INCOMMENSURABILITY IS NOT A THREAT TO THE RATIONALITY OF SCIENCE OR TO THE ANTI–DOGMATIC TRADITION

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1. The anti-dogmatic tradition.

In order to understand the nature and import of the present-day discussion on theory-ladenness and incommensurability, it is necessary to consider two major changes in the philosophy of science of our century. The first change is that attention is not centered any more at specific methods, which are claimed to be universally valid, but rather at a very general view on (scientific) rationality, which allows for changes in methodological rules. Most present-day philosophers of science are not very interested in specific theories of induction, of explanation, of hypothesis testing, and the like, or even believe that such theories are not valid for all times or in all domains. They are however, interested in general rules which enable us to understand as rational choices between theories *and* choices between methodological principles[1]. The second change concerns the recent stress on competitive pluralism. Present-day philosophers of science are not only convinced that it is sometimes impossible to make a rationally justifiable exclusive choice between competing theories or even between competing methodologies, but a large number of them believes that the co-existence of competing theories (and competing methodologies) is an essential component of the progressive character of the scientific enterprise — see, of course, especially Larry Laudan (1977).

Both changes fit in quite well with the anti-dogmatic cultural tradition, which is usually said to have started in Europe with the Enlightment, but which has ties with the Renaissance and with Rationalism. The history of this tradition cannot be understood

without reference to ideological and political factors. The struggle against the power of churches, especially of the Catholic Church, and against political institutions, gave a major impetus to this tradition. Another major impetus was provided by the successes of science; adherents of the anti-dogmatic tradition see science either as an offspring of their tradition or else as something that has been integrated in it. The combined struggle of this tradition against very different claims on unique authority enables us to understand that this tradition formed the intellectual background — or part of it — for Liberalism and Socialism, and for Positivism, and enables us to understand the anti-clerical character of these movements during certain periods, especially, but not solely, in Catholic countries[2].

The temptation to dogmatism has been constant, even from within the tradition. (If one feels extremely superior to one's opponent, one is tempted not to consider the opponent's objections at all). Relying on this tradition and on the successes of science, some have defended an extremely dogmatic form of scientism. Relying on this tradition, some have held dogmatic beliefs on the absolute reliability of empirical data, or on *the* scientific method, presumed to be a known and static set of methodological rules. Nothwithstanding all this, it should be kept in mind that the very basis of the tradition is the idea of free enquiry, applied to any domain, connected with the deep conviction that there is no revelation[3]. Given the fact that even very successful scientific theories and methodological views[4] became replaced by competitors, it is completely in the line of the tradition to consider theories and methodological rules as historically variable. Given the fact that a plurality of competing theories and methodological views existed during several periods in almost any scientific domain, it is completely in the line of the tradition to hold that clearcut justified choices are not always possible. Also, the idea that both beliefs and methods are subject to change and that clearcut choices between (sets of) beliefs and methods cannot always be justified, is not at all new to the anti-dogmatic tradition. So, e.g., the idea of a pluralistic society, in which autonomous thinking is propagated and in which conflicting views are respected (pluralism and toleration), has for centuries been defended and fought for by persons and movements belonging to this tradition[5].

Pluralism and toleration may be interpreted in a passive way : one merely tolerates and respects competing views because one realizes that one's own views are not justified in an absolute way.

In my (1974) and (1978) I have argued that in order to recognize one's own dogmas and prejudices, and in order to recognize the weaknesses of one's own views in general, it is necessary to cooperate in an active form of pluralism. Competing views should interact intellectually, they should try to demonstrate the weaknesses of their opponent's theories, they should try to defend themselves to such attacks, and they should transform themselves whenever this inter-action requires so.

From a diachronic point of view, the incommensurability theses constitute a threat towards the rationality of the evolution of science. Within the anti-dogmatic tradition, however, the incommensurability theses also form a threat from a synchronic point of view : they from a threat to the possibility of communica-tion and hence of intellectual fight between competing scientific theories. They are arguments in favour of the thesis that each view is at best open with respect to new (theory-laden) observational data, but is not open to criticism from competing views. If this were correct, any view would necessarily be dogmatic in some fundamental respects. But if each view were necessarily dogmatic because intellectual fight is impossible, then it also would be impossible to measure degrees of dogmatism and it would also be impossible to distinguish between different views in this respect. The upshot would be that the offsprings of the anti-dogmatic tradition would not be better off than the dogmatic views this tradition set out to fight in the first place.

