NICHOLAS RESCHER'S COHERENCE THEORY OF TRUTH

Diderik BATENS

1. A careful reader of Nicholas Rescher's *Hypothetical Reasoning* might have remarked that the logical machinery presented in that book has a much wider range of application than the explicit discussion of the book suggested. In *The Coherence Theory of Truth* (1) this machinery is not only applied to a wider domain of problems, but comes to play a central role in a preeminently philosophical theory, a theory of truth.

The label "coherence theory of truth" may be somewhat misleading at first sight. The main subject of the book is a *criterion* and not a definition. (Rescher himself defends the suitability of *defining* truth in correspondence terms.) Furthermore, correspondence considerations as well as pragmatic considerations are also made use of in Rescher's coherence theory. I shall first try to make roughly clear how these three components are interrelated.

The problem is to decide, in a given situation, the truth status of some proposition. The starting point is *data*. Unlike in a correspondence theory, these *data* are not taken as completely certain. They are not truths, but merely truth candidates. The coherence machinery is applied to the data, and only as a result of this process does one get truths. Pragmatic considerations come up in the justification of the whole procedure. Notwithstanding the important role of correspondence and pragmatic considerations, the criterion is essentially coherentist in that (a) the *data* as such guarantee no truth at all, and may indeed be flatly inconsistent, leading to truth only after they have been processed by the coherence machinery, and (b) the pragmatic considerations are used only in support of the validity of the whole procedure, and not in support of particular truth-claims.

(1) Nicholas RESCHER: *The coherence theory of truth*. Oxford, Clarendon Press, 1973.

It goes without saying that the objection that "a set of falsehoods may be as coherent as a set of truths" is pointless with respect to a theory which, as the one under consideration, starts from experiential *data*. Of course, one might repeat the objection in a more sophisticated way, and ask why the application of the coherence machinery to the *data* should lead to a set of true propositions. This objection can be directed against the specific coherence machinery presented in the book under discussion, or against any coherence machinery in general. I shall return to the first version of the objection in the sequel, and briefly consider the second version here. The concept of "truth" that the objector has in mind cannot be the coherence concept of truth itself; indeed, every reasonable criterion of truth would accord with his own truths for trivial reasons. What the objector has in mind is probably the definitional concept of truth as correspondence with the facts. The objection has then to do with the fallibility of the coherentist criterion, and not only with its fallibility with respect to its own different applications in time, but with respect to the facts as well.

Tarski's definition of truth is unexceptionable of course, not only for the trivial reason that it can be read as a (stipulative) definition, but also because it expresses without any doubt exactly what we mean by 'true'. But precisely because it is infallible, it is useless as a criterion or even as a basis for a criterion. Indeed, any criterion of truth will be either almost application-free (lead to an empty set of truths) or fallible, and this not for a theoretical reason, but for the simple and contingent reason that our experiential means are as they are. Consequently, all one can require from a *criterion* of truth is that it lead to 'p is true' only if there is a *rational warrant* for '*p* is true'. This rational warrant cannot be found in correspondence considerations for reasons mentioned before (protocol sentences are a myth). Nor can it be found immediately in pragmatic considerations, for they lead at best to useful tools, and, as Karl Popper argued in his *Logic of Scientific Discovery*, tools cannot be true or false. This suggests that a rational warrant has to depend at least to some extent on coherence considera-

tions. One can argue against either the relative import of coherence considerations in a criterion of truth, or the specific coherence machinery that is suitable, but the applicability of coherence considerations in general must be acknowledged. Returning to the question of whether any coherence criterion will guarantee the truth of its results, the answer seems to be that it never will *guarantee* truth in an absolute sense, but that some such theory will account for a rational warrant for truth, if any theory at all will. Of course, it never will do so on its own; without *data* it is completely worthless. And there surely is no *a priori* way to find out which specific coherence criterion is to be adopted, nor even to what extent coherentist considerations have to play a role. But given the kind of epistemic subjects we think ourselves to be, and given the incomplete reliability of our techniques for gathering *data*, a truth criterion without any coherence machinery can hardly be defended.

