research projects, but also on the training of the researchers, on their limited imagination with respect to subjects and methods of research, on the fact that researchers are subjected to social sanctions in their choice of a research project and of a research method, and on the fact that science and technology develop in part according to a proper dynamics (largely independent of the conscious intends of individuals or groups).

(b) Ideological factors play also a certain role in the proces of hypothesis formation and theory formation. In certain cases this role may be restricted to vague analogy mechanisms. Well-known

examples are the analogies between Linnaeus's and Darwin's biological theories and the social structure of the societies they lived in. Especially with respect to the human and social sciences the ideological influence on theory formation may have consequences for the direct ideological impact of the theory. Darwinism in biology may have had some ideological impact, but the so-called social Darwinism (Herbert Spencer) certainly had a lot. The ideological influence on theory formation is especially important since the basic principles of a theory will not become rejected unless in case competing theories with different basic principles are present[24].

(c) A scientific or technological theory may have numerous possible applications in principle, but it is obvious that only some of them have actually a chance to become realized, at least within a foreseeable future. The socio-economical structure of our society (societies) determines a rather limited number of alternative sets of ends and preferences. It furthermore determines, with respect to the economical means required by certain applications of a theory, which applications might actually be realized.

(d) The successes of science and technology have resulted in a widespread positive evaluation of scientific and technological knowledge, more precisely with respect to its reliability. These successes derive mainly from characteristics of the methods of science and technology. Through social-psychological mechanisms this positive evaluation has been transferred to science and technology themselves in their actual forms, including the body of actual scientific and technological knowledge and including their actual organization. The positive evaluation has furthermore been transferred to the separate actions as well as to the general practice by which science and technology are applied, and hence to these applications themselves. Obviously these applications will still be condemned morally in case they conflict patently with existing moral sentiments (e.g. using technological knowledge for exterminating large numbers of a human "race"), but if they do not patently conflict with such sentiments, they have a good chance of receiving a positive evaluation by virtue of their being applications of science and technology. The positive evaluation of scientific and technological knowledge has also had consequences–again : not by logical mechanisms but by social-psychological mechanisms–for the evaluation of problems and of fields of knowledge which cannot be approached by means of science as it stands. Either such problems are considered to be pseudo-problems, or else they are considered as not subject in principle to scientific methodology. The corresponding knowledge is either taken to be pseudo-knowledge, or else is taken to

be essentially different from scientific knowledge in nature.

(e) Analogous social-psychological mechanisms as those mentioned in (c) play a role in transferring both methodological rules and scientific or technological statements to extra-scientific contexts, even if this transfer cannot be justified by logical or methodological means. The transition from operationalism in science is operationalism "in society" is discussed at large by Marcuse[25]. That the transfer of scientific statements to non-scientific contexts may be unjustified and dangerous becomes immediately clear if one realizes that the same terms may have a relatively exact and clear descriptive meaning in a scientific context and at the same time be heavily laden with all kinds of connotations in other contexts. Just to take one example, consider the term "intelligence". Within a given psychological theory this term may have an exact descriptive meaning with reference to a test battery, whereas it has numerous evaluative, actional, and other connotations in everyday language.

(f) The continuous growing of scientific knowledge and of the reach of its applications have naturally lead to an idea of progress. On the other hand there has been a definite progress in the living conditions of people in certain parts of the world, especially Europe and North America, and this progress has been made possible to a large extent by the progress in scientific knowledge. Psychological mechanisms are responsible for the fact that an idea of progress was connected to the notion of "applying scientific knowledge" as such, strengthening the positive connotations which were already present with applications of science and technology (compare with (c)).

(g) Apart from the logical or conceptual connection between science and ideologies or value-systems, there are also psychological and socio-psychological connections between them. The conflicts between heliocentrims and Darwinism on the one hand and christian churches on the other hand cannot be reduced completely to a contradiction between the theories and a literal interpretation of the Bible. Apart from such a contradiction those scientific theories were also psychologically in conflict, at least for christians of these periods, with the christian ideas on man's place in nature. This does obviously not only hold for religions but for all kinds of ideologies[26].

Let us finally mention a few points concerning relations between science and values which are apparent in the consciousness of individuals and groups involved in scientific activities. First of all we are confronted here with the motivations for engaging in a certain scientific activity. These motivations are connected with the values and ends of the individual or group. It is clear that all

aforementioned factors, as far as the individual or group has (correct or incorrect) views in this connection, play a role within the individual's or group's motivation. Apart from the motivation one should also point to the attitudes of the individual or group with respect to science and technology. These attitudes, which will depend on the relevant attitudes in larger groups, will not only include a more or less extended evaluation of science, but will also contain specific attitudes towards the need for justifying one's involvement in science. In other words, these attitudes will, among other things, determine whether or not the individual or group will try to gain information about the relations between science and technology that were mentioned earlier.

4. *Rationality*.

A discussion of the justifiability of the present sciences is only meaningful if it can rely on a set of information and on a methodology which are at least provisionally accepted. This set of information and this methodology should be made as explicit as possible. To the extent that the methodology, which has among other things to determine which arguments are legitimate, is not explicit, such a discussion will rely on an implicit methodology, i.e. one which cannot itself be subjected to critical discussion. Analogously for the provisionally accepted information. The discussion of the justifiability of the sciences should proceed on the basis of an explicit "rationality".