2. The incommensurability theses.

In this paper I shall deal with what I consider to be the three major ways in which theories are claimed to be incommensurable by some present-day philosophers of science, viz. with those alleged forms of incommensurability which are argued by referring to the theory-ladenness of, respectively, observational data, terms, and methodological rules. Other forms of incommensurability are some-times mentioned, e.g., the incommensurability of observational data, or of statements, etc., but the nature and import of these reduce to the nature and import of the corresponding theory-ladenness theses. (For readers not familiar with the problem, I have given a brief survey of the three major incommensurability theses in the editorial preface to the preceding volume of this journal).

The three (major) forms of incommensurability are clearly of a

different form and constitute different kinds of problems, and not only different problems. I shall briefly consider each of them in order to show this.

The thesis that observations are theory-laden states that observations, or perhaps more correctly observational data, are never free of interpretation, of theoretical elements. In this generality, the thesis has been argued in so convincingly a way, that I cannot imagine anyone to question it. Of course important philosophers of the past have held the view that there are sensations or sense-data, which are direct results of sensory experience and which are uninterpreted. Since those days, however, the information we gained from the history of science, from psychology, and even from neurology, e.g., about the nerves connecting the eyebals with the brain, are so clear and convincing that the existence of sensations or sense data seems to be rejected in a final way.

It is another question, however, whether the alleged connected form of incommensurability does indeed follow from this. The problem, as I shall explain in a subsequent section, is to determine the set of theoretical elements which enter in the interpretation of a person's observational data. Popper seems to believe and Kuhn clearly believes that this set contains all relevant statements of the theory the person adheres to. (It is not very important whether 'theory' is used here in the broad sense of "paradigm" or "research tradition", or in the narrow, linguistic sense). If this is indeed the case, then it is quite possible that two persons, adhering to different theories respectively, gain different observational data while, say, looking at the same fact. The extent to which the data fit in with the corresponding theory (or with the other for that matter) is not very important. By all means a common observational basis for the two theories might be absent, if by the observational basis of a theory we mean the observational data gained by its adherents.

The theory-ladenness of terms is at first sight analogous to the theory-ladenness of observation. The meaning of a term, even of so-called observational terms, is said to be loaded with theoretical elements. Again this statement becomes clear only after the set of those elements has been specified. Usually this set is specified as containing all relevant (or even all) statements from the theory (either in the narrow sense or in the sense of a "paradigm") adhered to by some person. This is then used as an argument for the absence of any deductive relations between any two competing theories on the same domain (this is what Feyerabend (1977) claims to mean by 'incom-

mensurability'). This form of incommensurability is clearly related to the Quinean thesis of the indeterminacy of translation, as Kuhn (1970) noticed. Whether this incommensurability thesis is important or not depends on its exact meaning. More precisely, if this thesis merely boils down to the absence of deductive relations between theories, it need not in principle prevent one from making a rational choice between competing theories, nor need it prevent intellectual fights between adherents of competing theories. (I return to this in a subsequent section). But if the thesis moreover asserts that communication between adherents of competing theories is impossible, then of course it constitutes a severe threat to the rationality of scientific development and to the very basic presuppositions of the anti-dogmatic tradition.

The third incommensurability thesis is quite different in form from the other two. If one claims that methodological rules are theory-laden, one obviously does not mean that statements from some scientific theory (in the narrow sense) play a constitutive role with respect to the methodological rules that are taken to be correct by the adherents of the theory. Of course, such rule may have pre-suppositions about the world and about human beings qua knowing subjects. However, such presuppositions are very general, far too general to be significantly dependent on the statements of some theory. Yet, methodological views may be theory-laden in the sense that different such views go along with or form constitutive parts of different paradigms or research traditions. If, under these conditions, it is impossible for the scientists working in one research tradition to demonstrate to those working in the other, that their methodological view is superior, then any choice made between the two research traditions will always be a choice from within one of these traditions. This situation does indeed question the rationality of the scientific enterprise as well as the superiority of the anti-dogmatic tradition.

The picture might be a little more complex than I suggested in the preceding paragraph. It is indeed plausible to assume that the fact that some set of methodological rules led to (the discovery and acceptance of) one or several very successfull theories within some research tradition will be considered as an argument in favour of this set of rules by the adherents of this tradition. One of the better arguments for methodological views is indeed that they lead to successful theories. Hence some argumentative circularity cannot be avoided. One cannot simply reduce the problem in old-fashioned

hierarchical terms by saying that the methodological disagreement leads to the disagreement on paradigm choice or theory choice. All three forms of theory-ladenness have at least one thing in common, viz. that they point to forms of argumentative circularity.

