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MEANING, ACCEPTANCE AND DIALECTICS*

1. INTRODUCTION

It is the aim of this paper to present a general framework for meaning, acceptance, and their dynamics. The central adequacy condition for this framework is that it should enable us to understand science as a rational enterprise; and by 'science' I obviously mean the scientific enterprise as it is depicted by contemporary philosophy of science. I do indeed believe that the available frameworks prove inadequate if they are compared with the results arrived at by the philosophy of science. Apart from this, the framework presented in this paper should also enable us to understand meaning and rational belief as they occur outside the realm of science properly. The view that rationality is not specific to science is rapidly gaining consent, and rightly so. Nevertheless, science stays a privileged source for an inquiry into rationality because of its specific organizational aspects, the systematic way in which its results are organized, and the availability of detailed historical studies.

The final section of this paper deals with dynamic dialectical logics. My aim in adding this section is to show how, with the help of those logics, the outlook defended here enables us to solve several extremely important problems in a completely natural way.

My approach to meaning and acceptance (certainty, rational belief, etc.) is contextual in a specific sense. There is of course nothing very original about taking a contextual outlook to science and to knowledge in general. I should warn the reader, however, that the contextual outlook I shall defend is rather extreme.

The motivation for the approach lies in the fact that science is a dynamic, collective enterprise, and that its progressive character depends essentially on the communication between adherents of competing theories and competing research traditions. It follows that we should develop a theory about scientific communication and about its impact on scientific change.

J. C. Pitt (ed.), Change and Progress in Modern Science, 333–360.

What I mean by 'a context' is precisely a communication situation. A context is characterized by (i) a set of participants, (ii) the problem that one tries to solve, (iii) the set of statements that are regarded as certain and in this sense define the set of possible answers to the problem, (iv) the set of aspects that are considered relevant to the problem, and (v) the set of methodological do's and don'ts that are judged appropriate with respect to the problem. It would of course be a question-begging oversimplification if I were to suppose that the context, defined in this way, is identical for all participants. For this reason, I state from the outset that the context may be different for each participant, and that it may change – in the sense that one of the elements changes – for one participant, while remaining constant for the others. In other words, one participant should be singled out in order to describe a context.

In general, contexts may change very rapidly. If one of the participants judges that he is misunderstood by one of the others and starts explaining the sense in which he uses some term, the context changes and, if there is minimal communication, changes for all participants. If, after this *interludium*, the participants return to the original problem, it is quite possible that they do not return to their original contexts because some of the statements previously regarded as certain by some participant, may have been given up by this participant. Or consider a scientist who performs an experiment in the presence of another scientist, because he believes that the experiment constitutes a problem for the theory which the other accepts. There is a context in which the outcome of the experiment is determined. There is another context in which it is decided whether the outcome of the experiment does constitute a problem for the theory of the second participant. And there is a third context in which it is decided whether this problem, together with other problems, requires the rejection of one theory in favor of the other.

I do of course realize that some scientific activities occur in situations in which there is only one participant. Nevertheless, I shall still employ the term 'context', on the one hand because, such situations may be seen as limit cases of communication situations, and on the other hand because, if it really concerns a scientific context, the participant behaves in principle as if other participants were present.

2. MEANING

It goes without saying that philosophers of science are not in any direct way interested in problems about meaning. Some connected problems belong to their direct concern, however, e.g. the relation between linguistic entities and some set of human activities, and the problem of communication among scientists. It has been argued that such problems should be solved with the help of a theory of meaning, but the trouble is that the theories of meaning that are usually invoked in this connection fail to do the job. For example, they fail to explain how it is possible that there is communication between scientists that work in different research traditions. Some fail because their alleged explanation works only at the expense of a drastic simplification of the relation between different conceptual frameworks; others fail because they are inadequate even with respect to the actual situation as depicted by their adherents.

Needless to say, this failure constitutes a serious drawback. Nobody denies the possibility of communication between scientists that work in different research traditions or paradigms. Given that science is a collective enterprise, the problem of communication is a central one. And it clearly belongs to the job of a theory of meaning to explain how communication is possible.

Most views on meaning that we encounter in philosophy of science texts contain or presuppose two myths. The first is the MYTH ABOUT COMMUNICATION (MC):

> MC. Communication between X and Y is only possible if either all terms have the same meaning for X and Y (identity), or at least certain terms, viz. the relevant ones, have the same meaning for X and Y (overlapping), or the meanings of all or some terms resemble each other well enough (somewhat in the sense of Putnam's "charitable account of meaning", (Putnam 1978, p. 22) or all terms of some metalanguage have the same meaning for X and Y, or all terms of some specific part of the language have the same meaning for X and Y (common sense).

Feyerabend has argued against the myth about communication in numerous places, and it seems to me that his arguments are extremely convincing, whereas in their replies his opponents either oversimplify the actual differences between conceptual frameworks (e.g., Krajewski 1977), or else avoid differences in meaning by redefining 'meaning' in an extremely vague way (e.g., Putnam 1978).

The second myth is the MYTH ABOUT MEANING (MM):

> MM. For any term of a given language, there is *the* meaning of this term with respect to this language.

Incidentally, I am convinced that one of the reasons why this myth is widely accepted, is that it is believed to be a necessary condition for the possibility of communication. My objection to the myth about meaning is not that meanings may change over time or that there are different aspects to meaning, for all this is compatible with the myth. My objection is that meanings vary constantly from one context to the other. Even in periods in which the conceptual framework is said to remain constant, there is no such thing as *the* (stable) meaning of a term, not even for one and the same scientist.

I shall now try to sketch a view on meaning in which both myths are given up. I need not argue for the following two theses on meaning:

> M1. The meanings of terms (in a given context) determine and are determined in part (and only in part) by the criteria one applies (within this context) to (provisionally) accept statements on the basis of observation.