The reader might object that an explicit rationality does by no means provide us with a stable basis in this context, since it might be asked in turn whether this rationality itself is justified. Hence, the problem is only pushed a little further, without being solved. In general this objection is quite correct, and it is clear at once that there can be only two ways out. The first is that one accepts the rationality on some extra-rational basis, such as the belief that it is possible to provide a correct "naturalistic" definition of 'rational', e.g. referring to a deity, to history, and the like. However, a rationality is essentially a methodological tool. This means that it should be justified with respect to its usefulness. To accept a rationality on an irrational basis is by no means more justified than to accept an irrational methodology in the first place. To put it in other words : if a rationality may be based on an irrational ground, then it cannot offer *in principle* a rational argument in favor of itself and against alternative such rationalities. Consequently, it will only be a rationality for reason of stipulation and not for reason of

justification, and it will have to consider different rationalities as rational as itself, except for stipulations. Hence, a true rationality should be justified by rational means itself. In order to escape a *regressus ad infinitum*, this can only be done–and we now come to the second way out– by accepting a provisional rationality, the critical power of which can be applied to itself, and which is subject to a constant stream of new information which may lead to changes in the rationality itself (the well-known idea of self-correction). Only such a whole of methodological rules can in my opinion be truly called rational, as I have tried to argue at some length elsewhere[27].

Although one may have objections against particular aspects of the methods used presently by the sciences, I cannot see how one could rationally hold that one disposes of a better method for arriving at descriptive knowledge which proves useful with respect to application to action in and understanding of reality[28]. Consequently, if someone holds a view that conflicts with scientific knowledge, then he might very well be right, but it would nevertheless be irrational for him to rely on *his* presumed knowledge as long as he didn't warrant it by means of a method of which he can demonstrate that it is at least as reliable as the scientific methods relevant to the corresponding scientific knowledge. We all know, of course, that not all our scientific knowledge is warranted to the same extent. So, in some cases it might be easy enough to show that there is a more reliable alternative to the concrete method that was used to arrive at the given piece of scientific knowledge. This, however, does not weaken my point but strengthens it. From the present primacy of the scientific methods, it follows also that one should rely on scientific methods for gathering the knowledge that is needed for the elaboration and application of one's ideology. All this means that no ideology (in the broad sense of the term) may hope to have a claim to the label 'rational', unless it incorporates, at least provisionally and possibly with some reserve, the scientific methodology.

With respect to criticism on the misuses of scientific knowledge, Adolf Grünbaum wrote more than twenty years ago :

"It is both true and important to realize that scientific theories often exert powerful ideological influences outside the domain to which they are relevant and that they may be widely abused sociologically. Nevertheless, it is a fundamental error to forget that the truth or falsity of particular theories of psychology, biology, or physics is quite independent of the actual or potential abuses that may be made of them by groups in society. It is one thing

to say that a theory is false in the light of the pertinent evidence *and* that, in addition, it is being misappropriated for dubious social ends. But it is quite another thing to say that a given scientific theory is false *because* it is thus being misappropriated."[29]

Grünbaum's statement may easily be extended to other kinds of relations between science and values. Even with respect to those kinds of value-ladenness of science, which affect science in its contents and methods, a similar although somewhat less far-reaching reasoning applies. Indeed, no better method than the scientific is yet available. What is more, if an arguably better method would be devised at a given time, it would become by then the scientific method, and replace the older one.(The scientific community might, of course, have turned so irrational by the time, that it would stick to the old method, but the new method would nevertheless be demonstrably better than the old one.)

The fact that science is intimately connected with values and ideologies shows the desirability of an ideological pluralism–I mean, one in which the different ideologies have a full influence on different parts of the scientific community–for the development of science and for the discovery of truth. In the closing paragraphs of the last article that I quoted from (see last note), Adolf Grünbaum refers several times to "the ethical perplexity of modern man". As long as this ethical perplexity remains, an ideological pluralism will be necessary. Of course, such a pluralism is not enough to guarantee a fast development of scientific knowledge in a given situation. It is also necessary that the scientific community finally faces the falsehood of the Weberian absolute separation between science and values (and it is a fortunate event that people of the most unsuspectable sort are criticizing this absolute separation[30]). We should make the relation between science and ideology itself to a subject of scientific study (I return on this in section 5). Only in this way we might be able to put an end to the situation in which this relation is implicit and beyond control, and in which scientific activities are not so much influenced by the scientist's ideology as by the greatly unknown ideology that was implicit in earlier scientific activities and practice.

Nothwithstanding my strong defence of the role that science has to play within any sound rationality, I am not prepared to subscribe to the thesis that rationality coincides with science. The following statement by Wolfgang Stegmüller sounds rather puzzling to me :

"Die Entscheidung für den Beruf des Wissenschaftlers is
allerdings in *einer* Hinsicht etwas mehr als irgendeine
beliebige Berufswahl. Es ist die *Entscheidung zur
Rationalität.*"[3][1]

Let it be perfectly clear that Stegmüller does not intend here to
make a statement concerning the ends and motives that play a role in
the choice of a profession. Still, the sense of his statement is not
quite clear. The choice for the profession of a cabinet maker, of a
schoolteacher, or of a bus driver, is certainly not a choice for
irrationality. Presumably Stegmüller intends to say that science
equals rationality, although one never chooses the profession of "a
scientist" but always a profession in a specific science. But let us
return to the main point. Apart from science itself, the justification
of scientific methods, the application of scientific knowledge in
action and explanation, and the like, are certainly rational activities.
Of course, a proponent of the "rationality equals science" thesis
might reply that he considers such activities as belonging to science.
In order to avoid spilling words on a partly terminological point, let
us at once consider a patent case of a rational activity which turns
out to be not a scientific one. Let us take a case which falls
completely under the ends-means rationality, avoiding difficult
discussions about ethical rationality. Starting from such an example I
shall try to make two main points.

Consider a chap who walks around somewhere in 1850, suddenly
sees a troop of wolves running up in his direction, and climbs in a
tree. No doubt, his climbing in the tree has to be considered a
rational decision. Nevertheless, he did not verify by scientific means
that there was indeed a troop of wolves coming after him, he did not
confirm in any scientific way that wolves do not climb trees (whereas
they, e.g., do swim), and no "logic of decision and action" was
available at the time for justifying his specific course of action as
opposed to other possible ones (such as, e.g., jumping in a nearby
river). Let us first consider the problem connected with scientifically
warranted knowledge, and next the problem connected with
available scientific methods.