Philosophers have argued for a long time now, that all arguments, if pursued, are in the end either petitiones principii, or circular, or appealing to reductio ad infinitum. I have argued in my (1978) that circularity is an acceptable policy, that some form of circularity is the best we can get in certain cases, and that it is not in the way of progress and of rationality. (This is far from original, the notion of a healthy circle is well-known to American epistemologists). From this viewpoint, the aforementioned forms of theory-ladenness are not necessarily problematic. They are only problematic inasfar as they lead to forms of incommensurability that constitute threats to the rationality of human knowledge and to the possibility of intellectual fights between competing views.

3. A framework of "contexts".

In spite of all paper spent on it, it seems to me that the incommensurability problems are neither very important nor very difficult to solve. They are difficult and important with respect to the traditional and common views to human thinking and knowledge, but I am convinced these views are utterly wrong. According to such views, human knowledge (in the broadest sense of the term) is organized in a hierarchical way. The lower level items are justified by higher level items, and hence a justified choice between entities from one level presupposes that all relevant entities from all higher levels are justified. Those traditional views allow for (non-extreme) degrees of certainty, which may vary over time. But they presuppose that degrees of certainty are assigned in a consistent way to all statements (by some person or community), and that clear and general rules are available that justify changes in these assignments from one point in time to another. Moreover, the leading traditional views on human thinking and knowledge hold that the meanings of terms (and sentences) are defined with respect to stable (or very slowly changing) large scale entities such as languages or conceptual systems, and that either each linguistic entity has a unique meaning with respect to the large scale entity to which it belongs, or else, if linguistic entities may have different meanings in different contexts, that a general system is available which

determines with respect to each context a unique meaning for each linguistic entity.

I take all three theses to be wrong : the entities of knowledge, belief, adherence, etc., are not and should not be justified in terms of hierarchically higher such entities; degrees of certainty (and the like) are not and need not be assigned in a unique and consistent way to all statements, and changes to such degrees are not and need not be regulated by some preestablished system; linguistic entities do not have unique and stable meanings with respect to the language to which they belong, nor is their meaning defined or need it be defined with respect to each possible context by some general or preestablished system. In my (1984) I have outlined a systematic alternative approach to meaning and certainty, I have tried to defend it against a set of possible objections, and I have tried to demonstrate its merits over the common traditional approaches (and with respect to the approach by Isaac Levi (1980), which I take to be already extremely superior to the traditional approaches). My main type of argument was that an approach of the kind I outlined is necessary to understand and do justice to major results from present-day philosophy of science. Among them, I hope to demonstrate here, is the fact that theory-ladenness and incommensurability are dealt with in such a way (i) that justice is done to the arguments of the supporters of the incommensurability theses, but (ii) that these arguments form neither a threat to the rationality of science nor to the anti-dogmatic tradition.

I shall now briefly summarize the approach I defended in my (1984). I am somewhat reluctant to do so, because I realize that I might easily be misunderstood. For more details I refer the reader to the aformentioned paper (and warn him that even this paper is rather concise and difficult, and less explicit that it should be).

I use the term 'context' to denote my central notion. Perhaps 'communication situation' would be a better phrase; all forms of human inquiry may be seen as forms of communication, communication with oneself being a special case. Furthermore, I restrict my attention to contexts in which people try to solve problems, a term which I shall keep rather vague because I think that the statements I shall make about it apply to almost any kind of problem I know of.

My definition of a context goes as follows : a quintuple consisting of (i) a set of participants, (ii) the problem under consideration, (iii) the set of statements regarded as certain and defining

for this reason the set of possible answers to the problem, (iv) the set of aspects considered relevant to the problem, and (v) the set of methodological does and don'ts judged appropriate with respect to the problem.