> M2. The meanings of terms (in a given context) determine and are determined in part (and only in part) by the situations that are considered logically possible (within this context). (I shall from now on use the phrase 'possible worlds' for such situations).

The set of possible worlds is restricted by conscious decisions, which give rise to conventions, and by prejudices, which make some people believe in "intuitively obvious logical truths".

All of us agree that there is not actually some single set of all possible worlds; scientists working in different paradigms or research traditions do not actually share the same set of possible worlds. Most of us will even agree that there is not some single "correct" set of all possible worlds. Idealists of different brands will agree to this, as will certain realists, e.g., Ilkka Niiniluoto, who holds that THE WORLD "is not carved up into 'pieces' and 'facts' without human conceptualizing" (Niiniluoto 1980, p.441). Even if there were some single "correct" set of all possible worlds, it is obvious that what some person means by a term does not depend on this "correct" set, but depends on *the person's* set of possible worlds.

This brings me to a third thesis, which will require some more argumentation.

> M3. Sets of possible worlds, and hence meanings, vary with individuals, not with groups.

I do not want to say, of course, that there is no resemblance at all between the meanings of terms for two scientists, X and Y, that belong to the same research tradition. What I do claim is that in some cases it may be demonstrated that X and Y use terms with different meanings, and have even different sets of possible worlds; in general there is no good reason to believe that X and Y use all terms with the same meaning, or even that they share the same set of possible worlds. I mention three arguments.

The first argument is rather vague. If we take meaning seriously, if we take intensions and connotations into account, if we realize that an individual does not receive but, rather constructs meanings while interacting with this social environment, then we can conclude only that meanings vary from one individual to the other.

For the second argument I start by considering an objection. Suppose one describes Newtonian mechanics at some stage of its development, i.e., one describes the calculus, the intended domain of application, the set of exemplars, the instruments, the measuring techniques, etc.

One might claim that each term has some single meaning, which is fully determined by the enumerated entities. This claim is false for reasons that become clear later on. But more importantly, the claim is irrelevant to my present point. The theory under consideration is a fossil (and incidentally, a reconstructed one); philosophy of science, even where it takes a synchronic viewpoint, should care about real, living theories. The working, creative scientist does not deal with his theories in the way we may deal with fossils. For him the aforementioned aspects or elements of theories are not "given" but are worked upon. He has personal views, personal prejudices, personal misunderstandings, personal ideas, and he may be able to express some of these in language. So, he has *his* set of possible worlds and *his* meanings.

Now the third argument. The idea that meanings are common to all members of some group may seem to be acceptable from the point of view of Kuhn's paradigms. If you look closer, there are doubts as to whether this is possible. And if you look at Laudan's characterization of research traditions, then you can only reject the idea. Laudan points out the methodological importance of the relations between a scientific theory and theories from other domains, including metaphysical and other "non-scientific" theories. He shows that theories may travel from one research tradition to the other. And, most importantly, Laudan argues that a scientist may belong to different research traditions, some of which may even belong to another domain than the one the scientist is working in. In view of all this, no one can reasonably hold that meanings, or sets of possible worlds are shared by all scientists that work in the same research tradition.

In defending M3 I have been attacking the MYTH ABOUT MEANING. Yet, my attempt at destroying this MYTH is still incomplete, as may be seen from my fourth thesis about meaning;

> M4. Meanings are local, not global; they vary (for the same person) from one context to the other.

Expressed differently, a person may consider A to be logically possible in one context and not consider A a logical possibility in another context. In arguing for M4 I shall proceed in two steps.

First, the set of statements that are regarded as certain, varies from one context to the other. If an astronomer is trying to determine the position of a star, he accepts as certain, e.g., optical laws according to which his telescope has been constructed, a statement about the speed of light, and so on, and of course he accepts as certain that he is looking at the stars and not at a movie picture, that he is not having a dream, that the color of his eyes and the presence of a pot of flowers in the room are both irrelevant for the reliability of his result, etc. But it is obvious that each of these statements may be questioned in some other context. Examples of this kind may be repeated *ad nauseam*. We all know that lots of presuppositions are involved in any observation, in any actual explanation, in any prediction, in any judgment about the acceptability or rejectability of a hypothesis or theory. And it is obvious that the set of accepted statements may differ drastically from one context to the other. Incidentally, some people, e.g. Carnap (1968, pp.146-150) and Bar-Hillel (1968, pp.150-161), have argued that not a single general statement may ever be accepted, but I shall explain in Part Three why their arguments do not hold water.

Second, the variety of sets of statements that are regarded as contextually certain, results in a variety of contextual *logical* truths, and hence in a variety of contextual *meanings*. Let us use the term 'absolutists' for those who believe in context-invariant meanings. It should be pointed out that my disagreement with the absolutists is not a verbal one. The disagreement is not about how we use the phrase 'the meaning of a term'; it is about what people mean when using some term in some context.

With respect to each context there are statements that are regarded as certain, and there are other statements that are relevant to the problem that one tries to solve within the context. Some of the latter may be or become accepted in the context, but this is immaterial to my disagreement with the absolutists.

The absolutist will say that there are, within a given context C1, two kinds of statements that are regaded as certain, viz. logical truths and factual truths. Clearly, he can only make this distinction by referring to one or more other, related contexts C2, C3..., in which all "logical" truths of the first context C1 are kept, whereas no "factual" statement that is regarded as certain in C1, is regarded as certain in C2. I will not discuss the very complex implications of the absolutist's distinction in detail but merely point out one of them: the absolutist cannot accept that there is a context C3 (which is "more general" than C2) in which some logical truth of C1 is not a logical truth. If he were to accept this, he would admit that his choice of C2 to distinguish between logical truths and factual truths in C1, is plainly arbitrary; hence his criterion would break down. It follows that the absolutist must believe that there is some context, in which all and only logical truths are regarded as certain, and that there is no context in which some logical truth is questioned. Let us call this *the most general context*.

