Science and the search for values: motives and dangers *

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0. One of the main reasons for the central role of science in the contemporary search for a global macro-ethics in a finite world is the widespread belief that the knowledge science provides is the only real, the only respectable sort of knowledge. However this centrality brings with it the danger of scientism. To counter it, the principle boundary between fact and value must be recognized. Only in these terms can we recognize the proper relationship between science and ethics.

1. What is science? Demarcating science from pseudo-science: Rejection of unjustified claims.

What would be needed is not just a definition but an entire *image* of science. Such an image can be provided only by *methodology*, whose task it is to develop and improve our presciptive image of science. The history of science attempts to provide a descriptive image of science; but in doing so it must make use of a prescriptive image, if only to be able to say which developments constitute scientific progress, are worth studying.

Since the first world war philosophers of science have been engaged in the search for a *demarcation criterion* which draws a line between *science* and *non-science*, or what they called "metaphysics." Their intention was that of enlightenment: the Vienna Circle fought the various irrationalisms in its intellectual environment. It was, and still is, an urgent task. But, as we all know, the Vienna Circle and its successor, *Logical Empiricism*, did not succeed, neither intellectually nor politically. They formulated the problem as a semantic problem, and the history of the various criteria of Empirical Significance is that of degenerating problem shifts: from real methodological problems to problems created by the logical instruments themselves (1), and the criteria proposed eventually turned into what Mario Bunge has aptly labled theory-demolishing techniques.

At a very early stage of this development, *Karl Popper* saw that the proposed criteria of Meaningfullness could not fulfil their function and would, if taken seriously, destroy science. His classic of 1934 criticizes the Verificationism (with its sister Probabilism) and the Foundationalism of Logical Positivism. Popper too (as well as Lakatos) sees the problem of demarcation

as the basic issue in the philosophy of science. He developed his well-known Falsificationism (a theory is "scientific," respectable only if chaperoned by a potential falsifier): *falsification* functioning *as a method both of quality control and of improvement* (2), and definitely not a criterion of meaning (as the persistent Popper-legend would have it). Popperianism holds that methodology (3) has two basic tasks: theory appraisal and advice, that the first - quality control of the products of research - has to explicate the concept of Scientific Progress and to provide objective indicators of such progress (e.g. in Popper comparative "degree of corroboration"), and it is consistent with Popperianism to view the demarcation problem as being but quality control in black and white: the *demarcation criterion as a spill-over from the (prescriptive) image of science* developed by methodology.

The demarcation criterion certainly draws a boundary line against *metaphysics* in the sense of preconceptions at the level of world picture hypotheses. But it functions more like an admission criterion, i.e. with suitable modifications such preconceptions may not only play a heuristic role in science, they may also be of structural importance and are not totally immune against repercussions from scientific developments (4). However *demarcating science from pseudoscience is important primarily in political debate.* It also has grave implications for institutionalisation of criticism. As Lakatos points out, this is amply illustrated by history: from the ban of Copernicus' theory in 1616, with the official reason given that it was unscientific, and its rehabilitation in 1820, on the ground that it had been proved by the facts, to the ban on Mendelian genetics in 1949 in the Soviet Union on the ground that it was unscientific and to the idea of a "scientific" socialism. In todav's liberal Establishment of the West the hereditary thesis (which is not an attractive card in the political game in the United States) is attacked on the grounds that it is pseudoscience (5). It is obvious that in an age where science is regarded by so many as the only source of respectable knowledge, political powerholders and, in a decentralized system like the United States, pressure groups, will seek to create the impression that their dogmatic creeds are, in spite of all appearances, "scientific." Thus, this is *a boundary which must be clearly drawn (and only objective criteria can achieve this): science must be defended against this usurpation on the part of political forces.*

2. <u>The boundary between science and ethics: A critique of Scientism: excessive and in principle unfulfillable demands on science</u>.

consequence of the demarcation of science form non-science is second, still more important boundary, that between facts and lues (6), description and prescription: values are outside e domain of science. Norms and value judgments cannot be tified by means of science. This consequence was indeed

unpalatable for many, because it seemed to them to lead relentlessly to nihilism. For instance, Husserl came to this conclusion in his *Die Krisis der Europäischen Wissenschaften.*