The fact that there is fallibility, and the fact that there is rational warrant only, might suggest that we are dealing with a theory of acceptance rather than with a theory of truth. What is meant here, however, is not a theory of acceptance like many of those now extant, which account only for general hypotheses and predictions, including so called retrodictions. Most of these theories start comfortably from a set of unquestioned "evidence." However, a criterion of truth has to account even for this "evidence," or expressed more suitably, for the factual truths that are used as a basis for accepting predictions and general hypotheses. Supposed that we use *'theory of acceptance'* in the broader sense, what are the reasons why the coherence theory cannot be considered as such a theory ? As Rescher argues, the rational warrant "applies to the criterion at issue, and not (save obliquely) with the propositions to which that criterion addresses itself ..." (p. 16). Two other differences seem relevant here. If an acceptance theory has to be a useful *explicatum* for what happens, e.g., in the actual history of science, it seems that it has to allow, at least under definite conditions (a) the acceptance of inconsistent theories, and (b) the acceptance at the same time of some theory and of falsifying instances or even a falsifying lower level (read less

general) hypothesis. Such points are stressed in recent work by, e.g., Paul Feyerabend. Risto Hilpinen has (in another context) argued against this possibility in *Rules of Acceptance and Inductive Logic*, but his arguments are certainly not conclusive if one tolerates different kinds of acceptance (which seems realistic), and if one introduces a logic that allows the deduction of sound conclusions from such a kind of inconsistent propositions. Although this logic will differ from Rescher's coherence logic, it follows analogous lines and is surely not more complicated. It would unfortunately go beyond the limits of this paper to go further regarding this point. Anyhow, although an acceptance criterion might lead to the acceptance of "partly" falsified, and even contradictory hypotheses, this would surely be completely unsound for a criterion of truth. An inconsistent set of propositions can never be called true without vast violence to the term. A last difference that I want to mention here is that theories of acceptance differ from theories of truth in their aims and in the status of their results. The conditions for accepting a proposition p seem to depend on what we want to use the accepted p for, and on the amount of risk we can permit in this respect. But if 'truth' is meant in a serious sense, one cannot allow that truth would depend on such factors, even if one allows (and has to allow) fallibility in his claims to truth.

2. Let us now briefly have a look at the coherence theory advanced in Rescher's book. As indicated above, one starts from *data*, i.e., truth candidates. Each *datum* can be as complex a proposition as one wants, and the set of *data* is literally the set of these propositions (and not of conjuncts of them). No problem arises if the set is consistent; if one wants to classify the whole set as true, one immediately gets a consistent set of truths (I return to this in the sequel). The importance of the coherence machinery shows up if the set of *data* is inconsistent. In this case one has to make a choice: not every member of the set can be classified as true. Before going into more detail, it seems important to note that, according to Rescher's criterion, not only members of the set of *data*, but also consequences of this set, can be classified as true, even if these consequences

do not follow from the members of the set that are classified as true (what makes the characteristic non-trivial). This proves the considered coherence criterion to be more general, and more realistic, than a naive coherence theory would be. Returning to the main problem, which consequences of the set of *data* will be classified as true ?