Even the absolutist will agree that any conceptual system contains prejudices, that the meaning of any term contains factual presuppositions. Even if we do not know which are the prejudices or presuppositions, we know that there are such. Hence, the most general context cannot be most general *in principle*, and the absolutist may at best refer to the most general context that some scientist may *actually* imagine.

Consider a scientist X who accepts some theory T. He meets another scientist Y who adheres to a rival theory T', that was unknown to X, and they discuss their respective theories. X continues to accept T, but, as a consequence of the communication, (typically) discovers some prejudice in the conceptual framework of T. In other words, the most general context that X could actually imagine, is replaced by a more general one. The absolutist is then driven to the following absurd conclusion: although X continues to accept T, this is *not the same* theory T, for the meanings of its terms have change *in all contexts*.

The absolutist might try to reply that X may introduce a meaning postulate, and in this way bring the terms back to their original meanings. However, in using this argument the absolutist would contradict himself. Indeed, the meaning postulate is false in the most general context X may imagine, and consequently the meaning postulate is a *factual* truth by the very criterion the absolutist introduced in the first place.

According to my view, however, X may introduce such meaning postulates in some contexts, and, consequently, may keep believing the same theory as before. It is precisely by defining meaning as context-dependent that we may avoid the conclusion that our views about the world change whenever we realize that some term may have another meaning than the one in which we used it in some context.

Apart from this theoretical argument there is the very simple fact that we may all easily imagine a situation in which a chemist *means* something about molecules and atoms when he uses the term 'water'. Such examples however, do not convince absolutists, because they will claim that there is some way to analyse what is meant by the chemist in terms of a combination of meanings on the one hand and of accepted factual truths on the other. By offering the theoretical argument I hoped to show that such complications are superfluous and useless.

Incidentally, some will think that the term 'logical truth' is too strong in connection with contextual meaning, but it is not. The meaning of logical terms is just as context dependent as the meaning of referring terms, and I shall show in Part Four that the meaning of negation changes if we move to a larger set of possible worlds than the one considered by two-valued logicians.

Up to now I have mainly considered contexts in which there is only one participant. I did so in order to avoid unnecessary complications, but now it is time to mention some contexts in which there is more than one participant and to explain under which circumstances there is communication between them.

Let us consider a very simple example of an adherent of classical mechanics, X, and an adherent of relativistic mechanics Y who together want to measure with a given precision the length of some normal seized object. In order to describe the context for each of them, we have to state both for X and for Y which aspects are considered relevant to the problem and which is the set of statements regarded as certain. It is obvious that the context for X is different from the context for Y: their sets of logical truths are different; their worlds, although restricted to the relevant aspects, are populated by different objects; and the meaning of the term 'length' is different. Nevertheless, there will be communication in this pair of contexts. *In general* there is communication between X and Y in a given pair of contexts if both X and Y believe truly that there is a one-one relationship R between the possible worlds of X and the possible worlds of Y such that, if RwXwY, then X will decide in this context that wX obtains under the circumstances under which Y decides in his context that wY obtains, and *vice versa*.

Several points are in need of clarification. I require that X and Y believe this condition *truly* because, if they merely believe it, there might be no communication, even if they believed that there is. I also require that X and Y *believe* that the condition is fulfilled, for if they do not believe so, there can be no communication. Next, both X and Y may describe in their own language the circumstances under which they decide to wX and wY respectively. But this is not a hindrance for the communication: X believes that Y will decide to wY under the circumstances he describes by means of his own language; and analogously for Y. Finally, X and Y need not believe that they in general decide under the same circumstances to wX and wY respectively; all that is required is that they truly believe that they do in *this* pair of contexts.

The aforementioned criterion may easily be adapted to define communication between more than two persons, partial communication, "one-sided" communication, pseudo-communication with an imaginery partner like one's former self, or an actually absent person, etc.

On first sight it might seem that the fact that meanings are contextual is a hindrance for communication. It is true that communication would be easier and less partial if all humans shared the meanings of all terms. However, the very fact that this is not so, makes the contextual character of meanings a necessary condition for communication. If the set of possible worlds were not restricted to the aspects that are relevant to the problem of the context, the one-one relation mentioned in the criterion could never obtain. Moreover, if the absolutist's account were correct, and meanings would not be determined within some context by *all* statements regarded as certain, then again a one-one relation would never obtain between the respective sets of worlds of two scientists belonging to different research traditions.

It is important to point out that the kind of communication we arrived at is a very powerful one. It leads to a form of communication which allows for collective action, the exchange of observational reports, etc. Moreover, this form of communication is sufficient for the existence of scientific communities, for the possibility of cooperation in research. Within a scientific community, communication will be guaranteed with respect to a variety of problems: problems about observation, problems about the theories, problems about methodology. This communication is furthered by the fact that the members of a scientific community make the same decisions in a large number of contexts. For this reason we may consider scientific communities themselves as actors and as decision makers in a large number of contexts.

3. CONTEXTUAL CERTAINTY AND ITS DYNAMICS

There are not many left, today, who believe in the viability of Carnap's original program in inductive logic. Nevertheless, I think that it is instructive to take a quick look at part of the evolution of Carnapian inductive logic.

When Rudolf Carnap started developing his inductive logic in the early fourties, his aim was to find an explicatum for "the methods of inductive reasoning which are at present applied in science and statistics" (Carnap 1950, p. vii), and he meant inductive logic "in the wide sense of nondeductive or nondemonstrative reasoning" (Carnap 1950, p. vii). Carnap soon discovered that the most obvious "confirmation function" was inadequate with respect to his aim, and in 1952 presented an infinite continuum of confirmation functions that are defined with the help of a parameter which may be interpreted as expressing the degree of caution with respect to predictions based on inductive reasoning (Carnap 1952).

From 1965 on, Jaakko Hintikka presented a two-dimensional continuum in which a second parameter, α, was introduced.[1] It measures the degree of caution with respect to inductive generalization (I remember that, in infinite languages, the degree of confirmation of any contingent general sentence with respect to finite evidence is always zero for almost all confirmation functions of Carnap's continuum).

