IN-WORLD REALISM *vs.* REFLECTIVE REALISM

A WAY OUT OF THE CONTROVERSY ON SCIENTIFIC REALISM

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1. AIM OF THIS PAPER

In the present paper, we mainly try to make one point about the debate on scientific realism: the question whether the theoretical statements of our best theories are (approximately) true is fundamentally ambiguous.

In section 2, we list three groups of questions. Our aim is not to argue at length for the correctness of specific answers. We shall state what we take to be the correct answers to these questions, and briefly point to some main arguments. The central point we want to make, however, is that the answers are usually taken to be contradictory, but that in fact they are not.

It is quite bizarre that so many competent scholars have taken a stand in the debate without spelling out this point. That most if not all questions are ambiguous should be well known. Especially so to philosophers of science, as it has been common wisdom for years now in both the literature on scientific explanation and the literature on scientific discovery. The reason why realists and anti-realists alike seem to overlook the ambiguity is presumably related to the fact that, in agreement with the traditional epistemological view, they all share a monolithic view on knowledge systems. From that perspective, the proposed double interpretation is indeed objectionable.

For the last fifteen years, the first author has been working on an alternative epistemological approach. That a new epistemology is needed is a rather common view. And so are the basic facts that it should account for. His main endeavour was to push things to extreme consequences and to spell it all out. Some results have been published in English–see Batens [1985] and [1992a]–but the full account exists in Dutch only (Batens [1992b]–the forthcoming Greek translation will not help many of the present readers). This forces us to say a few words about that epistemology, but also to argue in a somewhat general manner.

Once the main point is made, viz. that the question is ambiguous, it will soon be apparent in which sense one should side with scientific realists and in which the anti-realist stand seems inescapable. We realize that our position may seem philosophically suspicious. Agreeing with everyone 'in a sense' is

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not in general the result of good philosophical thinking. All we can say in our defense is that our position is only a consequence of a study that had nothing to do with the debate on scientific realism, and that we shall by no means save all claims of both parties 'in a sense'.

2. Some Questions

Each of the following questions are directly related to the debate on scientific realism. Let us start with the most general ones.

(1a) Can we justifiedly consider the theoretical statements of some of our scientific theories as approximately true (as approximately accurate descriptions of reality)?

(1b) Do the theoretical statements of successive scientific theories converge towards the truth?

The difficulty of distinguishing between observational and theoretical statements is not very central with respect to questions (1a) and (1b). Some, like Bas Van Fraassen, maintain a very sharp distinction (that remains constant as long as the observers remain constant). Others, like Dudley Shapere, rely on the theory-ladenness of both observations and terms to make the distinction a temporal one; still others abolish it completely. All this is only indirectly relevant for the answer to questions (1a) and (1b). It will, however, be more important for some subsequent questions.

Question (1b) is a recent (nineteenth century) supplement to question (1a), and so is the 'approximately' in (1a). Seventeenth century realists would have answered the unqualified question (1a) in the positive. The reason for the supplement is a change in our views on both scientific progress and scientific discovery–see Laudan [1973] and [1980]. This is why, with respect to the scientific realism debate, references to the views of seventeenth and eighteenth century scientists risk to be misleading.

The following questions are related to the scientists' conception of scientific theories and to the generation of these theories.

(2a) Do scientists consider some theoretical statements of scientific theories as approximately true?

(2b) Do scientists intend and want to arrive at true theories?

(2c) Does the search for true theories (as opposed to the search for empirically adequate theories) play a (central) role in the epistemology of science?

Question (2c) may be interpreted descriptively–Did it play such a role historically?–but also normatively–Are there epistemological reasons why it should play such a role? Although the interpretations are different, the answers will be parallel. One good thing about philosophers of science is that they realise that the actual practice is richer than the output of philosophical speculation, and hence prefer looking at facts over arguing about Hume's gap.

We now come to a third bunch of questions some of which seem rather remote from the debate on scientific realism. That they are nevertheless directly related to it, appears from the ensuing question (3c).