Rescher distinguishes roughly four kinds of consequences of a set of propositions *S*. The set of *L-consequences* (logical consequences) will contain *all* propositions, because *S* is inconsistent. Needless to say, this cannot be considered as a set of truths. In order to make clear what the other kinds of consequences are, one has to remember that every inconsistent set *S* determines a definite number of "maximal consistent subsets" (m.c.s. for short), i.e., subsets of *S* which are consistent but would immediately become inconsistent if one more member of *S* was added to them. (Self-) inconsistent members of *S* will not occur in any m.c.s.; all other members of *S* will occur in at least one m.c.s. Rescher calls every proposition that follows from each m.c.s. an *I-consequence* (inevitable consequence) of *S*, and every proposition that follows from at least one m.c.s. a W-consequence (weak consequence) of *S*. The set of W-consequences will in general (i.e. unless *S* is consistent) be inconsistent again, whereas the set of I-consequences will be rather restricted. In general none of these sets can suitably be considered as the set of truths, but the set of truths has to be a subset of the set of W-consequences, and has to contain as a subset the set of I-consequences. To determine the set of truths a fourth consequence-relation is introduced: *P-consequence* (preferential consequence). Suppose one has some device for classifying certain m.c.s. as "preferred." The set of P-consequences of *S* (with respect to this preference distribution) is the set of all propositions that follow from each preferred m.c.s.

The concept of P-consequence is of utmost importance from a philosophical point of view. It allows one to express definite relationships that cannot be expressed by the purely logical concepts of L-, I-, and W-consequence. These relationships have to play, at least in my opinion, a central role in epistemo-

logy — and even in an analysis of what we mean by 'consequence' in factual situations. I return to the difference between the two kinds of consequence in a later point. Incidentally, it is worth noting here that "the way the 'content' of an inconsistent set of premisses is formulated ... [affects] decisively (the 'consequences' of the set" (p. 102.) This means that there are at least two relativising (and context dependent) factors: the preference distribution, and the way the *data* are formulated. The rationale for the first factor is obvious, the rationale for the second can be seen best from an example. Suppose each *datum* is a report of a different observer. If a part of such a report is disqualified, the whole report becomes questionable: there is a reason to distrust the reporter as to his accuracy. The formulation of the *data* is then important for identifying propositions that form a whole (each "whole" being expressed as, e.g., a conjunctive compound proposition).

However, there is a third relativising factor. The same method for introducing a preference distribution is not appropriate in all cases. Rescher elaborates some four different methods for preference distribution, and each of these is (more or less) suitable in different situations. It goes beyond the scope of this paper to explore these different methods. Let it only be mentioned that they are all worked out very well, and that arbitrary choices are ruled out as much as possible within each method. Whereas the relativizations as such seem necessary for a workable truth criterion, it seems unavoidable that a further development of the theory will require the construction of general rules determining the range of application of the different methods, and determining to some extent the methods themselves. One or more theories, viz., pragmatic ones in my opinion, have to do this job. Needless to say, the fact that these theories are not worked out in the book is not an objection. Rescher himself writes explicitly in the "Postscript" that he considers his book incomplete in certain respects, and that a good deal of further investigation is needed.

3. Some further remarks have to be made about the application of the procedure and its relation with our actual thinking. Rescher makes a sharp distinction, albeit almost implicitly,

between, on the one hand, the *data*, and, on the other hand, the reasons for choosing a definite method for introducing preferences as well as the different factors determining the definite preference distribution that one arrives at within the chosen method. A positive point in my opinion is that Rescher sees these "reasons" and "factors" as mostly factual in nature. But exactly because he does so, it is amazing to note that he makes such a sharp distinction between these "reasons" and "factors" on the one hand, and the *data* on the other hand. Of course, some "reasons" and "factors" have to do with the sources of the *data* rather than with the objects or facts that the *data* are about and that the truths we are looking for have to be about. But even this does not hold in general. There are at least two problems at hand here. First, can such a sharp distinction be drawn at all ? Next, to what is the truth criterion to apply ? If it applies to all factual truths — this indeed seems to be Rescher's conviction — the next question is whether it has to be applied separately to different aspects of reality (say, *data* about sources of information, and other *data*) or to all of them at the same time (this eventually with respect to different problem situations). However, if the truth criterion is applied separately to *data* about sources and to *data* produced by the sources, the situation becomes rather complex. First of all, the former kind of *data* have their sources themselves, but this is only a minor point. Next, it is impossible to apply the criterion definitively to one set of *data* before applying it to the other set: the results gained for each of the sets has repercussions on the application of the criterion to the other set. Here an oscillating process seems the only possible way out, always revising the result based on one set after a new result based on the other set is gained. This process may eventually come to an end, viz., lead to constant results from some point on.