Even before Hintikka's results were published, Carnap and Kemeny had tried to avoid an inadequacy by introducing yet a further parameter: Carnap's and Hintikka's. The inadequacy was that confirmation functions allow for induction with respect to predictions that are identical to elements of the factual evidence, but do not allow for induction with respect to predictions that are only partly analogous with respect to the elements of the evidence.[2] This means that, if I write my name on my pencil, I cannot predict what will happen when I drop it, for I have never observed a pencil which had my name on it and was dropped.

As Carnap and Kemeny's solution applied to a special case only, I tried, more than ten years ago to find a general solution. I soon discovered that the problem of analogy and partial analogy is far more complex than it seems at first sight. I found a solution, which was published in 1975 and which I still take to be right from the viewpoint of Carnap's program (Batens 1975).

However, I am now convinced that this solution shows that Carnap's program is *completely misguided*. I mean that a probability measure, defined for all sentences of some language, and which agrees at least with our most simple "intuitions" on inductive support, does not exist. The reason is that any such probability measure should solve the problem of partial analogy, and that the simplest such measure is too complex. I do not mean that it is too complex from a technical point of view. The fact that an explicatum for some set of intuitions is mathematically complex need not be an objection. What I mean is that the measure is too complex from a methodological point of view. In using arguments by analogy, we do not and cannot take into account all considerations that have to be taken into account according to the probability measure. And, incidentally, this measure is still far from complex enough to be applied to such generalizations as Galileo's law of free fall.[3]

There is a wide range of non-Carnapian approaches to induction, rational credence, etc. Instead of discussing some of these, I shall try to explain the outlook to which I came, partly in consequence of a long and intense involvement with Carnapian inductive logic. This outlook is a natural consequence of the contextual view on meaning I sketched in Part Two.

I shall defend a contextual view of rational belief, certainty, and induction. Let me point out from the beginning that making degrees of rational belief contextual is not in itself much of an advantage. After all, there are very general contexts, and we need degrees of rational belief in these too. As I see it, the main advantage of the approach I shall defend, is that it enables us to describe and understand as more or less rational the dynamics of our beliefs, and this includes the dynamics of our methodological beliefs. For quite some time now, Isaac Levi has been defending a contextual view on credal probability – I refer especially to his 1980 book – and many take his theory to be extreme; I shall argue that it is not extreme enough.

For a start, I claim that the statements that are regarded as certain within some context, and in this sense determine the certainty of possible worlds, are completely certain within the context: they are logically true, they receive probability 1, there is no room for doubt about them *within* the context. Of course, a scientist who accepts some singular or general statement within some context, may (under certain conditions on which I shall comment later) move to another context in which this statement is questioned; but this does not prevent the statement from being considered completely certain within the first context.

Some people will agree, but will add that such statements are only certain "in some sense". I claim that there is no meaningful and relevant sense in which those statements are not contextually certain. Maybe some statements are regarded as certain in more contexts than others. I doubt whether it is possible in general to numerically compare sets of possible contexts, but, even if it were, the number of contexts in which some statement is regarded as certain does not add anything to its certainty within a given context. And if the claim were that those statements are not, or at least should not be considered absolutely certain, then I readily agree, but only because no statement is justifiably considered absolutely certain, in that no statement is justifiably considered certain in all contexts.

In order to consider a statement as logically true and hence as completely certain within some context, it is not required that the certainty of the statement has been demonstrated or may in principle be demonstrated within some other context. Consider a theory that is regarded as logically true and completely certain by an engineer in a given context C1. It is quite possible and of course desirable that it has been demonstrated in some context C2 that the theory has to be preferred over its rivals and that it is reliable with respect to the domain to which the engineer applies it. But it is not and cannot be required that it has been demonstrated in C2 that the theory is true or that we may be absolutely certain about it. Or consider a context in which some measuring instrument is considered reliable. It obviously cannot be required that it has been demonstrated in some other context that we may be completely certain about the reliability of this instrument. No one could ever demonstrate this.

The fact that some statement is regaraded as logically true and hence gets probability 1, does not entail that we should keep believing forever that the statement is true. Of course, if A is logically true, its relative probability will always be 1. But this holds only within some context. We can move to another context, in which the statement is not logically true, in which, if it is an empirical generalization, it may be tested, or in which its acceptability is examined on the basis of some set of (contextually contingent) statements.

Before I proceed about contextual certainty, I shall spell out the way in which different contexts interact in general, and the way in which this interaction leads to changes to the set of beliefs, i.e. changes to the sets of contextual logical truths.

Any subject, whether an individual or a scientific community, has certain explicit or implicit beliefs about how to approach certain problems. For some problems, the set of beliefs will determine the selection of a set of logical truths, a set of relevant aspects and a set of methodological instructions. For kinds of problems the subject is not familiar with, the subject will first move to another context, in which the problem is to select the statements that have to be regarded as certain, the relevant aspects and the methodological rules that characterize the context in which the original problem has to be solved. Needless to say, any rational subject will sometimes declare itself incompetent to solve a problem.

Philosophers of science should not worry about how one arrives at one's first set of beliefs, but about the justificatory problems connected with given sets of beliefs. In this connection I am on the side of those who, like Isaac Levi, hold that a belief need not be questioned unless there is a good reason to do so. But, if there is a good reason, any belief should be open to investigation, even if it is methodological or logical in nature. One of the good reasons to question a belief might be the very fact that some subject sets itself the aim to question some set of beliefs. It would be wrong, however, to characterize science as an institutionalized form of questioning beliefs. The aim of science is to explore the world, to extend the available set of beliefs, and it is only by way of a fortunate side effect that the resulting process leads to good reasons to question beliefs.