(3a) Do we (humans) have a need for a correct (more or less embracing) view of reality?

If (3a) is answered positively, there are two further questions:

(3b) Are we justified in considering some world-views as superior to others (with respect to the need mentioned in (3a))?

(3c) Should scientific theories, and more specifically their theoretical statements, play an important role in choosing between alternative world-views?

If (3a)-(3c) are answered positively, a positive answer to (1a) seems unavoidable. If the theoretical parts of our best scientific theories are not approximately true, we cannot rely on them to evaluate world-views.

3. The Positivist Insight

Positivists (and some pragmatists, and others) have argued quite convincingly that there are no good reasons to answer (1a) and (1b) positively. They either consider the questions as ill-phrased, or answer them in the negative.

Their arguments are quite convincing. Moreover, the arguments are not obfuscated by the changes that our epistemological view underwent since the days of Mach. Mach believed in the existence of a single language of science in which all scientific theories, in as far as they are scientific, may be phrased (or at least translated). He believed that sensations (exist and) form an unquestionable basis for our knowledge. He believed that horses (and some other animals) do experiments. Giving up all this has not affected the force of the positivists' arguments against the referring character of theoretical terms.

The basic argument is simple: our only contact with reality are our empirical data; although our theories (cannot but) go beyond these–remember the conventionalist thesis–empirical data are the only and conclusive criterion to

judge theories.[1] Rephrased negatively (and in more modern terms): in order to estimate the truth-likeness of a scientific theory, we should know the truth (the true structure of reality); but we don't; our only access to truth are the empirical data. (And if we did know the truth, we would have no need for science.)

It follows immediately from this theoretical argument that empirical success cannot warrant the (approximate) truth of the theoretical parts of scientific theories. In other words: false theories may be empirically successful. We never have seen a serious direct reply to this argument. All arguments offered by realists are indirect. And there is a good reason for this: it simply is true that some false theories are successful.[2]

Let us illustrate this with an everyday theory that does not require any technical knowledge. Until some fifty years ago, most beekeepers believed that a swarm (up to some forty-thousand bees, including a queen, leaving the hive) could be forced to gather (ideally on a branch of a tree, but usually in a more difficult spot) and to stay there for a while by making the bees believe that a thunder-storm is coming up–bees' reactions on upcoming thunderstorms are well known. Beekeepers imitated thunder by making lots of noise, using kettles, pots, and guns; they imitated lightning by reflecting the sun on the gathering swarm with the help of a mirror; they imitated rain by dispersing water in the air. The procedure is quite effective. It was a part of common knowledge about bees, as may be seen from tens of engravings and read in many books.

The 'theory' is false. For one thing, bees lack a central nervous system. Beliefs are somewhat out of order for them. Moreover, von Frisch and others discovered that bees are deaf (to put it simply). So, imitating thunder is even empirically ineffective. Finally, bees' reactions to an upcoming thunderstorm are ('empirical' and) related to the darkening of the sky: the part of the sky that emits polarized light is quickly diminishing, which disorients them outside the immediate neighbourhood of the hive (where they rely on 'visual' clues). So, even if the reaction of a swarm on the imitated thunder-storm is somewhat similar to its reaction on a real thunder-storm, the stimuli causing the reactions are different. We have an explanation today why dispersing water is effective–all beekeepers still use that. We even have an explanation why the reflection of the sun on a gathered swarm might (very slightly) slow down its departure (by disorienting the bees' dances). So, we understand why the procedure was effective, even if the theory was false.

A while ago, Larry Laudan (who considers himself a pragmatist) has brilliantly devastated some realist's claims–see his [1981]. Ever since, quite a

1. Theories from different domains should also be coherent–remember, e.g., Mach's stress on unification. Laudan [1977] specified this by requiring that theories should not generate internal or external conceptual problems. Still, reality only interferes by virtue of empirical data.

2. Positivists would phrase this differently; for example as: the theoretical parts of some empirically highly successful theories have not been retained by their successors.

few realists try to make their case by refuting some points from that paper of Laudan's. Especially the claim that the theoretical entities of many older scientific theories do not refer, has impressed realists. They seem to forget that, even if they *were* able to refute this argument from the history of the sciences, there still are many (factual and analytic) arguments left (and Laudan explicitly mentions many of them).