Is it necessary, with respect to the standards of rationality, that we
warrant all our beliefs in a scientific way before acting on them ?
The question seems almost a rethoric one (the chap of the example
would have been eaten before ever have scientifically verified
anything). Nevertheless, some authors seem to answer the question in
the positive. In connection with the statement that irrational factors
interfere with science, Stegmüller distinguishes several cases, one of

them being that this statement is taken to be a hypothetical conjecture. To this case he comments :

> "Dann ist auch diese Hypothese, so wie alle erfahrungswissenschaftlichen Annahmen, einer *strengen empirischen Prüfung* zu unterziehen, sofern die Proponenten dieser Hypothese *daran interessiert sind*, als Wissenschaftler ernstgenommen zu werden und rational zu überzeugen."[3][2]

It is certainly correct that some of the statements about the interference of irrational factors with science, made by philosophers such as Adorno and other members of the Frankfurter Schule, should better be made precise and tested scientifically before being made the subject of long speculative discussions. But the requirement that all factual beliefs should be tested scientifically prior to their application in action cannot be rational in general. It would indeed be completely irrational to waste money in verifying by scientific means (such as the chemical analysis of food, blood analysis, etc.) that the present inhabitants of the Sahel are underfed. It is rational to accept the truth of certain statements on "common sense" evidence[33], even if these statements are not scientifically tested. Whether it is rational to test them scientifically nevertheless will depend on the combination of such factors as the reliability of the specific argument from "common sense", the relative importance of the statements to our actions and to our understanding of the world, the relative availability of means (people, theories, time, money, etc.) for testing them, ideological factors such as the ends pursued, etc. Does it follow that there is a special category of acceptable knowledge, say "common sense" knowledge, which is different in principle from scientifically warranted knowledge ? By no means so. To accept a statement (as true) is to accept that it would be verified if it were subjected to (further) scientific tests. However, it is important to the correct understanding of the preceding sentence, that one realizes that the expression 'scientific tests' does not necessarily refer to the *at the time* available scientific methods (see the following paragraph).

We now come to the second question. Does the fact that no logic of decision was available at the time turn the chap's decision to climb the tree into an irrational one ? Certainly not—but why not ? A proponent of the "rationality equals science" thesis might answer the question as follows : the action was rational because *we* know by this time that it fits "the" logic of decision and action. This answer

cannot mean that the action is rational *for us* but irrational *for him.* because then rationality would not equal science. So the answer must mean that the action was rational *for him because* it is rational *for us.* On any coherent account, the statement that some decision *was* (always) rational because it agrees with a now existing method, can only mean that the rationality of some decision is not time dependent and that some decision is rational if and only if it agrees with "the ideal scientific methodology". This position, however, is indefensible, for forbearing any claim on rationality by this time. Indeed, nothing is more clear than that we didn't yet reach "the ideal scientific methodology". If the proponent of the "rationality equals science" thesis were to claim that some method is rational, he would have to prove that it is a part of the ideal scientific methodology. But in order to prove this he would need a method of which he could prove in turn that it belongs to the ideal scientific methodology, and so on. So either he ends up in a *regressus ad infinitum* or else in a *petitio principi.* If one compares the age of science with the age of mankind, and if one considers the amount of new techniques and methods that were developed within the last decades, then the hypothesis that we are pretty close to the ideal scientific methodology becomes ridiculous. The statement that it is rational for us to believe that certain scientific methods will not be decisively changed in the future, can only be argued for with respect to (and within) a provisional rationality which is relative to our present knowledge and insights. The main conclusion of the latter paragraphs is that the "rationality equals science" thesis is false.

5. *Two ways in which science is tied up to the actual state of affairs.*

One of the objections against contemporary sciences, especially against the social sciences, points to the fact that they are determined by the actual state of affairs in such a way that they play a conservative role from an ethical,social, and political point of view. This relation between science and the actual state of affairs may be divided into two subrelations, to each of which corresponds a particular objection against the contemporary sciences (or against science in general).

The first subrelation is described by Theodor W. Adorno in passages like the following :

"Aber die adaequatio rei atque cogitationis bedarf erst noch der Selbstreflexion, um wahr zu werden. Ihr Recht ist einzig des kritische. In dem Augenblick, in dem man

den Zustand, den die Researchmethoden treffen zugleich und ausdrücken, als immanente Vernunft der Wissenschaft hypostasiert, anstatt ihn zum Gegenstand des Gedankens zu machen, trägt man, willentlich oder nicht, zu seiner Verewigung bei. Dann nimmt die Sozialforschung das Epiphänomenon, das, was die Welt aus uns gemacht hat, fälschlich für die Sache selbst."[3][4]

Even if one does not like Adorno's style and vocabulary, one must agree that the above statement is essentially correct. I do not believe, however, that, as Adorno more or less implies in the paper this quotation is taken from, one needs the kind of philosophy he is doing in order to state this point. And I *a fortiori* do not believe that a dialectical philosophy or a metaphysics is the only effective cure for the situation, as is claimed by Herbert Marcuse[35]. From a methodological point of view Adorno's intention may be rendered quite correctly by referring to the distinction between mere factual truth and lawful truth. As such Adorno's point fits perfectly within and may soundly be argued for in the frame of contemporary philosophy of science. It is a well-known fact that, in the early days of logical empiricism, there was a tendency to consider scientific thinking as restricted to description, and to disregard problems connected with lawfulness, causality, and explanation. This, however, does not mean that logical empiricists have ever defended the position that factual possibility coincides with actual truth. Furthermore, the aforementioned tendency has been rejected since a long time by them and by philosophers of science who are working in the same tradition. (In the very book in which Marcuse claims that science, supported in this respect by positivist philosophy, is tied up to the actual state of affairs, he quotes a text of Heisenberg which states that science describes possibilities and much less actual facts. Marcuse does not seem to see the relevance of this text to the former problem, but uses it as an illustration to his thesis that contemporary physics "suspends judgement on what reality itself may be, or considers the very question meaningless or unanswerable"[36].)

