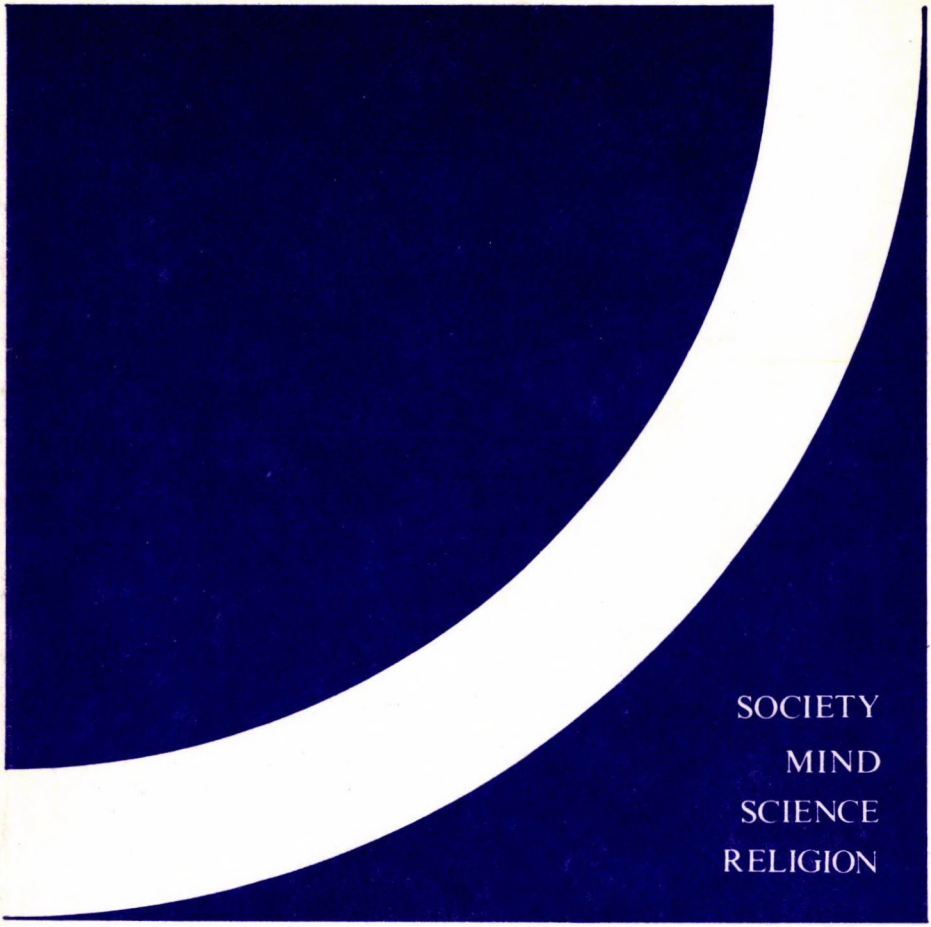
ΔΟΞΑ 6

PHILOSOPHICAL STUDIES



Budapest

DOXA

PHILOSOPHICAL STUDIES

6

Institute of Philosophy
Hungarian Academy of Sciences

Budapest

DOXA 6
1985

series editor
János Kelemen

Edited by
Imre Hronszky and Balázs Dajka

PREPRINT
HU-ISSN 0236-6932

Publisher: József Lukács, Director of the Institute
At the Printing Office of the Institute for Culture
Budapest, Hungary
Gábor Fazekas, printer

CONTENTS

PREFACE

There is much research currently being done in Hungary into the philosophy and the history of science. An important event in the process of developing international contacts in these fields was the 5th Joint International Conference on History and Philosophy of Science. This conference, organized jointly by the IUHPS and by Hungarian university and Academy institutes, was held in Veszprém, Hungary, in August 1984. Some of the papers included in this volume of DOXA were read to that gathering. Others, inspired by what was said there, were written later. Let us recall that DOXA 2 and 3 were published shortly before that conference and included some of the materials to be read. For the most part, however, the proceedings are forthcoming in *Logique et Analyse* (Brussels) and in a volume to be published by Reidel, Dordrecht.

DUCROT'S NOTION OF ARGUMENTATIVE DIRECTION*

Paul Gochet

This paper aims to show that Ducrot's and Anscombre's research on the relationships between argumentation, discourse, and lexicon have a bearing on issues discussed within the Montaguean tradition.

Lauri Karttunen and Stanley Peters have shown how the notion of conventional implicature can be given a rigorous treatment within an extension of the fragment of language studied by Montague in the epoch-making paper "The proper treatment of quantification in ordinary English". There is a significant overlap between the phenomena which the latter two and the former two authors try to account for. For instance they all tackle the problem raised by the exclusive contribution of words such as *even* or *almost* to the meaning of the sentences in which they occur. (Karttunen-Peters, 1979, 1-56)

Consider these two sentences

(a) Even Bill likes Mary

(b) If even Bill likes Mary, then all is well.

The truth conditions of (a) and (b) are clearly different. The second does not commit the speaker to "Bill likes Mary" whereas the first does. In spite of this the two sentences commit the speaker to (c) and (d)

(c) Other people besides Bill like Mary

(d) Of the people under consideration, Bill is the least likely to like Mary.

This clearly shows that "even" means something which has no effect on truth-conditions. To denote this something, Karttunen and Peters use the expression "conventional implicature" borrowed from Grice. As Montague's semantics treated only the truth-

*Paper presented to the 5th Joint International Conference on History and Philosophy of Science, Veszprém, Hungary. August 1984.

conditional aspect of meaning they extended his system in order to make it powerful enough to cover conventional implicature also. They associate two functions with each sentence. One of them takes the truth conditional aspect of meaning as value, the other takes as values propositions which specify the so-called conventional implicata of the sentence. Moreover Karttunen and Peters carry out the difficult task of presenting "a mechanism by which a finite system of rules can recursively associate with each of a language's infinitely many sentences the two required functions" (Karttunen-Peters, 1975, p. 270).

Let us now look at what Ducrot says about "even". Consider the sentence

(e) Even Peter has come.

To the question "In the semantic value of this statement what is to be ascribed to the adverb *even*", Ducrot answers: "it seems... that by using *even* the speaker presents himself as not only aiming to inform his addressee of what has happened: while letting him know... that Peter has come, he uses this coming to underpin some conclusion, insinuating that this coming supports this conclusion more than some other person's coming would (Ducrot, 1980, p. 12).

The adverb *almost* is also discussed by both Ducrot and by Karttunen and Peters. The latter offer an account in which the verb phrase adverb $almost^1$ is related to the sentence-adverb $almost^2$. The meaning of $almost^2$ is something like "come close to be the case that". The conventional implicature of $almost^2$ is given by these two statements together: (1) "asserting that something almost has a given property implicates that it doesn't", (2) if the verb phrase modified by the verb phrase adverb $almost^1$ already carries some implicatures, all these implicatures are inherited by the verb phrase which results from the introduction of the adverb. For instance, granted that "John failed to win the race" implicates that he tried to win, "John almost failed to win" inherits this implicature.

Let us now switch to Ducrot's account: "For us,... a sentence containing a quantitative indication *almost x* is directed

towards conclusions such that the same sentence would have provided a stronger argument for the same conclusion if *x* had been substituted for *almost x*" (Ducrot, 1980, p. 26). For instance "almost 80 % of the drivers respect the speed limits" goes in the same *argumentative direction* as "80 % of the drivers respect the speed limits". In the following sense: both sentences invite the same inferences (although the second sentence is a stronger premise).

It is my contention that Ducrot's account should not be seen as an *alternative* to Karttunen and Peters' account. These two accounts should be seen as *supplementing* one another. Ducrot's notion of argumentative direction enables us to explain why the following sentence is acceptable in spite of the prima facie contradiction which it contains: "the dinner is almost ready, and it is even ready" (Anscombre-Ducrot, 1983, p. 65), but Karttunen and Peter's account are welcome to explain why Anscombre and Ducrot's example involves a prima facie contradiction. Moreover Karttunen and Peter's devious mechanism of inheritance of implicature is not only welcome but absolutely necessary if one wants to account for more complex cases such as "John has almost failed, and even he has failed to win his bet to be here in time" which can be said in special circumstances: we see John opening the door at 12 o' clock whereas he was supposed to be in by that time. And a moment later we realize that our chronometer has stopped, and we straighten our claim by saying that John has failed after all.

Ducrot distinguishes between *sentences* which belong to language and which bear a *meaning* and *statements* which belong to discourse and carry a *sense*. A statement is a sentence in use. To find out what its sense is, we have to bring *the circumstances of use* to bear on the meaning of the corresponding sentence. In sense, Ducrot locates the argumentative direction and attracts our attention to the etymological connection between sense and direction in the words "sens", "senso" and

"sentido" of the romance languages (Anscombre-Ducrot, 1983. p. 87).

For meaning, Ducrot advocates a compositional account and sketches a programme similar to that already put into effect by Montague and his followers to a large extent: "As to the meaning of the sentence (materialized in a statement), I hold that it is possible to compute it beforehand, independently of any reference to precise conditions of use, by extracting it from the meaning ascribed to the elementary words out of which it is made up" (Ducrot, 1980, p. 21). But Ducrot goes further and envisages "a calculus which, for any statement whatsoever, would generate the formulae chosen to describe its sense, and which would obtain this result by having the formulae which represent the situation suited for its use operate on the formulae which constitute the meaning of the sentence realized in use" (Ducrot, ibid.)

Although Ducrot has not yet worked out a complete calculus accounting for the sense (and argumentative direction) of the statements, he has already spelt out some laws of the envisaged calculus, and supplied a mechanism of inheritance of the argumentative direction which is powerful enough to deal with the intricacies of cases in which presupposition and argumentative direction have both to be taken into account.

Among the laws of argumentation which Ducrot has formulated, the *law of negation* deserves special emphasis. This law reads as follows: "if a statement p is an argument for a statement q, then $\sim p$, i.e. the negation ...of p is an argument for $\sim q$" (Anscombre and Ducrot, 1983, p. 40). The expression "is an argument for" should not be construed as a sort of weak implication. Ducrot is not trying to salvage the well known fallacy embodied in the following inferential schema: "If p then q, then if ~p then ~q". The relation of being an argument for is not a truth-functional relation, not even a relation between truth values. It is rather a rhetorical relation which holds

between *p* and *q* when *p can* be used as an argument for *q*, i.e. points in the ***same argumentative direction*** as *q*. When a lenient mother tells her husband "If our child has got good marks at school offer him a gift, if he has not, offer him a gift nevertheless", she feels the need to use the adverb "nevertheless" which serves to convey that the argumentative direction has been inverted and to that extent she indirectly acknowledges that one would normally expect the following statement: "If our child has got good marks at school, offer him a gift, if he has not, do not offer him a gift", where the argumentative law of negation is applied.

Let us now see in a simple example how the argumentative law of negation together with the classical theory of semantic presupposition supply a solution to a linguistic problem.

There are several sorts of presuppositions: semantic presuppositions which can be characterized in terms of truth-conditional semantics and pragmatic presuppositions (which can in turn be divided into conventional implicatures and conversational implicatures). As far as semantic presuppositions are concerned, the received view is the account which Sir Peter Strawson has put forward in his *Introduction to Logical Theory*. On this view, whenever a sentence containing a presupposition is negated, the asserted content alone is inverted whereas the presuppositional content remains unaffected.

Consider the following example

(f) Peter is as tall as Jack.

In this sentence, it is asserted that Peter's height = Jack's height and it is presupposed that Jack is tall. If we negate sentence (f), the asserted content is negated and the presupposition is left unaffected. A problem arises, however, in connection with the negation of the asserted content. The negation of

(g) Peter's height = Jack's height

is

(h) Peter's height ≠ Jack's height

and (h) literally means that Peter is taller or shorter than Jack. Yet this is not the way in which an unbiased hearer will interpret it. The latter will interpret (h) as saying that Peter is shorter than Jack. How can we explain this preferred reading? Ducrot supplies a solution which brings the notion of argumentative direction to bear on the issue. He observes that "Peter is as tall as Jack" *points in the same direction as* "Peter is taller than Jack". This is shown by the possibility of adding to "Peter is as tall as Jack" the sequel "and even taller". This being granted we can apply the argumentative law of negation and reason as follows: the sentence "Peter is as tall as Jack" goes in the same argumentative direction as (*can* be used as an argument for) the sentence "Peter is taller than Jack", hence "Peter is not as tall as Jack" goes in the same direction as (*can* be used as an argument for) "Peter is shorter *than* Jack".

Let us now come to grips with a complicated case which requires the implementation of Ducrot's full system of interlocking calculi. The complex example consists of *nested* presuppositions and inherited directions. These nested presuppositions and inherited directions are a good test to check the non-ad-hoc-ness of Ducrot's system. Here there are four *diverging* observational data which we wish to explain by appealing to the *same* laws of discourse.

In the example we are going to analyze, the following two operators will play a crucial role

(1) "a mistakenly believes that p"

(2) "a takes it to be the case that p" (or, more colloquially "takes himself to be P").

There is a sort of chiasm between (1) and (2).

(1) *asserts* that ~p and presupposes that *a* believes that *p*

(2) *asserts* that *a* believes that *p* and presupposes that ~*p*.

In other words, when these operators are provided with an operand, they give rise to two sentences which are alike as far

as their truth-conditions are concerned but which differ as to the location of the speech-acts they bear. Each of them bears an assertive content and a presuppositional content. But sentence 1 asserts what sentence 2 presupposes and conversely.

Let us now consider the following four sentences in which the first (i) and the fourth (l) are acceptable. The problem is to provide an explanation of their acceptability.

(i) Peter mistakenly believes that he is as tall as Mary *but* he is rather tall

*(j) Peter mistakenly believes that he is as tall as Mary *but* he is rather short

*(k) Peter takes himself to be as tall as Mary *but* he is rather tall

(l) Peter takes himself to be as tall as Mary *but* he is rather short.

The sentence "Peter is as tall as Mary" carries both an asserted and a presupposed content. The asserted content is "Peter's height = Mary's height". The presupposed content is "Peter's height = Mary's height" and "Peter is tall" point in the same argumentative direction. Let us now insert the sentence "Peter is as tall as Mary" in the "p" of "Peter mistakenly believes that p". The asserted content of "a mistakenly believes that Peter is as tall as Mary" becomes "Peter's height ≠ Mary's height". The presupposed content is firstly "Peter believes that Peter's height ≠ Mary's height", and secondly the statement about the argumentative direction which is *inherited* from the sentence "Peter is as tall as Mary".

As the argumentative direction of a sentence is, Ducrot says, rooted in the assertive content of the sentence and not in the presuppositional content, we cannot exploit the previous inheritance. But at this stage Ducrot brings the argumentative law of negation to bear on the issue. Applied to the example under examination, the law of negation allows the following inference: if "Peter is as tall as Mary" points towards,

(is a possible argument for) "Peter is tall", then "Peter is not as tall as Mary" points towards (is a possible argument for) "Peter is short".

We come close to the stage where the use of "but" which marks a reversal in the argumentative direction will be explained, but we remain hindered by the principle which states that the argumentative direction is rooted in the asserted and not in the presupposed content. Here Ducrot takes a step which will be felt to be rather ad hoc, but which seems to have a genuine predictive power. He introduces the operation of *centring* (*centrage*) which "consists in deducing the content relative to the argumentative direction from the asserted content, by applying the rules of his second calculus (to which the law of negation belongs) to the contents supplied by the first calculus (the calculus which deals with the inheritance of presuppositions), i.e. by applying the rules of the second calculus to the presupposed contents (Anscombre and Ducrot, 1983, p. 103). In other words we have to centre the presupposed content upon the asserted content" and when this has been done, "the direction of the sentence is given by its argumentative presupposition, after the centring has taken place". To cut a long story short, Ducrot postulates a mechanism which removes the constraint about the argumentative direction which was initially said to be located in the asserted content alone.

In sentence (l), the law of negation cannot apply, hence there is no switch from "tall" to "short". Hence, if we add the sentence "he is rather short" we reverse the direction and have to use "but". Ducrot's calculus correctly predicts the acceptability of (i) and (l) and the oddity of (j) and (k).

The law of negation is perhaps the most questionable element in those of Ducrot's claims which I have examined. As Prof. H. von Wright observed (in conversation), although the fact that "he spends a lot of money" points to the conclusion

that "he is rich", it does not follow that "he spends little money" points to the conclusion that "he is not rich".

As a possible answer one might say that spending much money is more closely associated with being rich than spending little money is associated with being poor. This asymmetry reveals that a *topos*, i.e. a culture-specific commonplace is at work here (according to Raccah). I guess the topos here is the institution of "credit".

Topoi are needed anyway for other purposes. For instance the "but" in "It's a beautiful day *but* I have to work" can only be understood if we supply a missing premise: Fine wheather points towards the invited conclusion "One should go for a walk". On the proviso that we fill the gap in that way we can account for the use of "but". The topos which operates here is culture specific. It could not be used by people living in a country where the sun shines everyday. P.Y. Raccan to whom we owe this observation has sketched an account of the selection of topoi for a given sentence. (P.Y. Raccah, 1984). See also Ducrot (O. Ducrot 1984, p. 96) and the two monographs (P.Gochet 1980 and 1986) on the problems raised by the formal semantics assumed in this essay.

Seminar of Logic and Epistemology
University of Liège

Bibliography

(1) J.C. ANSCOMBRE & O. DUCROT, *L'argumentation dans la langue*, Bruxelles, Mardaga, 1983.

(2) O. DUCROT, *Les mots du discours*, Paris, les Editions de Minuit, 1980.

(3) O. DUCROT, *Le dire et le dit*, Paris, les Editions de Minuit, 1984.

(4) L. KARTTUNEN & S. PETERS, "Conventional Implicature" in *Syntax and Semantics Presupposition* 11 ed. by CHOON-KYU OH and David A. DINNEEN, Academic Press, 1979, pp. 1-56.

(5) L. KARTTUNEN & S. PETERS, "Conventional Implicature in Montague Grammar" *Proceedings of the First Annual Meeting of the Berkeley Linguistics Society*, 1975, pp. 266-279.

(6) R. MONTAGUE, *Formal Philosophy*, edited with an introduction by R. THOMASON, New Haven, Yale University Press, 1974.

(7) P.Y. RACCAH, "Argumentation in Representation Semantics", forthcoming in the *Proceedings of the 10th International Conference on Computational Linguistics 84"*.

(8) P.F. STRAWSON, *Introduction to Logical Theory*, London, Methuen, 1952.

(9) P. GOCHET, *Outline of a Nominalist Theory of Propositions*. Dordrecht, Reidel, Synthese Library 1980.

(10) P. GOCHET, *Ascent to Truth, a Critical Examination of Quine's Philosophy*, Munich, Philosophia Verlag 1986.

COMPROMISING ON TRUTH AND REALITY*

Cheryl Misak

A central issue in the realist/anti-realist debate concerns the notions of truth and evaluation. Anti-realists who argue that all knowledge is relative to social factors, values, or conceptual schemes, typically adopt some sort of coherence theory of truth - beliefs are true or false depending on whether or not they cohere with the rest of our belief network. But this theory has been charged with frustrating our goal of evaluation of observations, beliefs, and theories - an "anything goes" relativism appears to accompany anti-realism. Realists also have problems with the notion of truth. The realist package usually includes a correspondence theory of truth which takes beliefs to be true if they correspond to features of the world. Scientific knowledge, at least at the stage at which we evaluate and justify our beliefs, is alleged to be free from the taint of social factors. Many powerful anti-realist arguments, however, are directed against just this feature of the realist position. Given these problems with the two extreme theories of truth and reality, some philosophers have recently been led to believe that a compromise between them is called for.[1] In this paper I will, after attempting to clarify some of the terms and issues of the dispute, follow Putnam's suggestion, and try to show how the writings of C.S. Peirce may help us arrive at a plausible anti-realist position - a position sensitive to the fact that truth and reality are relative to human factors, yet one that escapes evaluational anarchy.

*Paper presented to the 5th Joint International Conference on History and Philosophy of Science, Veszprém, Hungary, August 1984.

I.

On one side of the fence, metaphysical realists construe reality as existing and having a certain character independently of any cognition, or minds. The world is "out there" and the job of science is to tell us what it is like. The source of divergence of observations or beliefs, on this account, is error. Since the world is simply the way it is, if descriptions of it are incompatible, only one can be correct, and the others must be erroneous. The correct one will be the one that stands in the right relationship to reality. As Putnam writes, proponents of this picture see the world as consisting of a fixed totality of mind-independent objects. Truth involves a correspondence between sign and external object, and there is one true and complete description of the way the world is.[2]

Those opposed to this realist position think that human factors are essential in the description of the world. Anti-realists argue that it is not the case that there is one unique reality of which our theories are true or false; reality is at least partly shaped by us. There are a number of loosely distinguishable positions opposed to realism. They are not mutually exclusive. One is "idealism", which asserts that the existence of "real things" depends on the existence of appropriate kinds of minds. Since "idealism" suggests certain historical positions claiming that the mind or the spiritual, as opposed to the material, is the fundamental stuff of the world, the label is rarely adopted by contemporary philosophers. A brand of idealism is "relativism", which holds either that the existence of objects, or the character objects have is relative to something else. This "something" varies with different versions of relativism, but it is always human or cultural in nature.

The third position I want to demarcate is "subjectivism". In its most general sense, it means "due to the subject", and hence is a kind of relativism; the nature of the world is relative to the subject that is perceiving/conceiving it. But

there can be degrees of subjectivity. In a weak sense, "subjective" is only partial; things can be more or less due to the subject. In a strong sense, however, it is complete, and doesn't admit of degrees. Relativism is also vague in this sense; the character of objects might be relative to something else, yet partially objective - that is, partially due to the object itself.

The "colourless"[3] term "anti-realist" is perhaps best characterized negatively. Anti-realists deny the central realist thesis of the existence of a mind-independent world; that is, they believe that the nature of the world is, at least to some extent, mind-dependent. The "to some extent" is important here, as idealism, relativism, and subjectivism are often regarded as being prefaced with the word "total", but it seems that they can come in degrees.

There are two other issues that I think have been run together in some of the literature. One is the question of whether or not the existence of the world is mind-independent, and the other is that of whether or not the characters or qualities that are predicated of the world are mind-independent. Anti-realists take "existence" to be dyadic: "—— exists to/for/because of ——." Realists take it to be monadic: "—— exists." Regarding qualities predicated of reality, the anti-realist takes them to be triadic: "——is —— to ——." And the realist takes them to be dyadic: "—— is ——."

It is a short step from the thesis of the relativity of the world (its existence or its nature) to the relativity of truth and evaluation. If "how the world is" depends ultimately upon cultural or human features, then on a total relativist view, there can be no grounds for claiming that one way of viewing the world is better than another. How we perceive/conceive of the world differs both synchronically and diachronically; different people and different cultures conceive of the world in different ways. On a coherentist view, it

seems that all internally consistent claims will be on a par with respect to their truth value. This is problematic, for the realist intuition that we can and do evaluate some beliefs as being better than others also seems to be correct.

II.

Understandably, some anti-realists want a divorce from their longstanding partner. That is, they want to avoid having to adopt a theory of truth which makes evaluation, rationality, and truth totally relative to conventions, cultures, or conceptual schemes. The recent Putnam is a clear example. He argues against the part of the realist program that makes truth correspondence, but he also argues against total relativism. He thinks a compromise between the two is in order, since he takes the first to face insurmountable problems regarding reference, and the second to be self-refuting.

In the early chapters of *Reason, Truth, and History*, Putnam gives what he takes to be a knock-down argument against the correspondence theory of truth. Since he thinks metaphysical realism has the correspondence theory as its core, it falls as well. His argument is based on model-theoretic considerations showing in a Quinean spirit that reference is indeterminate. There are, he argues, an infinite number of interpretations of any given sign that preserve truth value, and he gives an effective procedure for producing these different models. There is no way, he argues, for the realist to non-arbitrarily specify which relation is *the* reference relation. This challenges the correspondence theorist's claim that one and only one fact corresponds to a proposition. Realists argue that our terms refer to specific things in the world, but since reference is indeterminate, Putnam holds that the realist argument can't even get off the ground.

Putnam's less explicit argument against realism is that it naively fails to take account of the fact that all of our ex-

periential inputs into knowledge are conceptually contaminated; there aren't any inputs which are not themselves shaped by our concepts. We cannot stand outside of our own perspective to look at the world the way in which it "really is". Our world, he says, is a human world, and is dependent ultimately on our human judgements of likeness and difference.[4] He wants to make the "radical claim that what *counts* as the real world depends on our values",[5] for we judge the real world as that which is true and relevant, and relevance presupposes a wide set of interests and values. He concludes from this that the human world (what we count as the world) is determined by our values. Rationality is also value-laden, since meta-science, like science, evolves, and at different times people may have different conceptions of what it is to be rational. Rationality, he says, is equivalent to the ability to determine relevant questions and warranted answers, and hence "theory of truth presupposes theory of rationality which in turn presupposes our theory of the good."[6]

Putnam, however, sees the implications that some have drawn from this "internalist" thesis as disturbing, and wants to avoid them in forming his own position. The culprits are such "total relativists" as Kuhn (at times), Feyerabend, and Foucault.[7] Given that we "make the world", they conclude that truth is relative, and that there is no fact of the matter to which our statements correspond. Truth is only accuracy from a certain perspective, or within a certain paradigm, and so it is relative to backgrounds. Putnam argues that this total relativist thesis is self-refuting, for a relativist cannot plausibly claim that relativism is correct if she at the same time holds that all claims are totally relative; that we cannot evaluate any as being better than others.[8]

So Putnam argues for a compromise, which he identifies with a Peircean ideal limit theory of truth. "'Truth' ... is some sort of (idealized) rational acceptability - some sort of

ideal coherence of our beliefs with each other and with our experience *as those experiences are themselves represented in our belief system* - and not correspondence with mind-independent or discourse-independent 'states of affairs'".[9] Putnam, however, says virtually nothing about Peirce's theory, and his explication of his own "ideal" notion of truth does not seem to be very clear in its own right. Rather than attempting to work through Putnam's theory of truth and rationality in the short space allotted here, I will instead turn to Peirce's writings to see what kind of compromise Putnam's predecessor had in mind.

III.

Robert Almeder[10] claims that there are thirteen distinct interpretations of Peirce's theory of truth currently floating around in the secondary literature. This is some indication that Peirce, like Putnam, is not very transparent on this score. In what follows I will give a brief and very programatic sketch of my addition to this collection.

For Peirce, truth is that which would be the product of inquiry if inquiry were pursued indefinitely. The truth is what would be believed in the "final opinion" - the opinion which would be agreed upon by the community of investigators given the ultimate state of information. So truth is belief that is permanently settled - no further evidence, argumentation, or investigation could bring the matter back into doubt.[11] We might, and probably do, he says, already have permanently settled belief about many matters,[12] but we can never know which matters are so settled, and hence we can never know with certainty if any particular hypothesis is true. An apt paraphrase of Peirce's notion of truth is this: rational belief is the best belief given the available evidence, and true belief is the best belief given the available evidence when all the evidence is available. Since we can never know whether or not there is significant missing evidence for any

given belief, we can never know if we have a true belief. So truth, on this account, is not merely coherence with some body of knowledge. We may have good reason by our current canons of rationality to claim that a belief is settled, yet it may not be so. Peirce was a staunch fallibilist.

This theory of truth is thus far ambiguous with respect to the realism/anti-realism issue. In a sense, Peirce thinks that truth is a relational predicate. A true statement is one which corresponds to reality. He takes reality to be the object of true statements - that is, it is the object of statements which are made when all the evidence is in. Truth and reality are inseparable for Peirce, and so it might look as if his theory of truth is simply one of ideal correspondence. And since many correspondence theorists don't claim that we currently have the truth, but only that there is a fact of the matter which could be verified with sufficient investigation, they too have an ideal correspondence theory. Truth is correspondence (with an ultimate, mind-independent reality) under ideal conditions.

Although this is a common interpretation of Peirce, I think it misrepresents his position. The correspondence relation that Peirce argues for does not commit him to what is known as "the" correspondence theory of truth which is the core of realism. In fact, Peirce explicitly rejected this theory. Rather than ideal correspondence, his position is one of ideal coherence - the truth is that which *would* fit with our body of knowledge if we had all the evidence and argumentation available. The theory that is coherent under ideal conditions corresponds to reality, but this does not entail that reality is unique or mind-independent. Peirce does not rule out the possibility that the final opinion might coincide with, say, a Goodmanian one which characterizes the world as being many different "ways", and he does think reality is mind-dependent.

He takes the question to be: "Whether corresponding to our thoughts and sensations, and represented in some sense by them, there are realities, which are not only independent of the thought of you, and me, and any number of men, but which are absolutely independent of thought altogether." (7.336) The answer he gives is this: "The objective final opinion is independent of the thoughts of any particular men, but is not independent of thought *in general*". The footnote to this passage puts the point another way: the final opinion isn't a particular cognition in a particular mind at a particular time, although an individual opinion, if it is such that further investigation will not affect it, may chance to coincide with it. "The perversity or ignorance of mankind may make this thing or that to be held for true, for any number of generations, but it cannot affect what would be the result of sufficient experience and reasoning. And this it is which is meant by the final settled opinion. This therefore is no particular opinion but is entirely independent of what you, I, or any number of men may think about it; and therefore it directly satisfies the definition of reality". On Peirce's account, truth and reality are independent of minds only in a "here and now" sense. They are independent of any minds you might care to mention, but are dependent on a hypothetical body of minds. He says, "This theory of reality is instantly fatal to the idea of a thing in itself, - a thing existing independent of all relation of the mind's conception of it". (8.13)

So Peirce is not a metaphysical realist. But neither is he a total relativist. Although he holds that "if there were no thought, there would be no opinion, and therefore, no final opinion" (7.336), reality is, in a different sense, external to the mind. Reality is that which impinges upon us, and compels us to take note of it. It is paradigmatic of the second of Peirce's three categories; his ubiquitious classificatory

scheme. It is the element of compulsion - "the essence of truth lies in its resistance to being ignored". (2.139) Reality is the external cause of our beliefs, but all we can say of it is that "it is".

Peirce is not oblivious to the rather obvious tension here. On one characterization of reality, it depends on the beliefs of an ideal ultimate community of inquirers, and on another, it is the external cause of our beliefs. He says, "At first sight it seems no doubt a paradoxical statement that, »The object of final belief which exists only in consequence of the belief, should itself produce the belief.«" (7.340) But, he says, many predicates apply dispositionally. That is, predication can depend on hypothetical or dispositional conditions which may or may not obtain. The claim that an object is heavy, for example, is made in virtue of its disposition to resist when lifted, but its heaviness does not come into being at the moment it is first lifted - it was always heavy. Similarly, "x is real or true" holds solely in virtue of the disposition of some hypothetical ideal body of inquirers with perfect information to assert it, but if this "final community" never materializes (and Peirce doubts that it will), what would have been asserted is nevertheless what is true.

Perhaps the nature of Peirce's compromise between realism and idealism can be made clearer by looking at what he said about Kant: "The first step in Kant's thought ... is to recognize that all our knowledge is, and forever must be, relative to human experience and to the nature of the human mind ... Our knowledge of things in themselves is entirely *relative*, it is true; but all experience and all knowledge is knowledge of that which is, independently of being represented". (6.95) Peirce thinks that the only notion of reality and truth we can have is one in which they are relative to us; they are mind-dependent in the sense that the only way

to sensibly talk about them is in terms of what would be believed in the ultimate state of information. Nevertheless, he says, there must be an external cause of our beliefs.

So Peirce moves away from total relativism by arguing that truth and reality are in one sense mind dependent and in another, mind independent. Whether these two senses are compatible is still not clear, and an attempt at clarification will be an ambitious project. Although the coherence of Peirce's theory hinges on their compatibility, light is shed on the realism/anti-realism issue regardless of the ultimate success of Peirce's account. He has shown us that the two traditional theories of truth and reality do not exhaust the field, and has given us a suggestion regarding a possible compromise between them. I think Putnam is on the right track in attempting to dissolve this dichotomy, and I think he is right in taking the inspiration from Peirce, since Peirce's theory of truth seems to be one of the few alternatives to the standard theories. Moreover, it is a plausible one. It accounts for the fact that scientific "truths" are continually being revised - this phenomenon is to be expected, given that we are continually adding to our incomplete and imperfect body of information. It is fallibilistic - we cannot be certain with respect to any hypothesis, since we can't know if it is permanently settled. It coheres with our commonsense belief that something external causes our sensations and perceptions, and it connects us to the external world while avoiding the problematic thesis that our beliefs presently refer or correspond to specific things in that world. And finally, it allows us to evaluate hypotheses and theories according to our present standards of rationality with the promise that if we are pursuing the correct method of inquiry, we will get closer to the truth. In sum, while the realist will most likely think that Peirce's theory of truth and reality compromises

too much of the realist position, the anti-realist who balks at the standard offerings for theories of truth may well be advised to explore the Peircean avenue.

Balliol College, Oxford

Notes

I am grateful to Bernard Berofsky, Michael Kubara, and Maryann Ayim for commenting on an earlier draft of this paper.

1. Putnam's compromise, outlined in *Reason, Truth and History* (Cambridge University Press, 1981) will be discussed below. David Wiggins, in *Sameness and Substance* (Basil Blackwell, Oxford, 1980) wants to avoid the extreme positions of "any conceptual scheme goes" and the realist notion of a self-identifying object - the notion that things in the world have real natures that announce themselves to any receiving mind. And Dummett thinks that his view, whether "right or wrong for mathematics, (it) is available for other regions of reality as an alternative to the realist conception of the world ... we can abandon realism without falling into subjective idealism." (from "Truth", in *Truth and Other Enigmas*, Duckworth and Co., 1978, p. 18).
2. *Reason, Truth and History*, p. 49
3. The adjective "colourless" is due to Dummett, *Truth and Other Enigmas*, p. XXX, and p. 145.
4. *Reason, Truth and History*, p. 102.
5. *Reason, Truth and History*, p. 137.
6. *Reason, Truth and History*, p. 215.
7. Rorty can clearly be added to this list.

8. He also argues that total relativism, or "anarchy" is self-refuting because the Thesis of Incommensurability is self-refuting. See pp. 113 and 114.

9. *Reason, Truth and History*, p. 49.

10. Unpublished paper read at the C.S. Peirce Society meetings at the 10th World Congress of Philosophy, Montreal, 1983.

11. This fits with Peirce's characterization of inquiry as the passage from the unsatisfying state of doubt to the satisfying state of belief.

12. See 8.43. References to Peirce are to the *Collected Papers of Charles Sanders Peirce*, Harvard University Press. In citations, the first numeral refers to the volume number, and subsequent numerals to the paragraph number.

THE MODAL FEATURES OF KANTIAN EPISTEMOLOGY*

Márta Ujvári

The Anglo-Saxon interpretation of Kantian epistemology today seems to have two main strands: the standard Bennett-Strawson interpretation covering mainly the descriptive metaphysics of experience and Hintikka's interpretation covering the constructive metaphysics of pure sciences. The Kantian feature is present in the basic conception of quantified modal logic where the individuals "depend on our conceptual suppositions".[1] In fact, they cannot be described or individualized independently of these suppositions.

My view is that the Kantian attachment of the philosophy of quantified modal logic is grounded in the very nature of Kantian epistemology: namely, in its critical attitude which seeks to find an alternative to extensional, realist presentations of 'what there is'. And any deviation from the non-critical presentations of 'what there is' seems to lead now, in some form, to the metaphysics of modalities.

In this paper I analyze certain basic components of Kantian epistemology in modal terms, or, more precisely, in terms related to modalities, in order to show that the descriptive part based on a minimal empiricism and the constructive part make one coherent system. This will be done by elaborating, with the aid of modal terms, the idea that being dependent on our conceptual suppositions is not specific to the individuals of quantified modal logic. It also applies with the empirical individuals which depend on sensible *and* conceptual suppositions. Therefore the descriptive metaphysics of experience figures within a constructive Kantian framework.

*Paper presented to the 5th Joint International Conference on History and Philosophy of Science, Veszprém, Hungary, August 1984.

[1] See J. Hintikka: *Models for Modalities*. D. Reidel, Dordrecht, Holland, 1969.

The topics to be analyzed are: Kant's concept of intuition, his 'I think' as the expression for the original unity of apperception and his concept of 'possible experience'. Something will also be said of the relationship between Kantian epistemology and the theory of possible worlds.