Schopenhauer's dictum is more timely than ever: it is quite easy to produce an ethics, but terribly difficult to justify it. Western philosophy has been producing such "justifications" for at least to millenia. The development of science was itself one of the main sources of and a precondition for the enlightenment in which the traditional world-views were rendered susceptible to rational criticism and which by and large robbed the ruling tradition of its normative power. In this situation norms are no longer taken for granted; they are put into question and a justification for them is requested, for enlightenment is paid for in terms of a crisis in orientation. But the price is also paid in that a justification becomes more difficult and problematic than ever. The difficulty lies not only in the fact that a justification can no longer content itself with calling on the tradition and its world-view as an accepted authority. Of even more decisive importance is the fact that the justification of norms becomes oriented by the idea of objectivity associated with science - although scientific knowledge turns out to be itself in principle fallible (a fact that has been known at least since C.S. Peirce, but which is only today beginning to exercise a broader influence). At the same time, the industrialization which was made possible by the technological application of scientific discoveries has greatly increased man's practical possibilities - he can, as always, act to produce good or evil, but now on a much larger scale - and this makes the need for a global ethic which could be accepted as justified "by all" much more pressing. In order to deal with ethical problems, one needs rational deliberation, argumentation and knowledge. How are we to argue?

One who is unhappy with the sharp boundary between science and values and supposes it to be the root of the above dilemma will do everything possible to overcome it, to bridge the chasm. But today he does so in a situation where, as we have remarked, the knowledge science provides is regarded as the only authentic sort of knowledge. We might name this attitude the *epistemological aspect of scientism*: this aspect is a *totalization of the perspective of science.*

In such an intellectual situation it is only to be expected that in the ought-is (*Sollen-Sein*) problematic, *in the age-old attempt to justify norms in terms of factual knowledge, "knowledge" becomes to mean scientific knowledge.* This holds for attempts to justify norms of all sorts, be they ethical norms or some other type, extending even to the rules of (prescriptive) methodology itself. The general attitude which suggests such attempts, which even sees them as the method of solving all normative problems in terms of bridging the [illegible] between science and ethics, might be called the *practical* [illegible] *scientism*: scientism in the everyday sense of the word.

My concern at this point is to demonstrate that *these demands which are placed upon science are in principle unfulfillable*, thus making the expectations which accompany such demands doomed to disappointment. Thus, for example, the idea of leading a "scientific style of life," as put forward by the Vienna Circle, produced not the man moulded in the rational image of science, but rather the "Mann ohne Eigenschaften" (7) of Robert Musil's famous novel.

This scientism - or "reductive naturalism," or "reductive descriptivism," as one can call it if one wants to emphasize that in its generalized form it also takes in proto-scientific and everyday knowledge - has an *argumentative version* which is founded on a figure of thought which has become known as the "*naturalistic fallacy*" since G.E. Moore"s classic *Principia Ethica*. This is the attempt to derive a normative conclusion from a set of premises, which contains only descriptive, non-normative sentences. It is a fallacy because in a logically valid argument either not all premises are in fact purely descriptive (or analytic) or the conclusion is not normative (even if prima facie it may appear to be so).

A currently very popular variety of the naturalistic fallacy is the *genetic fallacy*: an argument in which the conclusion is a value judgment (e.g. "Theory T_2 is better than T_1 or X is of high easthetic merit") which is derived from premises which describe the *genesis* of the object in question: a move from the properties of the production process and of the producers to the evaluation of the product. (Thus, for example, it might be mentioned in evaluating a work of art that the work was elegantly, quickly or effectively produced, or that the producer has an handicap, or is very young, etc. Even if these references themselves contain evaluative as well as descriptive elements, this merely leads to a confusion of different evaluative problems, for reference to an appraisal of the producer is obviously irrelevant to the appraisal of the products as products.)

Since the so-called naturalistic fallacy is a logical fallacy, and *patently* so, less naive representatives of scientism attempt to evade it while still retaining their position. Aside from tricks such as arguments in which the conclusion is empty, analogous to a tautology: "Ought $(P \vee -P)$", there are *two possible strategies.*

a. *Introduction of an additional premise*: if we introduce an appropriate missing link, the fallacy is avoided. The original, invalid argument was of the following form: e.g., "The majority of ... assert that *N*; therefore *N*": the *sociological version of the scientistic fallacy*, or better, *"Naive sociologism."* In place of this original, we now have a *meta-linguistical argument* (8) in which the argument of naive sociologism is fortified with an additional premise. Such an argument takes the following form:

A says that *N*./ What *A* says is valid./ Therefore: "*N*" is valid./

Therefore: *N*. For example: The majority of scientists say that theory T_2 is better than T_1, is an instance of scientific progress./ What the majority of scientists say when they evaluate theories is valid./ Therefore the value judgment "T_2 is better than T_1" is valid. Therefore theory T_2 is better.