With some hindsight, the situation may be summarized as follows. There is no warrant for the approximate truth of our present scientific theories or for the convergence towards the truth of successive scientific theories. There is not even any reason to believe that the succession of concepts in some domain is bringing us any closer to the real structure of the world. Approaching the set of statements that are true within some language does not offer any warrant for approaching the truth; and approaching the linguistic (or conceptual) instrument that fits the world is a quite different matter.

4. The Role of the Search for Truth in the History of the Sciences

It is rather obvious that, before positivism and, later, quantum-mechanics came around, scientists were after a model of the world-out-there. For example, the discussions on heliocentrism, on phlogiston, or on evolution theory were not about saving the phenomena. Nicholas Rescher [1987] goes even further and calls it a truism that science seeks to describe reality (p. 39). And there are nice arguments, illustrated by examples, that scientists prefer the theories that offer a causal explanation–see Hacking [1983], Cartwright [1983], and McMullin [1993].

Especially the study of scientific discovery reveals that creative scientists do not merely manipulate formulas. Detailed case studies reveal that they start from *interpretations* (beliefs concerning the structure of the world), and adapt them as they get closer to a solution of the problem. Faraday is only one of many examples.[3] Admittedly, some scholars, especially those arguing that computer programs like BACON made impressive 'rediscoveries', seem to deny the importance of interpretations. Qin and Simon [1990], e.g., claim that explanatory hypotheses played no role in Kepler's discovery of the third law (and hence that BACON's simulation sheds light on the way in which Kepler arrived at his discovery). But the historical record in this and similar cases is unambiguous–see, e.g., Field [1988] and Kozhamthadam [1994].

The role of interpretations is usually misconstrued in science handbooks. Most of these are straightforwardly positivistic. And so are most science teachers, especially if they are not very creative; but sometimes even if they are. But presumably everyone knows by now that textbooks are not the right source for the history of science.

3. The way in which 'metaphysical' beliefs influenced the work of Faraday is found, e.g., in Nersessian [1984].

A more serious reason for worry is that several scientific realists try to reconstruct the concepts from older theories in modern terms. It is then said that Carnot's caloric was actually entropy (or, as others want it, internal energy), and that phlogiston was the absence of oxygen. It seems to us that this line of defense of scientific realism is bad policy. First, such reconstructions are very sloppy. The realist Mendoza has to admit that both Carnot's 'calorique' and 'chaleur' have to be interpreted in some cases as entropy and in other cases as heath (Mendoza, [1960; xvii]). And clearly, those bottles marked 'phlogiston' did not contain the absence of oxygen. Next, the function that caloric or phlogiston played in the origin and elaboration of thermodynamics and chemistry could not have been played by, respectively, entropy and the absence of oxygen. For example, the phlogistic view on combustion would never have originated under the terms of the reconstruction.

The beeswarm example proves useful to make a further point in simple terms. To reconstruct it, the realist should demonstrate (i) that some set of events is provoked both by the upcoming thunder-storm and by its imitation, and (ii) that a swarm (as a system) has the disposition to react on those events in a way that makes the desired behaviour probable. But precisely this is impossible: the stimuli that cause the reaction to the upcoming thunder-storm are different from those that cause the reaction to its imitation.

But, actually, what do realists intend to show by such reconstructions? The answer is presumably that (according to our present theories) there *is* something (a substance, a property, ...) in reality that is approximately captured by the concept from the old theory. This means that they need a *unified* reconstruction: they have to show that *all* occurrences of the old term, in empirical as well as in theoretical statements, can be replaced by the new term, *and* that the hypothesis-generating role of the old concept can be understood in terms of the new concept. But, as we saw, scientific realists fail precisely at this point. Scientific realists who offer such non-unified reconstructions not only fail to make their point. In admitting that the best reconstructions one may come up with are non-unified, they unintendedly support the thesis of the old positivists that just *any* interpretation may play an important heuristic role. For consider any theory that was rather empirically adequate. It is very likely that, for most of its statements, some statement from a contemporary theory has 'very roughly the same meaning'. If nothing more is being claimed, then nothing follows about the interpretations themselves (or about their continuity). If you think that, in the phlogistic account on combustion, phlogiston and the absence of oxygen mean roughly the same thing, imagine Priestly's face when you had told him so.