In the preceding paragraph I have argued that Adorno's point is correct, but that it fits completely within scientific rationality—or methodological rationality, if you wish. This does not mean that there is no problem, or that there is no problem for "rational scientists". One should take account of the psychological mechanisms that play a role in social research as it is structured nowadays. All too often social scientists are busy during long periods in painfully collecting and numerically analyzing data. We know

from social psychology that people get "commitment" to their activities, and that they make up "rationalisations" with respect to the meaningfulness of these activities. If one is busy measuring the amount of agression in the members of a sample, one might be tempted to forget that the sample is only representative with respect to the actual population, the properties of which might very well be historically contingent. I do not believe that, in order to eliminate these psychological mechanisms, it is necessary to introduce some or other "rationality" that refers explicitly to a set of correct ends and values. What we need in order to avoid these interfering psychological mechanisms is an organisation of sociological research which eliminates the aforementioned mechanisms. Empirical research should be preceded and followed by a scrutinizing methodological study. The ends of the empirical investigation, the relevance of the considered hypotheses with respect to this end, the sorts of empirical data relevant to the hypotheses, the relativity of the data, etc. should be studied before the investigation is started. And it should be examined afterwards to what extent the standards are met and, in as far as they are not, to which factual or theoretical hypotheses the collected data are relevant. One of the central aims of this methodological study should be to determine the extent to which the empirical evidence is biased as a consequence of the actually (and contingently) prevailing state of affairs. The latter might or might not be more or less determined by ideological elements. To the extent that it is determined by such elements, one should stimulate the elaboration of the considered theories (under construction) in such a way that they explicitly deal with the nomological relations concerning the impact of values on the studied properties of reality. Empirical research should furthermore be embedded as much as possible in an explicit theoretical framework, which will gradually replace the hidden and implicit framework which derives from everyday prejudices and from unsystematized ideologies.

Allow me to return for a moment to my statement that we do not need to introduce some "rationality" referring to a set of correct values, in order to eliminate the aforementioned psychological mechanisms. It would perhaps be more correct to say that such a rationality would be unable to help us out at this point. Let us return to the concrete example of the study of human agression. Let us suppose that we had agreed, however tentatively and on whatever provisional arguments, about such a "rationality". To take a concrete example, let us suppose that this "rationality" contained Marx's anthropological ideal of the non-alienated human being, and his sociological and economical translation of this ideal into the ideal of

a classless society. This "rationality" would certainly become completely irrational if it were to state the exact role which agression (in its different forms, "sublimated" or not, etc.) had to play in the ideal society. This does not only hold for a Marxian ideal, "rationality" or ideology. No ideology whatsoever can rationally uphold detailed views on the (or an) ideal situation unless in virtue of scientific research. (And Marx was certainly convinced of this; if he weren't, he wouldn't have passed days and days in studying the theories of Smith and Ricardo). It seems to me that we have come here to an important point. Ethical, social, and political values play and should play an important role in science. But they should play their role on the meta-level and on the meta-meta-level. If they play a role on the object level, then one is doomed to prejudices and dogmatism, hence to irrationalism, hence to an ideology which cannot provide any guarantee that it furthers the realisation of its own ends.

We now come to a second way in which science is centered on (and determined by) the actual state of affairs. Marcuse expresses it as follows :

> "In other words, the criteria for judging a given state of affairs are those offered by (or, since they are those of a well-functioning and firmly established social system, imposed by) the given state of affairs. The analysis is "locked"; the range of judgement is confined within a context of facts which excludes judging the context in which the facts are made, manmade, and in which their meaning, function, and development are determined."[3][7]

In the same book Marcuse puts it more generally as follows :

> "But this radical acceptance of the empirical violates the empirical, for in it speaks the mutilated, "abstract" individual who experiences (and expresses) only that which is *given* to him (given in the literal sense), who has only the facts and not the factors, whose behaviour is one-dimensional and manipulated. By virtue of the factual repression, the experienced world is the result of a restricted universe, and the positivist cleaning of the mind brings the mind in line with the restricted experience."[3][8]

Marcuse attacks this "radical acceptance of the empirical" which he claims to be present in everyday language, in science, and in positivist

philosophy. He considers behaviourism and operationalism as mainly responsible for it, and advocates a return to ("critical") philosophy, including metaphysics, as the cure required.