Consider now the case wherein the criterion is applied to the whole set of *data*. The interesting feature here is that no factual material can be used any more to determine which preference method has to be used, and no factual material can be used as an input for the chosen method. An exception has to be made perhaps for the sets of truths classified as such at

former points in time, in which case one has to be careful and prevent the overall method from becoming static. It seems interesting to investigate what would happen if we were to neglect the sets of propositions classified as true at a former point in time, because this would reveal what metaphysical, epistemological, or methodological considerations are basically behind the whole procedure. Although this is certainly an important problem, my proposal will probably lead to a somewhat unrealistic methodology, and perhaps also to inconclusive results; without any doubt one never neglects the set of propositions classified as true at a former point in time. Anyhow, further investigations seem at hand with respect to the *data* that serve as inputs for the coherence machinery and the *data* and considerations that (to continue the metaphor) determine the program of the coherence machinery.

There is in my opinion an important problem that Rescher does not consider. This problem is most clearly seen if one considers a case wherein the *data* are consistent. According to the coherence machinery, one ought to classify all these *data* as true. But this is clearly unsound, if one considers indeed *all* the data without any previous selection. An isolated statement made by an unreliable source is not likely to be classified as true. Essentially the same problem arises in cases wherein the *data* form an inconsistent set: one will want to reject even definite I-consequences. Although this is a weakness of the theory as literally presented in the book, it is clear that the only sound way to attack the problem is to do it following the lines of the general approach there, viz., from a *wholistic* point of view. One ought not only to consider then all m.c.s., but more generally, all "consistent subsets" of the set of *data*. The notion of P-consequence can be defined with respect to *these* subsets, in which case we will of course not have any more as a theorem that each I-consequence of S is a P-consequence of S. If this change in the coherence machinery is made, then the notion of the cohesiveness of truth, with respect to both the content of the set of truths as well as the methods that lead to truths (e.g., information about the way the *data* are gathered), would become still more important than it is now in the book.

Rescher writes: "The establishment of a plausibility-grading scheme in terms of the characteristics of these candidates — for example in the case of reports, a reference to the question of how reliable the sources from which they come have been in other instances — can clearly prove both useful and warranted." (pp. 111-112.) In general he always presupposes (a) that there is a given number of *data* that together form a propositional set, and (b) that we have certain information *about* the *data* that proves sufficient to apply the coherence machinery to them. However, there are situations wherein these presuppositions seem false or at least very artificial. First of all, we may be confronted with a set of *data* that belong to different conceptual structures. In what sense can such *data* be said to be consistent or inconsistent? This problem is related to a more important one that concerns the aforementioned presupposition (b). Is there any way to decide about the truth-status of *data* without referring to theories, or, if you wish, to our background-knowledge? Suppose that p is an I-consequence of some (consistent or inconsistent) set S, but that p conflicts with a well-established theory. In such a case, it is surely not always rational to classify p as true, and hence to commit oneself to the rejection of the well-established theory.

In other cases, *data* seem to be inconsistent, but are only so in virtue of theories. Rescher mentions the case that "... a man's sight indicates that a stick held partially submerged under water is bent and touch indicates that it is straight." (p. 301) and resolves the "inconsistency" by referring to the reliability of the two senses. But sight is *in general not* less reliable than touch. Furthermore, there is no inconsistency between the stick's looking bent and the stick's feeling straight. So, we need to consider theories — relating how something "is" with how something respectively looks and feels — in order to detect and, needless to say, in order to solve the inconsistency. But even this is not enough. How the matter has to be resolved will largely depend on theories about our senses, light-rays, solids, etc. In short, I want to argue that one has generally to consider the *data* in relation to theories in order to classify them as true or false. (For an interesting discussion of

part of this problem, see Adolf Grünbaum's *Falsifiability and Rationality*, to appear in 1973 by the University of Pittsburgh Press.)