In connection with the first element, the participants, I need to specify that not only the number of participants, but also their beliefs about the world (both observational and theoretical) and their beliefs about the beliefs of the other participants form part of the context. Moreover, I regard a context, defined in the aforementioned way, as in general different for each participant. With respect to problems, it is important to specify that I interpret these in a rather restricted way. If two people are discussing, and one of them specifies the meaning of some term he employed, or defends some criterion or rule he applied, the problem and hence the context changes. If, after this digression, they return to the original problem, it is quite possible that, at least for one of the participants, the return is not to the original context because some element of (iii) or (iv) may have been changed. Among the statements regarded as certain, see (iii), I do not distinguish between logical and non-logical certainties. In my (1984) I have argued at length that this distinction cannot be upheld. All contextual certainties function as logical truths and hence are determining in part the meanings of the terms employed in the context, and I also argued that there cannot be statements that are logically true in all contexts. Each contextual certainty, each belief about the relevance of some aspect, and each methodological rule may be questioned. But in all three cases this will be done within a different context from the one in which those entities play a constitutive role. As a final clarification to the definition of a context, I mention that the fourth element, viz. the set of aspects considered relevant, need occur in this definition because unrealistic and useless complications would arise if we were not to restrict a context with respect to relevant aspects. Clearly (iv) introduces restrictions to both (iii) and (v).

On this approach language is not a static system which defines meanings of terms in a unique way, but a plastic system which we may easily transform and indeed continuously transform according to our specific needs of the moment. Language is a communication instrument. While thinking, we adapt it in a creative way to the purpose it has to serve. While interacting with other people, we do the same thing, but we do this in an interactive way; and central to the purpose language has to serve here, is that it should make

communication possible. I shall make a statement about the circum-
stances under which communication obtains, but first I have to say
more about degrees of certainty and about the relation between
contexts.

Degrees of certainty may vary from one context to the other.
Some statements may even be assigned the value one in some context
and the value zero in another. In my (1984) I have tried to show that
this is actually the case, that there need be nothing irrational about
this, and that it is impossible to take care of such variations of
certainty within some closed probabilistic system in which degrees
of certainty are distributed to all statements in some single way. Do
such (and other) "inconsistencies" between contexts form a
problem? The answer depends on whether the person (or
community) under consideration is able to justify these "inconsisten-
cies". There is nothing wrong with considering some theory as
certain with respect to some set of applications, and during the same
period questioning the theory within some context of investigation.
(Nor may such variations by explained as deriving from changing
empirical evidence — as if this were absolutely certain in all con-
texts — for the variation is connected to a difference in problem,
and not to some growing set of, say, observational data.). However,
if the theory turns out to score low in the investigation contexts,
then of course there is a need to replace it by some theory which
scores better in these contexts but which at the same time may be
justifiedly considered as certain with respect to certain sets of
application.

As I implied above, the way in which a context is set up may be
questioned, and the answer to this question should be looked for in
a different context. The certainties relied upon in the latter,
however, should not be of a "higher level". Methodological beliefs
play a role in the justification of factual statements, but at the same
time factual statements about the world, about human beings, and
about the successes of methodological rules, play a role in the
justification of the latter.

Consider two scientists, X and Y, engaged in a discussion. Let
us denote by C_X and C_Y the contexts for X and Y respectively at
a given moment. With respect to C_X there is a set of possible
situations for X and with respect to C_Y a set of possible situations
for Y. I now propose the following definition : there is communi-
cation between X and Y in the given pair of contexts C_X and C_Y
if and only if both X and Y believe truly that there is a one-one

relationship R between the possible situations of X and the possible
situations of Y such that, if Rs_Xs_Y, then X will decide to s_X in C_X
under the same conditions under which Y will decide to s_Y in C_Y.
There are two points about this definition on which I want to put
some stress. The first is rather obvious : whether or not there is
communication between two people is a *local* matter. There may be
communication when they discuss certain problems under certain
conditions, whereas communication may be completely absent under
other conditions or when they discuss other problems. (Of course
there might be partial communication, or one-sided communication,
or fancied communication — the reader may easily see what I mean
by these if he reconsiders the previous definition). The second point
I wanted to emphasize is that, in order for communication to obtain
between X and Y, it is by no means necessary that they have the
same things in their minds — would this be possible in any sense ? —
or that their theories, or their relevant theories, have the same
structure. .

*4. Theory-ladenness and incommensurability within the contextual
framework.*

I shall now show that, within this contextual approach, the
arguments adduced in support of the incommensurability theses
form neither a threat to scientific rationality, nor to the anti-
dogmatic tradition. I readily concede that terms, observations, as
well as methodological views are theory-laden, although this theory-
ladenness is not as static as it has been suggested (see below). I
also concede that the statement that competing theories are in-
commensurable, is correct for specific meanings of 'incommen-
surable'. I disclaim, however, that such forms of incommensurability
form a problem in the sense specified above.