Basically, Kantian epistemology is made up of the theory of sensibility and the theory of understanding.

The theory of sensibility must presuppose the relative independence of intuitions ('Anschauungen') as modes of knowledge. Otherwise Kant could demonstrate neither the empirical significance of the categories and other concepts nor the synthetic character of geometrical/mathematical knowledge. Because in order to show the empirical significance of the categories and other concepts we have to find intuitions corresponding to them; and in order to show the synthetic character of geometrical/mathematical knowledge, we have to refer to the procedure of constructing, in pure intuitions, the geometrical/mathematical objects - thereby proving their existence.

Intuition is a singular representation relating immediately to one object (A 320/B 377). So the two criteria of intuition are singularity and immediacy.[2] If immediacy is emphasized, intuition tends to be assimilated to perception. I think this would represent a loss of originality for Kant's theory. With the singularity criterion, however, the manifold of intuition is the manifold of one single item of experience, that is, of an individual. Hence intuition is not the manifold of representations but the representation of the manifold *of* a unity. This is because to represent something as an individual is already to ascribe the manifold to a unity. But this can only be the result of our intellectual spontaneity; therefore intuitions rendered by their manifold as pieces of receptivity belong, at the same time, to spontaneity.[3]

[2]This was clarified by Ch.D. Parsons. See his *Kant's Philosophy of Arithmetic*, In: *Philosophy, Science and Method*, ed.: S. Morgenbesser, P. Suppes, M. White. MacMillan, London 1969.

[3]This was argued by W. Sellars. See his *Science and Metaphysics. Variations on Kantian Themes*. Routledge and Kegan Paul, London 1968.

Now the question arises whether intuition as singular representation is in fact the individual the manifold of which is actually not found in experience but rather postulated by us. Another possibility is that intuition is a mode of representing an object different from itself in which case the relation between intuition and object is yet to be specified. In this respect, intuitions are systematically ambiguous: for example, space and time are individuals and they are also modes of representation for other individuals. This ambiguity can be explained by the fact that being an object *of* representation and being a mode *of* representation have, with the same grammatical form, the same underlying epistemic construction. That is to say, both are determined in reference to the mental component. More precisely, both are *intentional*.[4] The objects of intuition under the modes of sensibility are objects existing *for us*. Therefore 'object' is an elliptical expression in Kant for 'object *for us*'.

The intentional interpretation of intuition can be used to explain the famous Kantian puzzle just referred to. In this space and time are 'Dinge' as well as intuitions, that is 'Undinge'. The explanation here is that being an *individual* for us and being a *cognitive mode* of knowing individuals for us are combined, with systematical ambiguity, in the intentional concept of intuition.

Intentionality can be discussed in its connection to intensionality. For example, intensions as mental products can be said to be intentional in the sense of not existing independently of the intentions of the mind. However, intentionality does not imply intensionality and I think this is specifically true for the Kantian epistemology. I would say that here phenomenal objects/intuitions are intentional in the sense of being formulated under the modes in which they can exist for us, and these are always part of their description. Phenomenal

[4]Intuition is intentionally interpreted by Ch.D. Parsons, R.P. Wolff, W. Sellars.

objects/intuitions, though, are not intensions. The critical position expressed by intentionality does not imply the position of transcendental idealism. In the same way, existential quantifiers do not lose their objectual reference for their figuring within contexts of cognitive attitudes.

To paraphrase Quine, 'to be an object in intuition is to be singularly represented'. Whether this representation is perceptual or non-perceptual is irrelevant. In fact, it cannot be perceptual or otherwise Kant's concept of intuition which involves synthetic a priori knowledge could not be extended to the interpretation of mathematics.

The other criterion of intuition already mentioned is singularity. The singularity criterion intending to pick out one individual relates to the problem of the *verbalization* of intuition. The question is whether we can give a verbal equivalent expressing the content of intuition. I think no verbal equivalent can be given because concept-words capable of application to the description of many different individuals do not represent one determinate content of an intuition, hence, go *beyond* the content of intuition. Intuition seizes hold of the individual regardless of our knowing that other individuals can or cannot be seized hold of in this way. Intuition is part of the metaphysics of experience; and experience is such, according to the famous locution, that it can tell us only what there is but cannot tell us that it could not be otherwise. Hence the idea of 'could not be otherwise' must not be built in the concept of intuition. Hence intuitions must not be taken as verbal expressions which select their objects and specify their uniqueness at the same time. Intuitions must fulfil the condition of singularity but they cannot fulfil that of uniqueness.[5] In this respect the limit of intuition is the limit of experience: the incapability of ruling out, on a priori grounds, the possibility of 'anders-sein'.

[5] This was pointed out by Manley Thompson. See his "Singular Terms and Intuitions in Kant's Epistemology" *The Review of Metaphysics* 1972, Vol. XXVI 314-348.

To sum up: intuitions are intentional and contain the conditions of individuals to be individuals for us. In fact, individuals can be presented as individuals only within the context of intuition.

Let us now look at the modal feature of Kant's theory of understanding. Here, instead of the dependence of the individuals on the conditions of sensibility, we find the dependence of all our knowledge claims on the 'original unity of apperception' expressed by 'I think'. Kant argues that to produce synthetic unity in concepts of objects, to conceptualize the manifold of experience, say, into properties and individuals, is possible only through the mechanism of transcendental connection. Therefore the original unity of apperception which fulfils this task is something more for Kant than a mere necessary component of mental occurrences. For him the necessary 'concept-carried connectedness of experience' (Strawson) presupposes, and is presupposed by, the application of 'I think' because it expresses a necessary unity and the consciousness of such unity without which no knowledge claim could be formulated. Actually, Kant connects this function of 'I think' with the deduction of a *given* categorial scheme for which he allowed no alternative. Now this connection of 'I think' with the deduction of a given and exclusive categorial scheme is a rather dubious step which I do not propose to discuss here. However, to assign the proper modal status of 'I think', I shall rely on its analytical interpretation. According to this, 'I think' is an analytical component of the statements expressing the 'necessary self-reflexiveness' (Strawson) and the unity of experiences and thoughts. *In this sense* it is the condition of knowledge claims in general. It is, I would say, the *implicate propositional attitude* which effects also in contexts traditionally called 'extensional'; for even such contexts, insofar as they represent pieces of mental unity, presuppose the idea of such unity which is located by Kant in 'I think'. This being the case, 'I think' is actually a *propositional attitude constant*. Now, for a phrase to be a propositional attitude constant might sound trivial from a modal point of view; but as a

feature of Kantian epistemology, 'I think' is part of the transcendental mechanism expressing the dependence of the items of knowledge on the idea of unity and self-reflexivity. This propositional attitude constant effects the concept-carried connectedness of experience. However, no special way of concept-carried connectedness and no determinate mode of conceptualization of experience can be grounded in it.

I shall now move on to a short analysis of the Kantian concept of 'possible experience'. It is generally accepted that allowance for possible states or possible worlds does not imply allowance for possible individuals. How does Kant see this? According to his famous thesis, the a priori conditions of possible experience in general are, at the same time, the conditions of the possibility of the objects of experience. That is, to be an object of experience is to belong necessarily under the a priori conditions of experience.

It must be clear that possible experience is not the experience of possible objects. First because Kant knew that there can be no difference made between the descriptions of possible and actual individuals. This is apparent from his criticism of the ontological argument. Second, because the logical type of 'possible experience' is not the 'possible something': the latter is an *object-construction*, the former is a hidden *propositional construction* being an elliptical expression for a set of statements yielding the conditions of experience. As a contrast to 'possible experience', we can represent to ourselves a chaotic state of the world. In contrast to 'possible objects', we can represent to ourselves nothing, as is actually the case with 'Dinge an sich'.

To conclude, in Kant's theory of experience we do not have possible objects. However, we do not have possible worlds here either because, as was stated previously, Kant rules out alternative conceptualizations of experience. This being the case, what then is the special sense in which Kant uses the term 'possible'? The Kantian sense of 'possible' is something not generally found in other epistemologies but quite usual in the constructive interpretation of pure sciences: namely,

what is *possible* in experience agrees *necessarily* with the a priori conditions of experience. Therefore possible experience has, for the objects of experience, the character of conceptual necessity and not that of empirical contingency. In short, possibility expresses, in the Kantian metaphysics of experience, a strong condition and not a loose admittance.

Finally, let me say something about the relationship between Kantian epistemology and the theory of possible worlds.[6] The greatest contribution of the former to the latter is the individual's dependence on sensible/conceptual conditions. This dependence is a transcendental supposition on the part of Kant determining the theories of sensibility and understanding without having, however, a direct effect on the description of the individuals. This stems from the Kantian lack of alternative descriptions, the alternative ways of identification already referred to. Therefore 'possible world' differs from 'possible experience': the latter has no genuine alternative, while the former rests precisely on the supposition that alternatives exist. Still, there is a Kantian feature in the theory of possible worlds: when reference is explicated within the context of conceptual suppositions or propositional attitudes then this treatment resembles Kant's treatment of the existence of empirical individuals. Kant says that the existence of empirical individuals can be known only in experience (he would therefore not accept substitutional quantification). However, he also says that empirical knowledge of these individuals must necessarily conform to the conditions of sensibility and that any existential claim can actually be formulated only in terms of these conditions (that is, objectual quantification has a transcendental dependence). The same line of thought was traced in the Kantian concept of intuition: to be an *individual for us* depends on the *cognitive mode* of knowing, in intuition, individuals for us.

Technical University, Budapest

[6]Here we need not consider any specific version of the theory of possible worlds only the common underlying thought.

THE ROLE AND TRANSFORMATION OF SCIENTIFIC ACTIVITY IN HUNGARY TOWARDS THE END OF THE 19th CENTURY*

Éva Katalin Vámos

Opinions on the possible roles and possibilities for Hungarian science in the academic and cultural life of the country at the end of the 19th century are decisively influenced by the following factors:

a) The state of development of scientific and cultural institutions, together with the level of their equipment; financial assistance for these institutions coming from public funds and the studies which determined the scale of this assistance. These studies took into account the demands made on science by agricultural and industrial development.

b) A basic function was given to the actual problems of a branch of science which engaged experts and also to the question: how quickly and with what effect did these problems find attention in Hungary?

c) A significant role was played by the interest of the Hungarian public in the science pursued at home and its opinion of studies conducted in Hungary. Also important is the position achieved in society by scientific institutions.

These factors together determined the orientation of Hungarian scientists, i.e. whether they embraced contemporary science by outlining, understanding, utilizing and transmitting it, or whether they presumed to set a new course for the development of science by means of their findings.

In the decades after the failure of the War of Independence (1848-49), the scientific and cultural activity in Hungary either ceased or was very limited. The first open general

*Paper presented to the 5th Joint International Conference on History and Philosophy of Science, Veszprém, Hungary, August 1984.

session of the Academy of Sciences was held only in 1858, whereas the Congresses of Hungarian Medical Practitioners and Natural Philosophers were not organized at all between 1847 and 1863. The Society of Natural Science, which later became the main forum for the popularization of science, was also dormant for 15 years, a period, however, during which much was discovered. Kálmán Szily aptly summed up these years in 1880. In his speech to mark the 40th anniversary of the Society he said: "Experts consulted and wrote to experts. In this way the Society fulfilled the role of the Academy in serving natural scientists at a time when the Academy's possibilities were limited". After the experimental and preparatory period of the 1860's, the last three decades of the 19th century were characterized by a stir in the nation's scientific and cultural life.

a) The development of scientific and cultural institutions

The Hungarian Academy of Sciences was given new statutes in 1869 and these made possible its independent activity. Under the terms of these new statutes, the Academy had great autonomy: it could choose half of its governing body while the other half was appointed. The purposes of the Academy were not just to promote the Hungarian language; the Academy was also intended to be the effective organizer of scientific study. Among its aims, the following is found: the Academy should foster "the cultivation and diffusion of science and literature in the Hungarian language". In order to achieve this purpose, its members undertook supported studies and journeys, wrote original works, translated important authors and announced scientific and literary competitions. The Academy disseminated knowledge through its own library, printing operations, translations and lectures and, "as far as possible", helped the outstanding researchers to continue their activity. The Academy was divided into departments: the Department of Linguistic and Literary Science, the Department of Philosophical, Social and Historical Sciences, and the Department of Mathematics and Natural Science.

Two institutions were given the rank of universities. One was the former József Polytechnic School which was reorganized as a university entitled "József Technical University" in 1871. This was the first such institution in the world to have the word "university" in its name.

The József Technical University trained mechanical engineers, architects and chemists but, in the 19th century, a doctor's degree could not be obtained there.

The other new university was the University of Kolozsvár. This was established in 1872, with the incorporation of two existing institutions of higher education, a law school and a school of doctors and surgeons. Significantly perhaps, it did not possess a faculty of theology.

During the time that Ágoston Trefort (1817-1888) was Minister of Education and Cults (1872-1888), the establishment of a third university was planned. However, although this scheme came to nothing, existing universities acquired additional premises. The Technical University was given its building in Museum Boulevard, and the University of Arts and Sciences gained its Library, its Faculty of Medicine and the clinics in Üllői Street. Independent departments were established for chemistry, geology, mineralogy, mathematics, physics, anthropology, zoology, botany and astronomy. A great number of natural science associations which continue their work even today were established at this time. These included the following (year of foundation given in brackets):

Forestry Association (1866)
Hungarian Geographical Association (1872)
Hungarian Association of Dentists (1878)
"Loránd Eötvös" Physics Society (1891)
"János Bolyai" Mathematics Society (1891)
Hungarian Association of Gynaecologists (1896)
Electricity Technology Association (1900)

Several societies, mainly medical, were also established but which have since ceased to exist. These include the National Public Health Association (1885), the Society of Balneology

(1810) and the National Federation of Physicians (1897).

b) The development of science in the world at large and scientific studies in Hungary

Public expectations of science and the attitude of the scientific community were (to use a popular saying of the time) that "We Hungarians should be among the first". The quick presentation of a new scientific finding or the rapid duplication of the basic experiments in Hungary were seen as national achievements. For example, William Conrad Röntgen (1845-1923) demonstrated his discovery in 1895 and, after some months, the Természettudományi Közlöny (Natural Science Bulletin) publicised it in Hungary. As X-rays became known the world over, it may be interesting to see when news of other important discoveries reached Hungary. For example, Heinrich R. Hertz (1857-1894) proved that the electromagnetic waves discovered by Maxwell did, in fact, exist and that they behaved in exactly the same way as light. His work on "The Connection of Light and Electromagnetism" was published in Hungary in 1891. Without doubt, the greatest interest was in medical discoveries. The Pasteur Institute started work in Paris in 1888. In Budapest a similar institute was founded two years later by Endre Hőgyes (1847-1906). Hőgyes was a professor of pathology and pharmacology, and a member of the Hungarian Academy of Sciences.

Translations which introduced to the country scientific findings worldwide were highly thought of at the end of the 19th century.

János Hunfalvy described the task of the Royal Society of Natural Science: "... it should be desirable and advisable if the Society gradually translated all works on the natural sciences into our language, in order to put them among the writings at our disposal". The Society also founded a publishing enterprise (1872) which, in the beginning especially, aimed to bring out these translations. Very famous Hungarian scientists translated the most well-known of these foreign works. For example: the first part of Hermann von Helmholtz's "Populare

wissenschaftliche Vorträge" (1865-1875) was translated in 1874 by Baron Lorand Eötvös and Jenő Jendrassik. Baron Eötvös (1848-1919) was a physicist, university professor, government minister and a member of the Hungarian Academy of Sciences. Jenő Jendrassik (1824-1891) was a physiologist, university professor and also a member of the Academy.

Although the prominent scientists did not regard translation to be beneath their dignity, this was an area where women interested in science could quickly play a part. Elisée Reclus' (1830-1905) "Histoire d'un ruisseau" (1868) was translated by Elma Hentaller in 1894. Among the translations of Sarolta Geőcze (1862-1928), we can find the same author's work, "The History of Mountains", published in 1881. Regard for translation is shown by József Szinnyei who, in his 14-volume work "The Life and Works of Hungarian Writers", (he considers 'Hungarian Writers' to be those writing anything in the fields of literature, science or journalism in Hungary up to the 1900's),lists all translations made by them. The bibliography of the "Scientific Reports" over the years 1841-1890 also lists the articles of authors contributing, and also the authors whose works they translated.

The importance of translation in Hungary at that time did not only lie in the fact that it made accessible fundamental and up-to-date scientific works to Hungarian experts. These works were already accessible in German, as the language of education was German until 1860 (except for 1848-49). The greater part of the middle class also spoke German. However, an important aspect of translation was to develop the special terminology in the Hungarian language of the natural sciences and technology. As these translated works contained the knowledge of "the more developed foreign lands", the public got to know them too. A cartoon in the satirical magazine "Borsszem Jankó" suggests something which cannot be found in official documents. The cartoon shows a famous scientist who is charged with a translation. The work is subcontracted from person to person until finally the village teacher does it. How true this was cannot be checked today.

Just as translations were seen as national achievements, the purchase of foreign, up-to-date equipment was in the same way regarded as a worthy exploit by Hungarians. For example, at the international exhibition in Paris in 1900, hundreds of Hungarian items presented. However, among them, there were quite a number described like this: "The Southern Railway exhibits its most modern points bought from a German firm". The scientific institutions were also very proud of their modern, foreign instruments. For example, the Institute of Physics at Kolozsvár University considered its Siemens' electric rheograph to be worth giving a lecture on. This was organized by the Transylvanian Museum Society.

c) Interested Hungarian laymen and Hungarian science

Hungarians interested in science in the country could only develop their opinion on scientific findings if they were informed clearly about the latest studies. It was characteristic of their way of thinking (which was influenced by a late positivism and a reviving national self-consciousness) that most prominent scientists regularly popularized their subject in lectures or wrote educational books. They thought that the popularization of science was work in the service of the public and therefore, without doubt, worthy of the scientist. The scientific institutions which published and reviewed the best scientific works wished to inform the public about their findings. Accordingly, some of their lectures and meetings were open to the public.

The greater of this popularizing work was done by the Royal Hungarian Society of Natural Science. Scientific evenings were organized from 1866 onwards. The host at these evenings was initially Károly Than (1834-1908), professor of chemistry, vice-president of the Society of Natural Science from 1862 and president between 1871 and 1879. At these gatherings, wide topics were covered. The lectures were often given by such excellent scientists as, for example, Frigyes Korányi (1827-1913), medical specialist, professor, and member of the Acad-

emy; he gave a lecture in the spring of 1880 entitled "The Methods of Recent Pathology and Therapy". Ferenc Pulszky (1814-1897), politician, archeologist, and member of the Academy also, spoke "On the Findings of Palaeoarchaeology and on Some Questions of Palaeoarchaeology". The lectures were published in a series entitled "Miscellaneous Booklets".

As these evenings introduced in 1866 had been a success, Károly Than invited the management of the Society of Natural Science to dinner in 1868. There it was decided to put the propagation of general knowledge into the forefront of the Society's activities. In 1869, the new articles of the Society said: "the task of the Society is to study the natural sciences in general, to investigate Hungary from this point of view and to propagate general knowledge". The Bulletin of Natural Science was started in the same year, while in 1872 the Natural Science Publishing Enterprise was founded. The country in general approved of the Society's work. This was proved by the fact that, in 1870 the Hungarian Parliament voted Ft 4000 to the Society "for research and publications in the national interest". This sum was voted annually throughout the whole period.

The results of these changes were summarized by Kálmán Szily on the 40th anniversary of the Society in 1881. He said: "We have learnt, and we shouldn't forget this, that the Society, near to the National Museum and the Academy of Sciences here in Budapest, derives its right to exist only by its propagation and popularization of science. This helped us to acquire public trust, and while we have this, we shall always have the possibility to study sciences."

In this period a lot of tasks were set for the scientists. They were expected to carry on independent research work, to teach at the universities, to be informed and to inform about scientific developments abroad and to propagate scientific knowledge. It may be interesting to see how the scientists reacted to these expectations.

At the beginning of the period, outstanding scientists tried to fulfil their tasks. They threw themselves whole-

heartedly into the establishment and reforming of scientific institutes and tried to raise the standard of special publications by contributing to them. Their writings are packed with the usual expressions of the period: "for the public benefit", "for the nation's prosperity", "In the great competition among the nations, we Hungarians should...". An example of an 'oeuvre' in the service of the nation is the work of Lajos Ilosvay (1851-1936). His name became famous in chemistry as a result of the Griess--Ilosvay reagent, with which one part of nitric acid can be detected in 352 million. His fields of interest were, among others, the examination of carbonyl sulphide and similar compounds, the analysis of gas for lighting purposes and the atmosphere in the famous Büdös-barlang (the "Stinking Cave") in Torda etc. In addition, he was a professor at the Technical University after 1883 and Rector of the same university from 1901 to 1903. Ilosvay was also active in the Society of Natural Science, serving as its secretary and president. He was a member of the Hungarian Academy of Sciences and later its Vice-President. Ilosvay started the Hungarian Chemistry Bulletin in 1897 and edited it. Later he was the editor of "Publications in Natural Science". After 1900, he was an MP and Under-Secretary at the Ministry of Education and Cults. Not only had he these titles but he also performed the duties attendant on them. In spite of all this, though, Ilosvay realized bitterly that a scientist is kept from his study if he tries to meet public demands. He writes: "In Hungary it is not easy to be a creative scientist and an untiring professor. In Hungary a professor is the expert and advisor of different associations, authorities and scientific societies." Elsewhere he also complains: "We teach and teach again, administer, organize societies which propagate science or practice charity, sit day after day on committees, and, if we happen to find a minute or two, can make sorties to our special field of interest".

Until the end of the last century scientific institutions continued to be founded and most of them were then stabilized in their present form. The relatively well-equipped labora-

tories in the new buildings made high-level research work possible. The enthusiastic statements on the role of science in the nation's prosperity became transformed into slogans. This is why the majority of scientists who were given professorships at the turn of the century did not endeavour to raise the standard of Hungarian journals with their findings, or to translate specialized foreign texts in order to present them to the public at home. They prefered to investigate the major problems of their field, to introduce their work abroad and to publish their results in important international periodicals. An example of this approach can be seen in the case of Lajos Winkler.

Lajos Winkler (1863-1939) was the professor of chemistry at the University of Budapest and a member of the Academy since 1896. His special field was analytical chemistry and pharmacology. His main topics were precision gravimetry, gas analysis, and the examination of water and medicines. His method to determine oxygen dissolved in water became famous as "Winkler's Iodimetric Determination". He did not take part regularly in the work of scientific associations. His name sometimes appears among those of editors of periodicals, but again not regularly. He consciously put aside time for research. Every day, he went to the university at noon and gave lectures from 2 to 3 o' clock. From 3 to 7 o' clock he completed his work in the institution and worked in his laboratory from 6 in the evening until 6 the next morning. Winkler's opinion was: "Man cannot work during daylight, he is always vexed then". He was obviously thought to be a little whimsical as a result of this way of life. Once he told his assistant: "I am not so foolish as I appear to be, or rather, I am not foolish at all, I only pretend to be - otherwise I wouldn't be left alone." This approach resulted in 242 pieces of work, 150 of them in German. All were published in leading periodicals.

To summarize, one can say that expectations and public opinion stimulated the scientific community to continue its studies, to keep largely in step with international develop-

ments in science, and to exchange views with scientists abroad. But this international contact did not only result in translation of foreign scientific works and the reproduction of experiments. In the last decade of the 19th century, high-level research work was also possible in the up-to-date institutions existing in Hungary.

National Museum of Technology,
Budapest

ATOMIC SHAPES AND ELEMENTARY TRIANGLES IN PLATO'S *TIMAEUS*

István M. Bodnár

The following pages are conceived as yet another contribution to the vexed issue of the structure of matter in Plato. The points I wish to make are, I believe, extremely simple ones and can be made very briefly. However, first let me summarize the ways in which this problem is treated in the various quarters of classical scholarship. This will be helpful to my examination of the issue and will, hopefully, give some insight into the scholarly debate conducted during this century.[1]

1

Before Friedländer the problem itself did not exist. Plato's text was thought to be fairly straightforward on all questions: different elements have different elementary structures - tetrahedra are fire, octahedra air, icosahedra water and cubes are earth. As the domain of physical science is open to constant uncertainty, this will not mean absolute uniformity. Plato's asides about different sizes within the same elementary structure (57d-59a) and his description of intermediate states, of *skotos* (darkness) and *homikhlê* (fog, 57d)[2] could be accomodated in this overall framework.

Friendländer, however, insisted that any consistent theory has to give a sound theoretical base for the existence of different sizes of the same element, which using a modern analogy he termed isotopes.[3] He and Professor Cornford, who later developed his own solution in a Friedländerian vein, proposed a theory of matter in Plato which observes these requirements. More interestingly, Cornford formulated his views so that they

happened to elucidate a very strange peculiarity in Plato's construction, hitherto relegated to a sheer curiosity.[4] However, to explain this I have to outline Plato's overall account.

In order to construct Plato's regular solids all we need is the geometrical shapes which constitute their faces, i.e. squares and regular triangles. Squares can be divided simply into two isosceles right-angled triangles. Then instead of a regular triangle, Plato resorts to a half of it as elementary, i.e. to the triangle of 30°, 60°, and 30°. Two such triangles make a square and an equilateral triangle respectively. Nevertheless when Plato, just after these considerations, gives the method for constructing the regular shapes, he uses a different approach (Fig. 1):

Square Triangle

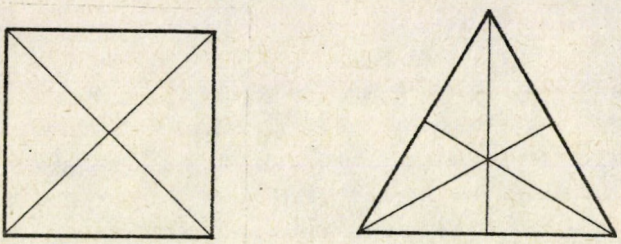
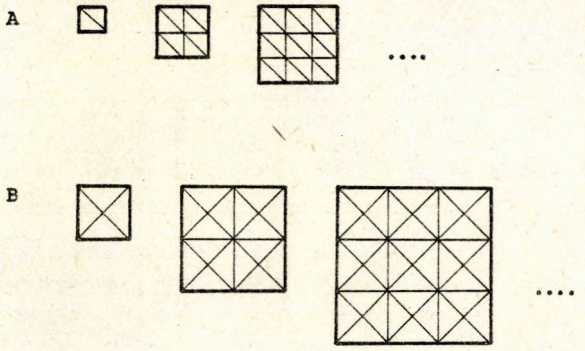


Fig. 1.

The elementary triangles are not only halves of the regular surfaces, as appears in their definition; from 4 or 6 of them respectively another regular element can be constructed.

The view is that Plato needs these two mutually irreducible basic constructions so as to be able to generate all the more varieties of matter, all the more isotopes. Simple regular surfaces can be combined into ever greater clusters, and if we have two starting points, we will have twice as many derivative surfaces in the two chains (Fig. 2):[5]

Squares



Triangles

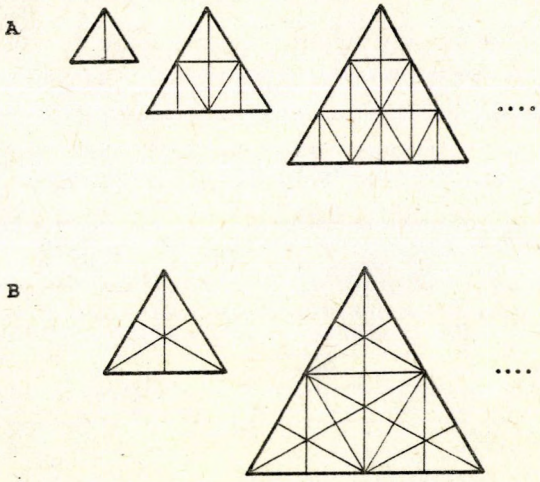


Fig. 2.

Had Plato blatantly opted for the regular surfaces themselves he would have acquired only one of these rows and would have been unable to account for all the variegated phenomena in the material world. Plato shows that he is fully aware of all this by choosing to give the construction of squares from 4 and triangles from 6 elementary triangles: "He chose to describe solids of a larger grade [i.e. not the minimal ones where the faces are from 2 elementary triangles] because he wanted to suggest that there are in fact several grades, ..."[6]. Cornford, in formulating his creed, confesses its extreme weakness: his 'wanted to suggest' clause tries to conceal and discredit the fact that nowhere in the *Timaeus* or in any other dialogue of his did Plato ever mention Cornford's grades. Moreover, Cornford's conjecture is bad mathematics.[7] As both chains [A] and [B] grow rapidly: sizes increase linearly, and areas quadratically, the density of the compound chain is not much greater than that of, say, chain [A]. And as M. Pohle pointed out in the paper referred to in note 7 we have no reason to suppose that Plato had to accept for his *minima* a size near the threshold of visibility. He might have chosen an extremely small atomic triangle and produce as many isotopes as he wished within a single chain.

Although this refutes Friedländer, Cornford and Vlastos, quests cannot be eliminated from the pursuit of knowledge. The particular theory may be discredited, but it leaves problems behind to all subsequent theoreticians, problems which were created first in the context of that particular theory. Here the question is why exactly did Plato construct the square out of 4 atomic triangles and the triangle out of 6.

2

Sir Karl Popper, though not criticizing Cornford's position explicitly, gives another answer to Friedländer's questions in his *Open Society* and *Conjectures and Refutations*.[8] According

to Popper, Plato, learning about irrationality in his later days was converted to the idea of irrationals and considered them so fundamental that he made an image of $\sqrt{2}$ and $\sqrt{3}$ the principle of his theory of physics. Figure 1 contains $\sqrt{2}$ and $\sqrt{3}$ in a perspicuous way. (Cf. the dialogue Meno, in which Socrates speaks about doubling the square with the same figure at hand.)

But Plato gives no hints that this would be the motive behind his constructions. He gives the equations for the triangles before the description of the constructions themselves. And if we take Plato at his word in the *Laws*, irrationality was by no means so trivial at that time as to leave it in a text cryptically and without further elucidation.

Popper's conjecture remains, then, without any textual foundation and apart from subjective factors, from a mere pseudo-religious reverence and awe for irrationality, it is incomprehensible why Plato should put the problem of irrationals into the core of theoretical physics.

3

Cornford dismissed a suggestion by Cook-Wilson and Tayler that the complicated construction of regular ploygons was needed to obtain "an absolute symmetry" within them.[9] Though the wording is not particularly precise, this is the most likely case, as I will show.

First let me state what the "absolute symmetry" amounts to. A number of lines of symmetry can be drawn on both figures:

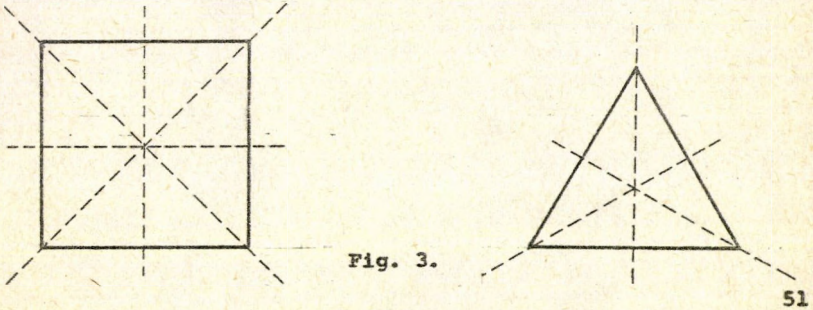


Fig. 3.

A common feature of the two figures can be described without recourse to the actual symmetry relationships as follows: in the case of both shapes, the square and the triangle have the same number of atomic triangles in each of their vertices. This would not be true in the case of the simple figures:

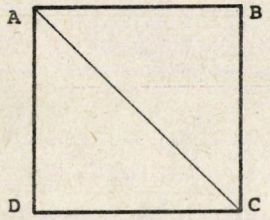


Fig. 4.

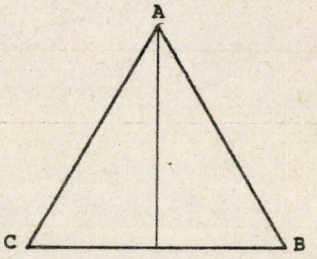


Fig. 5.

In Fig. 4 there are two atomic triangles at A and C and only one at B and D, in Fig. 5 there are two triangles at A and only one at B and C.

Cornford, in his objection, asks why symmetry is important here.[10] I can only guess at the answer to this. Symmetry may be important for somebody who is going to construct the physical world out of regular solids, i.e. solids which are "absolutely symmetrical" and so have the same number of faces at each vertex of them. Using faces which satisfy this criterion and which have the same number of atomic triangles at every vertex corroborates the regularity of the polyhedra: otherwise a cube would have 12 atomic triangles on its 6 faces, each of them reaches three vertices of the cube so the average number of triangles per vertex is $\frac{36}{8} = 4{,}5$, so it is simply impossible to have a construction where all vertices have the same number of atomic triangles.

Similar results may be obtained for octahedra and icosahedra. The octahedron would consist of 16 elementary triangles,

each of them falling to two vertices. The average number of triangles per vertex is $\frac{32}{6} = 5\frac{1}{3}$. In icosahedra the number of elementary triangles would be 40, each again at two vertices. The average number of triangles per vertex would then be $\frac{80}{12} = 6\frac{2}{3}$

With tetrahedra this is rather different. The resulting average is an integer, i.e. $\frac{16}{4} = 4$, and an appropriate construction is possible (Fig. 6):

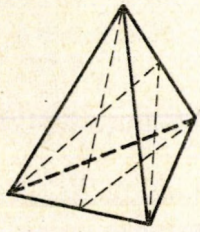


Fig. 6.

I would hesitate to call this construction symmetrical, and indeed the criterion for symmetricity for solids can be more closely defined to exclude this.[11] For example we can stipulate that all the faces should attach to a vertex with the same number of atomic triangles. This stipulation then simply entails that the faces themselves should have the same property too. But there is no need to use this stronger criterion as with the other three shapes a weaker principle suffices and accounts for the complicated construction of the faces from atomic triangles.

I have to admit that the solution I outlined above is conjecture only. Plato deliberately speaks in riddles here, and we therefore cannot put our finger on the real motive behind his construction. I merely propound this thesis as the best of those occurring to me, and give its consistency as the reason why I chose it in preference to the others. But as my position returns to a pre-Friedländerian panel, I have to deploy a final *caveat* against his considerations about the isotopes of the different elements.

Friedländer did more than just raise an interesting problem. He made a question of extra-theoretical curiosity the integral part of all subsequent doctrines about Plato's theory of matter. Now as I reject (at least some elements of) the A-series, there are two possibilities open before me if I wish to account for the different isotopes:

[1] We can mobilize the remaining A-series to provide for the different substructures,

or

[2] we can point out that Plato nowhere in the *Timaeus* mentions that his elementary triangles would be uniform in size. If they occur in a large variety, a single constructional type is enough to produce the different natural phenomena. Remember that Plato, though seeking uniformities in Nature, is fundamentally suspicious of her consistency, and so there is only *eikôs logos* about natural processes. This attitude is revealed at a place that can be in a way decisive for Plato's theory of matter.

Though in the first part of the *Timaeus* we learn that all shapes are composed out of two precisely defined triangles, when Timaeus proceeds to the description of the generation of animals, to the production of marrow, he says:

"The god set apart from their several kinds those triangles which, being unwarped and smooth, were originally able to produce fire, water, air, and earth of the most exact form." 73)[12]

Even if Plato had been committed to the A-series, such imperfect material as this sentence suggests would have prevented him from going too far: at some point these jumbled pieces of matter will simply not fit together. As Plato undoubtedly points at differences of size for the different varieties of matter, and the crippled building blocks of Nature seem to be unable to combine extensively, there can be only one way out. Different sizes of the same bricks have to be available.

Loránd Eötvös University
Budapest

Notes

1. The following books and articles will be referred to only by the name of the author and page:

   Cornford, F.M. *Plato's Cosmology*, RKP, London (First published 1937, I use the reprint of 1977)

   Friedländer, P. *Plato*, Princeton, UP, Princeton, N.J., 1958

   Pohle, W. "The Mathematical Foundations of Plato's Atomic Physics", *Isis* 82 (1971) 36-46

   Popper, K. *Conjectures and Refutations*, RKP, London, 1963

   Taylor, A.E. *A Commentary on Plato's Timaeus.*, Clarendon, Oxford, 1928

   Vlastos, G. *Plato's Universe*, Clarendon, Oxford, 1975

   Plato is quoted by the usual Stephanus numbers.