Good reasons to question beliefs are always internal. A subject would not question some belief which conflicts with empirical evidence, if he had not among his beliefs some methodological rules which sanction the empirical evidence as reliable. It is extremely important that we consider methodological rules as beliefs, and not as unquestionable statements of some higher type. In general, any context-independent hierarchy of statements should be rejected. Methodological statements are taken for granted in contexts in which the problem is to decide about the merits of some empirical generalization, but the latter are regarded as completely certain in contexts in which the problem is to decide about the merits of methodological rules.

Although good reasons to question beliefs are always internal in the sense explained, their origin may be either internal or external. It is internal if the attempt to solve some problem shows that the set of beliefs is either inconsistent or incomplete; it is external if a problem originates either from observation or from communication. The progressive character of science is essentially connected to the fact that some good reasons to question beliefs have an external origin, and especially that some find their origin in communication.

The good reasons of an internal origin may be of two types. Those of the first type arise when a context proves inadequate in that the problem under consideration is solved in a wrong way or in a way that appears later to be wrong. Expressed in Laudan's terminology, this corresponds to falsification in the case of empirical problems, to inconsistency in the case of internal conceptual problems, or to implausibility or inconsistency in the case of external conceptual problems. The other possibilities of unsolved empirical or conceptual problems lead also to an inadequacy of the context, vix. that the problem cannot be solved because the context is incomplete. Under such conditions one also moves to another context, viz. in order to complete the set of relevant beliefs, but no statement of the original context need to be questioned.

Scientists and scientific communities may spontaneously generate problems and this is the second internal source of good reasons to question beliefs. Theories may be subjected systematically to critical examination, or some empirical domain may be explored. In the absence of good reasons of an external origin, this spontaneous problem generation does not occur very frequently and is not very fruitful. The impediment is that, in the absence of good reasons of an external origin, it is not clear why, e.g., one accepted theory should be questioned rather than another. Moreover in a context in which one theory is questioned, several other theories will be regarded as logically true; if the questioned theory is rejected, this rejection will be contingent on the order in which theories are questioned. But as it is not clear why one theory should be questioned rather than another under the considered circumstances, the rejection will be arbitrary. If spontaneous problem generation leads to the exploration of some empirical domain, the impediment is that, in the absence of good reasons of an external origin, the results obtained may be completely uninteresting.

Some good reasons to question staements regarded as certain in some context, have an external origin. They are of two kinds. Those of the first kind derive from the inadequacy of the context to solve a problem which is not itself generated by the theory or research tradition. What I mean is this: it is a problem for a research tradition that its latest theories do not enable one to predict, say, whether A or not A will be the case; but whether or not A will be the case is itself a problem, and is not in any way generated by the research tradition. It seems to me, incidentally, that, in his *Progress and its Problems*, Laudan's formulation is sometimes confusing in this respect. Most of the time he discusses problems of the first kind, and such problems are indeed central for the comparative assessment of theories and of research traditions. But the problems which theories have to solve are those of the first kind. Scientific theories have to solve problems that have an external origin, whereas problems that have an internal origin are solved by the rejection or acceptance of existing theories, or by projecting new theories.

But let me return to good reasons to question contextually certain statements. Some good reasons of an external origin are of a second type; these are related to the communication between research traditions or, more generally, to the communication between subjects that hold conflicting views. In this connection I refer to the detailed and well argued analysis offered in Laudan's *Progress and its Problems*. This analysis enables us to see how, as a result of communication between rival research traditions, each of these gets confronted with new problems and hence with new contexts. It also enables us to see how, if a context proves inadequate, one may, essentially as a result of this communication between research traditions, have good reasons to question some accepted statements rather than others.

According to the view I am defending, it is excluded that a unique acceptance criterion would take care of the acceptance of all statements. The acceptability of some statement is essentially related to the question whether the contexts in which the statement is regarded as logically true or is accepted as a methodological instruction, are adequate with respect to solution of their problems. A unique acceptance criterion will not do because, first, there is a variety of contexts, in which a variety of problems has to be solved, and, second, the statements that may be accepted as certain within some context are quite different in nature. The justification of the acceptability of some such statements will be that they are acceptable as nomologically true; for other statements the justification will be that they are acceptable as (merely) actually true; for still other statements the justification will be that they are true in most (but not all) cases. Such distinctions are discussed in my 1975 book (pp. 280-298), and although my general outlook on induction has changed, the proposals I offered there may easily be adapted to fit to my present view. In general, acceptance criteria should depend on the contexts in which the accepted statements have to serve.

I hope that I have made the relations between contexts sufficiently clear by now, and return to the notions of contextual certainty and contextual probability.

The choice of a probability distribution for some context is a methodological choice. Once we take the view that methods are subject to discussion and change, there is no reason for not treating methodological statements on a par with other accepted statements. And there is overwhelming evidence that methods are subject to change, that such changes may be justified (Laudan 1981) and that non-methodological beliefs are relevant to the justification. Only an extreme contextual view can account for this.

The justification of the choice of an inductive method for some context is extremely different from and extremely less complicated than the choice of a general inductive method. The problem of partial analogy, to take one example, is almost completely eliminated in view of the presence of statements about the world and of accepted statements about relevant aspects that are regarded as certain. The latter statements will enable us to disregard all kinds of information and, if the problem under consideration is the choice of a prediction about a partly known object (or fact or process), they will save us the complications that derive from the fact that the object is partly analogous to other objects with respect to properties that are considered irrelevant. Also (other) accepted statements about the world will save us lots of complications. If a statement expressing the melting point of a crystalline chemical compound is regarded as certain, the prediction of the temperature at which a sample of this compound will melt, will not require any reference to partial analogy considerations. And if it is given that all crystalline chemical compounds have a melting point, then, even if nothing is specified about the melting point of some such compound, one statement about the temperature at which a sample of this compound melted will enable us to predict, without taking into accouont *any* partial analogy considerations, the point at which another sample of this compound will melt. Under the same circumstances we may derive a general statement about the melting point of this compound. In all such cases there is no need to choose some value of a parameter; we do not even need an inductive method. Several methodological choices required by Carnap's model are thus taken care of in the contextual model by accepted statements *about the world*. This argument for local induction is well known.