It seems to me that the particular use which is made of terms in science is indeed to some extent dependent on "the given state of affairs".[39] To be more precise, it seems clear that operationalism and behaviourism, especially with respect to the social and human sciences, are coresponsible for the fact that terms, originated in a prescientific language in which they had ideological and metaphysical connotations, are used in the sciences in a way which is to some extent "offered by the given state of affairs". In this sense these terms have lost what Marcuse calls their "transitive meaning". Furthermore, it is also clear that operationalism and behaviourism can be criticized, as was done indeed by philosophers that are on the wrong side for Marcuse–philosophers such as Adolf Grünbaum, Mario Bunge, and Grover Maxwell[40], just to name a few. But this does not mean that Marcuse's objections to behaviourism and operationalism would be accepted by contemporary philosophers of science, and it means even far less that his objections would be justified. As I see it, Marcuse's criticism does not only apply to behaviourism and operationalism, but does apply to the general requirement of using as exact concepts as possible in science. It is this general requirement which leads to the aforementioned shift in the meaning of terms. More importantly, it leads, along with requirements imposed specifically on theory formation and theory justification, to a situation in which the original prescientific problems are replaced by problems that can be treated within the scientific framework which is under development, and to a situation in which several problems will have to be replaced by their "translations" in order to be treated within this framework. It is quite possible that much of the original problem gets lost in the translation, and it will more specifically be the case that problems which involve value-laden or ideological subproblems, will be restated into more exact and descriptive terms, and hence will necessarily do injustice to the ideological subproblems. Suppose, to take a concrete example, that one wants to know how many people of a given country earn less than the minimal income. Obviously, this question involves an ideological component. A family's minimal income is the amount of money which is, in a given society, sufficient to satisfy the basic needs of the members of the family. Now first of all, the specification of "minimal income" into "a family's minimal income" involves already an impoverishment of the original question, since it leads to disregarding the finantial organization of the considered

family type. But, besides this, what are basic human needs ? Should all of them be satisfied, and to what extent ? How do the institutions and culture of the considered society interfere with the satisfaction of these basic needs ? All these questions are, at least in the present state of science, ideological in nature. Furthermore, no (concrete) ideology will be able to articulate the (its) answer to these questions in a way precise enough to provide a basis for scientific measurement. Consequently, any scientific treatment of problems concerning minimal income will necessarily impose interpretative and definitional decisions which will conflict with the ideology under consideration. In general an analogous mechanism plays for all sciences with respect to questions that originate from a metaphysical theory.

The translation of prescientific or extrascientific questions into the framework of a science involves a double disadvantage. First of all, the solution of the scientific problem might erroneously be taken to be a solution to the original problem. Next, the original problem might become forgotten, and related problems formulated within the same ideological framework might simply never be considered seriously. In view of the ideological character of the scientific treatment, depending on the aforementioned interpretative and definitional decisions, the very development of the scientific treatment might result in the imposing of implicit (and largely arbitrary) ideological elements.

Given the disadvantages originating from the scientific requirements concerning the exactness of scientific terms, how can they be avoided ? It certainly would be a bad solution to give up the exactness of scientific terms and statements. In the considered cases this would result in a kind of "knowledge" which consists of statements the meaning of which would be awfully indeterminate. Not only would the used terms be vague and inexact, their ideological character would also make them refer to concepts which are intrinsically indeterminate. Indeed, it seems to me that many ideologically laden terms are essentially "open" in this sense that they cannot be given an exact meaning within the ideological context itself, as a consequence of the *lack of information* one is confronted with at a given moment. Most ideologies are based on some or other intrinsically inexact notion of an ideal (or relatively ideal) state of affairs. Most concepts belonging to such an ideology will ultimately be linked to this ideal state of affairs, hence are intrinsically indeterminate, hence are not fit to figure within knowledge statements. Value-laden terms such as, to take a very simple example, 'murder' (obviously in its non-legal sense) are subject to this

indeterminacy. This is the reason why, in my opinion, some people feel that it is not a matter of vocabulary whether or not a certain form of abortion (abortus provocatus) is to be labelled as murder, and this independent of the fact whether they consider the abortion justifiable in the given situation or not.

It is of course possible that such ideological terms (and metaphysical terms for that matter) are so vague and indeterminate that the degree of communication they allow is almost nil. But this is not the point, Such terms, as understood (however solipsistically) be a given individual, are used in questions, which might provoke more precise questions, which might in turn provoke still more precise questions, and so on. From a given point on these questions will be considered precise enough to start a scientific investigation on the matter. This will in turn lead to a reformulation of the question it started from, but this does not mean that there was not a question which is meaningful at least in this sense that it led to a scientific question and that the latter may be a more or less adequate reformulation of the former. This is simply the *explicandum-explicatum* problem[41]. Summing up, questions which involve ideological and metaphysical elements are to be considered a kind of meaningful questions, although it does not make sense to look for their answers within the framework in which they are formulated[42]. One can but turn to a scientific treatment of reformulated questions, notwithstanding the disadvantages of such a treatment, viz. the ones originating from the requirements concerning the exactness of scientific terms.

The conclusion of the preceding paragraph does not mean that these disadvantages cannot be reduced or even avoided. To a large extent these disadvantages are not provoked by the requirements of exactness as such, but by the actual organization of scientific research in which the requirements operate. In order to modify this organization in the right direction, we need first of all a number of theories that enable us to describe and analyse in a way as exact as possible the existing ideological and metaphysical systems (including all their vagueness). Next we have to study the mechanisms that play a role in the reformulation of ideological and metaphysical questions into scientific questions. (This should not only be done in view of the solution of ideological and metaphysical questions, but also in view of the development of science.) All this presupposes a considerable extension of the present logical theories and of the present methodology of science. But only in this way would we be able to substantiate the widely held conviction that ideology and metaphysics are (actually and logically) of a heuristic value for

science[4 3]. It also would enable us to determine the extent to which the solution of a scientific problem fails to solve the original metaphysical or ideological problem. However, research directed to the construction of the aforementioned methodologically relevant theories would not help us out all by itself. We also have to make this research play a role in the methods used actually by all kinds of scientists. In other words, we should make sure that scientists do not confine their attention to problems which arise for intrascientific reasons, but also to problems that originate in contexts in which ideology and metaphysics play an explicit role. This can only be realized if a "metascience" is developed for every "object science". Such metasciences are interdisciplinary in nature. They presuppose research in the history of the specific object science, in its conceptual frameworks, in the metaphysical and ideological systems that gave and give rise to problems that are akin with the problems studied by the object science, etc. This project may seem to be unrealistic. But is it indeed more unrealistic than the wish that "philosophers", who have but a very fragmented knowledge of science and of its methodology but who, sitting in their chairs, tell vague and presumably false tales, would ever have any influence on the actual development of science ?