An even more perplexing realistic move is offered by Stathis Psillos, e.g., in his [1994], who argues that Laudan's 'pessimistic meta-induction' is no threat for scientific realism, because many former scientists themselves considered the non-referring terms of their theories as non-referring. It would be no problem then that 'caloric' is non-referring, because Sadi Carnot did not believe in its existence himself. Independent of the correctness of the histori-

cal claim, one wonders what convergent realism comes to under this reading. If we have to rely on the testimonies of past scientists,[4] then we should do the same for present-day scientists. But many of them portray themselves as adherents of a positivist stand, and hence may be taken not to believe in the referring character of the theoretical terms of present-day science. In this understanding, either there is nothing to converge to in the first place (if present-day science is to function as the criterion) or there clearly is no convergence (as most present-day scientists do not 'believe' in any theoretical entities, whereas some of their predecessors did).

After these remarks, our answer to questions (2a)-(2c) should be clear, and so should be its relation to scientific realism. There are many scientific discoveries (and other creative processes) in which the role of interpretations is central, not just as metaphors or as elements of a useful game, but as means in an attempt to obtain a true image of the world. These interpretations may, however, be mistaken. They need not coincide with the true structure of the world, not even approximately so. They are hypotheses about that structure, attempts to lay hands on it. It is these hypotheses that are the motor of discoveries. Even if Carnot would not have believed (at some point in time) in caloric, it was caloric that permitted him to rely on the water-wheel analogy to arrive at the idea of the 'chute de calorique'.

To recognize the role of interpretations is not unimportant. It entails that at least the popular reading of positivism is wrong where it reduces the problem of discovery to either arriving at a new theory by *any* means (the popular Vienna Circle) or by merely picking the best amongst *available* alternatives (the popular 1934 Popper). Scientific discovery requires that one devises new theories and solves problems connected with existing theories in a creative way. And the search for the true structure of the world plays an essential role in those activities.

Up to now we neglected the importance, for scientific discovery within some domains, of theories from other (sub)domains. Taking those into account as well will only strengthen our claim. Even the planning of experiments is performed by heavily relying upon (interpretations of) theoretical claims of accepted theories. And the interpretations that we referred to in the previous paragraphs will be required to be coherent (and preferably intimately interwoven) with the interpretations of accepted theories from other domains. In this respect too, the role of interpretations (in the sense of hypotheses about the unobservable structure of the world) is undeniable.

We did not clearly separate the epistemological questions from the historical questions in (2a)-(2c). The use of interpretations (in the sense of hypotheses about the structure of reality) seems to be a very effective way to arrive at a coherent and productive theory. And only by recurring to interpretations do we seem capable of understanding the creative aspects of the involved prob-

4. Psillos also suggests a relation between a scientist's beliefs and the statements she uses in her derivations. But we are unable to grasp the exact nature of this relation.

lem solving. The known alternatives, like the popular Reichenbach or Popper views, reduce discovery and creativity to chance, intuition, and the like, and neither of these has any explicative power–see Meheus [1995].

The answers to (2a)-(2c) may seem to agree with scientific realism. They certainly run counter popular versions of positivism. But they by no means challenge the positivistic answers to questions (1a) and (1b). Actually, the old positivists (Mach, Duhem and Poincaré) would have agreed with most of what we defended or stated in the present section. They all allowed for interpretations in as far as these had a heuristic value. Only, they contended that the merits of a theory had nothing whatsoever to do with the interpretations that led to it, and that interpretations should be eliminated from mature sciences because of (among other things) the answers to (1a) and (1b). (As far as science itself is concerned, Duhem is somewhat stricter in these quarters than the two others.)