The point of view I was arguing for is more wholistic than Rescher's, but there is no reason why his logic of coherence could not be used to articulate this point of view more precisely. All we have to do is to bring the *data* and theories together to make them form one inconsistent set. We will then have to assess plausibilities to the *data* as well as to the theories. This application of the coherence machinery does not require that one must be able to give an acceptable account of the whole process of induction in terms of this machinery. The application is sufficiently useful if it helps to resolve some problems in the short run, and if it helps to reveal problems that have to be resolved by the modification of old theories or by the invention of new ones.

4. The coherence machinery at issue has to lead from *data* to truth, viz., about facts that happened in the past and that the data give information about. Rescher also describes how essentially the same machinery can be used for inductive purposes. As the results in this case will again have to be truths, it follows that the move will be acceptable only for people who do not share the opinion of e.g., Carnap and Bar-Hillel, that induction leads only to more or less probable propositions, but never to "acceptable" propositions, i.e., leads to different degrees of rational belief, but never to acceptance. Incidentally, there is another difference between Rescher and i.a., Carnap in that Carnap starts from absolutely certain evidence (at least at some moment) whereas Rescher starts from mere *data*. But clearly there is no logical connection between these two kinds of difference.

The main idea of Rescher's inductive method runs as follows. The different competing hypotheses are added to the set of *data* to form one (of course inconsistent) set S, and the different m.c.s. of S are formulated. The specific problem is now to be solved by the preference distribution. In general, Rescher proposes "to rely on pattern-conformity considerations" (p. 227), but with respect to "general" hypotheses especially, he uses

probability considerations (applied to the m.c.s. rather than to the hypotheses themselves). Of course, these probability considerations can be regarded as a special case of pattern-conformity considerations. It is surely an advantage of the proposed probabilistic procedure that the *a priori* probabilities of the hypotheses have no very decisive influence on the final distribution. However, it seems to me that this procedure involves some problems which are not mentioned in the book. In cases where an infinite number of hypotheses is considered, there are an infinite number of m.c.s., each of which can hardly receive a higher probability than zero. Perhaps this problem may be solved according to Kemeny's solution of the problem of almost-L-false propositions ("A logical measure function," *J.S.L.*, 18(1953), 289). A second problem seems more difficult to solve. Although the distribution of absolute (*a priori*) probabilities seems to be more or less unimportant for Rescher's probability procedure, it is hard to see how the whole mechanism could ever constitute an adequate "logic of induction" before all (translated) problems of "the adequate m-function" are solved. This becomes clear if one looks at the more general formulation of the plausibility procedure: "rely on pattern-conformity considerations." There is obviously an immense number of pattern-conformities (or analogies, if you wish) in every non-simplistic case, given a language that is not awfully simplistic and poor. It seems furthermore completely unrealistic to suppose that one could take into account all kinds of analogies (which is perhaps a reason for optimism, although there seems to be no solution today as to what analogies have to be considered.) Of course it does not follow from this that Rescher's approach to induction is misoriented or has disadvantages, but it seems to follow that a definitive evaluation of this approach will depend on whether one will ever be able to fill the gaps that one is confronted with now. Anyhow, the application of the coherence machinery for inductive purposes seems to be much harder to evaluate and to specify than its application in the case wherein one wants to construct a set of truths out of a set of mere *data* about facts that happened in the past.

5. The main topic of the book reviewed here is, of course, epistemology. The most important problems concern the epistemological implications the theory of truth has, and the epistemological considerations the theory is based on. I shall first consider the coherentist epistemology as developed in the book, and comment in the next paragraph on the preeminently epistemological problem of the justification of the theory.