In other cases we shall have to make methodological statements within the context. The (well known) advantage of contextual models over global models in this connection is that in the former methodological choices are much more restricted and may be different for different contexts. Moreover, even for the same context, the methodological choices need not be identical with respect to all properties. So, for example, we may take analogy in some respects as relevant to some prediction, while taking analogy in other respects as irrelevant. In other words, the overall choice of the value of parameters in the Carnapian model may be replaced by specific statements about the inductive behavior of certain properties. And of course each of the specific statements may be questioned and investigated by moving to an appropriate other context. Incidentally, it follows that a number of the symmetry considerations which Carnap introduced, should be rejected on the contextual view.

Apart from these arguments for a contextual approach and against global inductive methods, I also refer to Isaac Levi's arguments in favor of the thesis that only "serious possibilities" (Levi 1980) should be considered. My present position is rather close to Levi's position. One of the differences, however, is that Levi distinguishes logical possibilities from serious possibilities, and that he considers *the* set of logical, set theoretical and mathematical truths as the minimal set of accepted statements. The arguments I gave in Part One may be employed in defense of my position against Levi's. In this connection it is worth considering an objection by Henry Kyburg (1976, pp.191-215) against an earlier version of Levi's position. Kyburg's argument may be paraphrased as follows: It does not help much to restrict probability measures to contexts, because we need more embracing probability measures as soon as a statement which was accepted at some point is questioned; eventually we need a global probability measure. I hope that it is clear that this objection does not apply to my contextual approach. If a statement that is regarded as certain in a context C1 is questioned, and we investigate its acceptability in context C2, the probability measure employed in C2 need not be an extension of the probability measure employed in C1. For one thing, most statements that are contingent and relevant in C1 will be either irrelevant or even non-contingent in C2.

There is another very important difference between the contextual view I am defending and Levi's position, and I need to comment on it because it concerns an essential aspect of my view, viz. that the set of statements regarded as certain may be different from one context to the other. According to Levi's model there is, at any time, a corpus of accepted statements. This corpus remains the same for all contexts, i.e. is not dependent on changing aims or problems. Under certain conditions statements may be added to the corpus (expansion); under other conditions statements may be removed from the corpus (contraction). It is essential for Levi as well as for me that such changes occur only if there is a good reason for them; as long as there is no reason to change the corpus, one should stick to it without having to justify its elements.

I do agree with Levi that it is possible in some cases to define such a corpus with respect to some set of contexts. It is perhaps even possible to define one corpus of accepted statements with respect to all contexts which concern the set of scientific problems as defined at some point in time with reference to some research tradition. The set of statements regarded as certain within some context will then consist of those statements from the corpus which are relevant to the problem investigated in the context.

Even if it is possible to define a corpus in this sense with respect to some research tradition at some point in time, the contextual view I am defending has some clear advantages. Consider a scientist X from this tradition who discusses with a scientist Y from a rival tradition about the merits of their respective theories. For the sake of the discussion both X and Y will have to move to a context in which some of their previously accepted statements are not regarded as certain. But suppose that the discussion does not convince X to give up his theory, and that X has not been able either to demonstrate that Y's theory is to be rejected. Under these circumstances X will certainly not give up his theory but neither will he decide that both his and Y's theory are equally acceptable and that he has to take both of them into account for all further purposes. It is extremely important that X sticks to his theory, that he keeps developing it, that he goes on using it to look at the world, and that, looking at the world from the viewpoint of his theory, he tries to find arguments against Y's theory. On Levi's model, however, this is impossible. X may stick to his theory without questioning it as long as there is no good reason to do so. But as the discussion compelled him to remove the theory from his corpus, he cannot, according to Levi's view, reintroduce it into his corpus unless he justifiably may do so. But the example is precisely a case in which X cannot justifiably choose between his and Y's theory.

A second argument against a context-independent corpus of accepted statements is that even a scientist may have his philosophical moments and question the truth-status of his theories in general. In such context he would be begging the question if he would stick to his background knowledge. Moreover, some form of Cartesian doubt, or some arguments for scepticism, or some arguments about the frequency of conceptual change in the history of science, may convince him that there is no good reason to believe that his present theories are true, or may even convince him that his theories are almost certainly false. But this will not, or by all means should not, prevent him from regarding those theories as certain within other types of contexts. If he became a sceptic even in the contexts in which he is engaged in solving problems as a scientist, he would become quite a failure as a scientist. To express it in a sloganlike way: a rational person may at the same time believe that some theory is completely certain and that it is almost certainly false, provided these beliefs relate to different contexts.

A third argument against a context-independent corpus of accepted statements is that the set of statements that are accepted by the members of a research tradition, may be inconsistent. The trouble is sometimes that the statements regarded as certain within some single context are inconsistent, but more often that the union of the sets of statements that are regarded as certain in different contexts, is inconsistent. Such a situation will constitute a problem, which should be solved within an appropriate context. But as long as the problem has not been solved, and perhaps in order to solve it, the scientists working in the research tradition will have to continue solving other problems in other contexts. According to my view, the set of statements regarded as certain may very well be consistent for most if not all of these contexts. But, according to Levi's position, the corpus is the same for all contexts. This argument may easily be generalized. Although the theories that are accepted within some research tradition at some time are, in the optimal case, those that deserve most to be accepted on the (methodological and other) premisses of this research tradition, those theories are far from unproblematic most of the time. This truism of present-day philosophy of science entails that all accepted statements are not and should not be regarded as certain in all contexts.