Let us start by looking at the theory-ladenness of terms. I
think this should be considered first, because, as long as the
connected form of incommensurability is not reduced to its real
impact, it might be used as an argument for the forms of incom-
mensurability connected to observation and methodology.

I first make a minor digression. It seems to me that 'the
meaning of t' might be a confusing phrase. It suggests indeed that
terms have a meaning independently of language users and of specific
times (and contexts in the sense of the previous section). Some
people defend the thesis that terms have meaning with respect to

certain communities at a given time, but even this is false. The intersection of the meaning of some term for different people and with respect to different contexts may clearly be empty. If confronted, e.g., with a stipulative definition, however unrelated to the ways in which one employed the term before, one is able at once to understand the term in the sense of the definition (if this is of sufficient clarity), and one is able to use the term in this sense when talking to other people who read or heard the same stipulative definition. As I remarked before, languages are not static systems determining unique meanings, but are plastic and may be adapted to our purposes of the moment (and this is indeed one of the main causes of the evolution of languages).

Terms are theory-laden in an obvious sense. Mathematical terms are related to calculi and other mathematical structures, employing them in describing some domain presupposes that this domain may be structured in this specific way. The same applies to logical terms — see my (1980) for arguments to this effect concerning negation. So-called observational terms are related to certain criteria and even this fact alone shows their theory-ladenness, for all such criteria presuppose the truth of certain factual statements. Theoretical terms need not be commented upon.

Having agreed that terms are theory-laden, I think a qualification is necessary. The meaning of a term does not depend on one's beliefs, i.e. on things one believes to be true, but rather on the possibilities one allows for, i.e. on the situations one believes to be possible. The confusion arises, it seems to me, from the fact that the contextual character of meanings is disregarded. Some physical law may be presupposed in the meaning of some term within some context, but this term cannot have the same meaning in a context in which the arguments for accepting this law are discussed. In other words, the same statement may be factual in one context and logical (but not necessarily a law of formal logic) in another context, all this for one and the same person and during the same period of time. Returning to the distinctions between truths and possibilities, we are all quite familiar with views different from ours but belonging to the same culture. If discussing with people that have a view different from ours, their views are relevant for our contextual possibilities (if we are somewhat sensible and open that is) and this will further communication. If we do not know their views, we may learn them (if we are interested). How easy this will be depends on a large number of things, among which the "cultural"

distance between us and them. I do not claim that I am able to com-
municate about any problem with any human being, I only claim
that I managed to communicate with a lot of people that do not at
all share all of my beliefs.

Does it follow from the theory-ladenness of terms that there are
no deductive relations between theories ? It does not in the following
sense : if both T and T' are formulated as calculi, and the under-
lying logic is the same, then all theorems of logic "follow from"
both T and T'; if some terms occur both in T and T', then possibly
T and T' will have logical consequences which are identical or which
are contradictory. Of course, this is not what Feyerabend meant
when claiming the absence of deductive relations. His claim is that
deductive relations are absent if we take the meanings of the terms
into account, where he views meanings as determined by the theory
adhered to by the language users. What about this view on meaning?
In a lot of contexts it is absolutely correct. It is even correct in all
contexts if we restrict the meaning of 'theory' to the set of state-
ments regarded as certain (element (iii) of a context). Where I take
Feyerabend to go wrong is where he presupposes, quite remarkable
so in view of his plea for creativity and liberty, that meanings are
constant over contexts. Feyerabend claims that meanings change as
soon as our theories change. I think we need to go one step further :
meanings change as soon as the context changes, more specifically,
as soon as the set of statements regarded as certain changes. (I am
talking about "communicative meaning" here, viz. the information
some participant wants to pass on to the others, and not about
something in the head of the participant, but Feyerabend too is not
thinking about the latter).

In some (pairs of) contexts adherents to competing theories
will disagree about statements which both parties derived from their
theories, in others they will agree. (In this sense there are deductive
relations). In still other (pairs of) contexts communication will be
absent or very distorted. But once the participants in the discussion
realize that they do not understand each other because each party
keeps up with its view, with its way of structuring the world, with
its contextual certainties, they may start discussing about these
views, structurings, certainties. Ways of structuring the world are
indeed not choosen between by comparing predictions — any
inventive person will be able to explain a negative so-called crucial
experiment — but by comparing the over-all merits of these
structurings, both with respect to empirical data and with respect

to the coherence of our world-view (covering descriptive dimensions, methodological dimensions, and so on).