2. The same problem is reproduced in Timaeus Locrus 42: hekaston de tôn tetorôn sômatôn polla eidea ekhei ... dia tân anisotâta tôn en hekastôi autôn trigônôn. (Edited by W.

Marg, E.J. Brill, Leiden 1972)

3. Vol.i., 255.

4. Cornford 215-9, 230-239.

5. Cornford's and Vlastos' notation is somewhat different here. Cf. Cornford 235-9, Vlastos 77 and 79.

6. Cornford 234. Cornford sees a suggestion in the text that 'when these larger solids are broken down into elements, those elements can be recombined in several ways'. This is not in the text, and Plato's apodictic tone while counting the elementary triangles of the different solids suggests just the opposite, i.e. that there is only one way to decompose a regular solid into its constitutive triangles.

7. Pohle 42-4.

8. Popper 88-90.

9. Taylor 374.

10. Cornford 217n2.

11. We can apply the same principle to the edges of the solids and postulate that these edges should contain the same number of elementary triangles. The results of the computations are respectively:

cube	$\frac{6 \times 2}{12} = 1$
tetrahedron	$\frac{4 \times 2}{6} = \frac{4}{3}$
octahedron	$\frac{8 \times 2}{12} = \frac{4}{3}$
icosahedron	$\frac{20 \times 2}{30} = \frac{4}{3}$

There is no figure that would pass both tests. (I owe this point to Mr. Sándor Ónodi.)

12. Cf. 82d. The condition of the triangles is a major factor in aging: while these triangles are new and strong, we are young, and oldness comes when they become weak (81b-c). Friedländer, P. "Structure and Destruction of the Atom according to Plato's Timaeus", *University of California Publications in Philosophy*, 1949, 236 mentions these texts and tries to explain them away, but his solution is not convincing.

THE RISE AND FALL OF CRUCIAL EXPERIMENTS

Márta Fehér

One and perhaps the most famous - or notorious, if you prefer - of Lakatos' last papers appeared in 1974, the year of his death. This dealt with the role of crucial experiments in science. In the paper, Lakatos puts the question whether we learn more about our theories from some experiments than from others. Crucial experiments are defined here as being those which, given n rival theories, refute $n-1$ of them and thereby prove the n-th. After denying the proving power of such experiments his next question is:

> Can we then at least learn from experiment that some theories are false?
>
> [Lakatos, 1974, p.310]

His answer is again a definite 'no'. He declares that:

> We cannot learn from experience the falsehood of any theory.
>
> [Lakatos, ibid.]

Note here how much stronger this answer is than that required by the original question. (It is stated within the first 2 pages of the paper at issue.) Lakatos began from the problem concerning crucial *experiments*, i.e. whether they have a special status or not, and ended in denying the falsificatory role of even *experience* in general. (In this way, Lakatos went beyond the mere rejection of experiments as yielding a special kind of experience).

Now, this indeed is a most radical and unexpected turn for a former Popperian to take.[1] For the Popperian, the most basic methodological norm should be the principle of empirical falsification, the conviction that, although we cannot confirm our theories, we can at least conclusively disconfirm them by

means of empirical evidence (by simple or experimentally achieved experience). Lakatos's standpoint is (as he writes in his *Rejoinder to Prof. Grünbaum's Criticism*)

> If crucial experiments are to provide experimental *disproof*, there can be no crucial experiments.
>
> [Lakatos, 1978. Vol.1. p.211]

He goes on to add that in fact:

> There have been no crucial experiments in science.
>
> [Ibid., subtitle]

While the latter is an *a posteriori*, factual, historical thesis, the former is an *a priori*, logical-epistemological one. By these Lakatos denies that there can be and that there has ever been a conclusive experimental disproof of theories, by experimentum crucis. As he says:

> Where a falsificationist sees a crucial negative experiment, I 'predict' that there was none. I predict that behind any alleged single fatal duel between theory and experiment one will find as a matter of fact a complex war of attrition between two research programmes during which one may establish what the relative strength (i.e. imaginative resources and empirical luck) of the two armies were at any given time.
>
> [Lakatos, 1978, Vol.1. p.212]

As is to be seen - from the above quotation as well as from the whole of Lakatos's 1974 argument - the thing he so firmly and expressedly denies is that crucial experiments could and did play any role in the decisions between two rival research programmes (i.e. series of theories) and not simply between two rival theories. By this he shifted the focus of consideration from the decision between single rival theories or hypotheses to the decision between whole series of theories. In his view, there are no single isolated scientific theories, only series of them, connected by a common hard core. It is the members of such series over which he denies that crucial experiments or experience in general could have any falsifying force.

This Lakatosian nadir in the esteem of the methodological value of crucial experiments follows upon a former zenith during the heyday of logical positivism and classical Popperianism when the possibility of conclusive empirical confirmation or disconfirmation was firmly believed. This zenith itself followed upon an earlier ebb in Duhem's philosophy of science, formulated and modified between 1892 and 1916 and put forward mainly in his (1906). Duhem's standpoint was that experiments (whether crucial or not) cannot be taken to play a *conclusive* role in the assessment of scientific theories (either in their confirmation or falsification). His argument for this view is the so-called Duhem thesis[2] which he elaborated first as a criticism of the Newtonian type of induction[3] and according to which:

> The physicist can never submit an isolated (theoretical) hypothesis to the controls of experiment, but only a whole set of hypotheses; when the experiment disagrees with the prediction, he learns that at least one of the hypotheses that constitute the set is erroneous and must be modified.
>
> [Duhem, 1964, p.187]

The argument which Duhem puts forward for this thesis rests on the distinction he makes between simple, direct observations, i.e. experiences and experiments performed by instruments, construed on their part by means of other theories (hypotheses) while the results they yield are and must be interpreted again by another set of hypotheses. This whole network of hypotheses is then involved in any experimental test:

> ... a physics experiment is not simply the observation of a phenomenon...

writes Duhem,

> It is... accompanied by the interpretation of these phenomena; this interpretation substitutes for the concrete given... some abstract and symbolic representations which correspond to the given by virtue of the physical theories admitted by the observer.
>
> [Duhem, ibid.]

What Duhem denies then, on the ground of the inseparability thesis, or, if you like, on that of the theory-ladenness of experimental observations, is the *conclusivity* of experimental tests regarding the truth-value of isolated hypotheses. In his view, given the inseparability of hypotheses for testing, the falsification of one or the other of them by experimental means, becomes a matter of arbitration.

It is highly interesting that Popper has never taken seriously enough the import of Duhem's thesis on the epistemological force of falsification. (That is why the drawing of the ultimate latent consequences of his methodological views had to wait for Lakatos). In the first and even in the subsequent, revised English version of his *Logic of Scientific Discovery*, he refers to Duhem in this connexion only twice and in footnotes. In one of these references, appended to his treatment of conventionalist objections to falsificationism, he declares sweepingly that:

> It may be mentioned that Duhem denies ... the possibility of crucial experiments because he thinks of them as verifications, while I assert the possibility of crucial *falsifying* experiments.
>
> [Popper, 1965, p.78n]

Which, as I believe is clear from the above quotations from Duhem, is a crude misrepresentation of Duhem's standpoint.

The second reference to Duhem is made by Popper in a footnote to Chapter 37 of his *LSD*. In this chapter he considers the case when it is impossible to decide by experiment between two theories without first improving our technique of measurement, since they differ so little in all fields of application. To this Popper adds in note that:

> This is a point which, I believe was wrongly interpreted by Duhem. See his *Aim and Structure of Physical Theory*,, p. 137 ff.
>
> [Popper, 1965, p. 124]

This again, I think, is quite beside the point made by Duhem in connexion with measurement.

In his later (1956, 1963) works, Popper returned again to consider Duhem's position and its impact on the possibility of crucial experiments, but he did not really come to grips with it. Although he elaborated three types of counter-arguments, as Ariew (1984) has recently shown, all of them fail against Duhem.

But what, in Popper's view, is the difference at all between a simple falsifying experiment and a crucial one? As he says:

> I mean by a crucial experiment one that is designed to refute a theory (if possible) and more especially one which is designed to bring about a decision between two competing theories by refuting (at least) one of them - without, of course, proving the other.
>
> [Popper, 1965, p.277,n2]

and

> In most cases we have, before falsifying a hypothesis, another one up our sleeves; for the falsifying experiment is usually a *crucial experiment* designed to decide between the two. That is to say it is suggested by the fact that the two hypotheses differ in some respect; and it makes use of this difference to refute (at least) one of them.
>
> [Popper, 1965, p87n]

As it appears from these formulations, there seems to be a mere pragmatic distinction between simple falsifying and crucial experiments. Later on, in his (1963 & 1972), he tries however to sharpen this distinction and make it more epistemological by suggesting that from some experiments we can learn more about our theories than from others. He argues that severe or crucial tests are conducive to the fruition of the scientific quest for truth. Against this claim, Grünbaum (1976) put forward counter-arguments. Returning to the Popper-Duhem controversy, the main difference, I think, between them is that Popper - being an epistemological atomist - does not accept the inseparability thesis on which Duhem's argument is founded, and further that Popper, himself a kind of conventionalist (or decisionist) is willing to admit that basic statements (e.g. those asserting

the outcomes of experiments) are accepted conventionally, but not that sets of hypotheses, whole theories or parts of theories are also accepted in this way, which is the moral of the Duhem thesis. And this is the most important conclusion of Lakatos' above-mentioned (1974) paper. (See p.318: "one may accept not only basic but also universal statements as conventions"). Popper is, of course, well aware that he has no telling arguments against the Duhem (or Dingler)-type conventionalism and admits in his (1959) that his "criterion of demarcation cannot be applied immediately to a system of statements". He adds:

> That the only way to avoid conventionalism is by taking a *decision*: the decision not to apply its methods. We decide that, in the case of a threat to our system, we will not save it by any kind of conventionalist stratagem.
>
> [Popper, 1959, p.82]

Ruled out, therefore, is the introduction of *ad hoc* hypotheses or modifications into the theory in question when a falsifying instance turns up. Thus, conventionalist arguments for the inconclusivity of crucial experiments and of falsification in general, are evaded by Popper with the help of a methodological *fiat*.

Lakatos' position was, however, more difficult since there appeared (in the 70's) new arguments to the effect that crucial experiments are not only inconclusive but that in the most important cases of theory-choice they are epistemologically as well as factually *impossible*. Those counter-arguments which Popper had to face were Duhem's inseparability thesis and his thesis of the theory-ladenness of experimental results. The new ones were Duhem's thesis as strengthened by Quine, i.e. the so-called Duhem-Quine thesis as well as Quine's thesis of the underdetermination of theories by all possible (and not merely by all available) empirical data, and, in addition, Kuhn's thesis of incommensurability. This latter argument was directed against the tacit assumption (common to Popper and Duhem) that

the rival theories to be tested by a crucial experiment always have common empirical consequences in the form of predictions (stated affirmatively resp. negatively).

The first three counter-arguments carry much weight as far as Lakatos is concerned. This is in spite of the fact that he regards the Duhem thesis (or, as he calls it in his [1970, in: 1978, 1, p.97] the weak interpretation of the Duhem-Quine thesis) to be effective against dogmatic,not methodological falsificationism. The reason is that:

> it only denies the possibility of a *disproof* of any separate component of theoretical systems.
>
> [Lakatos, 1978, Vol.1, p.97]

Lakatos rejects, however, the strong interpretation of the Duhem-Quine thesis which, excluding any rational selection among theoretical alternatives, is inconsistent with all forms of methodological falsificationism and unacceptable to even the sophisticated falsificationist. In Lakatos' words:

> The sophisticated falsificationist allows any part of the body of science to be replaced, *but* only on the condition that it is replaced in a 'progressive' way, so that the replacing theory successfully anticipates novel facts. In this rational reconstruction of falsification 'negative crucial experiments' play no role.
>
> [Lakatos, ibid, p.99]

Thus, for the sophisticated falsificationist, i.e. the Lakatos of the *Methodology of Scientific Research Programmes*, the method of the crucial experiments has no use as a norm of selection or preference among competing research programmes or series of theories. Nevertheless, he does not lack all criteria for choice (which would lead to a Kuhnian-type desperation), he uses instead a positive criterion for this end: the successful prediction of 'novel' facts.

In his later (1974) paper, lakatos is even more explicit in his rejection of Popperian-type empiricism and of crucial experiments which were all-important for Popper in the assessment of theories. Now that the unit of appraisal is no longer

the single hypothesis or theory, as was for Popper, but a series of theories:

> The falsificationist pattern of 'conjectures and refutations', that is, the pattern of trial-by-hypothesis followed by error-shown-by-experiment breaks down. A theory can only be eliminated by a better theory... And for this replacement of one theory by a better one, the first theory does not even have to be 'falsified' in the orthodox sense of the term. Thus progress and learning are marked by *instances verifying excess content* rather than falsifying instances, and 'falsification' and 'rejection' become logically independent.
>
> [Lakatos, 1974, pp.320-21, italics - M.F.]

And his conclusion is that:

> if we abandon negative crucial experiments in *method*, why stick to it in *metamethod*?
>
> [Lakatos, ibid.]

Instead, we should apply the methodology of methodological research programmes, i.e. try to organize the basic value judgements of the scientific élite into coherent frameworks and use them as a theory of rationality.

This radical extension of Popper's conventionalism into a Duhemian direction has been qualified by Hacking [1979, p.386] as a neo-Hegelian shift from the notion of truth as correspondence to the surrogate notion of truth as strict observance of the norms of methodology. It is the methodology of the methodological research programmes which secures the growth of knowledge and which defines what knowledge actually is. I think, however, that this Lakatosian turn may as well be considered to be a kind of sociological turn, since the "basic value judgements" which are decisive in the eyes of Lakatos, are issued by the "scientific élite" which can only be sociologically circumscribed, lest we get into a vicious circle.

Against Hacking's interpretation of Lakatos' later views, Newton-Smith rightly quotes [in his 1981, p. 100] the rather enigmatic words of Lakatos who, after admitting that his is a

very radical form of conventionalism (or instrumentalism) adds that:

> One needs to posit some extramethodological inductive principle to relate - even tenously - the scientific gambit of pragmatic acceptances and rejections to verisimilitude. Only such and inductive principle can turn science from a mere game into an epistemologically rational exercise from a set of light-hearted sceptical gambits pursued for intellectual lectual fun into a - more serious - fallibilist venture of approximating the Truth about the Universe.
>
> [Lakatos, 1978. p.113 quoted in Newton-Smith 1981.p.100]

But by taking recourse to an *inductive* principle (that of verification of excess content) Lakatos is performing another move which was anathema to orthodox Popperians.

The reason why I discuss here just Lakatos and his route to deviant Popperianism and thereon to hereticism, is that I think his intellectual career is more significant than that of any other 20th century empiricist philosophers of science. It shows, quite clearly how, in the new wave of scepticism in the philosophy of science, begun by Kuhn (spread and promulgated among others mainly by Feyerabend), but motivated by several intellectual and social factors outside the philosophy of science, even the most tolerant and flexible forms of empiricism lose self-confidence and become untenable. In this way, 20th century philosophy of science lost its trust in experience as an arbiter of theories and its faith in Nature as mankind's only trustworthy adviser in matters of knowledge. It also lost its faith in the epistemic authority of experience erected as such during the 17th century in the place of a former authority: Man's God-given intellectual and knowing power embodied in the Holy Tradition and the Church, which was swept away in the 17th century wave of scepticism.

Let me quote in this connexion the highly significant words of Popper:

> 'Once and for all', says Weyl, with whom I fully agree, 'I wish to record my unbounded admiration for the work

> of the experimenter in his struggle to wrest interpretable facts from an unyielding Nature who knows so well how to meet our theories with a decisive *No* - or with an inaudible *Yes'*.
>
> [Popper, 1965, p. 280]

To which Lakatos replies that:

> Nature may shout 'No' but human ingenuity - contrary to Weyl and Popper - may always be able to shout louder.
>
> [Lakatos, 1974, p. 319]

Thus we get to the picture of self-conceited Man who is unable or rather unwilling to take the advice of Nature, as in the 17th century he seemed unwilling or unable to take the alleged advice of God, in order to find Truth, that is, those repreentations which stand in privileged relations to reality. Behind this picture there lurks the Popperian vision of unyielding Nature with which Man has to struggle, which has to be coerced to give us advice. Also there is the idea that the relation of Man and Nature is competitive rather than cooperative. The roots of this firmly entrenched vision can also be found in the 17th century when Bacon inculcated in those seeking knowledge: that "we have to torture Nature's secrets out of her", as it was then usual with witches and reluctant witnesses. In the picture, however, which Lakatos offers us, Nature seems already to be defeated, her voice is no more decisive - as it was still for Weyl - and it is not loud enough, It is almost inaudible as that of a dying person. So, I think, it is no mere coincidence that the idea of crucial experiments originated in the 17th century with Francis Bacon (leaving aside the precedents of it in his namesake, Roger Bacon, and in Occam) and was rejected as unusable by Lakatos in our century.

Now that I have finished my brief survey of the recent downfall of the method of crucial experiments in the empiricist methodology, let me look back to the time when this norm came into existence. I shall then ask why it emerged at all into a fairly high-ranking position for a while and what epistemolog-

ical hopes it was expected to fulfil. In doing so, I hope that I may provide insight into the reasons of its present downfall.

The birth of crucial experiment: the Baconian idea

During the discussion, however, we should keep in mind that the time interval between the rise and fall of the method of crucial experiments is 350 years, during which scientific knowledge has changed from *episteme*, (i.e. a body of knowledge which was taken to be not only true, but necessarily so) through a stage of having been taken to be only contingently or probably true, to its recent position in which, to quote Popper again:

> ...our science is not *episteme*: it can never claim to have attained truth, or even a substitute for it such as probability.
>
> [Popper, 1965. p.278]

So, today we learn to live with the idea that scientific knowledge is conjectural, it has an uneliminable hypothetical character; but at the beginning of the 17th century, in the time of birth of the method of crucial experiments, the Aristotelian ideal of demonstrative knowledge was still valid. Therefore, everything which claimed to be genuine knowledge, *scientia* as opposed to mere *opinio* (opinion), had to conform to its requirements. The emergence of this new method of disconfirmation by experiment was due only to that intellectual situation in which the epistemic ideal was still valid but the methodological roads leading to its attainment became blurred and seemed to be no longer viable.

As it has already been said, the father as well as the godfather of the method of crucial experiment is Francis Bacon. Crucial experiment appears in his *Novum Organum* [1620] as the 14th of the 27 so-called prerogative instances and under the name of *instantia crucis*, i.e. crucial instance. (Although *instantia crucis* appears as "instance of the fingerpost" in

the 1875 standard English translation of the Novum Organum; I will not use this expression in what follows.) Bacon, however, lays no *special* emphasis upon this method, which he regards merely as one of the several means securing that the truths of knowledge emerging from inductive reasoning - he wished to recommended - be as certain and apodictic as it was required by the then valid Aristotelian ideal of scientific knowledge. Francis Bacon wanted merely to replace deductive, syllogistic reasonings in science (i.e. those starting from metaphysical first principles as major premisses containing the causes as middle terms of the syllogism, to statements about phenomena as conclusions), by inductive inferences starting from phenomena and ascending to their "forms" or natures, or - as he sometimes expressedly states: their laws (lex, leges, see Nov. Org. Bk.II.§.III.). He believed, however, that he had saved the demonstrative character of the knowledge acquired in this way.

As for crucial instances, their role according to Bacon, is to help us to avoid the pitfalls of simple enumerative induction and to enable us to make a decision whenever

> the understanding is so balanced as to be uncertain to which of the two or more natures the cause of the nature in question should be assigned.
>
> [Bacon, Nov. Org. Bk.II.§.36]

It is interesting that one of the examples given by Bacon to illustrate the working of the method of crucial instances is the case of the tides. The question to be decided is whether the tides are due to an advancing and retreating motion of the waters as if rocked to and for in a basin, or to a periodic lifting and falling of the waters. The basin hypothesis, according to Bacon, would be falsified if there were high tides at the same times at the coasts of Spain and Florida as well as of China and Peru lying opposite to each other. The experience regarding this phenomenon would be crucial since it excludes one of the two hypotheses and thus confirms the other.

A more important and serious example of the use of this method can be found, however, in Galileo's *Dialogue*. On the Fourth and last Day, Galileo uses the phenomenon of the tides as the crucial instance to decide between two astronomical hypotheses: between the Ptolemaic and the Copernican system. Although he does not use[4] the Baconian name for this method he might well have known it. (He corresponded with Bacon through mediators. Bacon was acquainted with, but rejected, Galileo's tidal theory, the outlines of which had been made known already in 1616.) The whole argumentation-structure of the *Dialogue* and the appearance of the tidal theory just at the end of it - as though the tidal theory crowned the edifice - clearly shows that Galileo regarded the phenomenon of the tides as an instance falsifying all other explanatory hypotheses save one: the Copernican. The phenomenon of the tides was the only one (from among several other terrestrial and celestial phenomena) accepted as existing by both astronomical theories (*demonstratio quia*), but was accounted for or explained (*demonstratio propter quid*) by only one of them - the Copernican theory. Or so it seemed to Galileo.

Therefore, in spite of Bacon's not attributing any privileged place or eminent role to these crucial instances among the other prerogatives he mentions in his *New Organon*, the method itself gained gradually in importance and was referred to by the name of "experimentum crucis" by Descartes (e.g. in his debate with Harvey on the cause of blood circulation). It was also referred to under this name by Boyle (in his writings on experiments with the air-pump), by Newton (in his early optical papers) and by many others philosophers of nature in the 17th century. The important thing with Bacon's crucial instances (experiences or experiments, as they are sometimes called[5]), is that they are assumed to give conclusive evidence in favour of one and against all the other alternative (incompatible) causal explanations. And they do this, according

to Bacon, moreover, with apodictic conclusivity, that is, they yield demonstrative knowledge. These rival explanations were, at the same time, not regarded by him as *hypotheses* (in the then usual sense) but as candidates for true knowledge, as giving the possible causes, only one of which is the true cause, and has to be selected.

The process of knowledge turning conjectural: the pluralisation of causal explanations

In the Aristotelian methodology, however, the plurality of accounts for the same phenomena was the indicator of conjectural, i.e. doxastic and not epistemic knowledge (demonstrative knowledge was unique). This was why neither of the different astronomical hypotheses developed in antiquity and in the Middle Ages did count as true knowledge. The main requirement for hypotheses was "to save the phenomena", to be in accord with (all the available) facts. There was no strict constraint of unicity and no strong requirement of choice toward them. They could be alternatively used for practical purposes. In antiquity and in the Middle Ages, nevertheless, two different choice criteria were offered in the case of plurality of hypotheses. One of them was Theon's: to adopt that hypothesis which was consistent (not only with the facts but) with the metaphysical first principles accepted by some School; the other was Ptolemy's: the criterion of simplicity. In the 16th century Aristotelian Jesuits (e.g. Ch. Clavius) proposed the following threefold criterion: the hypothesis should be consistent with (1) the facts, (2) with Aristotelian physics and (3) with the Scriptures. The hypothesis selected by means of these criteria (from which the first was the most important) was not taken to lose, however, its hypothetical character. (This was Cardinal Bellarmine's main argument against Galileo's claim that Copernican astronomy gives true knowledge of the ultimate reality and thus it is not a mere hypothesis.)

In the Bacionian methodology, even stranger to an orthodox Aristotelian, was the assumption of giving the decisive role to experience in the process of the choice of one from among rival causal accounts (thus not from among mere hypotheses). In the Aristotelian deductive-syllogisms, construed as demonstrations as well as explanations, the expressions specifying the four causes (material, formal, final and efficient) served (in answering the four scientific questions)[6] as middle terms connecting the subject and predicate of the major and minor premises in the conclusion. And so the question of whether a given cause is the real (or true) cause of the phenomenon (given in the conclusion) could be decided on logical grounds. It was simply a question of the validity of the syllogistic argument. Therefore, it was not a question to be decided by empirical means. By emphasizing this, I would not want to give the wrong impression that Aristotelian science was anti-empirical, or that it disregarded empirical evidence. I am merely saying that it was in answering questions *of fact*, (and those of existence) and not that of the *reasoned fact* (or of questions of essence) where experience could and had to be taken into account. But this means, in my view, that the Aristotelian and the Baconian-Galilean science differed in *where* (at what point of scientific reasonings or theory-construction) and *how* it took experience into account. (They differed, moreover, in the *kind* of experience they considered to be relevant, but more about this later). As, Mittelstrass has so excellently pointed out: in Aristotle the world of phenomena has already a conceptual structure. And thus:

> Both Aristotelian and Galilean physics represent *empirical* science: the former because it can be understood as a generalisation (objectivisation) of the knowledge of everyday experience (basic knowledge of concrete distinctions and demonstrated knowledge being in principle one and the same); the latter because it submits its propositions to objective, technically controlled experiences (basic knowledge of concrete distinctions and demonstrated knowledge diverging in this case). The

> Aristotelian and the Galilean concepts of experience stand for two different concepts of *empirical knowledge*.
>
> [Mittelstrass, 1978, p.43.]

They stand for two different ways of assigning an evidential role to experience. For the Aristotelians, the remote empirical consequences of the principles used in constructing demonstrative syllogisms could not have any bearing on their validity, since there was a separation between the establishment of the principles and the empirical consequences. The principles were given in metaphysics, and they were considered to be self-evident, thus their validity did not depend on empirical confirmation or disconfirmation. Also, the terms for causes standing as middle terms in *propter quid* demonstrations were merely mediating (hence their name "mediate causes") between the self-evidentially true first principles and the empirical facts of the everyday normal experience (phenomena). The selection of the true causes (middle terms) was, in the Aristotelian methodology, so to say, secured by theoretical means, given the two extremes (principles and phenomena) which they were to connect. Since the Aristotelian principles were essentially not transcending but merely conceptualising the world of normal experience, the mode of their connection was uniquely given within the framework itself. Thus no ambiguity and plurality regarding causal explanations turned up, and so there was no problem of selection from among different causal explanations. The two endpoints so closely and strictly connected in this Aristotelian chain of reasoning became, however, at the beginning of the 17th century, disconnected. (The main causes of which I will try to hint at later). By this loosening of the ties there emerged the possibility of connecting the two endpoints in several different ways.

This possibility was *not* specifically linked to the method of Baconian (simple) inductive method but also beset the Cartesian deductive methodology, and thus it was not a result of a simple deviation from the Aristotelian deductive-syllogistic

method, but that of a more profound change of the whole conceptual framework. The first part of this thesis can be attested by Descartes' following words from the *Discourse on Method* (Ch.VI)

> The power of nature is so ample and so vast, and these principles so simple and so general, that I almost never notice any particular effect such that I do not see right away that it can be derived from those principles in many different ways.
>
> [Descartes, Olscamp transl., 1965, p.52]

Thus even deductive method is no remedy against the pluralisation of causal explanations since the "particular effects" can be accounted for in many different ways, that is, by a variety of subsidiary causal schemes, which renders them hypothetical. And the choice between them even in Descartes, view can be made only by means of crucial experiments.[7]

As Buchdahl writes:

> We may say that in this sense the relationship between the primary principles and the particular phenomena is 'epistemically opaque', and the resultant certainty is no more than 'moral'. For Descartes, ontologically, the effects do not doubt depend upon the causes, in the sense that 'God willed them'. Unlike Leibniz, however, Descartes does not possess a special principle of selection between alternatives, supplying us with a 'reason' for God's choice.
>
> [Buchdahl, 1969, p. 122]

Thus it was on the level of the intermediary theories (subsidiary or subordinate principles) that the hypothetical or conjectural character of knowledge appeared in the 17th century. The level of basic metaphysical or first principles (of primary or ultimate causes) seemed for a while to remain intact from the wave of growing scepticism. The way out of this impasse seemed to be a reconsideration of the relationship between explanatory thories and experiences and then the making of the subordinate principles dependent on empirical evidence in their consequences. Since demonstrative knowledge could no

more be produced either by inductive or deductive means, the method of crucial experiments seemed for a while to serve as a surrogate means for demonstration in so far as it resulted in the *a posteriori* empirical confirmation of a conjectural causal explanation by a conclusive empirical *dis*confirmation of all its rivals. Bacon thinks so highly of his own inductive method that he even relegates Aristotelian syylogistic demonstration to the adjudication of only *opinions* and not of genuine knowledge. This is, I think very significant since it implies the devaluation of the former way of justification of knowledge claims, the very criteria of genuine apodictic truth. A shift towards empiricism seemed to solve the task of finding new ways and criteria for the evaluation of knowledge claims, but in the climate of the the prevailing (Pyrrhonian type) scepticism (emphasising the unreliability of experience) which characterized the general intellectual atmosphere of the 17th century [see Popkin, 1979], this process concluded later in renouncing even the Aristotelian ideal of science as unattainable.

The disappearance of apodicticity

Before going into details of this problem, let us first see the possible reasons and causes of why these above-mentioned subordinate principles or "middle propositions" became conjectural, i.e. acquired a hypothetical status. So why did the ties loosen between first principles and explainable phenomena? (With first principles remaining for a while unshaken, i.e. unquestioned and accepted as self-evident.) So far I have mentioned only their pluralisation as the most immediate reason. This, however, was itself due to other, more deeply-rooted reasons such as, *first*, the transposition of the problem of the plurality of views concerning the "system of the world", (the 'pluralité des mondes') i.e. of astronomical and cosmological assumptions to the realm of physics; and, by this, the

transposition of the method of *ex hypothesi* reasoning before this used exclusively in astronomy and mechanics (as mixed sciences) into physics (as pure science), instead of the formerly used *ex suppositione* reasoning (in *propter quid* explanations)[8]. This shift in method can very well be seen to appear in the methodology of Descartes, who employs the *ex hypothesi* method in physics (and who admits, unlike Bacon and, later, Newton even the hypothetical character of his theories, while formerly Galileo (as was shown by Wallace in 1976) tried to employ, inversely, the method of *ex suppositione* reasoning in astronomy (see the Dialogue) and mechanics (in the Two New Sciences) as well. This transposition of methods was, on its part, I think, due to the elimination then beginning of the boundaries separating celestial and terrestrial as well as natural and artificial phenomena and their respective sciences (astronomy vs. physics, resp. physics vs. mechanics).

The distinction between *ex hypothesi* and *ex suppositione* reasoning (since both have the same logical form: "if *p* then *q*") consists, according to Wallace that in *ex hypothesi* (and in modern hypothetico-deductive) reasoning:

> *p* formulates an hypothesis that does not pertain to the order of appearances, whereas *q* states a consequent that pertains to this order and so is empirically verifiable.

While in demonstration *ex suppositione:*

> *p* usually pertains to the order of appearances... with regard to *p*'s content, no claim is made for the absolute necessity or universality of such an observational regularity, since there are always impediments in nature that can prevent the realization of any ideal result. The logical consequent *q*, on the other hand, standing, as it does, for antecedent causes or conditions that produce the appearances, need not itself pertain to the order of appearances, at least initially, although it may subsequently be found to do so,...
>
> [Wallace, 1976, p.95]

The argumentation leads to a valid form of the *modus ponendo ponens* of the conditional syllogism

$\{p \supset (p \supset q)\} \supset q$,

i.e. to the demonstration of the truth of q. While *ex hypothesi* reasoning leads either to the invalid

$\{(p \supset q) \ \& \ q\} \supset p$

and with it to the problems of confirmation (since we can observe only q); or to the valid but merely falsificative:

$\{(p \supset q) \& \sim q\} \supset \sim p$;

so that the *truth* of p cannot be demonstrated, only its falsity can be *empirically* ascertained.

By this I come to the *second* reason I consider to be very important in turning the explanative theories conjectural. And this is: the shift of inquiry from the realm of visible and sensible things to the realm of invisible and in-principle - insensible ones. (as the ultimate constituents of sensible things). Let me refer here to McMullin [1978b] who says:

> ...Yet the most striking feature of 17th century science was its move into the "invisible realm" its introduction of a form of structural explanation that Aristotelian science excluded. The corpuscular philosophy of Galileo, Boyle, Gassendi, and the rest, postulated corpuscles which could be described in the same terms of extension, impenetrability, etc., that we would apply to objects of ordinary experience even though these corpuscles, in principle can never be experienced by us.
>
> [McMullin 1978b., p.15]

Closely connected to and following this shift there appeared another one: the shift from Baconian-type induction to transduction [McGuire 1970, or transdiction as McMullin, 1978b, calls it] and to retroduction [Peirce] - the former being induction to empirical generalisations, i.e. an inference from observed cases to non-observed (but in principle observable) ones; the latter two: induction to theories, i.e. inference from observables to *in principle* non-observables (transduction), and to generative mechanisms, constituent particles and remote

causes (retroduction). These two main types of induction are called by Laudan [1981] "plebeian" resp. "aristocratic" induction and are shown to lead to different kinds of problems concerning the justification of the knowledge-claims they generate. Systematic efforts to *reduce* transduction (and retroduction) to induction, to reduce causality to constant conjunction are to be seen, however, later in Hume's philosophy, in the 18th century. [Cf. Hacking, 1975]

The *third* important component in the process of scientific knowledge's becoming conjectural was the widespread use of the clockwork-metaphor in constructing explanations. Instead of the former organism-metaphor, it was now the machine which supplied the basic analogy. How widely used this clock-metaphor was, hardly needs demonstration.[9]

The main problem, from our point of view, within this picture of the universe as an immense clockwork mechanism, devised and put together by an inscrutable and voluntarist God[10] according to first principle (basic logical and even natural laws) intelligible and known to us, was, that the precise mechanism and detailed workings of this device were supposed to remain, veiled and hidden from our eyes (in the sensual as well as in the intellectual sense of this word). As Descartes writes:

> The Supreme Craftsman might have produced all that we see in a variety of ways, I freely admit the truth of this; I shall think I have done enough if only what I have written is such as to accord accurately with all natural phenomena.
>
> [Descartes, Anscombe-Geach transl., 1954, p. 237]

Since

> Just as a watchmaker could construct two watches which were externally similar and equally accurate in keeping time, but with very different internal mechanisms, so likewise God could have chosen a variety of causal mechanisms to produce the observable effects which we can observe in nature."
>
> [Clark, 1982, p. 115]

All this suggested that we can only make conjectures regarding the inner workings of the clock, like that of a "black" box which cannot be made transparent in spite of our knowing the basic principles of its functioning. The ultimate cause of all things was God, the Supreme Clockmaker, but his ways of procedure, the chain of secondary causes leading to observable phenomena became blurred before Man's eyes.

Closely connected to this picture (as a consequence of its acceptance) was the process of the reduction of the four Aristotelian *causes* to be investigated to only one worth finding. A similar process of reduction was taking place concerning the four Aristotelian types of *motion*.

The reduction of the types of causes and motions

The clockwork was, by definition, a mechanical instrument, the workings of which were taken to be due only to the (geometrical) configuration and locomotion (change of place) of its parts. Therefore, it seemed that the only connections worth scientifically investigating, were, besides spatial contacts, efficient (motive) causes on the one hand, and local motions (producing different configurations) on the other.

It is well known that local motion acquired a central, and even exclusive role, in Descartes' and Newton's physical theory. (all other types of motion were assumed to be reducible to this kind of motion.) Now, local motion, especially when treated only kinematically (as it was up to Newton), can be an effect of several different causes, (allows a lot of different explanations) and proceeds in a lot of different possible ways. (For instance the transition of a body from point A in inertial space to point B can take place in infinitely many possible ways and by infinitely many possible causes. To determine which the unique and real path is, and by which actual cause it is produced, needs the introduction of a selection principle. Leibniz introduced extremum principles, as, for instance, the

principle of least action, for this end. These principles served for him to select the real, actual world from among many different possible ones.)

As for the reduction of the four[11] Aristotelian causes, it had a serious impact on the character of knowledge gained in physical inquiry. In the Aristotelian model namely, a causal (*propter quid*) explanation had to be given in terms of all the four causes, and this presented knowledge of the *'reasoned fact'*, and not only that 'of the *fact*'. Explanations given in the terms of intrinsic (formal and material) causes were at the same time taken to be *superior* to those given in terms of extrinsic (final and efficient) causes. And in this tradition, efficient causes could only be used to explain existence *simpliciter*.