This is a typical mode of argumentation: norms are justified by majority decision - sophisticated sociologism as a sub-department of scientism (reductive naturalism), "sophisticated" because now no fallacy is involved. The premisses now contain a normative sentence, and the discussion must now concentrate on its justification. How are we to justify the normative sentence "What *A* says concerning *X* is valid?" If one offers as reason the fact that *A* has certain empirical properties (e.g. that *A* is a member of a specific class such as the "proletariate"), this will merely constitute a demonstration of the presence of a genetic fallacy (*N* is valid because it was produced in the "right" way, i.e. by producer *A*). If the reason offered is that *A* is a "good" producer, then the problem now concerns the articulation of criteria for the quality of producers which can be applied in the appraisal of the producers and the legitimation of such criteria. If these criteria are in turn defined in terms of the quality of the products produced, then the original problem simply rearises rather than being solved. Thus, such a line of thought must consequently execute a *problem shift*: a replacement of the *problem of product appraisal* by the *problem of producer appraisal*. The rationale for such a problem shift can only be the belief that it is impossible to develop and legitimate objective criteria for the appraisal of products.

Such a position has been adopted by those *contemporary philosophers of science who deny the possibility of objective criteria for theory appraisal*. Denying the possibility of a "statute law" in methodology, some argue that there might be at least "case law," i.e. valid theory appraisals are made at least in paradigmatic cases (Kuhn, Toulmin). If so, then you need "wise" judges (élites) who are in a position to identify the paradigmatic cases and interpret case law, or, as Polanyi thinks, "case law" by and large cannot be articulated, because it belongs to the "tacit dimension." Feyerabend goes so far as to say that *any* rules will hamper progress.

However it should be clearly noted that if the problem of objective theory appraisal is rejected as unsolvable, then strictly speaking we are no longer justified in speaking of "scientific progress" in the normative sense: we must restrict ourselves to a lexical definition. This is a *retreat from methodology to sociologism*. It is *a degenerating problem shift* because even if "good producer" is defined without reference to the quality criteria of product appraisal, and if, in addition, one in fact has such criteria, one would still have the problem corroborating and explaining the law hypothesis asserting that there is an empirical (causal) connection between

producer quality (according to criteria of producer appraisal) and the quality of the products (according to other, objective, criteria). Yet, if one possesses objective criteria for theory appraisal, there is no point in making the degenerating problem shift. (This problem shift on the level of methodology does have a parallel on the level of science policy making which, on that level, has a certain restricted validity: an inductivist rule of thumb which advises one that researchers who have hitherto produced good theories will be likely to do the same in the future, and hence should be supported.) The connection between the quality of products and that of producers is an internal connection: it makes no sense at all to first determine the one, then the other, and then proceed to investigate the relationship between the two. Thus, we must begin with one pole or the other, the explication of one pole must be parasitic upon the explication of the other. The entire thrust of my argument is that it makes no sense at all to begin this sort of investigation by focussing on producers.

But let us pursue the absurdities of this problem shift to the bitter end. We want to be able to use appraisals of the quality of producers as our principle in appraisals of the quality of products. How then are we to *identify the relevant group of producers* or the group of "wise" judges who create the case law? Polanyi suggests that in the case of theory appraisal, the relevant group with respect to methodological appraisals is the scientific community, which is a self-selected and pre-supposition-sharing community (C.S. Peirce). If the relevant group has been identified in one way or the other, one will have to rely on *consensus* in that group (or introduce a new principle which allows identification of the "best" members of the community). Lacking this, one will simply have to wait and see (as Toulmin suggests). This reliance on empirical consensus underlines the fact that we are witnessing a retreat into naive sociologism: factual consensus of any group whatsoever gives no guarantee for validity (9).

Reductive naturalism has become very influential in the *philosophy of science*, and influences the image of science sketched by those who embrace such a "scientistic" position. What we find here is not so much sociologism as a position that one might call "historiographism" (10): the use of the history of science as an arbiter in the appraisal of methodologies. Kuhn is perhaps the most famous representative of this position. Feyerabend would also fit in here, for he uses facts from the history of science to criticize methodological norms and applauds Lakatos' introduction of a meta-criterion (11) for his demarcation criterion which makes it criticizable by the history of science, thus coming dangerously close to the naturalistic fallacy.