6. *The justification of theories with respect to their applications.*

We now come to the question how an individual scientist or a group of scientists should justify his or their scientific work with respect to the possible applications of this work. There are two extreme answers to this question. The first is that a scientist should be concerned with intra-scientific standards only, i.e. with methodological standards. Every scientific result, it is argued by the proponents of this conviction, may be applied in view of different and conflicting ends, and hence the scientist can only be liable for presenting sound scientific results, whereas the decision as to which applications should be made of these results has to be left to politicians and political institutions, or to those who have the (political or economic) power to make these decisions. The same point of view is defended with respect to technological results, notwithstanding the ends that are explicitly taken into account by a given technological theory. Hans Albert writes :

"Abgesehen davon dürfte ein- und dasselbe technologische
System fast immer dazu verwandbar sein, entgegengesetzte
Wirkungen zu erzielen; denn wenn z.B. die Mittel bekannt

sind, mit deren Hilfe man das Preisniveau stabilisieren kann, lassen sich ohne weiteres Massnahmen ableiten, die die Realisierung dieser Zielsetzung verhindern."[44]

A second extreme answer to the aforementioned question is that a scientist should justify his work with respect to ethically, socially, and politically correct ends. This position is held in a more or less extreme form by people of the anti-scientism movement, but also by Marcuse and the Frankfurter Schule. Writes Marcuse :

> The processes of validation and of verification may be purely theoretical ones, but they never occur in a vacuum and they never terminate in a private, individual mind. The hypothetical system of forms and functions becomes dependent on another system—a pre-established universe of ends, in which and *for* which it develops. What appeared extraneous, foreign to the theoretical project, shows forth as part of its very structure (method and concepts); pure objectivity reveals itself as *object for a subjectivity* which provides the Telos, the ends. In the construction of the technological reality, there is no such thing as a purely rational scientific order; the process of technological rationality is a political process."[45]

Both answers are one-sided. The first answer not only makes the theoretical mistake of separating theory and praxis in an absolute way, but also fails to take account of the factual and obvious relation that exists between (scientific and technological) theories and their actual applications in numerous cases : scientific and technological research relevant to the construction of atomic and hydrogen bombs, research on advertising techniques, research on the segmentation of industrial task flows, and research on medical drugs, to mention only a few patent examples. The general objection against the presently discussed conviction might be phrased as follows. If the prevailing political system has ethnically, socially, or politically rejectable properties, and if it is plausible that certain scientific and technological results will be used, within the aforementioned system, to strengthen or to maintain the rejectable properties, then the research leading to these scientific and technological results cannot be taken to be justified. A methodological (or : intra-scientific) justification is clearly insufficient under these conditions. If preferences, values, and ends are taken serious, the latter statement is inescapable, and shows that the separation between one's scientific

activities (and their ends) and one's ideological convictions is unjustified and unjustifiable. It is important to stress that not the possible applications of scientific and technological results, but the applications that will actually be performed are relevant in this context.

The second extreme answer too has to be rejected for reasons of one-sidedness. In my opinion, there is a major practical objection against requiring an ethical, social, and political justification for scientific and technological work, viz. that it leads to the abolishment of any research. I am convinced that the objection against the first answer is a real one. But one shouldn't require *a priori* the warrant that research under consideration lead to a "better" social, economical, and political system. If this requirement were imposed, one could never justifiedly start any bit of research from the point of view of an ideology which does not coincide with the implicit ideology of the present social, economical, and political system. To consider the most extreme case at once, how could one ever hope that research, however conflicting its ends would be with respect to the existing political system, would not be system-confirming ? All over the world, one can only see political systems which violate basic human values. How could scientists ever hope to overrule these systems by means of their research and *qua* scientist ? The second extreme answer will, in my opinion, only lead to the result that no project whatsoever is actually justified. It will not lead to the justification, and hence the elaboration, of a definite project. Furthermore, this kind of answer risks to lead, and did in certain cases already lead, to an intellectual climate that is just as irrational as any "scientism", in that it fails to appreciate the importance and the necessity of scientific knowledge for a just social order and for human happiness. Whoever takes his ideology serious, and is not a dogmatic, will have to realize that he needs knowledge, and new scientific knowledge indeed, for the backing and elaboration of his ideological convictions. Even if this scientific knowledge will be abused by the existing economic, social, and political system, it would be irrational to restrict one's intellectual activities to a (correctly negative) criticism of existing and conceivable research projects. The requirement that research be ethically, socially, and politically justified with resepct to its future actual applications, leads to negativism, irrationality, and dogmatism.

People who like to set up tragic situations might rephrase the conclusion of the preceding discussion as follows. The ideologically minded scientist–and I here indeed disregard those who consider the system they live in as optimal or even as containing the roots which

will necessarily bloom into an optimal system–cannot escape from engaging in research of which he knows that it will further a political system which is opposite to his ideology. The way out of this tragic and perplexing situation contains three different steps. First of all we should engage in extra-scientific activities in order to further the ideals we believe in. In this way we may hope that the research we are doing now, notwithstanding abusions of its results by actual political systems, might ever prove useful within a better system. Next, we should engage in logical and methodological disciplines in a way as formal as possible, and we should arouse the interest of our students in these matters. More than any object-level knowledge (and more than any informal dialectic for that matter), these metalevel disciplines affront the weaknesses in the justifiability of the existing social, economical and political systems and help us to detect the weaknesses of our own ideologies. Finally, we should engage in scientific research which undermines the factual backing for the existing political systems as well as in scientific research which is relevant to the factual presuppositions of our own ideology. Apart from this three-headed way out, we can, in the present situation, only judge separate cases by means of our ideological convictions. A scientist should refrain from providing knowledge of which it is rational to accept that it will further the injustices of the present economic, social, and political systems. But there is no need for refraining from such research, if the scientist may warrantly accept that it will not lead to such effects, and if the results of the research might prove useful and required in the economic, social, and political system which he reasonably hopes to realize, especially by means of–remember the tragic situation–his extra-scientific activities.