Incidentally, the latter aspects of the old positivists' view do not seem very attractive today. For one thing, they presuppose a view on mature science that few people will consider sensible today–see Laudan [1977] for some hard arguments. And they presuppose that the context of discovery may be separated, both historically and conceptually, from the context of justification–a separation that Tom Nickles challenged very convincingly in the introduction to his [1980]. As we shall see in section 6, those aspects do cause, in view of our present insights, an apparent conflict with the positivist's answer to (1a) and (1b).

5. The Necessity of a World-View

In its loose sense, a world-view is a collection of beliefs about aspects of reality. In a stricter sense, these beliefs are organized and connected into a coherent whole. Everyone has an *implicit* world-view in the loose sense, which may be read from people's behaviour. But an implicit world-view does not allow one to consciously organize one's life. The central point we want to make is that people have a need for an (explicit) world-view. We are convinced that this need may only be fulfilled by a world-view that is well organized and coherent, but the weaker point is sufficient to establish the relation with the issue of the present paper.

There seems to be general agreement that humans have a need for a safe and efficient way of acting. But humans also want to understand their environment. They have a need for a correct world-view, both with respect to everyday practice and for the over-all organization of their lives. Especially the latter will require a world-view in the stricter sense. The history of mankind is full of examples of people searching for, debating and sometimes fighting about world-views. The central issues in all this concern what the world looks like, not a way of saving the phenomena. This holds for descriptive, evaluative, and normative questions alike. Ideological, scientific, artistic, and other traditions have taken part in such debates, and, although factual

claims are always involved, they hardly ever are just about observable facts. We all agree, of course, that most of the fighting should better have been avoided. But the need for a world-view is there all right.

There also is a more analytic argument. If interpretations and world-views (even in the loose sense) have to be eliminated from our reasoning, then not much worth reasoning about is left. Ethical and political stands concerning poverty, the South-North relation, exploitation, oppression and discrimination, fundamental rights (and their implications for divorce, abortion, information, ...) would all be pointless. At best, we would be left with a set of 'choices' that cannot be justified and for which it even does not make sense to analyze them. And, at best again, a set of such 'choices' would be available for ourselves, because other people's choices transcend our observational capacities; we would altogether be unable to understand other people as similar to ourselves, as beings that have aims and motivations, plan and reflect– see Alvin Goldman [1970]. Without a world-view in the stricter sense, any consideration concerning the meaning of life or the overall organization of one's life becomes spurious.

That people have a need for interpretations (and a world-view) in connection with the generation of scientific theories was an issue in section 4. But it is important to stress that this need manifests itself also with respect to applications of scientific theories and with respect to 'low level' empirical research.

Some people are desperately searching for ways to predict conflicts in the third world; there main difficulty is the lack of a theoretical framework. Factor analysis and similar techniques prove ineffective for their purpose. When they try to recur to Prigogine's dissipative structures or like theories, this is not because they consider them to be good instruments, but because they hope that some of them will enable one to understand what is really going on. But let us give a more positive example. Consider research on causal factors in the origin and evolution of some disease, say cancer. Logically speaking, the candidates are countless. Moreover, presumed charlatans claim to have evidence about a wide diversity of causal connections. How should researchers select the hypotheses to be tested? Sometimes observational data seem rather compelling, for example that some form of cancer is very rare in a specific region. To find an explanation for this is clearly a relevant empirical problem. In attempting to solve it, a selection is made implicitly. *Apart from this,* the selection of hypotheses can only be made on the basis of interpretations. Researchers will not test whether carrying a rabbit's paw in your pocket will influence breast cancer. But remark that no scientific *theory* as such entails that there cannot be a causal relation there. Only the *interpretation* of the contemporary sciences, the *world-view* that is connected to them, enables us to conclude that such causal relations do not exist. The better our theoretical understanding of a specific disease, the more one shall consider the judgement deriving from interpretations to be reliable. An endless list of similar examples may be produced, both from scientific and from everyday contexts.