The attempt to criticize or justify methodological rules by means of the history of science qua factual history is bound to miscarry: one insinuates that the validity of a certain methodological rule could be established by showing - in a

purely descriptive manner - that it leads to success, to real scientific progress, in a certain problem situation. But such an argument cannot show how and why it was successful, and why one has reason to trust that it would lead to success in similar situations in the future. But an even more fundamental objection would be that the problem of appraisal rearises when it comes to justifying the judgment that the result claimed to have been achieved is to be appraised as "scientific progress." (12) Thus, the irony of attempts to argue from the history of science is that they cannot even succeed in committing the naturalistic fallacy in spite of the intention to do so, and this for the simple reason that there cannot be a purely descriptive history of science. The history of science is concerned with investigating progressive developments (is, bluntly put, interested in *science* and not something else) in order to get a *better* understanding (*Verstehen*) and not in a neutral description of arbitrary historical events. Thus, the very identification of the relevant objects of investigation requires that a normative methodological position be in play. One cannot commit a naturalistic fallacy by calling on a *Geisteswissenschaft*.

b. The second strategy attempts to *deny the distinction between normative and non-normative sentences*. There are two main variants of this move, the first of which stems from *ordinary-language philosophy*. Here one calls on the fact that the illocutionary force of a statement varies with the context, such that a statement which is normally descriptive (constative) and which according to the semantical rules, makes an assertion, e.g., "It is two o'clock" or "The supreme court ruled in 1970 that ...", may in certain contexts function prescriptively, normatively. Such a claim is certainly correct, but irrelevant, for *within* the argument the sentence must not change its meaning.

A second naive but less harmless, *variant* of the *denial of the distinction between facts and values* consists in *a simple assertion of the synonymity* of certain norm sentences with descriptive sentences: a *semantical scientism*. In its most direct forms it may appear wildly implausible, but is it a popular sport. Thus, C.W, Churchman writes: "'*X* ought to do *A*' means '*A* is what mankind would choose *X* to do, given an opportunity of free choice.'" (13) Hence, the only problem of value is the problem of finding out what will be desired and how to get it. If the synonymity thesis is intended to be a lexical definition, it cannot justify anything. Thus, it must be meant to be a stipulative definition, and as an explication of this kind would itself have to be justified. This is not even attempted: it is simply asserted and then put into service.

But the phenomenon itself is interesting, in the first place because a historicistic optimism hides behind the straightforward assertion of the thesis, a certainty in salvation, for why else should the future choices of mankind (probably also

In The Long Run) automatically be correct? Secondly, and more important, because it offers no operative criteria for decision making, it is, as one reviewer, Henry Kyburg, points out (14) an open invitation to totalitarianism: Who will discover what "future mankind" really wants, if not a dedicated group seeing themselves as *Confidantes de la Providence*, "sociologist kings" or The Party?

This sort of attitude has led to a theocratic state in contemporary Russia, and in the West to the attempt by certain groups of intellectuals to take over the role of a kind of priesthood (15).

Interestingly enough, this naive scientism is often linked to the "anti-science movement" as represented, e.g. by H. Marcuse, a romantic view which 1) conflates evaluation of the results of the *application* of science with the evaluation of science, 2) postulates that science (as we know it) *necessarily* leads to a "repressive" domination of man and nature, and 3) dreams of another "emancipatory" science, which however, even if, per impossible, it existed, would not relieve one of the task of appraising that science which we now have and appraising the results of its application (16). In the until recently influential Marcuse the confusion is thus totalized and total. Here too is a boundary to be drawn in order to defend science against totally unjustified indictments.

3. The problem of value.

Hitherto we have only rejected attempts to base norms on science as constituting unreasonable and unfulfillable demands on science. Fortunately *the realm of Reason is wider than that of Scientific Rationality* (17).

Justifying a *prescription* (technological, methodological, etc.) consists in showing that the knowledge upon which it is based is authentic knowledge ("true,", high degree of verisimilitude, etc.) and that following the prescription will achieve or help to facilitate the realization of the presupposed goal, which must be clearly stated. Justifying the *goal* is another problem, a problem which eventually will lead to the problem of justifying the "ultimate ends" of the public-political sphere and of the private-existential sphere. This involves reflection, which is rightly called "philosophical" in the etymological sense of the word. Science and the humanities can contribute to this philosophical dialogue about ultimate aims, but no more.