7. *Concluding remarks*

Only some elements of a vast field of problems have been discussed in this paper. I hope nevertheless that the general tenure of the views I have defended might throw some light on a few non-trivial aspects, and might suggest ways to tackle other problems of the field. In the remaining paragraphs I shall comment on two topics. First of all I shall consider the general methodological problem connected with the justification of scientific and technological theories, and next I shall mention a question which I think to be especially important.

If one tries to answer the question as to the justification of the present sciences, then one is bound to take a stand, be it implicitly or explicitly, with respect to a methodological question, viz. which

arguments one will regard as legitimate. Since the justification problem under consideration is not meant as limited to the borders of some given theory, it seems desirable to allow all kinds of criticism. However, it will turn out readily after some examination that it is possible to offer arguments in favor as well as arguments against the "correctness" of any human procedure or of any set of statements. This means that a critical activity, which is not itself methodologically ordered, is doomed to be ineffective. Such a critical activity may seem to be interesting at first sight, it may lead to some or other result which looks impressive, and it may even have more or less influence on reality, including the behaviour of human beings, but nevertheless it will be intrinsically ineffective with respect to its own aim, viz. to lead to a sound judgment on the justifiability of some subjet. The methodological ordering of such a critical activity may be effected by different means; one may require the critical activity to start from a given set of values, one may also require it to start from a given view on reality (say, a theological one), one may also impose requirements on the critical arguments or on the conceptual framework the latter are formulated in ("negativity" of the concepts). However, such methodological orderings rely either on a-prioristic (hence unjustifiable) choices themselves, or on requirements which are not strong enough to provide for a methodological ordering which enables us to come to at least provisional conclusions on the basis of the resulting critical activity.

In my opinion, the only defensible way to allow in principle for a criticism as rich as possible, in other words the only defensible way not to rule out in advance and definitely some or other sort of criticism (each such kind might be legitimate), is to start from the kind of rationality which I have pointed at in section 4 and which I have pictured a bit more in detail in my aforementioned article on rationality. This kind of rationality leads immediately to the result that we should take account, at least provisionally, of the statements warranted by and the methods exemplified within the actual sciences. This is why I have defended the idea that we should set up metasciences which are explicitly directed towards the different kinds of criticism that we might want to make. In principle all kinds of criticism remain acceptable. The only requirement is that it be formulated in a way in which it may, to the best of our present judgement, be made precise and testable.

Let us now come to the comments on the second topic. Central to the whole problem of the justifiability of the sciences is the question whether, expressed roughly, a more correct ideology will lead to a

better science. Let us suppose that we are able to spell out an ideology of which it can rationally be demonstrated that it is both fairly correct and optimal in comparison with the other existing ideologies. Let us also suppose that we make this ideology have a maximal influence on scientific and technological practice whenever it is justified that an ideology should have such an influence. (Hence we will not, e.g. let this ideology interfere with the acceptance of object statements of an empirically well-backed scientific theory.) Will this result into a better science ? If so, in what respects ? This is, it seems to me, the central question in the present justification problem. A small part of this question may be answered easily enough in the positive. Indeed, the ends to which scientific research will be directed will trivially (and by definition) be better than the ends to which present disciplines are actually directed[46]. However, with respect to most aspects of science no precise answer can be given to the above question. Furthermore, we have no idea at all *in which way* the resulting science would be better, with respect to a given aspect, than the present science, even if we presumed it were better with respect to this aspect. Would we arrive at a better methodology ? And, if so, in which sense ? Would we arrive at more correct theories than the present ones ? Would we sooner identify the weaknesses of given theories ? None of these questions can be answered for the simple reason that we do not have the information for answering them. Before being able to answer these questions, we will need scientific research on the matter, i.e. research fulfilling the requirements of contemporary science, whatever disadvantages this science and these requirements might be laden with. The construction of an ideal world is an urgent matter. A design *cannot* be made in advance. We should not loose our time lamenting about the present world; we should start trying to transform it in the good direction.

<div align="right">

Rijksuniversiteit Gent
Limburgs Universitair Centrum

</div>

NOTES

[1] H. Albert, "Wissenschaft und Politik. Zum Problem der Anwendbarkeit einer wertfreien Sozialwissenschaft", in *Probleme der Wissenschaftstheorie*, Ernst Topitsch (ed.), Wien, Springer, 1960, p. 229.

[2] H. Boyko, "The need of a world academy of art and science", in *Science and the future of mankind*, H. Boyko (ed.), The Hague, Junk, 1964, p. 18.

[3] T. W. Adorno, "Einleitung" to *Der Positivismusstreit in der deutschen Soziologie*, (papers by Adorno, Dahrendorf, Pilot, Albert, Habermas, and Popper), Berlin, Luchterhand, 1970, p. 36.

[4] H. Marcuse, *One dimensional man*, London, Sphere Books, 1968, p. 14.

[5] From a paper published in *Survivre*, 9, 1971, which is reprinted in *(Auto)critique de la science*, A. Jaubert and J.-M. Lévy-Leblond (eds.), Paris, Editions du Seuil, 1973, p. 59.

[6] W. Stegmüller, *Personelle und statistische Wahrscheinlichkeit (= Probleme und Resultate der Wissenschaftstheorie und der Analytischen Philosophie*, vol. 4), Berlin, Springer, 1973, p. 60.

[7] A. Jaubert and J.-M. Lévy-Leblond (eds.), *(Auto)critique de la science, o.c.*

[8] 'Ideology' and 'ideological' are not used in a pejorative sense in this paper.