The other forms of incommensurability are easily dealt with now. Observational data are clearly theory-laden. Of course most mediaeval people were convinced that observation shows beyond any doubt that stones fall down (in an absolute sense of direction), and there cannot be the slightest doubt that the factual evidence we derive from observation is heavily loaded with prejudices. But what is the point of all this ? As soon as someone is confronted with a competing theory which one is able to learn (in that the cultural distance is not too large), one will recognize one's own prejudices, broaden one's set of possibilities, and hence arrive at an observational datum which is neutral with respect to both theories — see, e.g., Krajewski (1975). No observational data will ever be neutral in an absolute sense. But if they are neutral with respect to theories that coexist at a given time, adherents of the distinct competing theories may clearly distinguish between the observational datum and its interpretation with respect to each theory. Again, strong cultural differences may cause a lot of trouble, but I think it a little doubtful that these might have been a serious hindrance for Western science during the last decades. This is why I think it a baffling fact that Kuhn actually claimed that heliocentrists see a movement of the planet earth when looking at a sunrise. This is also why I cannot but consider the ongoing discussion about the incommensurability of "facts" and of problems a purely academic business.

As I remarked in a preceding section of this paper, the theory-ladenness of methodological rules concerns a problem which is quite distinct from the two aforementioned forms of theory-ladenness in that the connection between a theory and some methodological view is far less intimate than the connection between a theory and the meaning of terms or between a theory and observational data (but remember the qualification I made in the second section). Fundamental methodological disagreements are presumably more difficult to resolve than fundamental theoretical disagreements. Also, the fact that methodological disagreements may prevent a clearcut choice for some time is not very important. The absence of a clearly justifiable choice during some period of time is not an objection to rationality (if at least one realizes that rationality is itself an object of discussion, subject to improvement). The require-ment to have ready choices in all domains is even quite contrary to the anti-dogmatic tradition. Extremely important, however, is the

fact that rational discussion about methodological rules is possible, and that this discussion does not presuppose that one gives up all results gained with the help of the methodological views under discussion. This, I repeat, is an advantage of the contextual approach.

5. *Conclusion and a final remark.*

From the viewpoint of the contextual approach, neither form of theory-ladenness or incommensurability turns out to constitute a treath to the rationality of science or to the anti-dogmatic tradition. Rather than leading to the impossibility of rational interaction through communication between people adhering to different theories, this approach highlights the importance of communication between different research traditions for scientific progress : such communication leads in general to the detection of prejudices; with respect to the present discussion it leads to the detection of prejudices about the possibilities restricting possible solutions to problems — becoming acquainted with alternative conceptual systems broadens our perspective in approaching the world — prejudices in our observations — detecting these leads to observations which are less theory-laden — and methodological prejudices — detecting these is extremely important for methodological progress. Please remark that such improvements need not change all contexts, but make possible new, more open, less prejudiced approaches in contexts in which such approaches constitute improvements.

I quite realize that there is a weakness in the present "solution". It depends completely on the correctness of the contextual approach and this approach might be feared to lead, in real life situations, to a multiplicity of contexts which is out of any controll. I did of course refer to my (1984), but even there I have not answered this objection. I can only hope to have convinced the reader of the intuitive plausibility of the contextual approach, and of the problem-solving capacity it might prove to have after having been worked out in its technical details. I do realize, however, that the burden to engage in some years of mathematical work is on me.

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NOTES

[1] Paul Feyerabend forms an exception. He rejects and argues against the universal validity of specific methodological principles, but, at least in some papers, he even rejects the possibility of scientific rationality.

[2] When Désiré Mercier "founded" neo-Thomism, his main objective was to fight Kantianism and Positivism by trying to reconcile science with a Catholic philosophy. In his opening statement (1889) for the foundation of the Ecole Supérieure de Philosophie at the Catholic University of Louvain, he actually argues that catholics are able to do scientific research — a quite significant and revealing fact in several respects.

[3] Some outstanding present-day theologians will agree to this in that they consider "revelation" as an important source of inspiration for people trying to organize their lifes, but not as providing a direct answer to any question.

[4] See Larry Laudan (1981) for a collection of papers in which the dynamic character of scientific methodology is argued.

[5] At least in public matters, however, this fight presupposes limits to tolerance and pluralism. One should not be tolerant to the extent of letting dogmatists (churches, nazis, stalinists) take power in our societies.

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