As regards the *public-political sphere*, there are two main approaches, *two poles: the liberal and the totalitarian*. The kernel of the liberal position is that the ultimate end is taken to be the reduction of unnessessary human suffering (Popper). Not only is it easier to agree upon what constitutes "suffering," but the attempt to let the State, or Society, undertake the task of securing human happiness will relentlessly lead to totalitarianism. Attempts will be made to reduce the

existential-private sphere to the public-political sphere (and even to organize the latter on the model of the sphere of instrumental action and means-end rationality, i.e. in a scientistic manner). The "demand for a 'conscious social purpose' in everything" (18) will, as F.A. Hayek points out, permeate our whole form of life, and this "hubris of collectivism," if persistently pursued, "must lead to a system in which all members of society become merely instruments of the single directing mind (...)." (19) Fully developed forms of this "political collectivism" call on a pragmatic or instrumentalist conception of science (Bukharin, Hessen, Bernal, etc.), and hence sympathize with science policy making which treats pure research as a parasitic luxury (20) or even a symptom of capitalism (Bukharin). It can be demonstrated that "scientific socialism" is a real and dangerous enemy of science itself (21).

It is easy to give examples of contemporary attempts to reduce the existential-private sphere, to absorb it into the public-political. We need not call on the obvious example of the Soviet system, which exhibits all the characteristics of a theocratic state (22). Even in Western democracies: an increasing dependence of the individual on social welfare institutions eventually makes welfare itself into an instrument of domination. As Schelsky notes, "Politics (...) no longer be the day-to-day business of balancing interests off against one another (...), but becomes the mere vehicle for the production of the Final State of Society: 'the heavenly society' of the 'heavenly socialism,' (...)." (23) Thus, a politics which has in this way become pseudo-religion, a social religion which aims at salvation through administration, strives to actualize a "transcendence in this world." But at the same time it tries to clothe itself in the mantle of "science," for scientism remains a deep-rooted hope: the hope that science can and will be able to finally answer all of our questions and solve all of our riddles.

NOTES

1. Cf. Radnitzky, G. *Contemporary Schools of Metascience.* New York: Humanities Press, 1970, paper ed. Chicago: Regnery, 1973, Vol. I.
2. E.g., Radnitzky, G. "Popperian philosophy of science as an antidote against relativism," par. 2, forthcoming in Boston Studies in the Philosophy of Science, Imre Lakatos Memorial Volume, edited by R. Cohen, P. Feyerabend and M. Wartofsky. Reidel Publ. Co., early 1976.
3. On the function of methodology and the indispensability of objective criteria of scientific merit, cf,m ibid. par. 1.11-1.13.
4. Cf., e.g., Radnitzky, G., *Preconceptions in Research.* London: Literary Services and Production, 1974.
5. Cf., e.g., Ezrahi, Y. "The authority of science in politics," in: Thackray and Mendelsohn (eds.). *Science and Values.* Humanities Press, 1974, esp. par. V, and Flew, A. "The Jensen uproar," *Philosophy* 48:63-69 (1973).
6. Popper, K. *The Open Society and its enemies.* London: Routledge & Kegan Paul, 1945 ff. Vol. II, Add. par. 12, 13, 15, 16.