> Such efficient causes leave us in the realm of opinion about the *nature* of the effects and the *nature* of the causes which brought them about. For proper demonstrations we need formal, final and material (necessitating) cases...
>
> [Machamer, 1978, p. 173]

Therefore, while in the Aristotelian tradition the finding of all four proximate, necessitating causes delimited the number of possible *propter quid* explanations to one true one, now, the taking into consideration of only one, the efficient cause, left a greater freedom in theory construction, making at the same time the explanations produced more contingent and uncertain, and also giving rise to the problem of selection from among possible explanations.

Efficient causality, on the other hand, was very apt to be used in transductions, since efficient causes taken to be extrinsic to phenomena could be regarded as transcendent to them, (i.e. lying behind them, hidden from our eyes, not given *in* experience). The elimination of the other three intrinsic[12] causes already begins with Galileo and Bacon and proceeds through Descartes to Newton,[13] - to mention only the greatest scientific figures of the age. Under the conditions of the

aforementioned need for a principle of selection in the face of the plurality of rival explanatory hypotheses, when tradition, i.e. Aristotelian interpretation and methodological norms could no more be used, there seemed to remain one way open to evade scepticism and to resolve the problem. This was: to find another Authority to replace the lost one, or rather, to put aside the former mediator (the Aristotelian tradition) between Man seeking knowledge and God, that is, Nature; and to make unmediated experience the judge of truth. To regard Nature as a book containing the written word of God and try to read it without the mediation of Authority or tradition, as Protestants have read the Scriptures. [Cf. Hacking, 1975, p. 30]. The "book of Nature" metaphor was meant to make an assumedly unbiased, theoretically unprejudiced, or not pre-conceptualized experience the arbiter of theories, (first and secondary principles as well), through their empirical consequences. It could have meant, however, a radical *subjectivisation* of the criterion of truth, by making *individual* experience the supreme court of knowledge (leading to a kind of methodological solipsism.) This process did not take place, since the very notion of experience became at the same time problematic and had to be changed.

The question for Bacon and Galileo already was: what kind of experience to apply to for the justification of knowledge-claims. The indirect impact of Copernicanism was, a growing scepticism towards ordinary sense experience,[14] i.e. towards experience as defined by the Aristotelian tradition. (This suspicion was strengthened and gained additional support through the systematic arguments of Pyrrhonists against the reliability of the senses.) This scepticism was all the stronger since the most essential propositions of early modern science (those concerning the solar-system, free fall, inertial motion etc.) contradicted everyday experience, obtainable under normal conditions. (Remember Galileo's admiration for Copernicus because of the latter's courage to propound an

astronomy which flew in the face of commonly-accepted experience.)[15]

Thus, there began a process the result of which was that (in Mittelstrass' words) the Aristotelian and the Galilean-post-Galilean concept of experience stood for two different concept of empirical knowledge, (i.e. ... criterium naturalis philosophiae... vehementer saltem labefactari videtur - says Galileo).

The development of a new concept of experience then proceeds in a two-fold process:

(1) by the *reinterpretation* (in the light of the new supposition) of the ordinary, *commensense experiences*. (The first master of this was Galileo in the *Dialogue*: see his repudiation of the famous tower-argument), and

(2) by the *introduction* of new, formerly rejected and theoretically devaluated (invalid) sources of empirical knowledge, in order to obtain new, non-ordinary experiences. These are: the artificially-or instrumentally-gained experiences, i.e. the results of experiments (which were taken to be invalid as sources of genuine knowledge by the Aristotelians). Experiments were now given preeminence over ordinary ways of obtaining experience. This process was already started by Bacon[16] but the most ardent champions of it were Robert Boyle and Newton. It was mainly Robert Boyle in whose work (as was recently shown by S. Shapin 1984) we find a distinct and systematic effort to give experiments preeminence over ordinary experience, to develop the prqctice of simple, contemplative observation, and to create a new, *scientific community* (in Shapin's words: a public or social space) around experiments. He worked out the new, *pragmatic examples* of experimental procedures (he gave very detailed descriptions of his own procedure)[17] as opposed to the discredited clandestine manipulations of the alchemists. The main requirements of experimentation, repeatability and publicity, also stem from his work, together with the very important maxim that experiments should be made for some definite

purpose to test the implications of some theory, and not just randomly, out of a mere idle curiosity or as an illicit interventions into the workings of Creation. Boyle's technique as all as his writings therefore played an important role in securing a common assent, a favourable public opinion for accepting experiments as the new, *legitimate* sources of scientific knowledge. To create common assent and to so de-subjectivise paradigmatic experimental procedures and their results, was the more important since, before this time, we find [as pointed out by Clark 1982] in Descartes' (and Huygens') *oeuvre* a (though not too sharp) distinction between ordinary, common experience and experimental experience as[18]: experientia omnibus hominibus communis" (experience common to all men) versus "expériences particulieres", i.e.:

> Thos observations which mere not common to all untutored observers of nature, but requiered skill and scientific knowledge to make...
>
> [Clark, 1982. p.23]

Thus there was a need for the objectivisation and standardization of experimental procedures and results, since:

> Radical individualism - each individual setting himself up as the ultimate judge of knowledge - would destroy the conventional basis of knowledge, while the disciplined, collective social structure of experimental language game would create and sustain that factual basis. [...] Legitimate knowledge was objective insofar as it was produced by the collective, and agreed to voluntarily by those who comprised the collective. The objectification of knowledge proceeded through displays of the communal basis of generation and evaluation. Human coercion was to play no visible place in the experimental way of life.
>
> [Shapin, 1984, p.509]

And by making experiment the ultimate judge of the truth of knowledge, instead of traditional authority (first principles), or of the subjective experience of the individual, the grounds for universal assent shifted from a shared metaphysical

view of the world (including common faith or religious dogma), to "Nature" as the supreme court of knowledge. To refer to "Nature" (i.e. to experimental results) was tantamount to refer to a new, *secular* consensus of men of science (and this wa was an escape-route from methodological solipsism).

In this process of standardisation and objectivization of experimental method which seemed to exclude any idiosynchratic, subjective and sociological component, experimental experience was accepted as a legitimate source as well as the judge of knowledge claims. It was accepted as a judge in both senses of the word: to help arrive at a decision between rival, thus far observationally equivalent theories, and to adjudicate between their advocates, i.e. those scholars or groups scholars who endorsed them. In this was decisive or crucial experiments were to play an epistemological as well as a sociological or politico-scientific role in science.[19]

The epistemological role experiments were expected to play was to mediate Nature's verdicts on our theories as conjectures concerning her behaviour. That is why "experimenta crucis" came into the foreground in the course of the 17th century from among the 27 "prerogative" instances of Bacon. From this there emerged a new criterion for the assessment of theories: that of verifiability or falsifiability through their empirical consequences and with this: the requirement of empirical predictivity of theories, (i.e. that they lead to empirically-experimentally testable consequences). Against the former criteria of criticism viz.: intelligibility (compatibility with ordinary experiences obtained under normal conditions) and compatibility with privileged metaphysical (theological) principles, the new criteria brought in new, mobilising factors into the scientific enterprise (against the former rather more stabilising ones), propelling it towards new predictions and cleverly-devised experiments, as well as towards framing newer and newer hypotheses. In this way, the pluralisation of theo-

ries from a disquieting situation turned out to be a powerful motor of science. Early modern science found its way to (as Popkin, 1979, calls it) a "mitigated scepticism" (through Bacon and Descartes), instead of falling prey to devastating Pyrrhonism. The development of the epistemology and method of experiments saved in addition the "scientific rule of faith" from becoming logically vacuous (as Feyerabend, 1970 claimed they were). The standardization and objectivisation of experiments and the interpretation of their results enabled scientists "to identify what relevant scientific experience is" and further "to determine what experience tells us", that is, give interpretations to them in terms of the predictions of the theories in consideration.

Finally, to summarise the points of this study: the method of crucial experiments seems valuable in scientific methodology (and ideology or rhetorics) of science, when a revolutionary new theory is emerging and wants to fight against the élitistic dogmatism of its predecessor, its theories and methods (this was the case with Bacon, the early Newton, Mill and Popper in the 30's, the period of the emergence of 20th century revolutionary physics). The esteem of the method decreases, however, when the new theory and its methods are established and stabilised (as for the later Newton, Duhem and Lakatos) and a new scientific élite emerges.

Technical University,
Budapest

References

ARIEW, R. [1984]: The Duhem Thesis, Brit. J. Phil. Sci., 35, 313-325.

BACON, F.: The New Organon, The Great Instauration, in: The Works of F. Bacon, Vol.IV. English transl. Ellis-Spedding-Heath, London, [1875]

BERTI, E. (ed.): Aristotle on Science, The "Posterior Analytics", Padova [1981]

BUCHDAHL, G. [1969]: Metaphysics and the Philosophy of Science, Oxford.

CLARKE, D. [1982]: Descartes' Philosophy of Science, Manchester University, Press.

DESCARTES, R.: Discourse on Method, P.J. Olscamp transl., New York, [1965]

DUHEM, P. [1954]: The Aim and Structure of Physical Theory, transl.: P. Wiener, Princeton; Atheneum Press [1962].

FEHÉR, M. [1982]: Galileo and the Demonstrative Ideal of Science, Stud. Hist. Phil. Sci., 13/2., 87-110.

FEYERABEND, P. [1970]: Classical Empiricism, in: R. Butts (ed.) The Methodological Heritage of Newton, Bloomington.

GALILEO, G.: Dialogue, transl. S. Drake, Univ. of Calif. Press, 2d ed. 1967 : Dialogo, in: Le Opere, ed. nazionale, Vol. VII. Favoro, Firenze [1904].

GILSON, É. [1983]: Notes pour l'histoire de la cause éfficiente. in: Etudes Médiévales, Paris, Vrin, (repr. 1962. ADHLMA, XXIX).

GRÜNBAUM, A. [1976]: Is the Method of Bold Conjectures and Attempted Refutations *Justifiably* the Method of Science, Brit. J. Phil. Sci. 27/2, 105-136.

[1965, 1976]: The Duhemian Argument, Phil. Sci. 27/1, repr. in: S. Harding (ed.): Can Theories Be Refuted?, Reidel, Dordrecht.

HACKING, I. [1975]: The Emergence of Probability, Cambridge Univ. Press.

[1979] Imre Lakatos' Philosophy of Science, Brit. J. Phil. Sci. 30, 381-402.

KOERTGE, N. [1978]: Towards a New Theory of Scientific Inquiry, in: G. Radnitzky-G. Andersson eds.: Progress and Rationality in Science, Reidel, Dordrecht.

LAKATOS, I. [1970]: Falsification and the Methodology of Scientific Research Programmes, in: Lakatos-Musgrave eds.: Criticism and the Growth of Knowledge, Cambridge.

[1974]: The Role of Crucial Experiments in Science, Stud. Hist. Phil. Sci. 4, 309-325.

LAKATOS, I. [1978]: Rejoinder to Prof. Grünbaum, in: Phil. Papers of I. Lakatos, eds. Worrall-Currie, Cambridge, Vol.1.; Newton' Effect on Scientific Standards, in: ibid, Vol.1.

LAUDAN, L. [1981]: Science and Hypothesis, Reidel, Drodrecht.

[1976]: Grünbaum on "The Duhemian Arqument" Phil.Si. 32. [1965], repr. in: S. Harding (ed.) Can theories be refuted?, Reidel, Dorcdrecht 1976.

MACHAMER, P. [1978]: Galileo and the Causes, in: R. Butts-J. Pitt (eds.): New Perspectives on Galileo, Reidel, Dordrecht.

McGUIRE, J. [1970]: Atoms and the Analogy of Nature, Stud. Hist. Phil. Sci. Vol. 1/1. 3-58.

McMULLIN, E. [1978/a]: The Conception of Science in Galileo's Work, in: R. Butts-J. Pitt (eds.) New Perspectives on Galileo, Reidel, Dordrecht.

[1978/b]: Newton on Matter and Activity, Univ. of Notre Dame.

MITTELSTRASS, J. [1972]: The Galilean Revolution, Stud. Hist. Phil. Sci., 2/4, 297-328.

[1978]: Phaenomena Bene Fundata: From Saving the Appearances to the Mechanisation of the World Picture in: R. Bolgar (ed.) Classical Influences of Western Thought, Cambridge, 1650-1870.

NEWTON, I.: The Mathematical Principles of Natural Philosophy, transl. E. Motte, rev. and ed.: F. Cajori, Berkeley, Calif. [1934]

Optical Papers, ed.: A Shapiro, Cambridge [1984].

NEWTON-SMITH, W. [1981] The Rationality of Science, Oxford.

ORTEGA y GASSET, J. [1970]: L'Evolution de la théorie déductive, trad. par.: J.P. Borel, Gallimard, Paris.

POPKIN, R. [1979]: The History of Scepticism, Univ. of Calif. Berkeley.

POPPER, K. [1959, 2d ed.: 1965]: The Logic of Scientific Discovery, Harper Torchbooks, New York.

[1963]: Conjectures and Refutations, London.

[1982]: Objective Knowledge, Oxford, Clarendon.

SHAPIN, S. [1984]: Pump and Circumstance, Robert Boyle's Literary Technology, Soc. Tud. Sci., 14/4, 481-521.

WALLACE, W. [1972]: Causality and Scientific Explanation, 2 Vols., Univ. of Michigan Press, Ann Arbor.

[1976]: Galileo and the Reasoning *Ex Suppositione:* the Methodology of the Two New Sciences, in: R.S. Cohen et al. (eds.): PSA 1974, Reidel, Dordrecht.

Notes

1. I agree therefore with N. Koertge, according to whom Lakatos' methodology of the scientific research programmes is not a modification or revision of Popper's basic views, as LSE members qualify it, but an *inversion* , a turning upside down of the Popperian methodology. "Lakatos has moved science from the falsifiable to the unfalsifiable side of Popper's line of demarcation." - says Koertge [1978] p. 269.

2. Not to be confused with the so-called Duhem-Quine thesis, see a recent paper on the distinction by R. Ariew: The Duhem Thesis. Brit. J. Phil. Sci. 35 [1984], 313-325.

   One of the reasons why I am not dealing here with A. Grünbaum's [1960, 1976] refutation of the Duhem thesis, is, that he conflated "the views of Quine, Einstein, and Duhem" - as was shown by L. Laudan [1965, 1976], p.158. - and so his arguments are not all valid against Duhem specifically.

3. As Duhem argues against inductivism: "Induction can no longer be practiced in this (i.e. the Newtonian - M.F.) way when science no longer observes facts directly but substitutes from them measurements given by instruments, of magnitudes that only a mathematical theory has defined" [Duhem: Notice sur les titres et travaux scientifiques de P. Duhem, 1917. p.159. quoted by Ariew: op. cit. p. 319-20]

4. In the Drake translation of the *Dialogue*, at the beginning of the Fourth Day, after Salviati's propounding the outlines of Galileo's tidal theory, you will find Sagredo exclamating: "The proposition is crucial, both in itself and in what follows as a consequence;" [Dialogue Concerning the Two Chief World Systems, transl., by S. Drake, Univ. of Calif. Press, 2d. revised ed., 1967, p.417] The term "crucial" is, however, rather misleading here since Galileo actually uses a term meaning something like: "mighty". The original text is as follows: "La proposizione é grandissima, si per se stessa, si per quello ch'ella si tira in conseguenza;" [Opere, ed. Favoro, Vol.VII.]

5. There is no *sharp* distinction at this time between (simple) experience and experiment. As was shown by. D. Clarke [1982, Ch.2.] even Descartes' usage is indeterminate in this respect, and he seems sometimes to use these two terms interchangeably; though a distinction is being made between them by both Bacon and Descartes. There will be more about this later.

6. These were given by Aristotle in the *Posterior Analytics* as questions *of fact* (to hoti), questions of the *reasoned fact* (to dioti), - the answers to these were the par excellence causal explanations -, questions of existence (ei esti) and questions of nature or essence (ti estin). [Post. Anal. Bk. II, Ch.1,89b21-25]

   "Using a teaching he has already developed in the *Prior Analytics*, according to which the middle term of the ca-

tegorical syllogism gives the reason or explanation why a predicate may be applied to the subject under investigation, Aristotle reasons that all four of these questions are related to this middle term. The first and third questions, in his analysis really inquire whether there is an explanation or 'middle' (or, more radically, whether *any* explanation is possible), whereas the second and fourth questions, presupposing that an explanation or 'middle' can be sought, inquire precisely what this is. Moreover, since the explanation will show cause why the predicate can be attributed to the subject, the middle term will have to reveal the cause, and thus all four scientific questions are intimately connected with the concept of causality. It is this line of reasoning that leads Aristotle to the conclusion that causality is inseparably linked with explanation, understanding the latter in the sense of a middle term." [W. Wallace: 1972, Vol.I. p.12]

7. See: Clark [1982] and Buchdahl [1967].

8. See: W. Wallace: Galileo and Reasoning *Ex Suppositione:* the Methodology of the *Two New Sciences*, in: R.S. Cohen et al. (eds.): PSA 1974, Reidel 1976, 79-104.

9. See on this Laudan's excellent essay in his [1981, Ch.4], to mention only the most recent literature on this subject.

10. At least for Descartes and many other 17th century thinkers, though not for Galileo [See: Machamer, 1978, p.174.] This is *one* of the reasons why the conjectural character of scientific knowledge does not turn up in his epistemology. But - says Ortega - "Le Dieu d'Occam et de Descartes n'avait pas créé un monde *ad usum delphini*, comme celui d'Aristote ou celui de Leibniz, que nous allons voir - les "Dauphins" étant les hommes de science. Il n'avait pas créé un monde intelligible par avance. Il avait laissé l'homme plein de foi en Dieu mais plein de doutes sur le monde." [Ortega y Gasset, Borel trad., 1970, p.204.]

11. According to Gilson, there were in the Middle Ages some (e.g. Albert the Great, In V Metaphysicorum, I.3.) who took into account *five* causes, since they distinguished the efficient cause from the moving (or motive) cause (causa movens). Aquinas accepted four causes and regarded the efficient cause (causa efficiens) as the principle of motion. Occam separated the efficient and final cause from the other two, as being extrinsic, while the latter intrinsic. [See: E. Gilson: Notes pour l'histoire de la cause efficients, in: Etudes Médiévales, Paris, 1983. repr.]

12. From among the three (formal, material and final) the character of final cause was controversial since, as already said in n.11, it was taken by Occam as extrinsic, but, according to the more orthodox Aristotelian tradition it was rather intrinsic to the entity under investigation, being its *enthelechy*, its own inner destination.

13. *Bacon*'s view as given in the *Novum Organum* is that:

> It is a correct position that "true knowledge is knowledge by causes". And causes again are not improperly distributed into four kinds; the material, the formal, the efficient and the final. But of these the final cause rather corrupts than advances the sciences, except such as have to do with human action. The discovery of the formal is despaired of. The efficient and the material (as they are investigated and received, that is remote causes, without the latent process leading to the form) are but slight and superficial, and contribute little, if anything, to true and active science.
>
> [Nov. Org. Bk.II.§.2.]

And, though his standpoint is not quite univocal concerning causes, ad Wallace [1974] has shown, Bacon opts for efficient and material causality as the most important for physics. As he writes in the *De augmentis scientiarum:*

> Ant therefore to speak plain and go no further about, physics inquiries and handles the material and efficient causes, metaphysics the formal and final.
>
> [Bk.2, ch.4., quoted in: Wallace, 1974. Vol.II. p.84]

Galileo's position regarding causes, as reconstructed by Machamer [1978] and in my [1982], was that *formal* causes (giving the mathematical structure of the phenomenon) are the most important in scientific investigation. Being more i in conformity with the Aristotelian tradition of demonstration, he gives causal explanations sometimes in terms of all the four causes, moreover he even depreciates efficient causes (in the Two New Sciences) as not leading to *propter quid* but only to demonstrations of existence simpliciter. [See Machamer, 1978, pp. 169-173.] It is however important that we find in Galileo a stressing of the principle of the *unicity* of the cause responsible for an effect, in his words:

> For any effect there is one unique and most potent cause.
>
> [quoted in: Machamer, 1978, p. 171]

Which is, in my view, in line with the process of the delimitation of the causes taken into consideration in constructing causal explanations to only one (efficient or other cause).

Descartes' case seems to be the most clear example of efficient causality's gaining pre-eminence over the other types of causality. As Wallace [1974] summarises his views:

> Not only did he banish forms, of formal causality, form the realm of scientific explanation, but he also saw final causality as beyond human understanding; and though he endorsed matter, and in this sense subscribed to material causality, he severely restricted its scope over the interpretations of his predecessors. Thus, in effect, there is only one type of cause for Descartes, and this is the active or efficient cause, which hence forth would be at the base of all scientific explanations.
>
> [Wallace, 1974, Vol.II. p.14]

Newton's position cannot be discussed here in its full-complexity. It is in any case very well-known how dissatisfied he was with Cartesian "mechanistic explanations", i.e. with the consequent and exclusive use of extrinsic ef-

ficient causality in the explanation of astronomical and gravitational phenomena (the vortex hypothesis) and in what conceptual difficulties he became involved when he tried to explain gravitational phenomena by invoking the force of gravity, an attractive power exerted by a body upon another (by bodies upon each other) which was extrinsic to the body attracted and set in motion, but intrinsic to the body exerting it. For this idea he was accused by Cartesians as well as by Aristotelians with taking recourse to "occult qualities".

So I could say with Wallace that:

> What is inescapable is the conclusion that causality was far from being a dead issue with the founders of modern science. If anything, in fact, it served for them as a touchstone in terms of which they sought to test the truth or falsity of any explanation, and in this sense was an integral component of their scientific methodology.
>
> [Wallace, 1974, Vol.I. p.210]

14. An argument to this effect is voiced by Simplicio on the Second Day of the *Dialogue*: "Et primo, si opinio Copernici recipiatur, criterium naturalis philosophiae, ni prorsus tollatur, vehementer saltem labefactari videtur. (And first, if Copernicus's opinion is embraced, the criterion of science itself will be badly shaken if not completely overturned.) By which criterion, he means in agreement with philosophers of every school, that the senses and experience should be guide in philosophizing. But in the Copernican our position, the senses must deceive us when they visually show us, at close range and in a perfectly clear medium, the straight, perpendicular descent of very heavy bodies. Despite all, according to Copernicus, vision deceives us in even so plain a matter that the motion is not straight at all, but mixed straight and circular" - this is Galileo's new interpretation of facts. [Dialogue, Drake transl., 1967, p.248]

15. As Salviati says on the Third of the *Dialogue*: "I repeat, there is no limit to my astonishment when I reflect that Aristarchus and Copernicus were able to make reason to conquer sense that, in defiance of the latter, the former became mistress of their belief." [Drake transl., 1967. p. 328]

16. Bacon (in *The Great Instauration*) starts from the problem of the unreliability of the senses: "...the information of the sense I sift and examine in many ways. For certain it is, that the senses deceive; but then at the same time they supply the means of discovering their own errors; only the errors are here, the means of discovery are to seek... The sense fails in two ways: sometimes it gives no information, sometimes it gives false information. To meet these difficulties I have sought on all sides diligently and faithfully to provide helps for the sense - substitutions to supply its failures, rectifications to correct its errors; this I endeavour to accomplish not so much by instruments as by experiment. For the subtlety of experiments is far greater than that of the sense itself even when assisted by exquisite instruments, such experiments I mean as are skilfully and artificially devised for the express purpose of determining the point in question. To the immediate and proper perception of the sense therefore I do not give much weight; but I contrive that the office of the sense shall be only to judge of the experiment and that the experiment itself shall judge of the thing. [Plan of the Work, Ellis-Spedding-Heath transl. p.26., Vol.4.]

17. In Shapin's view the provision of circumstantial details of experimental scenes was a way of assuring readers that real experiments yielded the findings stipulated. It was also necessary, in Boyle's view, to offer readers circumstantial accounts of *failed* experiments. This performed two functions: first it allayed anxieties in those neophyte ex-

perimentalists whose expectations of success were not immediately fulfilled; second, it assured the reader that the relator was not wilfully suppressing inconvenient evidence, that he was in fact being faithful to reality." [Shapin, 1984, pp. 493-94]

18. Or as Descartes himself writes:

> ...experiences are of two kinds: one of them is easy and only presuposes that we reflect on those things which are spontaneously presented to our senses. The other kind of experience is more infrequent and more difficult, and cannot be had without some study and expense.
>
> [Descartes: Oeuvres, Adam et Tannery, Vol. XI. p.319.]

To which Clark adds:

> "It is clear from Descartes' correspondence that he understands an experiment in this sense: an experiment *presupposes some prior theory* or hypothesis; its objective is cognitive and it almost invariably involves the intervention of the observer into those features of natural phenomena which are less accessible than the features which are available for inspection by the non-scientist."
>
> [Clark, 1982, p.37, italics mine: M.F.]

Huygens, in a letter (of January 25, 1642) to Descartes, refers to Bacon's experiments as "experiments particulariseres" [see Clarke, 1982, p.42, n.17.].

19. As Shapin pointed out: it would have been "impolitic to acknowledge the existence of 'sects' in natural philosophy. One way by which one could hope to overcome sectarianism was to decline public recognition, that it existed: 'it is none of my design', Boyle said, 'to engage myself with or against any one sects of Naturalists...' The experiments will decide the case." [Shapin, 1984, p. 503]

This side of the matter became important for (e.g.) the British natural philosophers (in the second half of the 17th century) organising themselves into a body of gentlemen engaged in the impartial and unbiased inquiry of Nature,

standing above party divisions and ideological debates and united by the unicity of Truth disclosed by Nature. At least this was the image the Royal Society wanted to give about itself. That is why we find in the Transactions of the Royal Society and the writings of Newtonians, [MacLaurin, Reid] such frequent reference to Bacon's methods and Newton's accordance with them. [See, Laudan, 1981, ch.7, and Mittelstrass, 1972].

VEBLEN, SCHELER, BORKENAU ON THE SOCIAL HISTORY OF SCIENTIFIC COGNITION*

Imre Hronszky

It is frequently claimed that the social-historical approach to scientific cognition begins with the Marxist Boris Hessen's study on Newton written in 1931. Scholars committed to the "strong programme" of the sociology of science name Durkheim and Mannheim as their precursors. This invitation affords an opportunity to deal with other authors from the quite rich early history of the social-historical approach to scientific cognition, in outline, of course. Veblen only wrote down his basic ideas concerning the emergence and development of scientific cognition. Scheler provided a theoretical guideline to the detailed research undertaken. Borkenau wrote a vast book on the emergence of modern scientific thought. Each of these three writers is important in the history of ideas; each gives theoretical orientations concerning the history of scientific cognition.

To begin with, I would like to mention Thorstein *Veblen*, a prominent figure in America after the turn of the century. As an economist, he became famous primarily for his "institutional" sociology and his critique of capitalism put forward in *The Theory of the Leisure Class*. I intend to deal with those views of Veblen's which lie at the heart of his studies, "The Place of Science in Modern Civilisation" and "The Evolution of the Modern Scientific Point of View".[1] Science in modern civilisation has become a cult, something like a last tribunal, he says and poses the question of its emergence, predecessors and

*Paper read to the symposion on "Philosophies of History of Science" at the 17th International Congress on the History of Science and Technology (Berkeley, 1985)

validity.[2] Veblen answers these questions as follows. Science emerged mainly as the metaphysics of industry, its cultural predecessors are myths and legends. In science, thinking became appropriate to a society based on mechanized production - appropriate in its content and truth canons respectively. We can see that Veblen develops a cultural-anthropological approach to the problems stated.[3] Let us see how he deduces his answers.

Veblen's starting point is a presumed special "instinct". This is the "idle curiosity" or "irrelevant attention", as opposed to "pragmatic attention", which manifests itself even in animals. This "instinct" is responsible for the "esoteric knowledge", to be found in every society in history. The "esoteric knowledge" or "higher learning" has taken different forms in different types of society in history, developing from myths to a causal world picture. How did this change take place?

Veblen, by postulating the operation of "idle curiosity", rejects the possibility of an exclusively pragmatistic understanding of the history of cognition.[4] But, when explaining the emergence of knowledge systems which have no "pragmatic teleology", he does not acknowledge at all the idea of intellectual autonomy. In Veblen's "institutional sociology", there is an institution in every type of society which basically determines its character.[5] Thought is determined by life, the knowledge systems that come into being under the influence of "idle curiosity" will become appropriate to the "institution" prevailing in the given society. Changes in these knowledge systems also follow those of the "prevailing institutions", adapting themselves to the latter under the pressure of habituation. The scheme gained in this way will become a discipline. Accordingly, modern science came into existence depending on the process of how industry had become the decisive "institution" of society.[6] The happenings supposed to have taken place in the phenomena under observation have gone through gradual "disanthropomorphisation" in the course of history. For Veb-

len, modern science is dispassionate, impersonal, and hence "matter-of-fact" knowledge.

Here we can see that Veblens's social-antropological approach is not a full-fledged one. The basic barrier is that he presumes as *explanandum* the unconditionally objective character of modern scientific knowledge.[7] In essence Veblen says that the image of the world in its "factualness" was imposed by the development of industry based on machines.

Veblen claims that, in the first stage of industrial development, the world was "dramatized" as the relation between the craftsman and his product. So it was interpreted as the relation between efficient cause necessary to achieve a certain result and its effects. From the 19th century the world has been conceived of as a machine, or - the same thing according to Veblen - as a causal mechanism, a chain of consecutive changes, corresponding to the pressure exerted by industry on thinking.

This has amounted to a "machine made point of view", the "metaphysics of machine technology". There is the constraint of "hard-headed acknowledgement" of pure facts in life and this attitude has achieved its summit and symbol in science.

Veblen claims that the circumstances of experience and tradition to which the classes and members of a community are subject have not been uniform and in agreement with each other. There was a "bifurcate system of culture" throughout all history. The work experiences were allocated a lower place in the hierarchy of knowledge, and they were mediated by the "institutional" structure of society. However, in modern life based on industry they have received their right place. The direction of cognitive interest, the scheme of logic of search for knowledge, became the "logic of machine processes".

This "hard-headed acknowledgement" of the facts guaranteed a "decisive practical advance" for Western civilisation. But Veblen, owing to his specific attitude towards cultural criticism, would put Mr. Choakumchilde, the well-known Dickens he-

ro, in his place.[8] He absolutely denies in addition the view that modern science came into being for the sake of industry, by means of achieving some direct, pragmatic goals of the latter. The accord between industry and science is guaranteed by a connection at a higher level than that. "While even the scientist's curiosity is as idle as that of the pueblo myth-maker, ... the canons of validity under whose guidance he works are those imposed by the modern technology, through habituation to its requirements; and therefore his results are available for the technological purpose... Hence the easy copartnership between the two."[9] Therefore, history, we can say, operates like the *List der Vernunft*, and in industrial society it is precisely the "logic of machine process" that ensures the demand for "dramatic consistency" (and does so necessarily), while the latter ensures the technological applicability of knowledge.

With regard to Veblen we meet a possible basic difficulty in the externalist approach to scientific cognition. Although Veblen does not think that science is more or less an answer given to the direct needs of technology, as some of his contemporaries did, he assumes an *immediate* effect of "life" (industry) on science, and speaks of the effect of industry *previously* becoming the *ruling institution*. Science is an epiphenomenon, a mechanistic reaction to "archetypes", a mechanistic reflection.

*

It is well-known that at the turn of the century, a wide range of viewpoints came into being in social sciences and philosophy in Germany which claimed that society had played some constitutive role in the rise of positive science. Here we can refer to the views of Tönnies, Troeltsch, Simmel, Sombart, Max Weber, and others.

The idea at issue unified the anti-positivistic and anti-scientistic outlook on science with a critique of capitalism from the point of view of alienation. More exactly, these

thinkers conceived of the development of science as a part of the development of capitalism. Their common characteristic is that it was scientific rationality as a *social means*, and not an end in itself, which they considered to be the starting point. These inferences, though sometimes only in the form of scattered and occasional remarks, nevertheless enjoy a definite place in the systems of argument. They claimed that the source of the positive scientific attitude, i.e. the constitutive social conditions of its coming into being, was to be found in merchants' aspirations towards quantification as well as in the fact that machines and the study of them came into the foreground.. They developed a certain "functionalistic" approach to the problem of emergence and functioning of modern science. The functionality of scientific attitude was linked up with the teleology of bourgeois society, maintaining that scientific attitude had developed in connection with the fact that the respective function(s) was (were) brought into consciousness.

Nevertheless, in 1925 Max *Scheler* stated rightly that only sporadic remarks could be found concerning this topic - remarks which provided information only at the level of general conjecture. However, the parts of his book *Die Wissensformen und die Gesellschaft* which deal with the sociology of positive science are an attempt at the systematic elaboration of the issue. "It was not 'pure reason' or the 'absolute spirit' which at the beginning of the modern age sketched out the tremendous programme of the comprehensive mechanistic explanation of nature and man [...] but the new will to power over nature and the desire to work upon her on the part of the rising bourgeoisie." In this way Scheler sums up his views.[10]

In Scheler's conception, it is the special reversal of *Herrschaftswille* ('will to rule, will to power') that stands behind the rise of positive science. Instead of aspiration to rule directly over persons, the coming to the fore of aspirations that strive for "the productive transformation of things"

constitutes that "instinct" and ethos which is responsible for the emergence of positive science.[11] The *gemeinschaftliche Lebensform* (community lifeform), which was characteristic of the Middle Ages, was transformed into *gesellschaftliche Lebensform* (societal lifeform), and along with this, the "categorial-biomorph" *Weltanschauung* corresponding to it had, of necessity to change also. It had to give up its place to the mechanistic world picture and to positive science, Scheler says, since the "categorial-biomorph" world view does not afford any conception of Nature in which she can be regarded as governable - as that which can be transformed according to possible technical purposes.

According to Scheler, the process of the rise of positive science can be seen first of all in the extreme *restriction* of the goal of cognition. It is nothing but the goal restricted to the demand of the *regular prediction* of natural processes and that of the soul which exerts an influence on the outlook which deprives Nature of the objectivity of sensual qualities, restricts Nature to a mechanism, which limits its scope of interest to the measurable-quantitative aspects of the world and to the spatio-temporal relationships among phenomena. These are taken in their "so - und anderssein": i.e. that which seems to be quantifiable as dependent on possible motion-phenomena.

The description of these motion-phenomena in their law-like determination is a correlative of the outlook, belonging to the possible rule over natural phenomena. In this way, conceiving of the world as a mechanism is a point of view which is socially *pre*determined. The explanandum was for Scheler science understood as "savoir pour prévoir" with its system of laws securing this function. In this respect he transformed the Comtean task into the opposite, preserving his basic perspective on science itself: "*Die Güter zur 'Ware' quantifizierende Betrachtung*" of the social world serves as *explanans*.

In Scheler's view, the direct precondition of the coming into being of the cognitive attitude characteristic of positive

science was the meeting of two, earlier separated social strata. "Two social strata, which were separated in the beginning, gradually had to penetrate each other, so that a systematically elaborated, methodologically teleologic, cooperative professional knowledge should come into being..." These strata were namely the "free and contemplative people and that of those people, who gathered experiences in work and crafts rationally, and whose most intensive interest was - due to the internal instinct of the increasing social freedom and liberation - to create pictures and ideas of Nature which render possible the prediction and control of natural phenomena.[12]

In connection with the reversal from the "categorial-biomorph" outlook to the mechanistic approach, I would only like to touch upon some or Scheler's thoughts, which seem to be rather relevant, as regards the philosophical foundations of the present-day ideas on the value crisis concerning the scientific exploration of Nature. He states rightly that the separation of the intellectual and emotional functions of mind - which was expressed in the separation of, on the one hand, existential problems (*Seinsprobleme*) and, on the other, of value and *Sollenprobleme* - also belonged to the conditions of the emergence of positive science. The *Wertfreiheit des objektiv Daseienden* ("the value-freedom of objectively existing things") was a necessary theoretical presupposition for the conception of the world as the territorial object of possible rule... To conceive of the world in terms of value-freedom is a task set for a purpose of some value: for the purpose of the vital value of the rule and command of things."[13]

But in the course of the historical development, "the one-sided system of categories of the *gesellschaftlichen* way of thinking is gradually being put aside. However, we are far from the *lebensgemeinschaftliche* way of thinking of the Middle Ages.. but by superseding this contradiction between mechanical and teleological dependence, with the help of a new synthesis of conceptions concerning the world and science, by means of the

cognition of a comprehensive basic form of laws (*durch Erkenntnis einer übergreifenden Grundform der Gesetzmässigkeit*) and this basic form is neither mechanical nor teleological; by superseding such a conception which, in terms of sociology, finds it correlative in the connection with a new form of essence, in which Lebensgemeinschaft (community) and Gesellschaft (society) begin to supersede each other, in other words: *in the solidary-personal grouping of insubstitutable individuals.*"[14]

Although I do not agree with Scheler as regards the content of his assertions that values must also be regarded as objective (as Scheler himself understands the term "objective") - because as a Marxist, I have in mind rather the levelling off of value and material knowledge in a particular practice. I consider that Scheler's thought mentioned above, in which the change of the relation as a whole to nature is connected with the change of certain basic types of society, having its goal as ruling over things is exemplary.