A major characteristic of this theory of truth is that it is *non-foundationalist*. One does not gain truths from other truths, but from mere *data*. Consequently, both protocol sentences and immediate truths from intuition are considered as myths. This characteristic of coherence theories in general is without doubt their major strength in their competition with other theories of truth, and the importance of this book rests in no mean proportion on its articulation of a theory of such strength in both an exact and realistic way — the latter in that is that it takes into account at least some complaints of history-oriented philosophers of science. The weakness of a coherence theory lies obviously in the difficulty to get a sufficiently close relation between ontic truth and coherentist truth. The essential role of *data* and the specific machinery that leads from *data* to truths help to overcome this weakness for the specific coherence theory presented in this book to such an extent that one doubts whether another theory could do better (given the kind of epistemic subjects that we think ourselves to be).

Another characteristic of the coherence theory, viz., that it is *wholistic*, is extremely important in this respect. A foundationalist theory, as a very consequence of its nature, will have to develop a criterion that works for each particular proposition. The coherentist can do with a general criterion that starts from all *data* together, or at least from one set of *data* related to one problem, and leads to one body of truths (related by the very structure of the procedure). From this point of view, the relation between epistemic and ontic truth seems not at all to be a harder problem for a coherence theory than for any other theory of truth.

Of course, the foundationalist, and especially the empiricist, will persist in distrusting the coherence machinery, and insist

on getting "closer to the facts," e.g., in trying to eliminate as far as possible experiential errors. Although Rescher does not mention this point, I can see no reason why he would not applaud such a program. There is no reason why a coherentist would be "lazy" in looking for *data*, even if his theory does not especially demand his increasing the set of *data* or making them more precise. It goes without saying that the increase in the certainty of experiential techniques is one of the faiths "we all" have in common. It is also clear that modern science could not have been what it is now without both the introduction of experiments, and without the related improvement of observational (especially measuring) techniques, that reduce the role of our senses to more and more simple activities. To be sure, once the well-known "ideal science" will be there, all coherentist machinery will be superfluous. But as this unfortunately is not going to happen before the end of time, we are confronted in the meantime with at least two problems. The first problem is that we have to apply some criterion to the inconsistent sets of *data* that we still come upon. The second problem is that, given where we are now, any improvement has to start from some conjectures, and it would be comforting if these conjectures were at least partially true. Both the empiricist and the coherentist will use information about the liability of the different experiential reports, but thanks to the wholistic character of the coherence theory, the coherentist will not be obliged to apply his criterion to each *datum* separately. I thought the above (rather dynamic) considerations worth mentioning because one might misinterpret the fact that Rescher's book concentrates upon the static description, and conclude from this that it disregards the importance of improving experiential techniques, and even of increasing factual information (Rudolf Carnap's *Logical Foundations of Probability* was misinterpreted in the same manner).

Apart from being non-foundationalist and wholistic, the coherence theory under consideration is *fallibilistic*. "... all claims to factual knowledge can never be made categorically and definitely, but only conditionally and provisionally" (p. 325). I mentioned this at the very beginning, and I cannot see

how one could object to this without making the class of known truths almost empty. Another characteristic of the coherentist epistemology is what Rescher calls the "*dualism of contingent knowledge*": knowledge of something as a *datum* and knowledge of something as a fact. Perhaps "pluralism of factual knowledge" would even be preferable, for it does not seem clear to me that no other kind of knowledge is to be introduced. It seems plausible, for instance, to attribute some knowledge status to (perhaps multiple kinds of) accepted propositions, even if these cannot be considered as true. A further study of the relation between acceptance and truth would perhaps be instructive. It seems indeed advisable to distinguish different kinds of acceptance (and different criteria) with respect to the purpose the accepted propositions have to serve. It seems not advisable to extend the same multiplicity to truth — to have mutually inconsistent sets of truths seems to be a contradiction — but a comparative study could help to articulate more precisely the different factors that play a role in the plausibility procedure.