[9] A. Jaubert and J.-M. Lévy-Leblond (eds.), *o.c.*, p. 55.

[10] E. Topitsch, *Sozialphilosophie zwischen Ideologie und Wissenschaft*, Berlin, Luchterhand, 1966, p. 337.

[11] *o.c.*

[12] See notes (6), (1), and (10).

[13] H. Albert, *o.c.*, p. 216.

[14] M. Polanyi, *Science, faith and society*, Chicago, University of Chicago Press, 1964, p. 63 (first edition : 1946).

[15] *ibid.*, p. 72-73.

[16] *ibid.*, p. 69.

[17] K. Popper, *The logic of scientific discovery*, New York, Basic Books, 1959.

[18] See note (1).

[19] E. McMullin, "The history and philosophy of science : a taxonomy", in *Historical and philosophical perspectives of science (= Minnesota studies in the philosophy of science*, vol. 5), R. H. Stuwer (ed.), Minneapolis, University of Minnesota Press, 1970.

[20] M. Bunge, "Towards a philosophy of technology", in *Civilisation technique et humanisme*, (no editor), Bruxelles, Office International

de Librarie, 1968; H. Lenk, *Pragmatische Philosophie*, Hamburg, Hoffmann und Campe, 1975 (especially pp. 268-270, which contain numerous references).

[21] See my "Rationality and justification", this journal, 14, 1974, pp. 83-103.

[22] I realize quite well that these examples are very trivial, but they are only intended to show how the delimitation of the problems meant *sub* (a) might proceed by means of a theory of causality and by means of a general theory of action.

[23] T.S. Kuhn, *The structure of scientific revolutions*, Chicago, University of Chicago Press, 1962; N. R. Hanson, *Patterns of discovery*, New York, Cambridge University Press, 1960; P. K. Feyerabend, "On the "meaning" of scientific terms", *Journal of Philosophy*, 12, 1965, pp. 266-274.

[24] In other words, the falsification of a (falsifiable) theory will not necessarily lead to a new paradigm. As long as no alternative paradigm is presented, one will construct a new theory which is, from a logical point of view, different from (since inconsistent with) the old theory, but which will belong to the same paradigm.

[25] H. Marcuse, *One dimensional man*, o.c.

[26] This domain of research is still underdeveloped. Fortunately there has already been done some work in it, and even the connected methodological problems are discussed. See e.g. Robert Young, "The historiographic and ideological contexts of the nineteenth-century debate on man's place in nature", in *Changing perspectives in the history of science*, M. Teich and R. Young (eds.), London, Heinemann, 1973.

[27] See my "Rationality and justification", o.c.

[28] I consider action and understanding to be basic human activities which motivate the search for a rationality.

[29] A. Grünbaum, "Science and ideology", *The Scientific Monthly*, 79, 1954, pp. 13-19.

[30] E.g. Wolfgang Stegmüller in his book referred to in note (6).

[31] W. Stegmüller, *o.c.*, p. 64.

[32] *Ibid.*, p. 62.

[33] I am by no means supporting a "common sense" foundation of knowledge, but only claim that we should sometimes rely on "common sense" knowledge.

[34] T. W. Adorno, "Soziologie und empirische Forschung", in *Der*

Positivismusstreit in der deutschen Soziologie, o.c., p. 88.

[35] H. Marcuse, *o.c.*

[36] *Ibid.*, p. 125. The quotation from Heisenberg's paper figures on p. 124, is taken from "Ueber den Begriff 'Abgeschlossene Theorie'" (*Dialectica*, 2, 1948, p. 85), and reads : "Wass wir mathematisch festlegen, ist nur zum kleinen Teil ein 'objectieves Faktum', zum grösseren Teil eine Uebersicht über Möglichkeiten".

[37] H. Marcuse, *o.c.*, p. 99.

[38] *Ibid.*, p. 147-148.

[39] That this holds also for statements made by scientists outside the domain of their specialization is demonstrated by the quotation from Polanyi's book, figuring in section 1.

[40] A. Grünbaum, "Operationism and relativity", *The Scientific Monthly*, 79, 1954, pp. 228-231; M. Bunge, "The nonoperational nature of theoretical concepts", in *Problems in the philosophy of science*, I. Lakatos and A. Musgrave (eds.), Amsterdam, North-Holland, 1968, pp. 274-276; G. Maxwell, "Scientific methodology and the causal theory of perception", in *Problems in the Philosophy of science*, I. Lakatos and A. Musgrave (eds.), Amsterdam, North-Holland, 1968, pp. 148-160.

[41] See R. Carnap, *Logical foundations of probability*, Chicago, University of Chicago Press, 1950.

[42] It is not clear to me whether Marcuse wants to abolish the methodological requirements concerning the exactness of scientific terms, but it is clear that he considers metaphysics and "philosophy" (which "is ideology") as valid modes of knowledge. He seems to consider them so because they are "negative" and "transcending reality". But how could the mere fact that a (coherent) whole of statements which is "negative" with respect to the actual state of affairs, ever assure that this whole be reliable in any sense, or that it contain any form of intrinsic rationality.

[43] See e.g. K. Popper, *o.c.*; M. Wartofsky : "Metaphysics as a heuristic for science", in *Boston studies in the philosophy of science*, vol. 3, R. Cohen and M. Wartofsky (eds.), Dordrecht, Reidel, 1967, pp. 123-172.

[44] H. Albert, *o.c.*, p. 216.

[45] H. Marcuse, *o.c.*, p. 137.

[46] One should not, of course, give up methodological requirements,

unless this can be justified. So, one should e.g. not give up theoretical research on the basis of the naive consideration that this will not directly be aimed at a major end stated by the ideology.