According to Scheler, all knowledge is "social" in nature, but from this he does not imply that he *sociologises* natural scientific knowledge. In his view, it is only the operation of definite social conditions which open sluice-gates so to speak, and that form of mental act in which knowledge is gained that can make positive science a reality, and not merely leave it to be an eternal possibility - these are conditioned of necessity by society but the content and validity of knowledge are not. The knowledge gained by means of the mechanistic outlook is real, but restricted knowledge, of a partial type. Society only restricted the focus of interest,and knowledge gained as a result will be true knowledge, within this restriction.

Scheler the conservative thinker regards his enemies to be, on the one hand, the liberal defence of capitalism and the positivistic scientism (and, more generally, intellectualism) that pay lip service to it. On the other hand, he disputes aggressively with the defenders of socialism - something that had become a real possibility. More precisely, Scheler takes issue with those whom he thinks to be defenders of socialism, who, considered from the point of view of epistemology, are

the adherents of what he calls the purely "pragmatistic" perspective. In his explanatory system in which two independent variables are postulated in history, the self-development of mind, which becomes a reality as a result of the effect of the correlation of certain "realfactors", is an attempt at superseding both conceptions.

To illustrate the operation of Scheler's view of the sociology of scientific knowledge, I can make only two remarks. Scheler's conception leads him to deny that science came into being in the course of any *continuous* internal development, progressing from the Middle Ages to Modern Times. Such a way of looking at science is, he says, searching for antecedents without asking why at a particular given time things happened in the way that they did. In other words, this way of looking at things transforms factual mental predecessors - e.g. the spreading of Neoplatonism - into a necessary precondition. Scheler lays stress on the defence of the stance which states that, once it has come into being the function of scientific method can be guaranteed exclusively by its operation *according to its inherent laws*. Legitimated by its social function of predictive capacity, science autonomously develops. Scheler gives an answer to the question of the relation between science and technology in the light of this. Both being the result of the same *Trieb* (instinct) and ethos, their connection is therefore guaranteed structurally. At the same time this connection is historically *changing*, and, in the course of history, the development of the one has preceded that of the other just as many times as it happened the other way round. "The new science is not conditioned by technology (as it was supposed to be the case - one-sidedly - by Spengler), neither does new science condition technological advance and capitalism (cf. A. Comte). The logical system of categories is founded on the bourgeois species, in its new structure of instincts and new ethos as well as the original technological driving force to govern Nature."[15]

According to Scheler, the spirit of change is the bourgeoisie's thirst for power. This creates a world picture, in a

theoretical form, of the limited outlook on Nature that has its origin in work.

After Veblen, whose ideological stance is characterised by a special "socialist"-technocratic conception of one sort of industrial society, and Scheler, who is a conservative critic of capitalism, let us turn now to Franz *Borkenau*. During the time when his book was being written, Borkenau was still a prominent theoretician of a fundamental trend within Marxism. With this contention, we have touched upon an essential problem. The Marxist historians of science in the socialist countries, as well as authors of superficial non-Marxist reflections on the history of the Marxist history of science, frequently regard Boris *Hessen's* Newton study as the statement *par excellence* of the "authentic" Marxist conception of the history of science. But a closer analysis will reveal a more realistic picture of the matter: we should recognize that in terms of the social history of science there have been created two fundamental lines of interpretation of society. These came into being, eventually, on the basis of different political stances. One was the "leftist" position, whose most characteristic, albeit essentially different theoretical systems were elaborated by *Bogdanov* and *Lukács*, the latter in his theory of reification, respectively. According to these, "bourgeois science", meaning not only the form of organization, the institutions, but the knowledge and methodology as well are separated from the new society by a sharp *caesura* - in the same way as the whole of bourgeois society is. As Lukács states, "bourgeois science" is constituted by the domination of "formal rationality" which corresponds to reification and which is a constitutive part of it. Science in its forms developed in bourgeois society is the application of "formal", calculative rationality to Nature.[16] Its method allows the advance of knowledge within this framework and within the barriers this framework imposes. But the emergence of a new type of society will change the methodology of research, says Lukács in *History and Class Consciousness*, but without describing in detail his ideas about this problem.

A very different variant of a "leftist" understanding of science was worked out by Bogdanov. Bogdanov's conception was also based on the conception that different classes have different cognitive positions. But, in this conceptualization the bourgeosie needs knowledge for the purposes of control while workers need knowledge to be able to change the things fundamentally. (We have no place here even to sketch the important differences of Lukács's and Bogdanov's conceptions.) Exactly opposed to this "leftist" understanding was a technocratic - economistic one. It maintained that the motive forces in the development of science are to be found in the development of the forces of production. However, the development of these forces was in general very much restricted to the development of the means or production and often even to that of the working tools. Science was thought to be either the theory of the operation of machines, or immediately objective knowledge of nature. The scientific objectivism of positivism was taken over and used as an epistemological basis. From here originates the view that in order to enforce the scientific way of thinking, we should get rid of all the "ideologies" hindering it. On the other hand, the fact that its adherents, in line with the reductionistic view of society, saw the positive condition of the former in the improvement of the forces of production or more precisely, in the demands that manifest themselves in the development of technology and, ideologically, in the elaboration of correct philosophy. On the base of the scheme, demands are motivating factors and technology gives rise to tasks to be solved. The comprehensive survey of the latter calls for finding their theoretical foundations, that is the way how scientific theories are created.

At this point two remarks should be made. The first is that in this view the whole range of problems, in terms of epistemology and social theory, of the emergence of the modern scientific attitude *as an attitude* has been eliminated. Secondly, we should recognize that Boris Hessen, who is often considered to be the "father" of so-called externalism, (surprisingly enough) presupposed the *autonomy* of the development of

scientific thought, according to its inherent and timeless laws, just as any other positivist would have done. Hessen only supposed further that the needs of technology exert a fundamental influence, being the strongest motive force, on the speeding up of the elaboration of the theories. He was representative of an externalist internalism as an eclectic view.

It should not be surprising, even as regards the starting point of its political stance, that the "leftist" position in the epistemological foundation of the history of science brought to the fore the emergence of scientific cognition as an *attitude* and its inference in terms of social history. "Nature and its form of acquisition are social categories", Lukács repeats this in *History and Class Consciousness* and in the often emphasized thesis in his review of Wittfogel's book. Lukács goes on to say that the historical relation between natural science and bourgeois society is to be discussed as part of the "reification of consciousness" itself being a consequence of developed commodity production. This is the thesis which Borkenau (himself a committed "leftist" at the time) attempts to put forward in his book entitled *Der Übergang vom feudalen zum bürgerlichen Weltbild, Studien zur Geschichte der Philosophie der Manufakturperiode.*[17]

Borkenau's starting point was afforded by Cassirer, whose conclusion, purely descriptively, pointed to the emergence of the system of categories *common* to modern natural science and mechanistic philosophy, together with the emergence and rise of the attitude of criticism of knowledge - a conceptual structure that determines the changed manner of experience. Borkenau sets himself the task of explaining the emergence of this conceptual structure in terms of some sort of social ontology. In his hypothesis, it is the transformation of the structure of work organization, i.e. the substantiation of manufacture based on mechanical division of labour, which serves as the starting point. In this type of working process, matter is reduced to "pure matter", he asserts, to pure quantity having exclusively only spatiotemporal movement. But this outlook could become a world picture only in connection with those social struggles in

which the isolated individual appeared, getting into a mechanical relation with the given society. The new world picture and world view fitted in the social practice as a whole integratively and functionally, since it unified the bourgeois interest in the rationalization of the production and the transformation of social conditions, and afforded a science which guaranteed scientific knowledge corresponding to the new form of production and at the same time could function as an ideology.

The direct fate of Borkenau's hypothesis is quite well-known. I am thinking in particular of Henryk *Grossmann's* answer (at that time Grossmann was also a member of the Frankfurt School and both he and Borkenau had nothing in common with the later Critical Theory approach), in which he tried to point out with regard to all the essential issues, that Borkenau's picture of natural science as the logic of the new work organisation and the development of society in the Early Modern Period is untenable.[18] Grossmann pointed out rightly that at the beginning of the 17th century, organic manufacture simply did not exist, thus the explanation of the emergence of modern scientific attitude and world picture in terms of it is pure fiction. Grossmann, as the representative of the economist-technologist Marxist view of the development of society, wanted to emphasize in his critique the role of machines. This, however, was also done by many contemporary authors who were not Marxists. Grossmann set out to defend the ultimately decisive role of the development of the forces of production, and in line with this thesis connected the acceptance of science as an objective system of knowledge with an empiricist-inductivist epistemology. In his analysis, it was in the first place the development of early capitalism where the systematic application of the machines took place increasingly, and experimenting with them became more and more regular. As early as the time of Leonardo da Vinci this had led to recognition of the fundamental laws of mechanics as well as to the fact that the mechanical way of outlook had become a world picture. According to Grossmann, in Descartes' work, the achievements gained by means

of studying machines are summed up.

Considering the history of the responses to Borkenau's hypothesis, reactions fall into two periods, just as the publication of the work did. In the 1930's a *historian* like Lefèbvre pronounced in favour of it. In his selective reception he spoke in praise of Borkenau in his assertion that the development of science is embedded in the development of society, without reducing it to a pure epiphenomenon. Lefèbvre is right when stating that Borkenau, who was operating with *intrinsic contradictions* appearing of necessity in social life succeeded in evolving a synthetic outlook. Borkenau did not really try to understand the social determination of cognition as some mechanistic reflective process.

Also during the 1930's professional historians of science for the most part rejected Borkenau's hypothesis. I think that there were two reasons for this - one material, the other ideological but the two closely connected with each other. The material one was that the early conceptions of the social history of science in general did not perform anything like the "*conceptual analysis*" - as it was called afterwards by I.B. Cohen - which was developed later on by Koyré and reached great heights. They remained silent in forms of conceptual analysis, first because, in line with their reductionistic approach, they either did not feel the jump leading *du monde de l'à peu-près à l'univers de l'exactitude* (Grossmann for example) or they remained content with the examination of the external conditions of scientific knowledge (Hessen for example: whether they solved their task correctly or not is not the problem at the moment) or examined the emergence of modern scientific attitude as world view and ideology only in very general terms (Borkenau). Besides, rejection had an ideological cause as well. In the circumstances of the mid-1930's, the thesis of the autonomy of mind and the autonomy of science to the liberal outlook seemed increasingly to be a part of the defence of human freedom. There followed the age of Mertonian norms and of *Logik der Forschung*, ensuring scientific work, Koyré's conceptual analysis, Hall's history of ballistics and of the *Society for*

Freedom of Science laying down the fundamental outlines of how to conceive of science and its history. It is characteristic that the belief in the autonomy of science development (at least as a part of the autonomous development of mind) not only belonged to the self-identification of a historian or a philosopher of science but has been defended in a rather aggressive way after the Second World War. The changed atmosphere of the 60's and 70's gave a place again to the socio-historical conceptions of the development of scientific cognition, too. Within Marxism, for example, in its Western versions, there is some sort of renaissance of Borkenau's thoughts as a *theoretical orientation*.[19] This trend at the same time sets the task that now relying for example on Koyré's achievements as its base, the defence of the materialist epistemological stance should be unified with the socio-historical explanation of the rise and development of scientific knowledge in a differentiated reflection.

In the light of the theses of under-determination and theory-ladenness respectively, and in that of the problems raised by present-day trends in the sociology of knowledge, the trends in the social history of scientific cognition of the period ranging up to the 30's showed a mistake common to all of them. Namely, they presumed - for this or that reason - that scientific cognition does have some *autonomous* and, because of that, *closed* law of development when already emerged. By this assumption they ruled out in advance the possibility of putting the question in terms of "microsociology", allowing that scientific cognition advances in evolving alternatives and the decision between them could be decided on the basis of micro-social atmosphere. In other words, not thinking of that possibility they excluded too early the possible "social history" of scientific cognition on the level of the fine structure. All the same, they are distinguished positively from present-day "microsociological" trends by the acknowledgements of social macrosystems having an impact on the development of scientific cognition as attitude. So they cannot be accused as some "microsociological" analyses today that they wanted to explain

such most general transformations of scientific thinking as that of the new epistemological attitude behind the emerging quantum mechanics reducing the social structure to local factors exclusively.

For the positivists, science only needed the *ceasing* of social hidrances, because the scientific attitude was seen as a natural one. For Scheler just as for Veblen, society was a *necessary positive* condition for the emergence of modern scientific cognitive attitude itself being not a natural one, but understood nevertheless as *the* objective one. Because such early writers as they were not disturbed yet by those serious problems as that of the discontinuous change of ontologies, the possibility of different rightful experiences, the production of alternatives within scientific cognition, they only saw their task in explaining the emergence of the positivistically-understood scientific objectivity, trying to find the positive social conditions of the already-understood cognition processes.[20]

Technical University
Budapest

Notes

1. In : *The Place of Science in Modern Civilisation and Other Essays*, New York, Russel and Russel, 1961.
2. See especially the first article mentioned.
3. His sociological point of view, i.e. that of cultural anthropology, is clear from his characterisation of "esoteric knowledge", the content and truth canons of which are determined by the system of social "institutions", and the owners of which are specialists (e.g. magicians or scientists). Further, a given culture ascribes a great inherent value to the given type of this knowledge which, when viewed from the inside, appears even to specialists as a system of fundamental and eternal truths. Its specialists aim at developing it in a basically conservative manner, while they

are themselves the products of "group life". See: *The Higher Learning in America*, New York, 1957, Sagamore. His cultural anthropology is embedded in a Darwinian approach. (Main categories of it are adaptation to the milieu and selection.)

4. Pragmatism of that time still rejected the possibility of cognition without practical ends.

5. We have no space to deal here with the peculiar and rather ambiguous character of the term "institution" in Veblen's writings.

6. Veblen assumes that, besides this it was the coming to the fore of small-scale trade and then monetary processes, generally the "economic organisation of society", which transformed thinking.

7. Veblen did not deal with the problem of constituting experience, or with the consequences of certain conventionality of language usage. For him, the difference between observation made in the age of savagery or barbarism and that made in modern science was reduced to the difference between right and wrong observation. Due to the fact that he assumes a certain kind of continuity in history (cf. the term "matter-of-fact generalisation"), he rules out in advance any fruitful question concerning the difference between possible correct empiries.

8. Charles Dickens: *Hard Times*. We think of the problem underlying the advice "Stick to the facts, sir." Veblen insists on the belief that there is good cause "to be restive under its [factual science's] dominion".

9. This justification of the in principle technological usefulness appears, e.g. in the light of the "finalisation" researches of the Starnberg group, as too general. According to Veblen, the "pragmatic interest" can only be a hindrance in the process of theoretical research. We should think of the differences in the cases of preparadigmatic, paradigmatic and postparadigmatic stages of research.

(As regards the Middle Ages, Veblen says that the matter-of-fact theories were accepted in the form of technological maxims. Obviously, he does not sense the qualitative difference between the technological knowledge of rules and scientific generalisation.)

10. Max Scheler: *Die Wissensformen und die Gesellschaft*. Pancke Verlag, Bern-München, 1960, originally 1925.

11. It is not the place here to criticise the mode of explanation of history using the naturalistic term of "instinct" and the overthrow of this naturalism by postulating the work of an "ethos".

12. Op. cit. p.92.

13. Op. cit. p.101.

14. Op. cit. p.122. This empasis laid on the emergence of new historical social actors is common in all early sociological approaches to the development of scientific cognition. But only Scheler and the Marxists expressed their ideas concerning the future.

15. Op. cit. p.125.

16. We would like to remind the reader that in Max Weber's view, in history there operates an unstoppable process of rationalization (disenchantment) which brings along a new form of subordination. Lukács in his *History and Class Consciousness* tried to find an answer to (among others) this question too, regarding the "bourgeois" form of rationalization described by Weber merely as a half-way stage which should and could be surpassed. We should keep in mind that the view claiming the formal rationality of scientific cognition of their time was common to Lukács and Max Weber; they differed in the manner of the "externalist" way of looking at science.

17. *Der Übergang...* Alcan, Paris, 1934.

18. *Die Gesellschaftlichen Grundlagen der Mechanistischen Philosophie und die Manufaktur*, Alcan, Paris, 1935, in: *Z. für*

19. We have no place here to praise the excellent article of J-P Chrétien-Gonie and Christian Lazzeri (*Cahiers STS*, "L'esprit du mécanisme, science et société chez Franz Borkenau", 1985, Paris, ed. CNRS) that I could read in the last moment before my lecturing in Berkeley.

20. R.K. Merton [1938] looked for these positive conditions when, beside technology's direct needs he attached importance to the puritan ethos, to previously accepted values in society. But, contrary to Scheler, he was convinced that the most important factor in the emergence of modern scientific thinking was its internal development.

THE PROBLEM OF SCIENCE IN LUKÁCS'S AESTHETICS

János Kelemen

(1) "It was not our aim to elaborate, even in outline, the epistemology and methodology of scientific thought."[1] This is what Lukács declares in his *Aesthetics*, refraining from any systematic exposition relating to the philosophy of science. In fact, though one may not be justified in considering Lukács even to be philosopher of science, his own reluctance seems to be of hardly more than stylistic value. This is just one of the ways in which he wishes to express that, at a given moment he cannot tackle that question in detail. Alternatively he may intend to say that his otherwise lengthy pronouncements on the subject are meant to be mere hints, and that his analyses have no claim to completeness, etc.

In spite of the particular limitation of scope implied already in its title, *The Specificity of the Aesthetic* contains much more than a theory of aesthetic consciousness. It appears clear to any reader of this work that the author has in mind a general theory of reflection, within the framework of which "the specificity of the aesthetic" is defined in comparison and contrast to other forms of reflection, such as everyday consciousness and science. Consequently, the "specificity" of science (or of the "scientific") is also part of the subject matter of Lukács's *chef d'oeuvre*.

But we can say more than this. When writing about science, Lukács raises problems which he repeatedly dealt with in his earlier periods, most thoroughly in *History and Class Consciousness*. It may be useful to recall that *History and Class Consciousness* touched upon such central questions in the philosophy of science as the methodology of the social sciences, the epistemological and methodological dualism of the natural and the social sciences, the relations between science and society,

philosophy and the special sciences and empirical material and theory. We also find passages on the nature of historical knowledge and that of scientific rationality. These are in addition to questions about the relationship between the subjective representations formed by those who participate in the occurrence of facts and scientific descriptions of those facts.

Compared to this substantial list, the themes relevant to the philosophy of science which are examined in *The Specificity of the Aesthetic* appear on a more modest scale. But it is more important to notice that the treatments given to the above list of questions in *History and Class Consciousness* form a coherent theory[2], one which is radically different from the theory expounded, also with considerable coherence, in the later *Aesthetics*. It must be noted that there is more than a simple difference between the two theories: we can discern a systematic transformation of the contents and functions of certain basic categories and therefore a moment of continuity is preserved with respect to the initial questions. It is not difficult to locate the axis of this transformation: clues will be found in Lukács's 1967 Preface to *History and Class Consciousness*. In an act of self-criticism, Lukács emphasizes there, among other places, that the view expressed in *History and Class Consciousness* was strongly affected by the absence of the category of labour[3] and the rejection of the theory of reflection.[4]

The 1967 Preface was written in the period which gave rise to the *Ontology*, and Lukács's allusion to the crucial role of labour is obviously explained by that stage of his development. But many of his analyses contained already in *The Specificity of the Aesthetic* were based on the category of labour (the theory of everyday consciousness, the elaboration of Pavlov's theory of first and second signalling systems, or the comparison of the objectifications of labour and of science all come to mind). The fundamental difference between *History and Class Consciousness* and *The Specificity of the Aesthetics*

no doubt stems, therefore, primarily from the application of the theory of reflection.

(2) Now, what does the difference between the two conceptions consist in? How is the theory of science which characterizes *History and Class Consciousness* later transformed?

If, in order to answer these questions, we survey the main theses of *History and Class Consciousness*, it appears clear that those theses are strongly characterized by a critical attitude towards science. The theory of science inherent in that work is a typically antipositivistic *critique of science*. In this respect it has many points in common with post-Kuhnian sociologically-biased theories of science.

Another important point to note is Lukács's *epistemological and methodological dualism*, about which a few remarks must be made immediately. This dualism is closely linked to a conception of the relationship between science and society which considers *the independence of the social sciences from the methodological ideal of the natural sciences* to be a specific characteristic of proletarian science. Lukács is convinced that the features of natural science are connected to the capitalist structure of society. From which he logically concludes that those who adopt the model of the natural sciences in the social sciences remain captives of the capitalist phenomenal world.

His statement that "capitalist society is predisposed to harmonize with natural scientific method"[5] causes no problem for Lukács as regards the value of natural scientific knowledge. His dualism essentially means that while the application of the cognitive ideal of natural science to nature yields adequate knowledge, the same ideal will inevitably yield defective knowledge when applied to the field of society. Any adequate knowledge of society presupposes an *autonomous social science* which, as is obvious to every reader of *History and Class Consciousness*, is only possible from the standpoint of the proletariat. It is also well-known to readers of *His-*

tory and Class Consciousness that the adequate knowledge of society as a totality expresses the possible or imputed consciousness of the proletariat. This is, at the same time, the *self-knowledge* of the proletariat. (Lukács generally considers, at least for the sphere of history and society, that knowledge is self-knowledge. At one point he actually states, "every place of historical knowledge is an act of self-knowledge".[6])

In a way, the same dualistic philosophy of science is expressed in the young Lukács's much discussed conception of *dialectic*. According to this he accepts social dialectic as a real characteristic of the historical process (here he emphasizes the categories of totality and contradiction), but he rejects the dialectic of nature. Also, when he refers to *"the point of view of totality"* instead of "the primacy of economic motives" as a principal feature of Marxism[7], he speaks of the societal sphere: totality, like contradiction, is a category of social being and social knowledge and, as such, the methodological cornerstone of proletarian science. Among the various aspects or consequences of the application of the category of totality, special attention is to be paid to the idea of a *"unified science"* which, naturally enough, is proposed with reference to the social sciences. For Marxism, "there is nothing but a single, unified - dialectical and historical - science of the evolution of society as a totality."[8]

This is no place to pass judgement on the conception summarized above in broad outline. Many questions are left open in it but, as demonstrated by recent discussions in the philosophy of science, it no doubt represents a fruitful approach. For all his later self-criticisms, the older Lukács did not have a totally negative opinion of *History and Class Consciousness*. Ernst Joós may well exaggerate when he says that Lukács is a "recidivist" who "retracts his errors only to confirm them in a different way"[9] but, nevertheless, Lukács's self-criticisms are often only partial. Lukács said in his later Preface to *My Road to Marx* that "some mistaken statements of this book were correct at the core."[10]

Does this also apply to statements related to the philosophy of science in *History and Class Consciousness*? We find a unequivocal answer to this question also in this Preface to *My Road to Marx*. Lukács, in 1969, found a "progressive tendency of anticipation" in his early work which conceived Marxism "exclusively as a theory about society". He states: "the dialectic of social development cannot be grounded scientifically in an approach which does not derive, historically and ontologically, the highest level of development (i.e. social being) from the philosophically necessarily simpler existential categories of natural being but, on the contrary, looks to the latter for a methodological model to establish the laws of the higher forms of being."[11] In 1969, therefore, Lukács rejects once more the establishing of "the laws of the higher forms of being" on the basis of the "existential categories of natural being" or, put another way, the adoption of the methodological model of natural science. The quotation, of course, echoes the words of the *Ontology* and is not unconditionally valid for that great work preceding it, the *Aesthetics*.

Indeed, *The Specificity of the Aesthetic* - at least at first sight - appears different as regards the relationship between natural and social science. It does not speak of *social science based on autonomous principles* of methodology and epistemology. It is also completely devoid of any *criticism of science*. These two missing areas, in whatever way we evaluate and interpret them, are closely interdependent.

(3) First of all, let us see how the earlier *critique of science* is transformed and what we find in its place. *History and Class Consciousness* has been shown to take as its point of departure primarily the connection between science and a definite social structure (capitalism). This also means that it does not examine the structure and validity of scientific theories on the epistemological level, i.e. it does not approach them from the logical and empirical conditions of their justification. Here the adequacy (truth) of a theory is entirely

determined by the standpoint defined by the given social structure. In a more suggestive formulation, the contents of a theory are not determined by the theory's relation to the objective sphere (by the mode of reflection) but by the subjective side, the point of view necessarily defined by the social position. Capitalism guarantees the bourgeoisie an unlimited, adequate grasp of nature but essentially denies it a proper view of social reality. The bourgeois point of view therefore yields an *a priori* false social science. On the other hand, the point of view of the proletariat is accompanied by an *a priori* correct form of societal knowledge and self-knowledge. Such a critique of science does not extend to natural science: it is *ab ovo* directed at social science and bourgeois social science at that. All this implies quite difficult questions. If, for example, there is such an inherent relationship between natural science and capitalism, how then, if at all, is adequate knowledge of nature possible in other social formations? If everything depends on point of view, is it possible for the proponents of different theories to engage in discussion at all? Can disagreements be settled according to some standard independent of the individual starting points? These are more or less familiar questions. The first one receives no answer in the line adopted by *History and Class Consciousness* and this is a major defect of the Lukácsian theory. The answer to the latter is that the competition and struggle between rival theories and, ultimately the competition and struggle between bourgeois and proletarian social science are not resolved according to epistemological criteria but depend on the outcome of the class struggle.

In contrast to the critique of science in *History and Class Consciousness*, *The Specificity of the Aesthetic* has as a *leitmotif* that *science*, owing to its essence and in an unrestricted way, is of a *humane* character. It has a humanizing effect. Such statements are mostly connected with a concept which is theoretically fundamental to the *Aesthetics* and which is entirely new compared with those in *History and Class Consciousness*. This concept is that of "*the desanthropomorp-*

hizing reflection of reality". This is, in short, the principle of "desanthropomorphism", which is "in its essence progressive and humane".[12] At this point, the *critique of science* is replaced by an *apology for science* in general.

The introduction of the concept of desanthropomorphism into his theory of science is a concrete consequence of Lukács's adoption of the theory of reflection. Within the framework of that theory, science is defined as a mode of reflection, as one of the necessary forms of the universal human capacity for reflection which develops from labour and which has its stable structural characteristics independent of the given social structure or point of view. These structural characteristics are described by the concept of desanthropomorphism. To harmonize with this, Lukács does not stress here the analogy between the working of capitalism and procedures of natural science. He declares that "Greek philosophy [...] found the definite, though in its details frequently modified methodological model of the reflection of nature."[13]

It is remarkable, and by no means accidental, that the elements of an apology for science emerge precisely in connection with desanthropomorphism. The earlier-postulated contrast between bourgeois and proletarian science is now replaced by the struggle between the general tendencies of antropomorphism and desanthropomorphism. In this context, scientific desanthropomorphism is made to appear as an absolutely positive principle while anthropomorphism although sometimes intruding into science, is presented as a force external and alien to it. The critique of science *qua* science can have simply no place. Lukács traces back modern critiques of science to the conceptual confusion which mistakes desanthropomorphism for dehumanization: "the resistance stemming from the world outlook against this principle of genuine science always focuses on the point that desanthropomorphism equals inhumanity."[14] "The less the ruling class is able to tolerate the true reflection of reality the more inhuman or anti-humanistic it describes science in its ideology."[15] To counter such kinds

of fake humanism, he is obliged to emphasize, not just once but repeatedly, that "The transformation through thought and sentiment of the world, viewed desanthropomorphically [...] does not mean the nihilistic or relativistic dehumanization of human reality."[16] Genuine humanism, on the other hand, characterizes science for two reasons. The first of these is that, from the objective side, the "desanthropomorphization of science" ensures man's mastery over the world of objects. The second reason is that, from the subjective side, the same desanthropomorphization becomes a means to make men better and richer. Lukács himself states that "the scientific attitude" "leads to the more fruitful exploration of reality and thereby makes men richer, more complex and more humane than they could be otherwise."[17] Apart from stating the general characteristics of the scientific attitude and standpoint, Lukács does not raise any concrete epistemological questions and therefore makes no special mention of epistemological criticisms and doubts concerning scientific development. There may be a simple reason to explain this. Lukács considers that the epistemological criticism of the reliability, verifiability or justifiability of scientific knowledge, theories or hypotheses is simply irrelevant in an age when "it is no longer possible to oppose a concrete, anthropomorphizing world outlook" to science.[18]

This is merely dogmatic trust in science - as Lukács's critics may justly argue. But however justified such criticisms may be, and however much it may be true that Lukács is excessively self-assured when ignoring the epistemological difficulties produced in the course of scientific development, his argument in defence of science undoubtedly conveys a positive message to us. The newly-fashionable relativism, scepticism and methodological anarchism in the philosophy of science correspond to trends which Lukács himself identified with great accuracy many years ago. We should be naive to think that the revival of those tendencies is only due to the epistemological problems which have surfaced in the debates

over the commensurability of scientific theories and the possibility of distinguishing science from non-science etc. Much more is at stake: a struggle is going on for the scientific world outlook, not just as a consequence of such and such a development in the philosophy of science but, as Lukács saw correctly, as a permanent and necessary phenomenon of social and intellectual development. Epistemological difficulties often serve only as *casus belli*.

(4) It was mentioned earlier that the second missing element in *The Specificity of the Aesthetic* is its neglect of the idea of *autonomous social science* or, in other words, the rejection of the dualistic philosophy of science as expressed in *History and Class Consciousness*. Science in the later Lukács's work is a unified and indivisible form of consciousness which applies the same principle, namely desanthropomorphism, in reflecting both society and nature. This is a way of maintaining continuity - beyond the rupture - with the body of themes contained in *History and Class Consciousness*. Lukács never abandoned the principle of totality, which for *The Specificity of the Aesthetic* implies that the requirement of "a single, unified science" earlier restricted to the social sciences has now to be extended to science generally. Lukács states that, "in its tendency, [...] there is only one science, one approach from all sides to the uniform, objective world in itself."[19]

We should notice here that the requirement of "a single, unified science" is not only valid for Marxism at the moment. It is formulated as a principle without restriction and, as such, becomes the criterion of all science. For, as opposed to art, the specific feature of the scientific is that the individual sciences and branches of science form a unity in spite of their relative differences. That is to say, they are united in one overall picture of the totality of reality. Lukács expresses this view, stating that, contrary to the aesthetic sphere where individual works of art form a closed

world and the homogeneous medium of the work of art represents "something unique and ultimate", "the homogeneous medium of scientific reflection [...] is uniform for every branch of science."[20] In short, the totality of the sciences presupposes one single homogeneous medium.

All this amounts to the really basic *requirement of coherence* which states that the individual items of scientific knowledge must lend themselves to continuation, completion and criticism in the light of other items. No piece of scientific knowledge is self-sufficient, and each can have a claim to validity only as part of the whole system of knowledge. The "totality requirement of epistemology" concerns the sciences as a whole, not the individual sciences, and especially not particular scientific theories. At this point it is quite clear how the principle of totality (which characterizes an earlier period) and the theory of reflection are linked together. The justification of the above-mentioned requirement of coherence lies in the fact that the thing in itself - understood as objective reality - is also a totality and therefore, "from a strictly epistemological point of view", "only the 'totality for us' developed into a synthesis can count as the concrete antipole of the thing in itself."[21]

Of course, there are many arguments for the Lukácsian idea of a unified science. But is it not one of its consequences that the specificity of the social sciences is effaced? Some emphatic remarks by Lukács suggest an answer in the affirmative.

Lukács, whose last message was the ontology of society and who was a powerful proponent of the point of view of Praxis wrote in his *Aesthetics*: "The essential characteristic in common is that what is studied is always the objectiveness of reality existing independently of man. Even if man himself is made the subject matter of biological or socio-historical investigation, the aim - in the final analysis - is to explore such objective 'Gegenständlichkeiten', or processes."[22] In other words, the human sciences also perform desanthropomorphizing reflection. It is not necessarily paradoxical to speak

about desanthropomorhpism in connection with the human sciences but it is not unproblematic either. However, Lukács does not refer to any problem presenting itself to him at this point. He only says that the contradictory nature of social being "makes it difficult for bourgeois thought to apply to the social sciences concretely and fruitfully, the theory of desanthropomorphizing reflection"[23] Thus the social sciences have as their only specific feature, which does not in the least stem from their subject matter, "that in bourgeois society the desanthropomorphizing methods can only be applied to the social sciences with restrictions."[24]

Such a limited possibility for desanthropomorphism leaves only two ways open for bourgeois thought. These are either "the solidifying into lifeless formalism" of the socio-historical process or the "irrationalization" of historical life.[25] Here Lukács is right, as it is testified by the history of science, But is the range of the problems of the social sciences or of human sciences exhausted by the impossibility of complete desanthropomorphization? Apart from this contingent determination, external to science, is there not an aspect of the subject-matter and goals of science itself which hinders the application of the desanthropomorphizing point of view in principle? It is quite interesting that Lukács mentions few examples from the social sciences. In fact, he refers almost exclusively to economics as a standard example of the unifying process and of desanthropomorphizing thought. It is clear that he did not fully carry out his investigations in this field. We must remember that, in *The Specificity of the Aesthetic*, Lukács separated dialectical from historical materialism, however much he emphasized their active interrelation, and then never went on to write what he intended to be his section on historical materialism. No-one can know which direction his theory of science would have taken in that unwritten part and, for example, what place he would have assigned to the hermeneutical methods of "understanding" which are difficult to include within the category of desanthropomorphism.

(5) From what Lukács did in fact write, we can infer that he could not see a difference between the subject-matters of the natural and the social sciences that would define the structures of these two spheres of science. This is ultimately in accordance with his general philosophical point of departure, that is the interpretation of the material unity of the world in such a way that does not approach the difference between the aesthetic and the scientific spheres from the object of reflection either: "If [...] we want to examine the differences between the reflections realized in everyday life, science and art, we must constantly keep it in mind that all three forms represent the same reality."[26] That is to say, Lukács would firmly reject the Kuhnian idea that adherents of the different scientific paradigms are not describing the same world, or "are not working in the same world."[27]

Naturally enough, it is a basic requirement for every kind of materialism to recognize that the world is not only of a material but also of a uniform character, and that it is ultimately one and the same for everyone irrespective of one's subjective relationship to it. This is, however, only a requirement in the final analysis and which cannot obscure the fact that the world as objectivity is not given by itself. Lukács, of course, in no way wishes to deny the active nature of reflection. But, nevertheless, throughout the elaboration of his theory of reflection, he pays more attention to the *manner* of reflection (desanthropomorphism, anthropomorphism) than to its *object*. He does not expound the thesis that the active nature of reflection (in our case, of scientific knowledge) means more than the active construction of the image of the object. Science also creates the object of cognition,[28] this act being part of the constitution of the object, not just a mere objective precondition of the cognitive process. The different ways in which the particular sciences constitute their objects produce differences in the objects of reflection or knowledge. That is why one cannot stay with the statement that "the object of all reflection is this unique and uniform reality"[29]

and that in everyday thinking, science and art reflect the same contents.[30]

The main line of the reflection theory expounded in the *Aesthetics* is no doubt the opposition, or "precise separation of the objective and the subjective".[31] In his overview of the history of science, Lukács also finds the main tendency to be the development of this separation. As we have seen, he extends the validity of all this to the social sciences as well. Thus "the identity of subject and object" claimed in *History and Class Consciousness* disappears. Scientific knowledge cannot then be interpreted as self-knowledge even in the fields of social and historical knowledge. This is because social sciences have to face an object in itself through desanthropomorphization in the same way as the natural sciences.