Although I have argued against Levi's idea of a corpus of accepted statements, my kind of contextual approach requires that something very close to Levi's corpus is to be sought. In a sense it is an optimal situation for a research tradition that there be a corpus of statements that are accepted in all contexts, except for those in which the investigated problem presupposes that they are not accepted. There are three reasons why this is an optimal situation: (i) the need to understand reality as a coherent system, (ii) the fact that, in the presence of such a corpus, the organization of the beliefs is drastically simpler and more economic, and (iii) the fact that the presence of such a corpus will permit the generation of an appropriate context for dealing with new, unexpected problems. Levi's idea of a corpus, if modified in the aforementioned way, may be considered as an ideal for scientific research traditions. But even if so modified, it is not an appropriate means to deal with those numerous cases in which, unfortunately, scientific research traditions do not meet this ideal.

4. DYNAMIC DIALECTICAL LOGICS

One of the typical properties of the contextual approach is that even formal logical systems may differ from one context to the other. In order to show the extremely important advantages of this situation, I now describe some logical systems which differ drastically from the usual ones, which are required for the solution of a family of central methodological problems, and the application of which should clearly be restricted to some contexts only.

It has been argued for quite some time now that inconsistencies frequently occur in connection with actual scientific theories. Some theories are inconsistent all by themselves. In other cases there is an inconsistency between two accepted theories. In still other cases there is an inconsistency between one or more theories and a set of accepted observational statements. In all such cases we face two different problems: (i) how to continue scientific research in the presence of an inconsistency, and (ii) how to get rid of the inconsistency. It appears that scientists solve such problems frequently. But they do not know exactly how they do it, and philosophers of science will fail to understand what is going on, as long as they try to approach those cases in terms of classical logic.

There is some similarity between the aforementioned problems and Hegel's notion of a dialectical process, and for this reason I use the expression 'dialectical logics'; I devised these logics precisely in order to deal with theories which, although they were intended to be consistent and were believed to be consistent for some time, turned out to be inconsistent. The addition of the term 'dynamic' is clearly redundant from Hegel's viewpoint, but it happens that Richard Routley and Robert K. Meyer use the term 'dialectical logic' for systems which they consider as a first step in the formalization of dialectics, but which are *static*, as Routley and Meyer explicitly admit.

The dynamical character of the logics lies not with the fact that one theory is replaced by another, but with the inferential process itself. Whenever an inconsistency has actually been arrived at in a derivation from a set of premisses, the rules of inference change. I do not mean that they are changed or should be changed by us; they change all by themselves. Moreover, this change is local; it affects only some statements, viz. those that proved to be inconsistent. As a result, some statements that were derivable before the inconsistency has been arrived at, may not be derivable afterwards. This applies even to inconsistencies. One or both "halves" of an inconsistency that has been derived at some point, may not be derivable any more at a later point as a consequence of the derivation of another inconsistency.

I realize that all this sounds quite extravagant, and for that reason want to point out that I am not talking about some vaguely formulated obscure system, but about a completely decent logic, which has an exactly formulated syntax and semantics (completeness and soundness proofs are available), a logic which has an exactly formulated derivability relation, an exact definition of the consequence set, etc. The consequence set is defined for any set of premisses, i.e., for any set of premisses, it is determined from the outset to which set of consequences the dynamic inferential process will lead if, as is also required for the more common logics, it is carried out in a suitable way. All such technical details may be found in my "Dynamical Dialectical Logics" (Batens 1983); I shall merely present two applicataions of dynamic dialectical logics here. Before doing so, however, I have to make two further short introductory remarks.

The first is that my only technical paper on the matter deals with the propositional level only, whereas I need predicate logic for the applications. I can, however, assure the reader that there are no special troubles on the predicative level, except for some undecidability results.

The second remark is that both applications concern the first of the two problems I mentioned at the outset, viz. how to continue scientific research in the presence of an inconsistency. The removal of the inconsistency should lead to an "enrichment" of the theory. This problem is typically a heuristic one, and although some possible enrichments may very well be arrived at by algorithmic means, no algorithm could possibly lead to the or a "optimal enrichment".

As a first application of dynamic dialectical logics, consider a theory which was meant and believed to be consistent, and for this reason was formulated with classical logic as its basis. Suppose, however, that we derive an inconsistency from the theory as it stands. We might of course simply reject the theory because it is trivial and false, but if no rival theory is available, this decision will have disastrous consequences. Furthermore, we know that scientists do not in general reject a theory merely because it is inconsistent (in itself or with another theory, or with observational reports), even if competitors are available. And several philosophers of science have argued that, under certain circumstances, scientists are justified in not rejecting an inconsistent theory.

Another alternative is that we change the theory drastically in that we replace its logic by a logic which does not, in the presence of an inconsistency, lead to triviality. One such logic, or rather logical machinary, has been presented at least as early as 1964 by Nicholas Rescher. This machinary, however, leads to results which are essentially dependent on the way in which the theory is axiomatized, and in some cases this is a disadvantage. A further possibility is to replace the logic of the theory by one of the numerous paraconsistent logics which have been presented from 1963 on.[4] If we do so, however, then the theory becomes extremely poor. In order to avoid that any statement be derivable from an inconsistent set of premisses, paraconsistent logics drastically cut down the rules of inference. As a consequence, they do not only avoid that an inconsistency leads to triviality, but also eliminate a large number of consequences that are in no way whatsoever related to the aforementioned inconsistency. Of course, this is not what scientists want or should want. What they want, and what they actually use, is the theory *in its full richness, except for the pernicious consequences of its inconsistency*. This is *exactly* what some dynamic dialectical logics lead to.

The idea is that we start from an extensional paraconsistent logic, i.e. a logic which is exactly like classical logic, except in that the meaning of negation has been weakened in order to make inconsistencies logically possible (Batens 1980, pp.196-234). For all inferences that are correct according to classical logic but are incorrect according to the paraconsistent logic, we have the following result within the paraconsistent logic: there is a set of statements such that either each of these statements is true together with its negation, or else the inference is correct.

I now define a dynamic dialectical logic by stating under which conditions a statement is derivable at some point in a demonstration. There are two cases.