7. This is translated as "The Man Without Properties," but "The Man Without an Identity" would be more accurate.
8. As Popper has indicated (*Open Society,* Vol. I, p. 234) this involves viewing a sentence such as "You ought ...(not to steal)" as being equivalent to the sentence "The norm 'you ought not to steal' is valid, correct ("true")." As Popper indicates with his reference to Tarski, full precision requires the following: What *A* says is valid./ Therefore: '*n*' is valid (whereby "*n*" is a meta-linguistic sign for the normative sentence which in a given case takes the place of "*N*")./ Therefore *N* (or the translation of "*N*" in the meta-language, should one wish to admit only *one* language into the argument). This is now a valid argument (valid in an appropriate deontic logic or in a "semantics" à la Tarski in which Validity is a central concept). Also presupposed is that, analogous to the similarity criterion between explicandum and explicatum for Tarski's explicatum of the concept of truth, a criterion (as a necessary condition) for the adequacy of the explicatum of "validity" is introduced, namely: a definition of "valid" is adequate in the sense of the similarity criterion if and only if from the definition by means of which "valid" is introduced into the meta-language, every sentence of the object language with the following form can be derived: "*n* is valid if and only if *N*" (using the convention mentioned above according to which "*n*" is a meta-linguistic sing nor the normative sentence which stands for "*N*" in a given case).
9. At this point we again encounter a parallel between the legitimation of *truth* claims of statements and the legitimation of *validity* claims of norms (mentioned above in footnote 8). If one wants to eschew the concept of objective truth or correspondence, relying instead on a consensus theory of truth, then it is obvious that factual consensus cannot do the job. Thus, consensus is idealized, and truth is defined as that about which the ideal communication community *would* reach consensus in the long run (K.-O. Apel). But this does *not* give you any indicators (not even fallible ones, like the ones Popper offers) concerning the truth or "Verisimilitude" of a certain hypothesis and hence no justification of the *action* of (provisionally) accepting that hypothesis; and the original problem of truth reappears, because what could the consensus be about if not about truth claims with respect to assertions or validity claims with respect to norms? Moreover, underlying all similar schemes appears to be a *historicistic* assumption: that there is some guarantee for progress In The Long Run. A retreat from the search for objective criteria of appraisal leads first to sociologism and then into an idealization based on historicistic metaphysics analogous to a salvation story.
10. This is the modern version of the psychologism of traditional epistemology: the explication of criteria of knowledge in terms of experiences, be they intellectual (rationalism) or conceptual (empiricism). The error common to both varieties lies in the failure to see that sentences can only be justified by sentences, and that the *subjective* concept of knowledge inevitably leads to scepticism and irrationalism, as Popper's critique has clearly shown.
11. Cf. Lakatos in the Schilpp Volume: *The Philosophy of Karl Popper,* Vol. I, p. 251; cf. also par. 1.12 of my paper "Popperian..." mentioned in footnote 2 above.

12. A criticism of the use of history as an arbiter in the appraisal of methodologies is given in par. 1.14 of "Popperian philosophy ..." mentioned in footnote 2 and 11 above.
13. Churchman, C. *Prediction and optimal decision. Philosophical issues of a science of values. (The problem of modern decision making: value vs. fact)*, Englewood Cliffs, N.J.: Prentice Hall, 1961, p. 367.
14. in *Journal of Philosophy* No. 20, pp. 551 ff.
15. A brilliant and enlightening study of this phenomenon is Schelsky, H. *Die Arbeit tun die anderen. Klassenkampf und Priesterherrschaft der Intellektuellen.* Opladen: Westdeutscher Verlag, 1975.
16. Cf. G. Andersson's contribution to the Fourth *ICUS*.
17. This is the message in ch. 24 of Popper's *Open Society*, Vol. II.
18. Cf. Hayek, F. *The counter-revolution of science. Studies on the abuse of reason.* Glencoe: Free Press, 1955 (original ed. 1952), p. 218.
19. (Hayek, 1955) p. 92. When one has recognized the paramount importance of *unintended* (and often unwanted) consequences of social action, one has seen also that the only sensible mode of trying to bring about changes which constitute an improvement is by *piece-meal "engineering"*: cautious small-scale experimentation which does not risk losing past achievements (such as the comparatively high degree of personal liberty in Western democracies).
20. (Hayek, 1955) p. 218 and L. v. Bertalanffy "The psychopathology of scientism," in Schoeck, H. and Wiggings, J. (eds.) *Scientism and Values*, Princeton: Van Nostrand, 1960, pp. 202-218, esp. p. 207.
21. Andersson, G. and Radnitzky, G. "Kritische oder traditionelle Wissenschaftsforschung?" *Wirtschaft und Wissenschaft*, vol. 23, No. 1 (1975), esp. p. 11.
    Andersson, G. *Forskningens nytta och frihet* ("The Freedom and Utility of Research") *forthcoming* at Esselte Studium, Stockholm, Sweden.
22. Cf., e.g. Lübbe, H. "Traditionsverlust und Fortschrittskrise. Sozialer Wandel als Orientierungsproblem," in: *Wolfenbütteler Studien zur Aufklärung* (ed. G. Schulz) Vol. 1, Bremen: Jacobi Verlag, pp. 12-33, esp. pp. 22 ff.
23. Schelsky, op. cit. (in footnote 15 above) p. 79, cf. also pp. 43, 143, 128, 130, 70, 77 - the general thesis of the book. For a case study see also Huntford, R. *The new totalitarians.* London: Allan Lane, The Penguin Press, 1971 ff.

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