But the concept of "knowledge as self-knowledge" does not disappear altogether: it is transformed and transposed to the sphere of artistic reflection. It is true that Lukács loosens the relation of "identical subject and object" in the aesthetic sphere as well, and expects mimesis to "reflect the reality independent of human consciousness". However, it remains one of the principal messages of his work that "art is the most adequate mode of expression for the highest order of man's self-consciousness."[32] It is beyond the scope of the present analysis to deal with the intricate questions arising at this point. Even some problems which are more pertinent to the philosophy of science have to be omitted for lack of space.

The few analyses which have been presented in this paper seem to warrant the following conclusions. An immediate consequence of his adopting the theory of reflection is Lukács's abandonment of dualism in the philosophy of science. The idea of a unified science covering the *whole of science* imposes the requirement of objectivity which the category of desanthropomorphism is designed to express. Consequently, the contrast between bourgeois and proletarian ideology and, in general, between the ideologies of the reactionary and the progressive classes does not appear within science but in the opposition of science to non-science, or desanthropomorphism and anthro-

pomorphism. At the same time, the idea of a unified science is not cast in a form which would help to clarify the obviously specific features of the social sciences. The category of desanthropomorphism seems to be insufficient to settle this problem. Furthermore, it is an important point that social science and historical knowledge can no longer be conceived as self-knowledge - in the same way as the consciousness of the proletariat is no longer identical with the self-knowledge of the totality. The function of self-knowledge or self-consciousness is transferred to the arts but, even so, here the subject is not a class but the whole of mankind.

The scientific, which corresponds to desanthropomorphism universally characterizing the whole of science, becomes a fundamental value in the *Aesthetics*. Unlike all other critiques of sciences, this gives rise to a pathetic apology for science. Apart from recommending the acceptance of this pathos as a lasting element in the Lukácsian heritage, we must underline one thing: the apology for science and the scientific does not imply an uncritical attitude. According to Lukács, desanthropomorphism must be extended to both the subject and the object. The desanthropomorphization of the subject is nothing but perpetual self-control and self-criticism. It is the attitude of the subject towards reality which permits him to practise "incessant control over his own outlook, ideas and concept formation".[33]

Loránd Eötvös University
Institute of Philosophy
Budapest

Notes

1. Georg Lukács, *Die Eigenart des Ästhetischen*. Luchterhand, Neuwied, 1963, Vol.1, p. 200.
2. An analysis of that theory is found in J. Kelemen, "Philosophy of Science and Its Critique in *History and Class*

Consciousness", a paper read in Ferrara, 2nd May 1985, repr. in: DOXA 4

3. Georg Lukács, *History and Class Consciousness*. Cambridge, Mass.: The MIT Press, 1969, p. XVIII.
4. Ibid., p. XXV.
5. Ibid., p.7.
6. Ibid., p.237.
7. Ibid., p.27.
8. Ibid., p.28.
9. Ernest Joós, *Lukács's Last Autocriticism*. Atlantic Highlands, N.J.: Humanities Press, 1983, p.13.
10. György Lukács, *Utam Marxhoz*. Magvetõ Kiadó, Budapest, 1971. Vol.1, p.22.
11. Ibid.
12. Georg Lukács, *Die Eigenart des Ästhetischen*. Vol.1, p.197.
13. Ibid., Vol.1, p.145.
14. Ibid., Vol.1, pp.174-175.
15. Ibid., Vol.1, p.167.
16. Ibid., Vol.1, p.177.
17. Ibid., Vol.1, p.158.
18. Ibid., Vol.1, p.166.
19. Ibid., Vol.1, p.181.
20. Ibid.
21. Ibid., Vol.2, p.290.
22. Ibid., Vol.1, p.181.
23. Ibid., Vol.1, p.199.
24. Ibid., Vol.1, p.202.

25. Ibid., Vol.1, p.199.

26. Ibid., Vol.1, p.35.

27. Thomas Kuhn, *The Structure of Scientific Revolutions*. Chicago-London: University of Chicago P., 1970, p.150.

28. Among the many analyses bringing out this point let us recall Althusser's distinction between the "real object" and the "object of knowledge". Louis Althusser-Etienne Balibar: *Reading Capital*. London: NLB, 1970, p.41.

29. Georg Lukács, *Die Eigenart des Ästhetischen*. Vol.1, p.35.

30. Ibid., Vol.1, p.55.

31. Ibid., Vol.2, p.278.

32. Ibid., Vol.1, p.617.

33. Ibid., Vol.1, p.146.

TRADITION AND PRACTICAL KNOWLEDGE

Kristóf J. Nyiri

The title of the present paper being what it is, my first task is to indicate how the topic of *practical knowledge* might involve, or why it should involve, an analysis of the notion of *tradition*. Such an indication is in fact not difficult to give. After all, both practical knowledge and knowledge embedded in tradition are kinds of knowledge that seem to lie outside the domain of reflection or reasoning. Both presuppose an epistemological subject whose activity encompasses more than the life of pure cognition - a subject to whose make-up traits other than mental essentially belong. No wonder, then, that philosophers concerned with the practical dimension of knowledge will usually draw attention to the special ways in which that dimension is transmitted. They will examine the customs and institutions concerned with this, in other words, tradition.

In this connection, Ryle stresses that learning *how* is different from learning *that*: the former involves, as the latter does not, inculcation,[1] i.e. persistent, inprinting repetition. In a similar vein, Michael Polanyi, after having argued that the rules of scientific discovery are but *rules of art*, goes on to point out that since "an art cannot be precisely defined, it can be transmitted only by examples of the practice which embodies it."[2] Science, Polanyi writes at another place, "is operated by the skill of the scientist"[3], by a skill that, again, can only be passed on by example. But to learn by example is "to submit to authority... By watching the master and emulating his efforts in the presence of his example, the apprentice unconsciously picks up the rules of the art, including those which are not explicitly known to the master himself. These hidden rules can be assimilated only by a person who sur-

renders himself to that extent uncritically to the imitation of another", by a person who will "submit to tradition".[4]

Oakeshott, too, points out that "the coherence of scientific activity" does not "lie in a body of principles or rules to be observed by the scientist, a 'scientific method'"; that coherence, he stresses, "lies nowhere but in the way the scientist goes about his investigation, in the traditions of scientific inquiry".[5] And one of the main claims of T.S. Kuhn is, of course, that "we have too long ignored the manner in which knowledge of nature can be tacitly embodied in whole experiences without intervening abstraction of criteria or generalizations. Those experiences are presented to us during education and professional initiation by a generation which already knows what they are exemplars of."[6] This seems also to be the idea taken up by David Bloor when he writes (referring, incidentally, to Mary Hesse's *Structure of Scientific Inference*):

> [P]redicates are learnt on the basis of a finite number of instances. These are provided by teachers or authorities who must simultaneously inform and control the behaviour of the learner. The learner's task is to acquire a sense of the similarity between the cases to which he is exposed as instances of a given concept. His sense of similarity and difference must be matched to those of other language users. This involves grasping the *conventions* which are involved in the judgements about similarity and difference.[7]

Even Feyerabend, having in *Science in a Free Society*, once more made his peace with Wittgenstein, writes of "standards or rules" we could not use were they not "well integrated parts of a rather complex and in places quite opaque practice or tradition".[8] As to Wittgenstein himself, one need recall only the central role his arguments played in turning into a philosophical issue the idea of knowledge embedded in, or constituted by, practice. When von Wright, interpreting Wittgenstein's *On Certainty*, coined the notion of "pre-knowledge", remarking of course that the same "is not propositional knowledge" but rather a *praxis*,[9] philosophers were quick to point out that the appropriate term here was not "pre-" knowledge,

but, precisely, *practical knowledge*.[10] I would like to underline that in those arguments of Wittgenstein in which the idea of practical knowledge essentially figures, the concept of *tradition*, too, inevitably presents itself. It finds its expression in the terms *Gepflogenheit*, *Gebrauch*, *Institution*, *Lebensform*, or *Autorität*.[11]

My point of departure is, then, roughly as follows. Since practical knowledge encompasses, or serves as a foundation for, much of what we know, and since such knowledge appears to be tacit, non-propositional, and indeed inarticulable,[12] channels of communication other than explicit discourse have indispensable functions to fulfil. *Traditions* represent just such channels. That this initial position immediately leads to a number of questions, is clear; and the most this paper can do is to indicate the rudiments of a strategy for approaching these questions.

The first difficulty is presented by the notion of practical knowledge itself. *Skills* are, or embody, such knowledge, but not all skills presuppose a social context. Take cycling, one of Polanyi's favourite examples.[13] Cycling involves a vast amount of tacit knowledge in the sense that the mathematical description of what happens at every moment whilst one adjusts the curvature of one's bicycle's path in proportion to the ratio of one's inbalance over the square of one's speed is, of course, unknown to the cyclist, and would not help him in his performance even were it known. But I don't see what is, in principle, inarticulable about this knowledge; and I certainly cannot recall anything like a state of apprenticeship when learning to ride my first bicycle. I saw what other people were doing, but I did not learn by imitating them, I learnt by constantly falling off, and then by sometimes not falling off. It seems there are *technical* skills - like cycling - and *social* skills - like counting - and the former do not presuppose a tradition in the immediate sense in which the latter do. Or take medical diagnosis, another of Polanyi's examples. "Unless a doctor can recognize certain symptoms",

writes Polanyi,

> e.g. the accentuation of the second sound of the pulmonary artery, there is no use in his reading the description of syndromes of which this symptom forms part. He must personally know that symptom and he can learn this only by repeatedly being given cases for auscultation in which the symptom is authoritatively known to be present, side by side with other cases in which it is authoritatively known to be absent, until he has fully realized the difference between them and can demonstrate his knowledge practically to the satisfaction of an expert.[14]

It was similar, or related, observations that led Ludwik Fleck in the early Thirties to his traditionalist, pre-Kuhnian theory of science. Thus in his explanations of the Wassermann reaction, Fleck stresses that the "reaction occurs according to a fixed scheme, but every laboratory uses its own modified procedure, which is based upon precise quantitative calculations; nevertheless, the experienced eye or the 'serological touch'" - *das 'serologische Fühlen'* - "is much more important than calculation."[15] The field of serology, Fleck writes, "is a little world of its own and therefore can no more be fully described in words than any other field of science."[16]

It is however a fact that important areas of medical diagnosis are today conducted by means of computer programmes. It would seem strange to speak of "personal knowledge" or "touch" with respect to a piece of software. Of course, these programmes are based on the knowledge of experienced human experts, and indeed it is quite a problem to present that knowledge in software-digestible form. Two computer specialists write:

> Human experts have acquired their expertise not only from explicit knowledge found in textbooks and lectures, but also from experience: by doing things again and again, failing, succeeding, wasting time and effort, then learning to save them, getting a feel for a problem, learning when to go by the book and when to break the rules. They therefore build up a repertory of working rules of thumb, or "heuristics", that, combined with book knowledge, make them expert practitioners.[17]

Heuristic knowledge "is the knowledge of good practice and good judgement in a field". It is "hardest to get at because experts - or anyone else - rarely have the self-awareness to recognize what it is. So it must be mined out of their heads painstakingly, one jewel at a time."[18] But now, tacit knowledge as here described does not seem to possess any philosophically interesting characteristics at all, and it is quite disturbing to realize that the *faculty of judgment*, the ability to subsume particular instances under a given rule, or the ability to *apply rules*, can be imparted to a suitable machine without further ado, without extended training on the learner's side, without the full social context that seemed so essential for this kind of acquisition. After all, for Kant already the *application of rules* seemed to embody a specific philosophical problem:

> If understanding in general is to be viewed as the faculty of rules, judgment will be the faculty of subsuming under rules; that is, of distinguishing whether something does or does not stand under a given rule... General logic contains, and can contain, no rules for judgment. ... If it sought to give general instructions how we are to subsume under these rules, that is, to distinguish whether something does or does not come under them, that could only be by means of another rule. This is turn, for the very reason that it is a rule, again demands guidance from judgment. And thus it appears that, though understanding is capable of being instructed, and of being equipped with rules, judgment is a peculiar talent which can be practised only, and cannot be taught. It is the specific quality of so-called motherwit... Deficiency in judgment is just what is ordinarily called stupidity, and for such a failing there is no remedy. ... A physician, a judge, or a ruler may have at command many excellent pathological, legal, or political rules, even to the degree that he may become a profound teacher of them, and yet, none the less, may easily stumble in their application. For, although admirable in understanding, he may be wanting in, natural power of judgment. He may comprehend the universal *in abstracto*, and yet not be able to distinguish whether a case *in concreto* comes under it. Or the error may be due to his not having received, through examples and actual practice, adequate training for this particular act of judgment. Such sharpening of the judgment is indeed the one great benefit of examples.[19]

Ryle, too, stresses that *stupidity* is not the same thing as ig-

norance, pointing out that:

> The consideration of propositions is itself an operation the execution of which can be more or less intelligent, less or more stupid. But if, for any operation to be intelligently executed, a prior theoretical operation had first to be performed and performed intelligently, it would be a logical impossibility for anyone ever to break the circle.[20]

Similar infinite regress arguments play a central role in Wittgenstein's later philosophy,[21] nor are they missing from Polanyi's writings - "The application of rules must always rely ultimately on acts not determined by rule"[22] - or, for that matter, from F.A. von Hayek's:

> there will always be some rules governing a mind which that mind in its then prevailing state cannot communicate, and ... if it ever were to acquire the capacity of communicating these rules, this would presuppose that it had acquired further higher rules which make the communication of the former possible but which themselves will still be incommunicable.[23]

But it is exactly this infinite regress argument, seemingly so central to all philosophizing about practical knowledge, which somehow loses its magic once the nature of knowledge built into artificial intelligence expert systems has been considered.

Or take the case of Ryle's "well-trained sailor boy", who "can both tie complex knots and discern whether someone else is tying them correctly or incorrectly, deftly or clumsily. But he is probably incapable of the difficult task of describing in words how the knots should be tied."[24] Knots are more easily tied than explained, but the boy's presumed inability to do the latter does not seem to carry a philosophical message. He might be unable to explaing *anything*. Or a detailed terminology of knots could be developed, helped by which the boy would have no difficulties at all in describing and criticizing. Of course, the usual way to explain tying knots is through *pictures* rather than through words. And here one should perhaps say that though knowledge conveyed through pictures might be non-propositional, it does not therefore necessarily

follow that it is practical, i.e. non-theoretical, in the sense of the present paper.

It might be useful, at this stage, to distinguish between two positions with regard to the issue of practical knowledge. According to the first, this knowledge is a *practical abbreviation* within the texture, or flow, of knowledge as such; a device of paramount pragmatic importance perhaps, but not something the discovery of which should basically transform our epistemological convictions. According to the second position, there is a layer, or dimension, of practical knowledge which could in no sense be dissolved into knowing *that*. Or perhaps - and this would be a stronger version of the same position - there is a hard layer of practical knowledge which serves as the bedrock upon which *all* knowledge rests. Or indeed - to formulate a yet stronger version - all theoretical knowledge represents but an articulating, a spelling out, of a knowledge which is, in the last analysis, invariably reducible to practice. Philosophers like Wittgenstein, Oakeshott, or Kuhn clearly support some version of the second position; but Ryle, too, flatly states that "theorising is one practice amongst others".[25]

Now, each of these positions has its counterpart within the theory of traditions. Let us distinguish between *primary* and *secondary* traditions, and say that secondary traditions contain and convey, in an abbreviated and perhaps emotionally bolstered form, information which could in principle, though perhaps only with a loss of convenience, be communicated also in a purely discursive fashion. The information embedded in primary traditions, on the other hand, cannot be separated from the way in which it is handed down, or rather it can be so separated only within a context different in kind from that in which these traditions were originally functioning. In other words, secondary traditions can be dissolved without essentially impairing that activity the transmission of which they serve; primary traditions cannot. The thesis to the effect that there *are* primary traditions, a thesis to which I sub-

scribe, I shall call the *strong traditionalist thesis*, and contrast it with the *weak traditionalist thesis* which denies the existence of primary traditions but recognizes the existence, and usefulness, of secondary ones. The position *denying* this usefulness might then properly ba called *anti-traditionalist*. I take the hard-core view of practical knowledge to imply, and be implied by, the strong traditionalist thesis. In what follows I will, very briefly, call attention to some of the issues which have bearing on this thesis; before doing that, however, I would like to touch upon two other, closely related topics.

The first is rationality. Reason and tradition are usually conceived of as opposed,[26] and even traditionalist arguments are often phrased in such a way as to maintain this opposition. The power of the irrational - or of the arational - is stressed, along with the importance of traditions, as creating a dimension of coherence in the non-rational realm, as bringing, through their very irrationality, cohesion into society. It is in this sense that Karl Popper, quite a traditionalist in his way, writes:

> What we call social life can exist only if we can know, and can have confidence, that there are things and events which must be so and cannot be otherwise. - It is here that the part played by tradition in our lives becomes understandable. We should be anxious, terrified, and frustrated, and we could not live in the social world, did it not contain a considerable amount of order, a great number of regularites to which we can adjust ourselves. The mere existence of these regularities is perhaps more important than their peculiar merits or demerits. They are needed as regularities, and therefore handed on as traditions, whether or not they are in other respects rational or necessary or good or beautiful or what you will. There is a need for tradition in social life.[27]

Now if the strong traditionalist thesis holds, this way of formulating the matter is misleading. For this thesis implies that reason itself is ultimately grounded in traditions, or, as Oakeshott eloquently puts it: "'Rationality' is the certificate we give to any conduct which can maintain a place in the flow of symphathy, the coherence of activity, which composes a way of

living."[28] It will not do to regard rationality, as Feyerabend does, as "one tradition among many rather than a standard to which traditions must conform",[29] since this would still amount to an unjustifiable picking out of some single tradition. A formula is needed which will preserve our intuitive grasp of what "rational" amounts to, without however introducing any arbitrary criteria. I think Oakeshott comes close to finding such a formula when, after writing that "no conduct, no action or series of actions, can be 'rational' or 'irrational' out of relation to the idiom of activity to which they belong", he goes on to state that "an activity as a whole (science, cooking, historical investigation, politics or poetry) cannot be said either to be 'rational' or 'irrational' unless we conceive all idioms of activity to be embraced in a single universe of activity."[30]

But the author who, in my opinion, really pointed the way here, even if for sixty years no one seems to have embarked upon it, was Maurice Halbwachs, with his *Les cadres sociaux de la mémoire*.[31] "Reason", Halbwachs wrote, "is actually a striving to raise oneself from a narrower to a broader tradition, into which latter the memories not merely of one class, but those of all groups will fit. ... Reason faces tradition as a broader society faces a narrower one."[32] The tradition capable of absorbing a variety of other traditions, or the tradition that emerges as an amalgam of various particular ones, will then possess, or amount to, what might be called *relative rationality*; and of course all rationality is relative.

The second topic I feel should be touched upon in the present context is the relation between traditionalism and the philosophy of mind. It seems to me that the strong traditionalist thesis is simply incompatible with what is usually called mentalism or intellectualism: the view of an autonomous, sovereign mind, of a mind intimately acquainted with, and freely operating upon, its own contents (images, concepts, and the like), a mind for which language, in particular, is a mere instrument of communication, an external vehicle expressing, and indeed guided by, inner thought-processes.

Wittgenstein and Ryle are of course well-known critics of this view, but their arguments are seldom taken notice of by traditionalist writers, generally insensitive to the epistemological presuppositions and implications of their position. Two notable exceptions were Edmund Burke and T.S. Eliot, who did indeed realize those implications. In his essay "Tradition and the Individual Talent", Eliot wrote:

> The point of view which I am struggling to attack is perhaps related to the metaphysical theory of the substantial unity of the soul: for my meaning is, that the poet has, not a "personality" to express, but a particular medium, which is only a medium and not a personality, in which impressions and experiences combine in peculiar and unexpected ways. ... The emotion of art is impersonal. And the poet cannot reach this impersonality ... unless he lives in what is not merely the present, but the present moment of the past...[33]

And as to Burke, he not only had a theory of traditions, but in fact the rudiments of a theory of meaning to match the former. Examining the "common notion", according to which words "affect the mind by raising in it ideas of those things for which custom has appointed them to stand", Burke does "not find that once in twenty times" any such idea or "picture" is formed, and indeed when it is, "there is most commonly a particular effort of the imagination for that purpose. Burke gives a charming example. "Suppose", he writes,

> we were to read a passage to this effect: "The river Danube rises in a moist and mountainous soil in the heart of Germany, where, winding to and fro, it waters several principalities, until, turning into Austria, and laving the walls of Vienna, it passes into Hungary; there with a vast flood, augmented by the Save and the Drave, it quits Christendom, and rolling on the barbarous countries which border on Tartary, it enters by many mouths into the Black Sea." In this description many things are mentioned, as mountains, rivers, cities, the sea, &c. But let anybody examine himself, and see whether he has had impressed on his imagination any pictures of a river, mountain, watery soil, Germany, &c. Indeed it is impossible, in the rapidity and quick succession of words in conversation, to have ideas both of the sound of the word, and of the thing represented; ... nor is it necessary that we should.

In the ordinary course of conversation, Burke concludes, "we are sufficiently understood without raising any images of the things concerning which we speak."[34] This is, clearly, an approach to meaning which does not presuppose or suggest mentalist views; it is compatible with the idea of language as an essentially social institution; it is, in particular, compatible with the strong traditionalist thesis.

Returning now to a brief examination of this thesis itself, we have to take into account, first of all, that the term "tradition" is surrounded by a family of related terms. This family would include terms like "authority", "convention", "custom", "disposition", "habit", "institution", "mentality", "mode", "mores", "norm", "paradigm", "practice", "prejudice", "rule", "style", "taste", "technique". The interconnections within this family are far from unequivocal, the meanings of most of the terms vary and overlap. Clearly, both a survey of connotations and a list of stipulations is called for.

To acquire a foretaste of the endeavour that seems to be necessary here, let us consider, first, the term "authority". According to Halbwachs, it is traditions which confer authority upon certain roles and persons.[35] Polanyi, on the other hand, stresses that only by "*a previous act of affiliation*", by a "combined action of authority and trust", will the assimilation of basic traditions become possible at all.[36] Wittgenstein writes that one has "learned an enormous amount and accepted it on human authority",[37] he asks if it is not the case that one "must recognize certain authorities in order to make judgments at all",[38] and seems to suggest a certain parallel between authority and tradition when declaring: "Tradition is not something a man can learn; not a thread he can pick up when he feels like it; any more than a man can choose his own ancestors."[39]

Or take the term "convention". For Hume and for Burke this notion was related rather than opposed to that of tradition. As Wilkins has put it:

> .Social conventions such as rules for the acquisition and transmission of property are artificial in the sense of being man-made, but given man's social nature and the mutual dependence of men there is a sense in which they are natural as well. The important thing for understanding both Hume and Burke is their general refusal to equate artificial with arbitrary.[40]

In a rather different context, in the domain of the philosophy of science, Fleck, too, strives to show that the element of arbitrariness has no primary role to play in the connotation of the term "convention". He speaks of the "cultural-historical dependence" of the "alleged epistemological choice - the alleged convention", stressing "how little such conventions, which from the point of view of logic may seem equally possible, are in fact felt to be of equal value".[41] And in the domain of the philosophy of art it is e.g. Arnold Hauser who draws a close terminological parallel between convention and tradition. "[S]pontaneity and convention, originality and tradition", he writes, are

> inseparable from each other... [E]very work, every form, and even the minutest attempt at expression ... are always the result of a conflict between spontaneity and convention, originality and tradition... The process is not one in which spontaneous personal experiences become communicable and accessible only through conventional forms, but one in which the experiences to be depicted move from the outset along conventionally regulated lines. ... Artistic expression comes about not in spite of, but thanks to, the resistance which convention offers to it.[42]

Clearly, Hauser is a strict traditionalist as far as the issue of artistic creativity goes, but it is the term "convention", not the term "tradition", that carries the weight of his argument. The connotations of "convention" are however no less blurred than those of "tradition". And here, most modern authors would seem to agree with Halbwachs e.g., for whom convention means *free agreement*: he contrasts the "purely conventional" with the "purely traditional".[43]

Or consider, again, the next term on our list, "custom". It is a term extremely rich in meanings. Burton Leiser in his book on the subject lists at least nine main ones, ranging from mere *habits*, through sanctioned *regulations*, to so-called *constitutive rules*, rules which, by their very definition, could not be broken.[44]

Before turning now to the term *tradition* proper, let me select one more term from that list of related notions, namely the term "prejudice". It was in connection with this term that Burke formulated one of his most often-quoted passages. "Instead of casting away all our old prejudices", Burke wrote,

> we cherish them to a very considerable degree; and, to take more shame to ourselves, we cherish them because they are prejudices... Many of our men of speculation, instead of exploding general prejudices, employ their sagacity to discover the latent wisdom which prevails in them. If they find what they seek, (and they seldom fail,) they think it more wise to continue with the prejudice, with the reason involved, than to cast away the coat of prejudice, and to leave nothing but the naked reason; because prejudice, with its reason, has a motive to give action to that reason, and an affection which will give it permanence. Prejudice is of ready application in the emergency; it previously engages the mind in a steady course of wisdom and virtue, and does not leave the man hesitating in the moment of decision, skeptical, puzzled, and unresolved. Prejudice renders a man's virtue his habit, and not a series of unconnected acts. Through just prejudice, his duty becomes a part of his nature.[45]

Note Burke's reluctance to "leave nothing but the naked reason", a reluctance characteristic of the strong traditionalist attitude; but note also the concluding reference to "just" prejudice, with its implication that not all prejudices are just. And it is of course the idea of the unjust, the malign, prejudice which constitutes the generally accepted meaning of this term. It is in this sense that Ernst Mach could speak of "the fetters of inherited prejudice",[46] or of the "terrible power" of what we call - as the translation puts it - "prejudgment or prejudice", i.e. "habitual judgment, applied to a

new case without antecedent tests".[47] But even Mach, definitely no traditionalist, concedes that without certain "fixed habitudes of thought"[48] new problems would not become perceivable at all. "No one could exist intellectually", Mach writes,

> if he had to form judgments on every passing experience, instead of allowing himself to be controlled by the judgments he has already formed. ... On prejudices, that is, on habitual judgments not tested in every case to which they are applied, reposes a goodly portion of the thought and work of the natural scientist. On prejudices reposes most of the conduct of society. With the sudden disappearance of prejudice society would hopelessly dissolve.[49]

Of the term "tradition", the *Oxford English Dictionary* provides some excellent definitions. Tradition, it says, is the "action of handing over (something material) to another; delivery, transfer." It is the delivery, "*esp.* oral delivery, of information or instruction". It is the "act of transmitting or handing down or fact of being handed down, from one to another, or from generation to generation; transmission of statements, beliefs, rules, customs, or the like, esp. by word of mouth, or by practice without writing." It is, also, that "which is thus handed down; a statement, belief, or practice transmitted (esp. orally) from generation to generation". "More vaguely", the *OED* goes on, a tradition is a "long established and generally accepted custom, or method of procedure, having almost the force of a law; an immemorial usage".

Clearly these explications, however apt, do not solve our theoretical problems, partly since the explanatory terms they employ - "handing down", "rule", "custom", "practice", "law" - themselves stand in need of elucidation, and partly because, as I tried to show in the foregoing, a host of yet other notions would seem to be of relevance here. Obviously, a nominal explanation of the concept of tradition, though necessary, is not sufficient. Especially not if it actually fails to rise

above, or indeed falls below, the dictionary level, as when Edward Shils writes:

> Tradition means many things. In its barest, most elementary sense, it means simply a *traditum*; it is anything which is transmitted or handed down from the past to the present. It makes no statement about what is handed down or in what particular combination or whether it is a physical object or a cultural construction; it says nothing about how long it has been handed down or in what manner... The degree of rational deliberation which has entered into its creation, presentation, and reception likewise has nothing to do with whether it is a tradition. ... Tradition - that which is handed down - includes material objects, beliefs about all sorts of things, images of persons and events, practices and institutions. It includes buildings, monuments, landscapes, sculptures, paintings, books, tools, machines..., practices and institutions made up of human actions.[50]

Rather more interesting are *particular* definitions like the one Hobsbawm gives of "invented" traditions:

> "Invented tradition" is taken to mean a set of practices, normally governed by overtly or tacitly accepted rules and of a ritual or[?] symbolic nature, which seek to inculcate certain values and norms of behaviour by repetition, which automatically implies continuity with the past.[51]

Useful, too, are explications such as e.g. those by J.G.A. Pocock. Tradition, Pocock writes, is

> the handing on of formed ways of acting, a formed way of living, to those beginning or developing their social membership... A tradition, in its simplest form, may be thought of as an indefinite series of repetitions of an action, which on each occasion is performed on the assumption that it has been performed before; its performance is authorised - though the nature of authorisation may vary widely - by the knowledge, or the assumption, of previous performance. In the pure state, as it were, such a tradition is without a conceivable beginning; each performance presupposes a previous performance, in infinite regress. Furthermore, it may well be that it is the assumption, rather than the factual information, of previous performance that is operative.[52]

Still, what we need is not so much definitions - as much rather a detailed examination of the ways in which traditions, in all their forms and varieties, *function* at the different levels and in the different spheres of social life. Such spheres are: *language, science, art, law, politics, education,* and beyond them, or common to them, general phenomena like *spontaneous orders, deviance and normality, creativity, group behaviour,* and so on. Also the issue of so-called *national* or *ethnic* traditions, as well as the *culture/civilization* contrast would, in particular, merit special attention.

Here there already exists a substantial body of important research upon which one can draw. And I think much of that research directly supports the strong traditionalist thesis as formulated above. Thus with all the recent stress on linguistic universals and on the biological foundations of language, there has not survived, in the literature, any serious attempt to question the existence of essential linguistic layers culturally structured and traditionally transmitted. Noam Chomsky's oddly impoverished notion of linguistic creativity,[53] a creativity determined by genetic inheritance and following inborn patterns, has became a curio of the past. In a 1982 study, Slobin and Bever could, once more, revert to Bloomfield's classic dictum "We speak ... by certain well-practiced schemes, - sentence-skeletons that require but the variation of a few words from utterance to utterance", and point to the language-specific nature and broad contextual setting of "schema-development".[54]

With respect to science, the role of traditions is an issue which, due to the Popper-Oakeshott controversy,[55] and especially to the controversy surrounding Kuhn's work,[56] has recently received ample attention. Important here is David Hollinger's observation that Kuhn has in fact applied to the history of science *the* conventional historiographic view of the part played by traditions in politics, arts, and the life of society in

general.[57] "Kuhn's notion of the 'paradigm', his most celebrated and maligned term", writes Hollinger,

> embodies the sense that activities are defined and controlled by tradition, and that radition consists of a set of devices, or principles, that have proven their ability to order the experience of a given social constituency. An operative tradition provides a community with criteria to distinguish one activity from another, sets priorities among those activities, and enables the community to perform whatever common activities make it a community at all. Insofar as the community's common experience is contingent, that experience presents itself as a series of "problems" to be solved by tradition, which validates itself by transforming the contingency of experience into some thing comprehensible and subject to maximum control. Tradition, then, is socially grounded, and its function is that of organization. Organization may be achieved through a number of modes and devices, ranging from formal institutions to informal habits and from codes of abstract principles to concrete examples of how problems of a given class have been solved in the past. Whether it is conduct or perceptions that require organization, whether the task is prescriptive or cognitive, the organizating devices have enough flexibility to sustain them through successive, contingent experiences; to the extent that a tradition can expand and adapt, like the English common law, it is that much more likely to retain its constituency.[58]

Thus, as Hollinger on the other hand points out, in different communities - of which the community of modern-age natural scientists is only one specific kind - the role played by traditions may vary widely. Kuhn himself has written an essay in which he draws attention to the particular way traditions function in art, as contrasted with science. In art, but not in science, Kuhn emphasizes, a tradition might be dead yet its products still living; or again, "though resistance to innovation is a characteristic common to both art and science, posthumous recognition recurs with regularity only in the arts."[59] Also, artists "can and sometimes do voluntarily undertake dramatic changes in style on one or more occasions during their lives", whereas such changes are rare, and never voluntary, in the career of the individual scientist.[60] Still, not only will

"most artists begin by painting in the style of their masters"[61], but one can also assume, Kuhn suggests, that even if styles might not, *pictures* do indeed serve as genuine "paradigms" in art.[62] - Mention has been made above of the traditionalist theory of art of Arnold Hauser. Again and again Hauser emphasizes that "[e]very artist expresses himself in the language of his predecessors, his models, and his teachers", that "every newly created work owes more to other works than to the invention and experience of its creator".[63] Wittgenstein expresses a similar view when he says that "every composer changed the rules, but the variation was very slight; not all the rules were changed. The music was still good by a great many of the old rules."[64] According to Robert Musil even the spontaneity of an artist is inconceivable without handed-down forms and concepts - it is those very handed-down forms that become a source of originality in the creative process.[65] Or, to quote Hauser again:

> what is most significant is not the fact that every expression uses conventional forms from the very beginning but the fact that conventional forms of expression themselves create in part the content of what is being expressed. ... It is true that expression always moves on well-worn tracks, but the tracks multiply and bifurcate as they are being traveled.[66]

And a related position has been developed, perhaps surprisingly, by Karl Popper. According to a "conjecture" of his

> it was the canonization of Church melodies, the *dogmatic restrictions* on them, which produced the *cantus firmus* against which the counterpoint could develop. It was the established *cantus firmus* which provided the framework, the order, the regularity that made possible inventive freedom without chaos.[67]

In theories of law, politics, and of social life in general - theories in which fetishist categories like truth and beauty never really played a role - the idea of an order imposed by mere traditions of course always had a relatively stronger ap-

peal. The words of Carl Menger, the inaugurator of the trend that has become known as the Austrian School of Economics, might convey a suggestion of the unlikely parallels here obtaining between Anglo-Saxon and German thought. Menger definitely displays strong sympathies towards views such as those of Burke, and of F.K. v. Savigny (chief representative of the *historische Rechtsschule*). As Menger writes:

> Burke was probably the first, who, trained for it by the spirit of English jurisprudence, emphasized with full awareness the significance of the organic structures of social life and the partly unintended origin of these.[68]

Burke taught that numerous institutions of his country

> were not the result of positive legislation or of the conscious common will of society directed toward establishing these, but the unintended result of historical development. He first taught that what existed and had stood the test, what had developed historically, was again to be respected, in contrast to the projects of immature desire for innovation. Herewith he made the first breach in the one-sided rationalism and pragmatism of the Anglo-French Age of Enlightenment.[69]

There is, Menger maintains, a "subconscious wisdom" manifested in those institutions that come about organically; and the meddlesome advocates of reform "would do well less to trust their own insight and energy than to leave the reshaping of society to the 'historical process of development'".[70] - In a similar spirit, to-day's leading exponent of the Austrian School, F.A. von Hayek stresses that "since we owe the order of our society to a tradition of rules which we only imperfectly understand, *all* progress must be based on tradition."[71] But the grand old man of contemporary German philosophy, Hans-Georg Gadamer, realizes also that the ordering of life through the rules of law and morality always amounts to more than the application of general principles. "Immer wird auch unser Wissen um Recht und Sitte", he writes, "vom Einzellfall her ergänzt, ja geradezu produktiv bestimmt. Der Richter wendet nicht nur das Gesetz in concreto an, sondern trägt durch seinen

Richterspruch selber zur Entfaltung des Rechts bei..."[72] And in this domain of theory, too, the ideas of the later Wittgenstein have provided new impetus. It was partly under his influence that H.L.A. Hart has developed his conception of law as a combination of "primary" and "secondary" social rules. Hart's primary rules seem to be a proper subclass of the *primary traditions* we described above. They are customs supported by strong social pressure, coming into being through "the slow process of growth, whereby courses of conduct once thought optional become first habitual or usual, and then obligatory..."[73] Without their prior existence, no legal system could be built up.