(i) If, at some point in a demonstration, A is derivable from previous steps according to the paraconsistent logic, then A is derivable at this point and from those steps according to the dynamic dialectical logic.

(ii) If, at some point in a demonstration, A is derivable from previous steps according to the paraconsistent logic *on the condition* that the statements $B_i, \ldots, B_n$ behave consistently, at this point in the demonstration, then A is derivable at this point and from those steps according to the dynamic dialectical logic.

On later points in the demonstration A may not be derivable any more because one of the B may have been shown to behave inconsistently. (I leave out one small complication which would require too much time but is perfectly natural).

It is possible to define the notion of a *final consequence* of a set of premisses in terms of the set of all demonstrations that fulfill certain regularity conditions. The set of final consequences of a set of premisses is provably independent of the way in which a specific demonstration proceeds. This is very important, because we do not want the notion of derivability to be contingent on the accidental way in which the proof proceeds. Semantically the set of all final consequences of some set of premisses is exactly the set of all statements that are true in all models in which all premisses are true and which are precisely as inconsistent as is required to make the premisses true. In other words, the set of final consequences gives us the theory in its full strength, except for the pernicious consequences of its inconsistency.

I now quickly consider another application. Consider a scientist who has to make predictions on the basis of a falsified theory together with some set of observational statements. If there is only one falsifying instance, the theory will obviously not be considered as falsified; we would rather reject the falsifying instance. So, if the theory is falsified, we have at least an accepted empirical generalization which contradicts the theory. Suppose that 'All A are B' is derivable from the theory, whereas 'All A that are C, are not B' is the accepted empirical generalization (to take a simple example). Under such circumstances the scientist will predict that an object does not have property B if he knows that it has properties A and C, whereas he will predict that an object has property B if he knows that it has property A and not property C. If he does not know about C, he will refrain from making a prediction about B.

Incidentally, the scientist may have good reasons to make predictions in another way, but this is irrelevant to the point I am making. All I need is that the scientist will have a more or less demarcated opinion about the interplay between the theory and the accepted empirical generalization for his decision to predict B or not-B or neither. .Whichever the criterion, the scientist may apply the aforementioned dynamic dialectical logic to make derivations from the theory together with the empirical generalization and observational statements, and then apply this criterion in order to eliminate, whenever possible, one half or both halves of the inconsistency (and to eliminate also all other statements for the derivation of which the eliminated half of the inconsistency has been employed). If one half of the inconsistency has been eliminated, he will refrain from making any prediction on the matter. This result corresponds exactly with what he wanted to do intuitively.

By way of a final remark, I want to stress that it would be completely wrong to conclude from all this that dynamic dialectical logic is *the* correct logic because it leads to correct results with respect to both consistent and inconsistent theories. What I did want to show is precisely the opposite, viz. that if some context proves inadequate to solve the problem under consideration, we have to move to another context to decide what is the right remedy; and in some cases the right remedy is to change the logic, and to change it only in some context, because only this context is affected by this inadequacy.

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NOTES

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For references see, e.g., Niiniluoto and Tuomela (1973).
[2] Expressed more precisely, some confirmation functions lead to the desired results with respect to predictions that share all their known properties with other known objects; but no confirmation function of those continua lead to the desired results with respect to predictions about objects that do not share all their known properties with some other known object.
[3] Carnap distinguished clearly between inductive logic and the methodology of induction; cf., e.g., his (1950 p. 202, ff). According to the spirit of Carnap's position, all analogy problems mentioned should be solved by inductive logic in that some set of "confirmation functions" should lead to the desired results; the task of the methodology of induction is merely to guide us in the application of inductive logic.
For an instructive overview, see A. Arruda (1983.)

REFERENCES

Arruda, A. (1983), "Aspects of the Historical Development of Paraconsistent Logic", *Paraconsistent Logic*. eds. R. Routley and G. Priest. (in press)

Bar-Hillel, Y. (1968) "The Acceptance-Syndrome", *The Problem of Inductive Logic*. Amsterdam: North-Holland.

Batens, D. (1975) *Studies in the Logic of Induction and in the Logic of Explanation*. Brugge: De Tempel.

Bar-Hillel, Y. (1968) "The Acceptance-Syndrome", *The Problem of Inductive Logic*. Amsterdam: North-Holland.

Batens, D. (1975) *Studies in the Logic of Induction and in the Logic of Explanation*. Brugge: De Tempel.

________ (1980) "Paraconsistent Extensional Propositional Logics", *Logique et Analyse*.

________ (1983) "Dynamical Dialectical Logics", *Paraconsistent Logic*. eds. R. Routley and G. Priest. (in press)

Carnap, R. (1950) *Logical Foundations of Probability*. Chicago: University of Chicago Press.

________ (1952) *The Continuum of Inductive Methods*. Chicago: University of Chicago Press.

________ (1959) *Induktive Logik und Wahrscheinlichkeit*. Wien: Springer.

________ (1968) "On Rules of Acceptance", *The Problem of Inductive Logic*. Amsterdam: North Holland.

Krajewski, W. (1977) *Correspondence Principle and the Growth of Science*. Dordrecht: Reidel.

Kyburg, H. (1976) "Local and Global Induction", *Local Induction*. Dordrecht: Reidel.

Laudan, L. (1977) *Progress and its Problems*. Berkeley: University of California Press.

________ (1981) *Science and Hypothesis*. Dordrecht: Reidel.

Levi, I. (1980) *The Enterprise of Knowledge*. Cambridge, Mass.: MIT Press.

Niiniluoto, I. and R. Tuomela (1973) *Theoretical Concepts and Hypothetico-Inductive Inference*. Dordrecht: Reidel.

____________ (1980) "Scientific Progress", *Synthese*, 45.

Putnam, H. (1978) *Meaning and the Moral Sciences*. London: Routledge & Kegan Paul.

Rescher, N. (1964) *Hypothetical Reasoning*. Amsterdam: North-Holland.