Two brief remarks are appropriate. As we suggested before, the intended selection may be determined indirectly by empirical evidence that generates an empirical problem. Remark that the presence of such problems may undermine the reliability of the current interpretation(s) of our best theories. Only after such problems have been solved shall we justifiedly rely on the interpretation(s) of the (modified) theory. Second remark: the present argument for answering (3a) in the positive is related to–actually a generalization of–the answers given to (2a)-(2c). Whenever, in order to solve a problem, no ready-made method is available, we have to rely on interpretations offered by our world-view. To put it slogan-like: as long as there are problems that cannot be solved by merely deriving *empirical* consequences from the available theories, we shall have to rely on interpretations and hence need interpretations and a world-view.

The last sentence brings us back to the main issue of the present section. If one replaces 'problems' by 'action problems' in that sentence, it appears unlikely that we shall ever be able to waive world-views. But most problems that are related to understanding make world-views altogether unavoidable. For example, it always was a mystery to us how Mach and others could claim that the sciences should replace the myths in this respect, if all interpretations have to be removed from mature sciences.

Of course, one might answer (3a) in the positive and (3b) in the negative. This position concedes that there is a need all right, but contends that it can only be satisfied in an arbitrary way; or maybe the need should just be suppressed. We see four main arguments against such a position. First, unless we have very good reasons to prefer the position over its alternative, the alternative is superior for methodological reasons. If it is possible to justifiedly prefer some world-views over others, then failing to do so may cause terrible injury. Next, it is not sufficient to show that we cannot have a final and unquestionable foundation for some or other world-view. Indeed, this still leaves open the possibility that some world-views are more correct than others. Even if some world-views were only more *efficient* than others (with respect to the purposes they have to serve), this might be a good reason to make a choice that is justified in that respect. Third, it simply is bewildering that people seriously consider all world-views on a par, especially if they want to make strong claims about observables. As the existence of uninterpreted, absolutely reliable empirical data has been given up by everyone, an unbridgeable gap between observables and theoretical entities seems undefensible. (Van Fraassen [1980] has been sufficiently criticized on this point). Fourth, the consequences of answering (3b) in the negative seem rather unattractive. Consider, for example, the fundamentalist claim that God created the world a while ago, and created all the fossils as we find them today.[5]

5. A handy fundamentalist would admit that evolution is the best *scientific* hypothesis, but would deny that it has any relevance for our world-view.

Whoever answers (3b) in the negative has no *argument* to oppose this. Or take another example. Do positivists really claim that, in as far as truth is concerned, the psychological explanation for the effects of carrying a talisman (on people that believe in it) is on a par with the superstitious explanation?

Implicitly we already gave a positive answer to (3c). Actually, apart from fundamentalists, people that answer (3b) in the positive will do so for (3c) as well. They may disagree about the relevance of scientific theories in this respect. Typically, some deny its relevance for 'fundamental' issues (like the meaning of life). Some go as far as denying that scientific knowledge might be directly or indirectly relevant for someone's decision to commit suicide. We deeply disagree, but this is not the place to argue about it. Let us stay content with the fact that anyone who is sensible enough to consider some world-views as better than others, agrees that scientific theories are relevant for many aspects of our world-views. These hardly questioned aspects range from the structure of matter, the structure of the cosmos, and the origin of life to such 'empirical' matters as selecting food. In this time, where going by smell and taste is a warrant for dying early, not only the instructions applied by controlling state agencies but even cookbooks are full of theoretical terms (at least in the sense of referring to entities that van Fraassen labels unobservable).

6. An Apparent Contradiction

In section 3, we pointed out that positivists have convincing arguments against the claim that the theoretical statements of some of our scientific theories are approximately accurate descriptions of reality *and* against the claim that successive scientific theories converge towards the truth. In the two subsequent sections, we maintained that the theoretical parts of our theories do not only organize our past experiences and do not only lead to predictions that are (closely or more remotely) similar to past experiences, but that, in some decisions, their interpretations play an essential role.