When led by a sense for primary traditions, the discriminating eye will soon find faults with much of the prevailing views on education. Here again, the writings of T.S. Kuhn have shed new light on some crucial problems. In having developed the truly revolutionary notion of *normal science*, Kuhn underscored the need for rigid traditions within particular scientific groups. In the absence of such traditions, scientific innovation appears neither structurally nor psychologically possible.[74] This view has immediate consequences for educational theory. As Kuhn has pointed out, scientific progress is, at least in the basic sciences, *not* achieved by "liberal" education, by encouraging "divergent" thinking.[75] And one can add that, at the elementary level, *all* learning seems to require a measure of external rigidity. It was Wittgenstein who, in his later philosophy, has explored the reasons for this, and it is significant that an elementary spelling book, his *Wörterbuch für Volksschulen* [1926] was the prelude to that philosophy.[76] In spelling, as in elementary mathematics, Wittgenstein believed in authoritarian teaching methods. That these beliefs, rooted in sentiment but based on analysis, were far from being erroneous, to-day clearly emerges from a number of educational surveys and reports.[77]

Theoretical attitudes on deviance are in many ways bound up with those on education, and it is to be expected that an awaraness of the essential organizational role more or less

rigid traditions play in human communities will, again, preclude the acceptance of radically permissive sociological arguments.[78] But that very awareness - the conviction that only a social fabric entirely destroyed can be devoid of traditional elements - will also enable one to see through the claims of excessive traditionalism, to recognize invented traditions for what they are, to withstand the romantic yearning for bonds. Nationalism on the one hand, and the attacks on contemporary "civilization" in the name of some more authentic "culture" on the other, are two notable instances of an excessive traditionalist ideology. National divisions and nationalist sentiments are invariably bound up with material conditions surrounding the living. "Instead of being automatically united by a shared history", K.W. Deutsch wrote, "men ... cannot share the historical events through which they live, unless they are already in some sense united."[79] Nationalism as often as not forfeits the politico-economic present while focussing on an imagined past. Similarly, the foe of "civilization", while yearning for the fictitious warmth of an age that never existed, is blind to the real traditions of the society, the actual form of life, surrounding him. A seldom-quoted remark by Wittgenstein seems to be appropriate here. "It is very *remarkable*", he wrote in 1946, "that we should be inclined to think of civilization - houses, trees, cars, etc. - as separating man from his origins, from what is lofty and eternal, etc. Our civilized environment, along with its trees and plants, strikes us then as though it were cheaply wrapped in cellophane and isolated from everything great, from God, as it were. That is a remarkable picture that intrudes on us."[80]

Loránd Eötvös University
Budapest

Notes

1. Gilbert Ryle, *The Concept of Mind*. London: Hutchinson's University Library, 1949, p. 59.

2. Michael Polanyi, *Science, Faith and Society* [1946]. Enl. ed., Chicago: Univ. of Chicago Press, 1964, p. 14. The quote is from the 1964 Introduction, but similar passages occur in the main body of the text, e.g. on pp. 42f. and 76.

3. Michael Polanyi, *Personal Knowledge: Towards a Post-Critical Philosophy* [1958]. London: Routledge & Kegan Paul, 1983, p.49.

4. Ibid., p.53. Similar formulations can be found in John Ziman's *Public Knowledge: An Essay Concerning the Social Dimension of Science*, Cambridge: At the University Press, 1968, e.g. on pp. 7 and 10: "The fact is that scientific investigation ... is a practical art. It is not learnt out of books, but by imitation and experience. ... The young scientist does not study formal logic, but he learns by imitation and experience a number of conventions that embody strong social relationships."

5. Michael Oakeshott, *Rationalism in Politics*. London: Methuen, 1962, pp.102f. The quote is from the essay "Rational Conduct" [1950].

6. "Reflections on my Critics", p.275, in: Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge*, Cambridge: At the University Press, 1970.

7. David Bloor, "The Strengths of the Strong Programme" [1981], repr. in: J.R. Brown, ed., *Scientific Rationality: The Sociological Turn*, Dordrecht: Reidel, 1984, p.88. - The parallels (and differences) between Oakeshott and Kuhn are illuminatingly brought out in M.D. King, "Reason, Tradition, and the Progressiveness of Science" [1971], repr. in Gary Gutting, ed., *Paradigms and Revolutions: Appraisals and Applications of Thomas Kuhn's Philosophy of Science*, Notre Dame: Univ. of Notre Dame Press, 1980. Kuhn, King writes,

> states emphatically that the term 'paradigm' denotes not a world-view but a specific example of actual scientific practice which serves as a model for a research community

and implicitly defines the legitimate problems and methods of a research field for successive generations of practitioners. ... Faithfulness to the traditions which spring from paradigms or sets of paradigms is the hallmark of genuine 'science'. To break faith with established tradition is to risk being labelled a crank, a charlatan, or being made an 'outlaw'. - A sociologist reading Kuhn's attack on scientific rationalism can hardly fail to be struck by how closely it resembles Oakeshott's famous onslaught against political rationalism; Kuhn's science like Oakeshott's politics is subject to authority of concrete traditions rather than that of abstract 'reason'. Both are seen as practical activities that, to use Oakeshott's distinction, involve not merely technical knowledge (or technique) which 'is susceptible of formulation in rules, principles, directions, and maxims' and which may therefore be learned from a book and thereafter 'applied', but also practical knowledge which cannot be reduced to rules, cannot be written down and therefore 'can neither be taught nor learned, but only imparted.'

(Quoted from the Gutting volume; the Oakeshott reference is to his "Rationalism in Politics" [1947].)

8. Paul Feyerabend, *Science in a Free Society* [1978]. London: Verso, 1982, p.26.

9. G.H. von Wright, *Wittgenstein*. Oxford: Basil Blackwell, 1982, p.178.

10. Cf. Rudolf Haller, *Urteile und Ereignisse: Studien zur philosophischen Logik und Erkenntnistheorie*. Freiburg: Verlag Karl Alber, 1982, p.184.

11. The crucial passages being *Philosophical Investigations* §§85, 198-208, 239-242.

12. As Plato already seems to have thought. See the reconstruction in Wolfgang Wieland, *Platon und die Formen des Wissens*, Göttingen: Vandenhoeck & Ruprecht, 1982, cf. esp. p.254: "Freilich wird derartiges Wissen" - e.g. the expert knowledge possessed by craftsmen -

> immer nur in einem Prozess von Anleitung und Einübung vermittelt. Es lässt sich niemals wie ein Gegenstand transferieren. Für das Wissen des Handwerkers gilt auf exemplarische Weise, dass sich sein Inhaber von ihm nicht distanzieren kann... Es lässt sich nicht objektivieren, weil es, wie es eine glücklich gewählte Metapher [Platons] einmal ausdrückt, gleichsam in die Handlungen eingewachsen ist.

13. *Personal Knowledge*, pp.49f.

14. Ibid., pp.54f.

15. Ludwik Fleck, *Genesis and Development of a Scientific Fact*. Chicago: The Univ. of Chicago Press, 1979, p.53. Cf. Ludwik Fleck, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache: Einführung in die Lehre vom Denkstil und Denkkollektiv* [1935], Frankfurt/M.: Suhrkamp, 1980, p.72. - Incidentally, the notion of practical knowledge is, in *modern* literature, foreshadowed in the work of Max Scheler, who presumably had some, direct or indirect, influence on Fleck (cf. Fleck, *Entstehung*, p.64, n.29). As Scheler wrote in his "Der Formalismus in der Ethik und die materiale Wertethik" [1913]:

> There is something like "practical" obeying and "disobeying" of laws, but not of laws which "control" natural acting as natural laws control, in the sense that natural acting would conform "to" them in an objective manner. The laws that we have in mind are not at all given as laws (in a form of perception, of "being conscious of..."); they are *experienced* as fulfilled or broken *in* the execution of acting. And it is only in these experiences that they are given. In this sense the acting artist is "controlled" by the aesthetic laws of his art without "applying" them; nor does he realize their fulfilment or violation only in the effect, i.e., in the work of art produced. In this sense, too, it belongs to the essence of the "crime" that he who breaks laws experiences himself as breaking them while acting; these are laws with which he reckons in *practice*, whether he or others are concerned, without having to have the slightest *knowledge* of such laws, and without having to have "thought" about them.
>
> (Quoted from Scheler, *Formalism in Ethics and Non-Formal Ethics of Values*, transl. M.S. Frings and R.L. Funk, Evanston: Northwestern University Press, 1973, pp.141f. The notion approximated here is of course *not* the *"practical-technical intelligence"* described e.g. in Scheler, *Problems of a Sociology of Knowledge*, London: Routledge and Kegan Paul, 1980, cf. esp. p.79.)

16. Ibid.

17. Edward A. Feigenbaum and Pamela McCorduck, *The Fifth Generation*. New York: Signet, 1984. p.67.

18. Ibid., p.82.

19. *Critique of Pure Reason*, A132-134.

20. *The Concept of Mind*, pp.29f.

21. Cf., e.g., *Philosophical Investigations* §§82-86 and 198ff.

22. *Science, Faith and Society*, p.14.

23. Hayek, *Studies in Philosophy, Politics and Economics*, London: Routledge and Kegan Paul, 1967, p.62.

24. *The Concept of Mind*, p.56.

25. Ibid., p.26. - Similarly Feyerabend: "What is called 'reason' and 'practice' are ... two different types of practice", *Science in a Free Society*, p.26. Also Arnold Gehlen, even if on the basis of some rather crude arguments; "Das menschliche Erkennen ... ist geradezu als *Phase der Handlung* zu definieren", *Der Mensch* (Berlin: Junker und Dünnhaupt, 1940, p.52).

26. See e.g. the discussion in Samuel Coleman, "Is There Reason in Tradition?", in: Preston King and B.C. Parekh, eds., *Politics and Experience*, Cambridge: Cambridge University Press, 1968, cf. esp. pp.242ff.

27. Karl R. Popper, *Conjectures and Refutations*, London: Routledge and Kegan Paul, 1972 printing, pp. 130f. From the paper "Towards a Rational Theory of Tradition" [1948].

28. *Rationalism in Politics*, p.109. - Recently the same point was made by O. Schwemmer. One participates, writes Schwemmer, in the "*Handlungskultur*", i.e. in the universally available forms of activity, of a given group or society; and by the very possibility of such participation the rational character of those forms is established, "[d]ie Fähigkeit der Handelnden, ... für andere verständlich zu handeln ... verleiht dem Handeln damit bereits eine elementare Rationalität". (Oswald Schwemmer, "Aspekte der Handlungsrationalität. Überlegungen zur historischen und dialogischen Struktur unseres Handelns", in: H Schnä-

delbach, ed., *Rationalität*, Frankfurt/M.: Suhrkamp, 1984. p.191.)

29. *Science in a Free Society*, p.7.

30. *Rationalism in Politics*, p.102.

31. Paris: 1925. In Peter L. Berger's and Thomas Luckmann's *The Social Construction of Reality*, a book which amply stresses the significance of the "pretheoretical level" of knowledge in society (e.g. on p.65 of the 1967 Anchor Books edition), mention is made of Halbwachs' category of "collective memory" (ibid., p.202) - but not of his combining "memory" and "tradition" with *reason*.

32. Quoted from the German edition: *Das Gedächtnis*, Berlin: Luchterhand, 1966, pp.348f. and 383. - Halbwachs' suggestion actually represents a third way between the usual alternatives of *either* equating rationality with an attitude having some unique, standard structural characteristics, an attitude marred only by false logic, traditions, and emotions; *or* by accepting as rational any views or positions that are felt, *by the groups or persons holding them*, to be appropriate under the obtaining circumstances. These are the two alternatives called - rather misleadingly - the *"traditionelle Rationalitätskonzeption"* and the *"anti-traditionalistisches Rationalitätskonzept"* by Karl Acham, in his essay "Über einige Rationalitätskonzeptionen in den Sozialwissenschaften", in: H. Schnädelbach, ed., *Rationalität*.

33. The essay was first published in 1917. Quoted from Eliot, *The Sacred Wood: Essays on Poetry and Criticism* [1920], London: Methuen, 1960, pp.56-59.

34. Edmund Burke, "A Philosophical Inquiry into the Origin of our Ideas of the Sublime and Beautiful" (1756/57), in: Burke, *The Works: Twelve Volumes in Six*, 1887, Vol.1, pp.246-252.

35. Halbwachs, *Das Gedächtnis*, p.355.

36. Polanyi, *Personal Knowledge*, pp.207f.

37. Wittgenstein, *On Certainty*, §.161.

38. Ibid., §493.

39. Wittgenstein, *Culture and Value*, ed. by G.H. von Wright, transl. by Peter Winch, Oxford: Basil Blackwell, 1980, p.76e.

40. Burleigh Taylor Wilkins, *The Problem of Burke's Political Philosophy*. Oxford: Clarendon Press, 1967, p.61.

41. Fleck, *Genesis*, p.9.

42. Arnold Hauser, *The Sociology of Art*, transl. by Kenneth J. Northcott, London: Routledge and Kegan Paul, 1978, pp.28, 30, 21.

43. Halbwachs, *Gedächtnis*, p.389.

44. Burton M. Leiser, *Custom, Law, and Morality: Conflict and Continuity in Social Behavior*. Garden City, N.Y.: Anchor Books, 1969, pp.7-47.

45. Burke,"Reflections on the Revolution in France" [1790], in: Burke, *The Works: Twelve Volumes in Six*, 1887, Vol.III, pp.346f.

46. Ernst Mach, *Popular Scientific Lectures* [1895]. Fifth Edition, La Salle, Illinois: Open Court, 1943, p.214.

47. Ibid., p.232.

48. Ibid., p.227.

49. Ibid., p.232. - It was in this spirit what the later Robert Musil wrote: "In his potentialities, plans and emotions, man must first of all be hedged in by prejudices, traditions, difficulties and limitations of every kind, like a lunatic in his strait-jacket, and only then will whatever he is capable of bringing forth perhaps have some value, solidity and permanence." (*The Man Without Qualities* [1930], transl. by E. Wilkins and E. Kaiser, London: Panther Books, 1968. Vol.I, p.52.)

50. Edward Shils, *Tradition*. London: Faber and Faber, 1981, p.12.

51. Eric Hobsbawm, "Introduction: Inventing Traditions", in: Hobsbawm and Ranger, eds., *The Invention of Tradition*, Cambridge: Cambridge Univ. Press, 1983, p.1.

52. "Time, Institutions and Action: An Essay on Traditions and their Understanding", in: King and Parekh, eds., *Politics and Experience*, pp.209 and 212.

53. Cf. e.g. Geoffrey Sampson, *Liberty and Language*, Oxford: Oxford University Press, 1979, pp.7 and 105.

54. Dan I. Slobin - Thomas G. Bever, "Children use canonical sentence schemas: A crosslinguistic study of word order and inflections", *Cognition* 12 [1982], see esp. pp.229 and 253.

55. See Oakeshott's *Rationalism in Politics*, and Popper's paper referred to in note 27.

56. See esp. the Lakatos-Musgrave and Gutting volumes referred to in notes 6 and 7, as well as Kuhn's collection *The Essential Tension: Selected Studies in Scientific Tradition and Change*, Chicago: The Univ. of Chicago Press, 1977.

57. David Hollinger, "T.S. Kuhn's Theory of Science and Its Implications for History", in: Gutting, ed., *Paradigms and Revolutions*, pp. 196ff.

58. Ibid., pp.197f.

59. "Comment on the Relations of Science and Art", in: T.S. Kuhn, *The Essential Tension*, pp.346 and 348.

60. Ibid., p.349.

61. Ibid.

62. Ibid., p.351.

63. *The Sociology of Art*, pp.30f.

64. L. Wittgenstein, *Lectures and Conversations on Aesthetics, Psychology and Religious Belief*, ed. by Cyril Barrett, Berkeley: University of California Press, 1967, p.6.

65. There are concepts, Musil writes, which for the poet constitute

> die Begriffe seiner Überlieferung, mit deren Hilfe er sein persönliches Selbst mühsam gefestigt hat. Er braucht ihnen gar nicht allen beizupflichten, er kann bestrebt sein, sie zu verändern, so bleibt er ihnen doch allen verhaftet, weit mehr, als man dem Boden verhaftet ist, auf dem man wandelt. Der Dichter ist nicht nur der Ausdruck einer augenblicklichen Geistesverfassung, mag sie selbst eine neue Zeit einleiten. Seine Überlieferung ist nicht Jahrzehnte, sondern jahrtausende alt.

("Der Dichter in dieser Zeit" [1934], in: Robert Musil, *Gesammelte Werke*, ed. by Adolf Frisé, Reinbek bei Hamburg: Rowohlt, 1978, Vol.8, p.1250.) Or, as he put in the essay "Literat und Literatur" [1931]: "[A]uch der unabhängigste Schriftsteller [bringt] nichts hervor..., was sich nicht fast restlos als abhängig von Überlieferungen der Form und des Inhalts nachweisen liesse... [m]an kann ... nur dort 'von Originalität sprechen, wo es eine Überlieferung gibt." (Ibid., p.1207) - The connection between creativity and underlying traditions is explored in greater detail in W. Grassl and B. Smith, "A Theory of Austria", in: J.C. Nyiri, ed., *The Tradition of Austrian Philosophy*, Wien: Hölder-Pichler-Tempsky, 1986.

66. *The Sociology of Art*, pp.31 and 21.

67. Karl Popper, *Unended Quest: An Intellectual Autobiography*, rev. ed., Fontana/Collins, 1976, p.58.

68. Carl Menger, *Untersuchungen über die Methode der Sozialwissenschaften* [1883], in *The Collected Works of Carl Menger*, ed. F.A. von Hayek, Vol.2, London: The London School of Economics, 1934, p.173.

69. Ibid.

70. Ibid., p.91.

71. F.A. von Hayek, *Law, Legislation and Liberty*, Vol.3, London: Routledge and Kegan Paul, 1979, p.167.

72. Gadamer, *Wahrheit und Methode*, 4th ed., Tübingen: J.C.B. Mohr (Paul Siebeck), p.35.

73. H.L.A. Hart, *The Concept of Law*, 2nd ed., Oxford: Clarendon Press, 1963, p.90.

74. See ch.III of his *The Structure of Scientific Revolutions*, and esp. the paper "The Essential Tension: Tradition and Innovation in Scientific Research" [1959] in *The Essential Tension: Selected Studies in Scientific Tradition and Change*.

75. "The Essential Tension", pp.226ff.

76. "Only a dictionary", wrote Wittgenstein in his Preface, "makes it possible to hold the student completely responsible for the spelling of what he has written because it furnishes him with reliable measures for finding and correcting his mistakes, provided he has a mind to do so." Ludwig Wittgenstein, *Wörterbuch für Volksschulen*, ed. by A. Hübner et al., Wien: Hölder-Pichler-Tempsky, 1977, p. XXXI.

77. Cf. e.g. Neville Bennett, *Teaching Styles and Pupil Progress*, London: Open Books, 1976. - Current West-German perceptions are especially instructive. There, in the early 1970's, it has been declared that "broadening of linguistic competence" should supplant "training in the norms of 'standard German'" in general and the "learning of orthography" in particular. The results, as the progressive weekly Der Spiegel tells us, are by now catastrophic:

> Den Reformern ging es nicht nur darum, die unterschiedlichen Ausgangspositionen auszugleichen zwischen Schülern, die aus einfachem Hause kamen, und anderen, die einem sprachlich besser ausgestatteten Milieu entstammten - ein erstrebenswertes Ziel. Nicht nur der manchmal aberwitzige Drill sollte agbeschafft werden, der Stumpfsinn, der oft die Deutschstunde beherrschte. - Dem Entwurf war auch zu entnehmen, "dass diese 'Hochsprache' bislang stets eine Gruppensprache gewesen ist, die als verbindliche Sprache durchgesetzt und bei der Schichtung der Gesellschaft als Mittel zur Stabilisierung dieser Schichtung benutzt worden ist". ... Zum höheren Bildungsgut,

runs the Spiegel's mocking quote, "zählt seit jenen Tagen

die 'Erweiterung der Fähigkeit, sich in umgangssprachlicher Kommunikation als realer Sprecher-Hörer mit anderen zu verständigen'." *Der Spiegel*, July 9, 1984. But the ability of young Germans to write correctly, to read, and indeed to express themselves, has deteriorated drastically. And what sort of democracy is this, asks the magazine, where citizens are not capable of articulating their views?

78. "What makes an individual a member of society and gives him claims is that he obeys its rules", writes F.A. von Hayek. "Wholly contradictory views may give him rights in other societies but not in ours. For the science of anthropology all cultures or morals may be equally good (though I doubt that this is true), but we maintain our society by treating others as less so." *Law, Legislation and Liberty*, Vol.3, London: Routledge and Kegan Paul, 1979, p.172.

79. Karl W. Deutsch, *Nationalism and Social Communication: An Inquiry into the Foundations of Nationality*, London: 1953, p.5. - On some important material determinants giving rise to feelings of nationalism see also Ernest Gellner, "Nationalism", in his *Thought and Change*, London: Weidenfeld and Nicolson, 1964.

80. L. Wittgenstein, *Culture and Value*, p.50e.

HUMANISM, PHILOSOPHY OF LANGUAGE AND THEORY OF KNOWLEDGE IN ADAM SCHAFF

Augusto Ponzio

In the works of Adam Schaff, problems concerning the "human individual", "language", and "knowledge" constitute an organic whole. This is a reflection of the *real and objective* relation connecting these three fields: it is precisely on this aspect that I will concentrate, even if certain disciplines not only keep the three fields separate, but tend to fractionize them even further according to their specific interests.

In his philosophical research, Schaff concentrates on three main areas: a) the philosophy of language, b) the philosophy of the human individual, c) the theory of knowledge. In his most recent books (1974; 1975), Schaff attempts to unify his research and insists on expressing his disapproval of the tendency towards dividing these three fields on the one hand, and towards the suppression of their autonomy on the other.

During an interview I had with Schaff in 1977, I asked what the unifying element of his research was. He answered that it was the human individual, an issue concerning the philosophy of language, the theory of knowledge and the philosophy of man, that is, all the human sciences in general. This does not imply that this issue is necessarily dealt with in such areas of study, but on the contrary, despite its primary importance in historical, social, linguistic, cognitive and economic processes, the human individual tends often to be ignored. This fact explains the abstract nature of so many theories. In Schaff's opinion, the problem of the human individual is of fundamental importance for the scientific foundation of theory, and consequently for the capacity of theory for analysing and explaining any particular phenomenon. Research in philosophical anthropology and the philosophy of man remains

isolated if not applied to the various fields of the human sciences, for results obtained in the latter act as some kind of feedback which in its turn enriches the former - provided that researchers do not limit themselves to mere speculation and metaphysics.

The fact that Schaff places so much importance on the problem of the human individual is particularly relevant for the question of "what it means to be a Marxist today". In fact, Schaff believes that the problem of the human individual is particularly relevant in a Marxist perspective not only because of what the Marxist classics have said about it, but also because of what certain Marxists have tended not so say, thus denying the legitimacy of such an issue. Furthermore, certain philosophers reject this problem in the name of "true Marxism", and of the "true Marx" (that is, the later as compared to the early Marx), as they consider it to be a bourgeois ideological residue. They use the human individual and related issues (alienation, Marxist ethics and so forth), as a means of discriminating between "orthodoxy" and "revisionism" and formulate negative judgements a priori wherever such concepts as "alienation", "human individual", "humanism", and "Marxist ethics", appear.

As an example we have the discussion between Schaff and Lucien Sève on the translation-interpretation of Marx's *Thesen über Feuerbach*, published in the journal "L'homme et la société" (1971-72). This discussion clearly demonstrates the superficiality of those who consider "Marxist humanism" in estremely vague terms, regarding it as evidence of revisionism and who thus propose an even vaguer "antihumanism" as a token of true Marxism and "guarantee" of its scientific character.

The expression "Marxist humanism" can be referred to the theories of both Schaff and Sève. Both philosophers, in fact, support the interpretation of Marxism as scientific humanism and refuse Althusser's "theoretical anti-humanism". However, they take completely different points of view: they diverge in their interpretation of the Marxist classics and therefore in their ideological and political stances as their polemics

reveal. And this despite their common defence of Marxist humanism and of the maintainance of such concepts as "man", and "human individual" within Marxism.

The human individual and the related issues of alienation, socialist humanism, and Marxist ethics are not to be underrated or considered alien to Marxism itself but, on the contrary, should receive particular attention within a Marxist perspective. And this especially when like Schaff we consider Marxism as an "open system", a scientific system open to continual transformation, discussion and modification, and not as a set of fixed principles demanding absolute loyalty, dogmatic and orthodox acceptance. Marxism should not be viewed as a set of principles established once and for all, free of the risk of confutation: such a "risk" rather, is of vital importance to a system which aims at being scientific.

Some of Marx's earlier works and certain problems, therefore, concerning the human individual such as alienation, have often been labelled as revisionist and anti-communist. This had led to the refusal of those very issues by certain Marxists and to the acceptance of the division between "the early and the later Marx": the former being an ideologist and humanist, the latter a scientist and anti-humanist. It is absurd to tax any Marxist position which refers to Marx's juvenile production with "revisionism", just as it is absurd to distinguish between "ideology"and "science". This is a reflection of the tendency of attributing exorcizing functions and magical powers to such words as "ideology" "humanism", "revisionism", and "science".

It is certainly true that the word "humanism" can be ambiguous given all the different meanings which have been attached to it in the course of tradition: when applied to Marxism it has often given rise to revisionist and speculative interpretations. It is no less true, however, that misunderstandings and ideological mistakes have arisen because rather than making a precise criticism of certain interpretations of Marxism, the latter has been interpreted in terms of a generic anti-humanism.

When dealing with humanism in connection to Marxism, Sève has shown how we may use the same approach as when dealing with "materialism", "dialectics", "philosophy", "socialism", that is, all those concepts which are considered to be in direct contrast with Marxism. Despite pre-Marxist interpretations of materialism, Marxism asserts itself as materialism, historical-dialectic materialism, and as scientific materialism. In their aim of attaining a "highly-developed materialism", Marx and Engels criticize a certain type of materialism. However, this must not lead to confusion of such criticism with the absolute refusal of materialism, as Lenin shows in his criticism of the Russian Machists, of idealistic and subjectivistic stances, that is, of "low idealism". Though vitiated by Hegelian idealism, Marxian theory maintains the term "dialectics", using it with a new meaning. Moreover, Marxism puts an end to traditional philosophy, but to refuse to recognize that Marxism itself is a philosophy on the belief that it is possible to rid oneself of philosophy once and for all, is the worst kind of philosophy, it would mean acting as "slaves to the worst vulgar residues of the worst philosophies", as Engelst puts it. Similarly, it would be arbitrary to conclude that Marxism is a form of "theoretic anti-socialism" simply because Marx and Engels criticize utopian forms of socialism. Just as Marxism is the transformation of utopian socialism into scientific socialism, of metaphysical materialism into scientific materialism, it is also the transformation of speculative humanism into scientific humanism.

As Schaff observes, by denying Marxism the character of humanism (as did certain Polish Marxists even before Althusser during a debate in Poland in 1947 on the relation between Marxism and humanism), we strengthen the position of those who oppose Marxism and communism and consolidate the aim of dividing the proletariat using humanism as the discriminating factor.

We are now able to understand why Schaff attaches so much importance to the problem of the human individual which lies at the very centre of his theories of language and know-

ledge. I was once granted an interview by Schaff who claimed that:

> neglect of the problem of the human individual leads to the impoverishment of Marxism at the theoretic level and to its distortion at the practical level. In this mistake lies the deep secret of Stalinism. This is why the protagonists of "true" Marxism - where the individual is absent - are so dangerous. I am referring not only to those who put Stalinism into practice, but also to its theorist, whose various political lucubrations and theoretical mistakes have resulted in the thesis that Marxism is anti-humanism. If this were the case, it would be necessary to fight against it. But it is a pure lie: Marxism is humanism, and it is the concern of Marxists to fight in the name of this humanism. This has always been my firm belief, as a Marxist and as a Communist. And this fact explains the choice of the leitmotif of my philosophical works
>
> [Schaff 1977a]

Schaff has dealt with the issue of the human individual and with socialist humanism since 1947. Writings from this period prove the fallacy of the thesis of an existentialist influence on Schaff's Marxism. Among other things they testify to the presence of anthropological issues among Polish Marxists even before the diffusion of existentialism - and Schaff's own position is indicative of this. In fact, as early as 1947 we already have a discussion of two main tendencies which, though seemingly opposed, are both based upon the division between Marxism and "humanism". On the basis of the assumption that such a separation exists, the first tendency proposes to "integrate" Marxism and humanism; the second tendency maintains that Marxism is intimately opposed to "humanism". In contrast to these two main tendencies, on his part Schaff believes that Marxism is the humanism of our time. In fact, differently to other forms of humanism, insofar as Marxism is scientific socialism, it also has the real capacity of indicating the way to a profound transformation of the current relations of inequality and exploitation.

In his essay of 1947a and 1947b, Schaff formulates the problem of Marxist humanism with the same methodological procedure of *Strukturalismus und Marxismus* [1974] which deals with the same issue, only twenty-seven years later. In the

latter, Schaff criticizes Althusser's anti-humanism and demonstrates how it is misleading to speak of "humanism" (as well as of "anti-humanism"), without specifying the meaning of such terms in relation to particular historical and social conditions. Words like "freedom", "democracy", "justice", "equality", "property", receive an appropriate meaning only when related to particular historical and social conditions. Similarly the word "humanism" also needs a historical specification. Only on this condition is it possible to avoid making a moralistic use of the term which renders it inefficacious for the transformation of the capitalist system. By specifying the term humanism historically, we are also able to eliminate the semantic ambiguity and stereotyped component present in it which makes it liable to exploitation by those aiming at the preservation of current order and at anti-communist propaganda. In his 1947 essay Schaff wrote:

> Humanism does not exist in itself, just as man taken in himself and for himself does not exist. Only concrete man exists, man set in a particular age, living in a particular country, belonging to a particular social class, representing a particular tradition and particular personal ideals.

In his criticism of Althusser in *Strukturalismus und Marxismus*, Schaff demonstrates how, to a certain extent, the semantic ambiguity of the word "humanism" is responsible for the separation and opposition between Marxism and humanism. Althusser uses this word as though its meaning were univocous and in no need, therefore, of specification. Here too, Schaff shows how no real alternative between Marxism and humanism actually exists while it does, in fact, exist between Marxism and anti-Marxism. Now, anti-humanism is precisely a form of anti-Marxism. Schaff also brings to attention the mystifying character of Althusser's structuralist anti-humanism: in Althusser "humanism" implies an idealistic and speculative conception of the essence of man while, on the contrary, his "anti-humanism" is not symmetric to "humanism". Althusser's anti-humanism states rather that production relations are not relations among human individuals and that the human individ-

ual has no role in history. Althusser attributes this last statement to Marx (the later as opposed to the early Marx).

In *Entfremdung als soziales Phänomen* [1977], Schaff analyses such concepts as alienation, fetishism, revolution, reformism, Marxism, revisionism, and socialism. He deals with problems related to Marxist humanism and the human individual concentrating on two main aspects: a) the demand, intimately connected to the historical-materialist approach, for "historical specifications"; b) the demand for linguistic analysis which is considered to be of major importance, and not only when dealing strictly with language problems (his book, in fact, is full of "semantic digressions").

Such issues are not forgotten in the 1973 essay "Marxismus und das Problem der gewaltsamen Revolution" (now in Schaff, 1975). They are particularly relevant in the last chapter of *Entfremdung als soziales Phänomen* (written especially for the Italian edition where he deals with the particular approach to socialism on the part of the Italian, French, and Spanish Communist parties). Schaff refuses to acknowledge violence as a means of achieving the socialist revolution: he specifies that the Marxist concept of the "social revolution" is one thing, and the way in which it can be carried out is another. The expression "social revolution" implies a qualitative transformation of both social foundations and of the superstructures. Such transformation may be achieved either violently or pacifically by means of reform. The choice cannot be decided upon the basis of abstract theory but rather is determined by the specific historical and social conditions of a country. There are no fixed formulas as far as the way of building socialism is concerned, even if some scholars are convinced of the contrary and search the classics of Marxism for a solution able to overthrow the capitalistic system. In reality, the solution changes with the different situations, accordingly taking on different forms such as the juridical, constitutional, or trade-union form. By rendering Marxism static, we betray one of its most essential characteristics, that of "historical specification". The resort to reforms does

not necessarily imply renouncing class struggle or the building of a socialist society with the acquisition of power by the class. Use of the word "reformism" in relation to the enacting of reforms is justifiable when the aim of reform rather than being social revolution and transformation of the social relations of capitalistic production, is instead the preservation and reproduction of those very relations. The choice of a peaceful way to socialism (with particular reference to Western European Communist Parties), when the historical conditions make this possible, is not at all a "disavowal of Marxism". But rather it is the refusal of Marxism in the form of dogma and "orthodoxy" isolated from the dialectic relation to social praxis and concrete historical circumstance.

Between the second half of the Fifties and the beginning of the Sixties, existentialism - especially in the version given to it by Sartre -, deeply influenced certain Polish Marxist intellectuals; a phenomenon related to the "crisis of Stalinism" and the events of the Polish and Hungarian October. It was in that period that Schaff put into evidence the profound "incompatibility" between existentialism and Marxism. Historical materialism explains human behaviour in terms of social conditioning, the human being is viewed as the result of social relations (Marx's fourth thesis on Feuerbach); on the other hand, existentialism explains social phenomena in terms of individual freedom considered as an absolute, natural and non-historical fact. In his criticism of existentialism, Schaff places great importance on semantic definitions: in fact, he underlines the ambiguous nature of the notions and arguments employed by Sartre. This is one of the most recurrent aspects of his analysis in *Critique de la raison dialectique*.

However, the same reasoning which led Schaff to oppose Marxism to existentialism, and to polemize against certain Marxists (e.g. Leszek Kolakowski) for having accepted the existentialist conception of the human individual, induced him

to reject the oversimplified criticism of existentialism on the part of other Marxists. The latter, in fact, concluded their discussion of the matter by simply taxing existentialism with "bourgeois ideology", "revisionism", and "idealism". This kind of criticism did nothing but confuse the problems examined by existentialism with the approach adopted to such problems. Schaff himself supports the Marxist rather than the existentialist approach, but shares interest in the same problems concerning the human individual. Though certain problems have often been neglected by Marxism, he believes that they are not at all extraneous to Marxism at the theoretical level. In fact they belong to the same sphere of interest which generates Marxism and which gives a more profound meaning to the Marxian analysis of the social relations of production. Certain superficial critics have confused an open and constructive criticism of existentialism with a form of existentialist Marxism. The German title of Schaff's book of 1961, *Marx oder Sartre?*, is indicative of the relation he establishes between existentialism on the one hand and Marxism on the other.

In *Marksizm a jednostka ludzka* [1965] - which Schaff considers to be a full-length political as well as philosophical study (the same could also be said of *Entfremdung als soziales Phänomen*) - the human being is taken as the direct object of analysis. In his *Beitrag*, part of a debate organized by the Polish review "Studia Filozoficzne", which took Schaff's book as the take-off point for discussion, Schaff declares that anthropological issues must not be neglected in the ideological struggle. In such a perspective, the importance of assuming the human individual as the focal point of Marxism at the theoretical level, is determined by two main factors: the frist is that anti-Marxism has taken advantage of the fact that Marxism has ignored these problems, and as a consequence itself proceeds to insisting upon the relation between the individual and society, giving particular attention to such issues as freedom, individual happiness and so forth. The second is that these problems are particularly evident - unless we are blind or insincere - in socialist countries today.

Alienation is a major issue at both the theoretical and the practical levels in the building of a socialist society. The acknowledgement of the existence of such a phenomenon is of primary importance to the development of socialism. Some people believe it is contradictory to take great pains to eliminate alienation on the one hand, while stating on the other that alienation can never be permanently eliminated given that it constantly occurs in different forms. During a debate organized by "Nowe Drogi" for the discussion of Schaff's book *Marksizm a jednostka ludzka* [1965] Schaff insists that such a contradiction can in fact be easily explained in the terms of Marxist dialectics. Particularly convincing is the comparison he establishes between the theory of alienation and the Marxist theory of truth. Although the cognitive process is endless, it does not exclude the objectivity and truth of knowledge, nor does it exonerate us from the search for truth. Similarly, the unending struggle against alienation does not exclude the possibility of overcoming such alienation by means of the transformation of specific social relations, nor can the fact that the struggle is unending be used as a pretext for leaving things unchanged.