6. Rescher presents an interesting chapter on the problem of validating a theory of truth in general, and discusses the different alternatives before us. He comes to the conclusion that one has to turn to pragmatic considerations at this point. Just as correspondence considerations also play a role in the coherence theory under the form of *data*, pragmatic considerations play a role in the justification of the theory. As the coherence theory offers a criterion, i.e., a method, the "appropriate mode of validation is to show *that it works*" (p. 232). This, of course, cannot be done better by anything other than pragmatic considerations. The main difference with a pragmatic theory of truth is that Rescher applies the pragmatic considerations for validating the whole method, and not for validating separate claims to truth. This pragmatic turn saves the coherence theory from an *a priori* metaphysics, and stresses in the same manner as did the correspondence turn with respect to the *data*, the importance of factual and practical considerations. The problem is of course what we are going to use as information for judging whether the method works or not. One can

clearly not validate the criterion in saying that it works in that it leads to truths. This would be plainly circular. To escape circularity, Rescher turns again to the *data*. These are, to be sure, not true, but they have nevertheless some cognitive status (cf. supra). The argument has then to rely on the fact that the method delivers us a vast majority of the most plausible *data* as truths.

It seems to me that a stronger and more plausible validation of some criterion of truth (or some method in general) could be reached if attention is paid to the facts (a) that there is no need for accepting one criterion once and for all, and (b) that a criterion is not applied at one point in time but during some interval of time. In applying a truth criterion more than once and starting from different sets of *data*, one will be forced to consider previous decisions about the truth of certain propositions as mistakes. A criterion that *cannot* lead to mistakes must be rejected *a priori* for reasons that will become clear later on. The fact that a criterion leads to mistakes provides some kind of information of its accuracy in the short run, judged "by its own standards." If one may suppose that there is some accuracy in the results of the criterion (and note that there was surely some accuracy already in pre-scientific thought — mankind did among other things not die out), and if one may suppose that later applications lead to more trustworthy results than former ones (i.a., because they deal with more *data*), one has reasons to substitute the used criterion for one that would have made less mistakes. The fact that one is able to perform this kind of "counterfactual" reasoning about criteria, entails that, if one is able to invent at least variations on the method he uses, then he may be able to substitute some criterion by "its" (in the strong sense) improvement — and I think indeed that any improvement of our methods proceeds along these lines. The problem whether and how one is able to invent improvements of methods is less important here, as the main aim of this argument has to do with the justification of some method once it is invented. One may of course doubt whether such improvements occur on the general level of the truth criterion, or rather on the lower level of measuring techniques,

etc. However, there seem to me to be various reasons for building in this dynamic on the most general level. The reasons for doing so with respect to Rescher's truth criterion are among others the following: (a) there would be no need for a truth criterion at all if our observations were completely accurate, a supposition from which people probably started originally, (b) some of the alternative methods (to be applied in different contexts) that Rescher offers are certainly not used in that exact and sophisticated manner today, (c) the invention of such an alternative does certainly not determine its exact field of application, and (d) to consider such an alternative makes sense only if formerly used methods were at least to some extent less satisfactory. A more general reason for building in this dynamic on the most general level is that it seems to be the best warrant for the reliability of the validating argument for any theory of truth.

The above comments are surely not an actual validation of Rescher's coherence theory with respect to contemporary science. They have to do with a metatheory that has to guide changes in the used method, viz., the truth criterion. The validation of this criterion rests on the very fact that it is guided by such a metamethod. Also Rescher's arguments have to do with a way of validating a criterion rather than that they constitute an actual validation; the latter would indeed require to sum up factual information about the application of the criterion of truth. But if we are concerned with a *method* of validation of a criterion of truth, it seems to me that we have to make it rest on *dynamic* considerations, and that it is only too optimistic to believe that we could validate a criterion at one point in time, stopping as it were to apply the criterion for a short time, and reflecting upon the fact that it is a good one. It seems furthermore clear to me that we shall have to improve a criterion many times; and we need a method to guide us here. If this is so, then the validation of any criterion of truth has to rest above all on the characteristics of the metatheory that determines the changes in the criterion. It should be noted incidentally, that we need a "self-correcting" criterion

for truth if we want to be able to "correct" the criterion itself by a metatheory.