In his *Marksizm a jednostka ludzka*, Schaff analyses the different aspects of alienation as it appears in socialist countries. He examines the issue even more closely in his *Entfremdung als soziales Phänomen*, especially in the chapters entitled "Sozialismus und Entfremdung" and "Sull'alienazione nella rivoluzione". We could ask those who pose themselves the problem of alienation in socialism the insinuating question, to whose benefit is all this? This question was asked in relation to Schaff's book *Marksizm a jednostka ludzka*, in the above-mentioned debate in "Nowe Drogi". Schaff answers that by bringing to attention and analysing the contradictions and diverse forms of alienation inherent in the building of socialism, rather than favour anti-communist propaganda, in the long run we actually favour the communist movement and Marxism. In fact, the critical capacity of Marxism is broad-

ened so that it is able to deal with problems which have been generally monopolized by anti-communist propaganda. In this way we contribute to the development of a socialist society and to the shaping of man in such a society.

Despite attempts by certain Marxists at "exorcizing" the problem of alienation by considering it a "non-scientific" and "non-Marxist" notion, "alienation" is an adequate label for certain social phenomena for which solutions have been attempted through practice based on Marxist theory. Such attempts at exorcism become increasingly frequent when analysis of the various forms of alienation is extended to the different socialist countries, and when it is considered that the struggle against alienation is endless given that it cannot be eliminated once and for all.

Marxism involves a struggle against the different historical forms of social alienation whereby the individual is prevented from being a conscious protagonist of his own history. Furthermore, in Schaff's opinion, it is also a *radical, positive* and *materialist humanism*. It is a combattant humanism, that is, it is committed to a historical social reality where it is desired that the history of men be a very human history. Marxism takes an interest in the human individual historically specified by the relations of production of the particular country he lives in, and because of this it opposes the interpretation of alienation in the abstract terms of "human essence" and "human nature".

2.

Linguistic analysis is particularly useful in the study of the historical-social structure of the human individual, given that it is especially through language that the historical and social conditioning inherent in the shaping of the individual is made possible. Language is a social fact and constitutes the social background to consciousness, thought and speech. Language is a social product as well as being a genetic phenomenon and is functional to human praxis. This is at the basis

of the historical-materialistic and dialectic character of the "active role" of the subject both at the level of cognitive processes as well as of practical action. The individual is able to act upon the historical-social situation which is pre-existent to him conditioning him from the outset, through his use of language (it too a social product). Language is not only an instrument for the expression of meanings, but it is also the material which goes to form meaning and without which meaning could not exist. Consequently, what we call the "subjective", does not at all mean the abstractly individual or absolutely autonomous, but rather it is the concretely individual and that which is conditioned, that is, a social product with a social function: the "subjective has an objective and social-historical character."

The linguistic sciences are able to grasp the socio-historical nature of language thus ridding themselves of both biologistic, innatistic, conventional prejudices on one hand, and of related mechanistic and idealistic conceptions on the other, merely by reconsidering the human individual in the perspective of historical-dialectic materialism. It follows that language is neither wholly natural nor wholly unnatural and conventional. Like any human fact, it is first of all a socio-historical phenomenon resulting from historically determined needs, mediating between needs and the satisfaction of such needs.

An innatistic and biologistic interpretation of language, as that of Chomsky and Lenneberg, can be maintained only by reducing the status of man to the mere product of natural evolution, as if his biological history were uninfluenced by his social history. In such a perspective he is viewed as "man in general", as abstract man, rather than as a historical and social being conceived in his concreteness, in his special historical specification according to the social system he belongs to, the specific division of labour, class and level of productive forces.

Furthermore, many authors agree that what is *innate* in language in only the *capacity of learning to speak* (which undoubtedly depends upon the hereditary structure of the brain, the vocal apparatus, and so forth), while the *concrete realization* of language is determined by *social relations*. Though true, this explanation is insufficient for it does not eliminate the dangers of a biological interpretation of language: in Lenneberg's work, for instance, social relations and the relations among individuals of the same species are placed at the same level. They are considered to be the same as relations existing in the animal kingdom at large.

It is absolutely necessary to found the theory of language on the interpretation of man and of interhuman relations where there is no backward step towards naturalistic positions with respect to the scientific achievements of historical-dialectic materialism. In a Marxian perspective, social relations are characterized by relations of production, they represent a particular form of production, they are historical, non-natural relations.

If we wish to free ourselves of what Schaff called the "fetishism of the sign" referring to the Marxist notion of the "fetishism of goods", we must view the analogy and typology of signs in connection with the issue of the human individual and social relations. In fact, to give up a reified conception of the relations between signs as well as between signifier and signified, it is necessary to take the social process of communication as the starting point of our analysis, and to consider the sign-relation as a relation among men who use and produce signs in specific social conditions. All analyses should start from the "social condition of the individual" and from the notion of the individual as a social product. This would prevent us from considering communication as a set of relations among originally separate and abstract subjects, as well as removing idealistic and materialistic-mechanistic explanations of the communication process.

The question of the relation of language to reality is closely connected to both the theory of knowledge and to the conception of the human individual. Does language create our image of reality? Or does language reflect and reproduce reality? Does language have an active, creative function in the cognitive process? To answer these questions implies taking a definite stance as regards the three fundamental models of the theory of cognition: idealism, mechanistic materialism, dialectic materialism. The latter two refer the problem of the relation between language and reality to the theory of reflection. All three concern the role of the subject in the cognitive process and consequently the problems related to the human individual.

The subjective-idealistic and materialistic-dialectic models differ from each other in their interpretation of the active role which both, in contrast to mechanistic materialism, assign to the subject and consequently to language in the cognitive process. In Schaff's opinion, in comparison to naive materialism, materialistic-dialectic theory recognizes the superiority of language theories which stress the active function of language in the cognitive process (even if from an idealistic point of view) and the connection between language and *Weltanschauung*, between language and the "image of reality" (think of Humboldt, Sapir, and Whorf). However, in the perspective of a Marxian interpretation, the human being should be considered as the result of social relations, and language as the product of social praxis. This interpretation recognizes the active function of the cognitive subject and, at the same time, maintains that far from being the starting point of the cognitive process, the subjective element is the result - and a complex one at that - of specific social influences. In a certain sense, the subject may be considered as the resultant construction of cognitive processes.

The concept of "reflection" is closely related to the concept of the "human individual", and it is precisely on the basis of the interpretation of such notions that we mark the

difference between dialectic and mechanistic materialism in connection with the theory of knowledge. As Schaff writes:

> the specific interpretation of the theory of reflection in the Marxian system is directly related to the interpretation of the concept of the *human individual*
>
> [*Jezyk a poznanie*, 1964; It. 1973, p.158]

The connection between the theory of language and the theory of knowledge is evident if we acknowledge the interaction between language and thought, and the indivisibility of meaning and concept. Schaff recalls Lenin's "On Dialectics", in which the latter outlines the programme for Marxist gnoseology with reference to the history of language, as sufficient evidence of this, maintaining that:

> ...when in accordance with the materialistic analysis of the cognitive process we consider thought and human consciousness as linguistic thought, as thought made of language (Marx maintained that language is "my consciousness and that of others"), it is evident that any analysis of the cognitive process must also be the analysis of the linguistic process, without which thought is simply impossible.
>
> [*Szkice z filozofii jezyka*, 1967; It. 1969, pp. 20-21]

"Pure" thought which is subsequently to find expression in a specific language does not exist, on the contrary, there exists a *language-thought process*. Any form of human speech implies the use of a particular language; thinking always takes place in a certain language. In reply to the Würzburg School, Vygotskij demonstrated the unity of thought and language, and of meaning and concept, through experimental research in the formation and development of conceptual thought.

Such unity of thought and language highlights the active function of language in the reflection upon reality, as well as the social character of individual thought, its status as a social product.

On defining the sign in general (that is, at the semiotic level) and on dealing with semantics, it is impossible to leave the theory of knowledge out of consideration. For example to consider the problem of the referent, or the material object,

as irrelevant to a semiotic point of view does not at all mean separating semiotics from the theory of knowledge and, therefore, allowing semiotics to remain neutral as regards such a theory, as certain authors believe. On the contrary, it means assuming a specific standpoint in relation to the theory of knowledge, which would be described as conventional-idealistic for the insistence upon the autonomy of the code and of the message with respect to material reality.

Semantics and the theory of knowledge are both implied whenever we ask the following questions: "What is meaning?"; "What is the relation between meaning and the sign-vehicle?"; "What is the relation between meaning and object?"; "What kind of existence do we refer to when we say that meaning exists?"; and so forth.

On the other hand, all the problems with which the theory of knowledge deals, insofar as they are problems concerning language, imply semantics. This does not mean that the theory of cognition should be exclusively a semantic analysis or that language should be the *sole* object of any philosophical research, as is maintained by semantic philosophy. The Marxist theory of reflection clearly puts into evidence all the implications existing between semantics and the theory of knowledge, rejecting any schematic attitude typical of conventional and idealistic relativistic standpoints. Certain philosophical trends such as Cassirer's neo-kantianism, neo-positivism, Russell's logical atomism, the linguistic philosophy of the Oxford School connected with Wittgenstein's later work, the semantic analysis of the Warsaw School and so forth, deserve recognition for having maintained and demonstrated that *language is not merely the instrument, but also the object of philosophical research.*

The theory of knowledge is not the only theory in need of support from studies on language. The philosophy of the human individual - to the extent that it deals with the function of the individual in social relations and with problems of traditional ethics, though rejecting any form of moralism -,

must inevitably consider that individual behaviour is conditioned by society mainly through the influence of language. This leads us to a new vision of issues related to language. the problem of the connection between language and ideology, concept and stereotype, language and social praxis. On considering the concepts of "choice", "responsibility", "individual freedom", we need to take account of the "tyranny of words", of the problem of "linguistic alienation". We should reject the idealistic and conservative point of view which refers contradictions and individual alienation to a semantic origin thus maintaining, similarly to the young Hegelians, that man can be "set free" by simply clarifying the meaning of words and by substituting true ideas for false ones.

3.

The relation between Marxist dialectics and formal logic clearly puts into evidence the connection between the theory of knowledge and the analysis of language. Schaff shows how the word "contradiction" has two different meanings depending on whether it is considered from a Marxist dialectical, or formal logical point of view; this implies that Marxist dialectics does not exclude the logical principle of non-contradiction. From the point of view of formal logic, the term "contradiction" signifies a relation between two sentences, or utterances, one of which maintains that something is in a given relation with an object at a given moment, while the other denies this. On the contrary, from the point of view of Marxist dialectics "contradiction" means "unity of antithesis", that is, unity of contrasting tendencies, aspects and forces; in this way, dialectics is the constitutive element of every phenomenon.

When Marx maintains that at a certain level of their development the productive material forces of society are in contradiction with the existing relations of production, the word "contradiction" does not express the relation between a positive and negative judgement (as in formal logic), but

rather the juxtaposition between opposed and yet complementary tendencies which form the unity of a certain system, but at the same time the mainspring of its transformation. In this case, the word "contradiction" - notwithstanding the misunderstandings it can give rise to - when intended as an objective rejection of the logical principle of non-contradiction, has a specific meaningfulness which justifies its use. In this particular case, the word "contradiction" underlines a contrast characterized by inadequacy and discordance such as to interfere with the functioning of the social mechanism to the point of causing its collapse.

A central point in Schaff's analysis of the relation between dialectics and the principle of non-contradiction is his demonstration that consideration of movement as a confutation of the logical principle of non-contradiction, is unfounded. Engels too falls into this trap. In Plekhanov's opinion we have to face the following dilemma: either we acknowledge the existence of the fundamental laws of formal logic and we deny movement, or, on the contrary, we acknowledge movement and deny these laws. Schaff observes that this is a false dilemma. It is caused because of the interpretation of movement as an objective confutation of the logical principle of non-contradiction, as something which is and is not at the same time in the same place. This interpretation which the Marxist classics derive from Hegel, in reality originates from the ancient Eleatic philosophers:

> Die Eleaten bejahten den Satz vom Widerspruch und negierten folglich die Objektivität der Bewegung; Hegel stand umgekehrt auf dem Standpunkt der Objektivität der Bewegung und verwarf infolgendessen die Gültigkeit des Satzes vom Widerspruch in der Beschreibung der Bewegung.
>
> [Schaff, 1975, p.26]

Schaff establishes a connection between the fact that Marx and Engels accepted the Hegelian interpretation of movement (as something which is and is not at the same time in the same place) and the level of development of the mathematics of the

time, in particular of differential calculus. Newton and Leibniz's conception of the infinitesimal entity, considered to be a quantity equal to and different from zero, strengthened the influence of the Eleatic-Hegelian principles concerning movement, on Marx and Engels.

As far as the relations between Marx and the mathematics of his time are concerned, today we can say that things are different from how they were described by Schaff in 1955. Thanks to the publication of Marx's *Mathematical Manuscripts* (Moscow 1968), today we are familiar with Marx's critical analyses of Newton's and Leibniz's "mystical" differential calculus, of D'Alembert's and Euler's rationalistic method, and of Lagrange's purely algebraical method. In criticising Newton and Leibniz's differential calculus, Marx highlights the presence, in their theory, of metaphysical notions and of the use of procedures which contrast the laws of mathematics. Though making use of Lagrange's work, through such criticism Marx independently reaches positions attained by such 19th century mathematicians as Cauchy and Weierstrass, who accomplish the transition from a simpler to a more profound and scientific stage of calculus. Schaff's considerations can certainly be referred to Engels but not to Marx. What Engels writes in his *Anti-Dühring* about the *differential* relation gives the impression that he accepts exactly that kind of interpretation of differential calculus which Marx defines as "mystical". Marx maintains that differential calculus is mystical in character; in fact it attains exact results by means of algebraically inexact procedures, as Marx says, it makes use of *exceptional laws*, that is, it confers contrasting properties to the terms employed; it resorts to devices devoid of any mathematical rigour, it resorts, that is, to "conjuring tricks". In Marx's opinion, calculus is to be dealt with in strictly mathematical terms, and in this sense he keeps account of Lagrange's contribution for the attempt of founding calculus on pure algebraic grounds. If procedures not founded upon demonstration were employed in differential calculus, this was not due to the dialectic character of such procedures, as Engels seemed to

believe when he explained that the lack of understanding, on the part of contemporary mathematicians, of Leibniz's differential calculus was caused by the impossibility of understanding the principles of calculus on the basis of formal logic. On the contrary, it was due to the fact that differential calculus was based on metaphysical and non-dialectical definitions.

4.

The Marxist conception of the individual is founded, of its very essence, upon criticism of naturalistic, innatistic, and biological interpretations of human behaviour. With reference to Marx's *Theses* on Feuerbach, Schaff shows how naturalism is materialism though in a limited form. Man is reduced to the mere status of the biological specimen and human relations are simply viewed as relations among individuals of the same species. The human being is certainly biological, a specimen of the species *homo sapiens*, but in his specific reality as man, he is the product of historically determined social relations. The description of man as a mere biological specimen is not enough to characterize him given that he is determined not only by biological conditions but also by social conditions: he is fundamentally a historical and social being. His "natural" delimitations are the result of an evolutionary development conditioned by social and historical situations.

On the basis of molecular biology, especially in research pertaining to the genetic code, biologistic interpretations of man are formulated. These, however, cannot be proven in the present state of scientific research. Despite this, however, a biologistic interpretation goes as far as expecting to explain something which is intrinsically socio-anthropological, that is, language - which together with material work constitutes the basis of the human and cultural world. The success of molecular biology explains Chomsky's belief in innate ideas and their translation into biological terms by Lenneberg.

In his essay "Gramatyka generatywna a koncepcja idei wrodzonych" [1972], Schaff analyses Chomsky's conception of language and deals with the possibilities of either accepting or rejecting the existence of innate and universal grammatical structures. This issue has been discussed by linguists and philosophers of language and many arguments have been brought forward both by those who agree with the innatistic theory such as Chomsky, Katz, Lenneberg, as well as by those who reject it.

Schaff makes a specific contribution to the debate: he is aware that this problem can be settled by neither philosophers nor linguists, but only by the specialists of natural sciences, particularly molecular biology. In Schaff's opinion, given that scientific research in its current state cannot give an answer of any kind, neither the innatistic nor the non-innatistic point of view can be scientifically proven. Schaff's purpose is not to solve the problem but to prospect it in the right terms; he aims at uncovering implicit assumptions and the logic of the arguments put forward by the parties involved.

In his analysis of generative transformational grammar, not only does Schaff underline the links with so called "Cartesian linguistics" (that is the philosophical tradition from Descartes to Humboldt), but also with contemporary mathematical logic, and particularly the school of the logical analysis of language (above all the Vienna Circle and the Lvov-Warsaw School). From this point of view, we have two main forerunners of generative transformational grammar: Carnap and Ajdukiewicz. For an understanding of the "filiation of ideas" underlying generative transformational grammar we need to consult Carnap's *The Logical Syntax of Language*, and Ajdukiewicz's works published by "Erkenntnis" in 1930. Rules of the theory of language (rules of meaning and of syntax), absent, as Chomsky points out, in traditional structuralism, while on the contrary, fundamental to the conceptual apparatus of generative grammar, were particularly developed by neopositivism with Ajdukiewicz as one of its major representatives (see *Gramatyka generatywna*, 1972). Thus the semantic component of transformational grammar (the others being the syntactic and the phono-

logical), gives deep structures semantic meaning and behaves in the same way as Ajdukiewicz's *rules of meaning*.

The theory of generative grammar aims at being a universal model capable of explaining the creativity of language also, that is, it presents itself as a model capable of generating and understanding an infinite number of sentences on the basis of a finite number of elements and a limited experience of language. The conception of innate structures underlying lingusitic behaviour and the linguistic apparatus is, therefore, fundamental to generative grammar. It is on the basis of this thesis that the universality of grammar and of deep structures is asserted. In Schaff's opinion, the thesis of "linguistic universals" is essential to generative grammar in the same way that the thesis of "linguistic differentials" is essential to the theory of linguistic relativity as conceived of by Sapir and Whorf.

In Chomsky's work, the assumption that innate and universal structures exist constitutes a preliminary axiom of generative grammar which therefore appears as a hypothetical-deductive model. Such an assumption not only takes on the value of a thesis to be verified, that is, a hypothesis, but also appears as an empirical thesis which has already been demonstrated, though this is not the case.

Schaff underlines the fact that Chomsky's conception of innate structures - which in his 1957 review of Skinner's *Verbal Behaviour* was simply put forward as a hypothesis and a prudently formulated postulate - was presented in increasingly stronger terms in his later works. This is particularly true of *Recent Contributions to the Theory of Innate Ideas* [1967] and of *Language and Mind* [1968]. What Schaff wants to stress is that such a development is not the result of scientific research but of reference back to a certain philosophical tradition. In fact, Chomsky presents Generative Grammar as a return to rationalism and to the tradition of "Cartesian Linguistics".

One of Schaff's main criticisms of the innatistic theory of language is that Chomsky, Katz and Lenneberg claim an empi-

rical character for their theses when, in fact, the natural sciences (and particularly molecular biology which should be the eventual source for the solution to such issues), are not, as we have seen, in a position to give a satisfactory answer at the present moment. This does not mean, however, that the problem is empirically insoluble. Schaff demonstrates this with his analysis of two of the most important representatives of modern biology: François Jacob and Jacques Monod.

Jacob observes that the more the nervous system of animals is developed, the less rigid is their hereditary nature. In the genetic code we may distinguish between a fixed component and an open one, which assures a certain amount of differentiation between one individual and another in ontogenetic development. In Jacob's opinion, speech is determined genetically but at the same time, it is related to the second and open component, that is, the capacity of learning any language is a possibility, a potential. This is very different from maintaining that every man is in possession of an innate generative grammar, as Chomsky asserts. Moreover, Jacob believes that human behaviour is characterized by the lack of any rigid conditioning on the part of a genetic code, so that symbolic systems mediate and act as a filter in the interaction between any organism and his environment. Culture is viewed as a second genetic system which overlaps biological heritage; therefore, the human world - historical and social reality - cannot be explained solely in biological terms. Jacob does not take a clear stance concerning the concept of innate structures, but he does agree that science is not yet ready to give and answer.

On the contrary, Jacques Monod agrees with Chomsky's conception. As Schaff points out, however, Monod has no scientific proof to support this conception which appears as a hypothesis that he leans towards more than as a scientific theory.

If, on one hand, the conception of innate ideas is legitimate and cannot be rejected as such, on the other it cannot be given scientific status as it has not yet been empirically proven. Consequently, in Schaff's opinion, given the impossibility of arriving at a solution to the problem, any remarks

concerning Chomsky's innatism should be restricted to the formulation of the problem and to the criteria employed to deal with it.

As regards this aspect, Schaff points out that we cannot accept the hypothesis of innate linguistic structures simply because there are no available alternative scientific theories to confute it with. Moreover, Sapir and Whorf offer an opposite hypothesis - that of linguistic relativism - which in its turn has never been empirically confuted. To verify Chomsky's thesis, not only would we have to prove the existence of *innate* learning mechanisms, but we would also have to prove that such a mechanism is universal, that is, that linguistic structures are the same for the whole of the *homo sapiens* species.

One of the weakest aspects of Chomsky's theory of language is that while he insists on innatism, the language sciences, and especially sociolinguistics and ethnolinguistics, insist on stressing the historical and social character of language. Rather than being considered in the terms of something which is either natural or non-natural, the latter is a social and historical phenomenon. Chomsky and Lenneberg could not deny that the learning and the use of language are conditioned by society, but they reduce social relations to relations among individuals of the same species. The social environment is viewed in the terms of any natural environment necessary to the development of attitudes peculiar to the species. The social factor is nothing more than input formed from sentences pronounced by people living in the same environment, it sets off the innate mechanism of language learning and creates the linguistic competence inherent in the particular language the subject is exposed to. Concerning such an interpretation of social conditioning, first of all, for Schaff the statement that the quantity of input (that is, the quantity of sentences we are exposed to in childhood) does not affect the output (that is, the production of spoken language), is false. In fact, if a child is exposed to incorrect language, he too will speak incorrectly as compared to official grammar. Secondly,

the social factor does not merely consist of sentences listened to by the speaker, it is also the relation between language and social praxis where language develops according to particular social and historical structures. Language itself is the product of social praxis, it is the means by which the individual receives his historical heritage. The individual belongs to a specific social system: he speaks, thinks and behaves according to specific social values and causes which, as part of a society divided into classes, have a class character.

5.

Linguistic analysis and the sociology of knowledge together with Marxism, contribute to putting into evidence the social character of thought and consequently its social and ideological nature.

Concept and meaning are two faces of the same phenomenon: this phenomenon is thought-language. There is no meaning outside natural language or independently of linguistic signs. However, the verbal sign is not only closely connected to concept, but also to what Schaff calls the *stereotype*. It is related to beliefs, established opinion, emotional tendencies, group and class interests, and so forth. The stereotype is a specific reflection of reality related to specific linguistic signs; but since it involves emotional, volitive, and evaluational elements, not only does it play a particular role in relation to cognitive processes, but also in relation to praxis. The stereotype is not simply a category of logical thought, it is also a pragmatic category. From language we receive concepts as the product of a certain society in the course of history; in the same way we receive stereotypes which carry with them specific tendencies, behaviour patterns and reactions.

This means that speech is always more or less ideological since it is connected to social praxis.

Schaff maintains that reflection upon the stereotype is characterized by a high degree of "intrusion of the subjective

factor" in the form of emotional, volitive and evaluational elements. This "subjective factor", however, is *social* and *not individual* in nature, it is linked to interests of social groups (social classes, ethnic groups which speak the same language and so forth). Seen in these terms the "subjective factor" is present in any form of reflection upon reality as well as in scientific knowledge. Schaff says:

> Science and ideology are closely connected to each other, in spite of those pedants who would like to separate them. In any case, since social praxis, which produces and promotes the development of language, is the common basis for both the relatively objective knowledge of the world, and for attitudes of evaluation, a genetic link exists
>
> [*Szkice z filozofli jezyka*, 1967, It. trans., 1969. p.127]

Schaff singles out the following relation between stereotype and ideology: "it is not possible to directly identify the stereotype with ideology but the latter could not subsist without the stereotype".

We may also deal with problems concerning ideology and the "subjective factor" of human knowledge - where the subject, as we have seen, is viewed as a social rather than individual product - from the viewpoint of the sociology of knowledge. This discipline, in fact, acknowledges the subject as a socially produced and conditioned individual. As Schaff frequently stresses, the sociology of knowledge derives from Marxism and particularly from the structure and superstructure theory; it is also directly related to gnoseology and to the theory of knowledge.

Schaff divides the definition of the concept of ideology into three groups so as to avoid any ambiguity and equivocations: a) the *genetic definition*, which examines the conditions of development of ideology; b) the *structural definition*, which attempts to define the specific character of ideology, and therefore to establish the differences, from the logical point of view, between the structure of ideological discourse and the structure of scientific discourse; c) the *functional definition*, which underlines the functions fulfilled by ide-

ology in relation to social, group, and class interests, etc.

Furthermore, Schaff believes in the necessity of distinguishing between the problem of the definition of ideology on one hand, and the problem of the value of ideology in relation to objective truth, on the other. Though related, these problems are different and should not be confused: the definition of ideology is one thing, while its value in relation to the question of objective knowledge is another. Therefore, though apparently a definition, the statement "ideology is false consciousness", is not, in fact, a definition, but is rather an answer to the question of the value of ideology. The main error made by Mannheim in his theory of ideology and in his criticism of Marxism, lies in his having mistaken the statement "ideology is false consciousness" for a definition of ideology.

We also need to distinguish between the meaning Marx and Engels gave to the word "ideology", and the meaning it was given in the Marxist tradition (especially from Lenin onwards). Such expressions as "bourgeois ideology", and "ideological science", are very much in use. They characterize ideology on the basis of its function. In Schaff's opinion, therefore, we may give the following functional definition of ideology: by ideology we mean a system of opinions related to the aims of social development which are founded upon a system of values; these opinions are at the basis of specific attitudes and behavioural patterns in the different objective situations.

Marx and Engels employed the word "ideology" in a narrow sense, that is, in reference to the "ideology" of the bourgeois class. Leading-class ideology aims at the preservation of a society divided into classes. Consequently it aims at concealing those contradictions which reveal the necessity of transformation in the current structures of productive relations. Bourgeois ideology is thus characterized by Marx and Engels as *false consciousness* with respect to objective consciousness. Marx and Engels consider ideology as false consciousness because they use the word in a narrow sense, that is, in reference to the ideology of the bourgeoisie, and not in the broad sense where the reference is to the "ideology of the proletar-

iat", to "scientific ideology", and so forth. When Mannheim maintains that if ideology is generally false consciousness, then Marxist ideology is also false, he makes a mistake for he identifies ideology in the narrow sense with ideology in the broad sense (cf. Schaff 1970: *Historia i prawda*).

We may summarize what we have said with the following points: 1) the statement "ideology is false consciousness" is not a definition; 2) when we speak of ideology as false consciousness we are referring to bourgeois ideology which aims at the reproduction of class society and of social inequalities; 3) use of such expressions as "ideology of the proletariat", and "bourgeois ideology", is now frequent. In Schaff's opinion, by considering these points we become aware of the necessity of defining the word ideology in such a way as to explain its different meanings on one hand, and so that it suits the Marxian perspective on the other. In this sense, ideology may be defined as either all those opinions formed under the influence of the interests of a specific class (*genetic definition*), or, as those opinions useful to the defence of the interests of a specific class (*functional definition*).

It is by considering ideology in relation to its genesis and to its function that we are able to more properly face the problem of the value of ideology as related to the objective and scientific knowledge of reality.

It must immediately be said that according to Schaff this problem cannot be dealt with on the basis of a linguistic-*structural definition*. Ideological discourse does not have a specific structure which distinguishes it from scientific discourse. It is an error to maintain that the difference between science and ideology lies in the structure of their propositions. According to such an opinion, ideological discourse would mainly consist in evaluative and normative propositions, whereas scientific discourse would consist of descriptive propositions. Schaff severely criticizes the neo-positivistic dichotomy between judgements of facts and judgements of value. This dichotomy appears in Marxism in the forms of the division between science and ideology.

The difference between science and ideology does not consist in the fact that the "subjective factor" (which, as we have seen, is social and not individual), is present in science and absent in ideology. It consists, rather, in the diversity of the *role* of the "subjective factor" which *is present in both science and ideology*.

Scientific analysis and the sociology of knowledge have made an important contribution to the destruction of the myth of the *pure objectivity* of scientific propositions. Given that both science and ideology are conditioned by society, both are in a certain sense subjective (at least because language without which human thought is impossible, introduces subjective elements in every form of human knowledge). Therefore, in Schaff's words

> in contrast to the thesis which sets science against ideology, another thesis is here presented. It maintains that not only are the propositions of science and of ideology linked, but in some cases they are identical
>
> ["La définition fonctionelle de l'ideologie", 1967, p.51]

to the point of being able to speak of "ideological science" and of "scientific ideologies".

Schaff stresses that recognition of the fact that every discourse is more or less ideological because of social and historical conditioning, does not imply that all ideologies are distorted and to be placed, therefore, at the same level. We need to distinguish between true ideologies and ideologies as *distortions* of reality; between *scientific ideologies* and *forms of false consciousness*. This distinction is determined by the different genesis and the different function of ideologies.

University of Science
Bari

References

Borbé, Tasso [1973] (editor) *Der Mensch Subjekt und Objekt. Festschrift für Adam Schaff*, Wien, Europa Verlag.

Colletti, Lucio [1974] *Intervista politico-filosofica*, Bari, Laterza.

Geymonat, Ludovico [1977] *Scienza e realismo*, Milano, Feltrinelli.

Luporini, Cesare [1974] *Dialettica e materialismo*, Roma, Editori Riuniti.

Petrilli, Susan [1984] (editor) *Dialogue, Iconicity and Meaning. Readings*, Bari, Adriatica

Marx, Karl [1968] *Matematicheskie rukopisi*, Moskva (It. trans. Bari, Dedalo, 1975).

Ponzio, Augusto [1973] *Produzione linguistica e ideologia sociale*, Bari, De Donato (Yugoslav trans. *Jezicna proizvodnja i drustvena ideologia*, Zagreb, Skolska knjiga, 1978).

[1974a] *Persona umana, linguaggio e conoscenza in Adam Schaff*, Bari, Dedalo.

[1974b] *Filosofia del linguaggio eprassi sociale*, Lecce, Milella.

[1975a] *Dialettica e verità. Scienza e materialismo storico-dialettico*, Bari, Dedalo.

[1975b] "Il problema dell'individuo umano e la traduzione e il senso delle tesi di Marx su Feuerbach", introduction to A. Schaff, L. Sève, *Marxismo e umanesimo. Per un'analisi semantica delle tesi su Feuerbach*, Bari, Dedalo, 1975, pp. 5-46.

[1976] *La semiotica in Italia*, Bari, Dedalo.

[1977a] "Filosofia del linguaggio, logica formale e dialettica marxista", introd. to A. Schaff, *Theoria della*

conoscenza, logica e semantica, Saggi filosofici/1, Bari, Dedalo, 1977, pp. 5-25.

[1977b] "Introduzione ad Adam Schaff", in *Annali della facoltà di lingue e letterature straniere dell'Università di Bari*, pp. 407-429.

[1977c] "Intervista a Adam Schaff": s. Schaff 1977a

[1978a] "Il marxismo come scienza e come ideologia", introd. to A. Schaff, *Che cosa significa essere marxista, Saggi filosofici*/2, Bari, Dedalo, 1978, pp. 5-34.

[1978b] "Il significato politico dell'umanesimo marxista", introd. to A. Schaff, *La questione dell'umanesimo marxista, Saggi filosofici*/3, Bari, Dedalo, 1978, pp. 5-26.

[1978c] "Semiosis, Referent and Sign Production in a Theory of Semiotics", in *Ars Semeiotica*, 2, 1978, pp. 17-35.

[1978d] *Marxismo, scienza e problema dell'uomo*. Con un'intervista ad Adam Schaff, Verona, Bertani, 1977.

[1979a] "Preface" to Adam Schaff, *L'alienazione come fenomeno sociale*, Roma, Editori Riuniti, 1979, pp. 7-18.

[1979b] "Note su semiotica e marxismo" in *Versus*, Bompiani, 1979, pp. 3-14, Engl. transl. in PETRILLI, S. (ed.) [1984]: "Notes on Semiotics and Marxism".

[1979c] "Note sul concetto di contraddizione in Marx", in *Scienze umane*, 2, 1979, pp. 126-146.

[1980] *Michail Bachtin. Alle origini della semiotica sovietica*, Bari, Dedalo.

[1983] "Abduzione e alterità", in *Versus*, 34, Bompiani, 1983; Engl. transl. in PETRILLI, S. (ed.) [1984]: "Symbol, Alterity and Abduction".

[1984] "La semiotica fra Peirce e Bachtin", *Documenti*/1 of the "Centro Romano di Semiotica", Roma; Engl. transl. in PETRILLI, S. (ed) [1984]: "Semiotics between Peirce and Bachtin".

Sartre, Jean-Paul [1960] *Critique de la raison dialectique, Paris, Gallimard.*

Schaff, Adam [1947a] "Humanizm socjalistyczny", *Kuźnica*, 19.2 and 26.2.1947; in Schaff 1975.

[1947b] "Sens dyskusji o humanizmie socjalistycznym", *Kuźnica*, 18.8 and 25.8.1947; in Schaff 1975.

[1960] *Wstęp do semantyki*, Warschau, Verlag PWN.

[1961] *Marksizm a egzystencjalizm*, Warschau, Verlag "Książka i Wiedza".

[1964] *Język a poznanie*, Warschau, Verlag PWN.

[1965] *Marksizm a jednostka ludzka*, Warschau, Verlag PWN.

[1967] "La définition fonctionnelle de l'idéologie et le problème de la 'fin du siècle de l'idéologie'", *L'homme et la société*, 3, 1967.

[1970] *Historia i prawda*, Warschau, "Książka i Wiedza".

[1972] *Gramatyka generatywna a koncepcja idei wrodzonych*, Warschau, "Książka i Wiedza".

[1974] *Strukturalismus und Marxismus*, Wien, Europa Verlag

[1975] *Humanismus, Sprachphilosophie, Erkenntnistheorie des Marxismus*, Wien, Europa Verlag.

[1977a] "Strutture, linguaggio e ideologia. Tre domande ad Adam Schaff", edit. by A. Ponzio, *Lares*, 2, 1977, pp. 295-309.

[1977b] *Entfremdung als soziales Phänomen*, Wien, Europa Verlag.

[1979] *L'alienazione come fenomeno sociale*, pref. by A. Ponzio, Roma, Editori Riuniti. Enlarged edition by Schaff [1977]: contains the chapter "Sull'alienazione nella rivoluzione", pp. 387-418.

[1980] *Stereotypen und das menschliche Handeln*, Wien, Europa Verlag.

[1982] *Die kommunistische Bewegung am Scheideweg*, Wien, Europa Verlag.

[1983] *Polen Heute*, Wien, Europa Verlag.

Schaff, Adam; Sève, Lucien [1975] *Marxismo e strutturalismo. Per un'analisi semantica delle Tesi su Feuerbach*, Bari, Dedalo 1975, edit. by A. Ponzio (contains the papers by Schaff and Sève relative to the debate in *L'homme et la société*, 1971-72)

Semerari, Giuseppe [1974] *Filosofia e potere*, Bari, Dedalo.

[1979] *Civiltà dei mezzi e civiltà dei fini*, Verona, Bertani.

Sève, Lucien [1973] *Marxismo e teoria della personalità?* Torino, Einaudi.

ALREADY APPEARED:

1984
DOXA I (in Hungarian)

Contents:

1984

DOXA 2 (In English)

(Special issue on the occassion of the 5th Joint Conference on History and Philosophy of Science, 14-20 August, Veszprém, Hungary)

Contents:

1984

DOXA 3 (In English)

(Special issue on the occassion of the 5th Joint Conference on History and Philosophy of Science, 14-20 August, Veszprém, Hungary)

Contents:

1985
DOXA 4

Contents:

1985

DOXA 5

(Special issue on logical negation)

Contents:

TO APPEAR:

1986

DOXA 7 (In Hungarian)

Proceedings of the Conference "Lukács and Present-day Culture" held in Budapest, 29-31 October 1985, Vol.1.

Contents:

1986

DOXA 8 (In Hungarian)

Proceedings of the Conference "Lukács and Present-day Culture" held in Budapest, 29-31 October 1985, Vol.2.

Contents:

For back issues, write to:

MTA Filozófiai Intézete - DOXA
1054 Budapest, Szemere u. 10
Hungary

DOXA 6

FILOZÓFIAI MŰHELY

Filozófiai Intézet
Magyar Tudományos Akadémia * Budapest