Although no sufficient validation of Rescher's approach to truth has been given, I hope to have made clear that this *kind* of approach has to be followed up and elaborated if we ever want to come to a sound theory of epistemic truth.

7. In this last point, I want first of all to say something about a very general difference between this book and many other works on epistemology, methodology, and logic. Rescher takes into account and *builds in* in various parts of his theory factors that I shall label in general as *contextual.* Consider first of all the logic of coherence. In standard kinds of logic we are told what can be derived from some set of propositions; once these are given as premisses, the consequences are determined in an absolute way irrespective of its purpose. Even if some logicians are so kind as to allow different logics to express different kinds of consequence relations, most of them will not allow one and the same consequence relation to be different according to the context. However, in Rescher's logic of coherence exactly such a kind of diversification exists: different preference distributions will determine different sets of P-consequences of one and the same (inconsistent) set of propositions. This gives the logic a special attractiveness and allows for the possibility of dealing with non-simplistic cases. It enables us to give to the different premisses a different status with respect to the derivations that can be made from them. Hence, we are not only able to take into account the presupposed and unique truth value of a proposition, but also its "epistemological status." Not only in its application in the theory of truth, but also in its application to a large number of other cases of "reasoning from inconsistent premisses" (a whole chapter of the book is devoted to such cases) does such a logic seem to be the only way to get sound results in systematizing our actual cognitive behavior and explicating the concepts we have in mind.

Analogous characteristics can be taken notice of in the construction of the methodological part of the theory. There is a variety of specific methods, each of which is to be applied in

the situation wherein it is suitable, and although there is certainly work to be done about the delineation of such methods, there is no reason for objecting to the multiplicity of such methods. What is determinative in all these cases is the context (in the broadest sense) of the application of the truth criterion. Here again, this contextual relativization gives at least hope for a sound result from the point of view of our actual behavior and of the concepts we have in mind, however vague they might be. One has only to compare these characteristics with a book as excellent as Rudolf Carnap's *Logical Foundations of Probability* to see the striking difference. To be sure, this contextual relativization makes the exact articulation of the theory more complex and difficult, but at least it saves one from the lures of a too elegant theory, wherein the disadvantages of its very simplicity are all too easily overlooked.

Let it be mentioned that Rescher's book contains a lot of interesting and well developed formal theories, such as the logic of coherence, an orthodox non-classical theory of truth (in its relation with three-valued logic), a formal theory of plausibilities, etc. All these do not only help to articulate the coherence theory of truth in an exact way, but are interesting for their own sake. Furthermore, the wide range of application of the coherence machinery as brought forth in the book opens interesting perspectives on numerous philosophical problems. A special chapter is devoted to the applications of the coherence analysis in information processing. These applications include, among other things, determining written passages of a book, spoken utterances, and group consensus reports — all in the face of conflicting information — and collating the results of different and conflicting sensory reports (but compare point 3.). In all such cases a rather clear distinction can be drawn between *data* about the "facts" and *data* about the "sources," and the problems I mentioned before in this respect do consequently not arise here. The application of the coherence machinery to such problems leads to plausible results and enlightening insights. All this stresses the power of the coherence machinery.

In concluding this essay, it should be mentioned that this book of Rescher's combines a real philosophical point of view with a realistic perspective on the actual development of our scientific knowledge. It is written in a clear language (sometimes perhaps a bit too redundant) without violating the difficult epistemological problems of the subject. I hope that this paper shows the importance of the philosophical theory presented in this book and of its further development, and that it suggests some tentative roads for such development.

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