That interpretations play such a role with respect to scientific discovery is not itself in contradiction with the positivist answer to (1a)–remember that Mach himself recognized that role. Still, this role becomes problematic in a period in which the existence of mature sciences is seen as highly questionable–see already 'In Defense of 'Immature' Science' in Laudan [1977]. If, as Laudan points out, there is no major (physical) science in which 'foundational debate has been questioned' or in which 'the disdain for anomaly and the indifference for extra-programmatic conceptual problems have been the prevailing features', then it is highly problematic whether some science may ever free itself from interpretations, as Mach required. But that interpretations should never be eradicated from the sciences seems to conflict with the positivist answer to (1a). That interpretations, and hence (aspired and hypothesized) truth, are bound to play forever a role in the sciences seems to

conflict with the claim that no theoretical statement can justifiedly be labelled true. Nickles's [1980] convincing arguments against a temporal or systematic separation of the context of discovery and the context of justification underline this conflict. If they cannot be separated, the role of interpretations cannot be restricted to one of them.

The role of theoretical statements with respect to world-views makes the conflict even more manifest. If we need a world-view for acting and understanding and if we have to rely on our best scientific theories to construct it, then it seems that we have to consider these theories as approximately true.

It is worth pointing out in this connection, that world-views cannot be reduced to *mere* guides to act. If you want to get someone on the phone, you may have many reasons to call the number of a particular office without actually believing that the person is in that office. Maybe it is the most probable location for her to be, even if the probability is very low; maybe it is the most probable location of those that you may reach by phone; maybe she is more likely in another location, but you prefer not to talk to the person that will pick up the phone there; or maybe, of all locations where she might be, there is only one of which you have the phone number. All of these are good reasons to dial that number, but none of them gives you a reason to *believe* that she is there.

The role of scientific theories with respect to world-views is quite different from those considerations, because world-views commit to belief. In the example from the previous paragraph, the decision may be said to be (in the different cases) the best among the alternatives. Similarly, those parts of scientific theories of which the interpretations will be integrated in a world-view, will be the best among the alternatives. But here this phrase does not (only) pertain to action. Those parts of theories will have to be the best with respect to truth: the best of the available hypotheses about some aspects of the structure of the world.[6] Even probabilities do not allow one to appraise scientific theories in the required respect.[7] All known approaches to estimate the probability of scientific theories (mainly Carnap's tradition and some related personalistic traditions) offer at best an estimate of their empirical reliability; they are even defined with respect to languages in which it is impossible to (seriously) express causal relations or nomological statements.

One might try to circumvent the aforementioned conflict by distinguishing between epistemological realism and intentional realism.[8] Humans would have no reason to consider (some) scientific theories as approximately true,

6. In the third paragraph of section , we shall say some more on the criteria for considering parts of theories as true. There will be comparative as well as non-comparative criteria, and synchronic as well as diachronic ones.

7. In the third paragraph of section 8, it will also become clear that criteria for the truth of parts of theories cannot all be expressed by means of probabilities.

8. Rescher [1987; 39-41] defends a rather extreme form of realism of intent and opposes it to a realism of achievement, which he rejects.

but nevertheless *seek* to describe reality, especially in doing science. This is not, however, very convincing. With respect to world-views, humans do not only aim at the truth, but believe, and on good grounds, that some (parts of some) scientific theories *are* (approximately) true. But then the conflict surfaces again. The negative answer to (1a) may be compatible with scientists' aiming at the truth, but seems to conflict with their belief that some parts of some scientific theories are true.

Another attempt to bypass the conflict consists in reducing the positivist's arguments for answering (1a) in the negative to a form of general scepticism. Everyone knows, it is then said, that we may be mistaken, that we have no absolute warrant for the truth of our beliefs, and this is the only argument the anti-realist offers; but the argument applies to *any* claim on knowledge. Anti-realists are not correctly depicted here. They have a very convincing direct argument (that empirical data form our only access to reality) as well as a bunch of indirect arguments. Nothing in these arguments commits anti-realists to a more general form of scepticism; the arguments do neither undermine the reality of observables, nor the possibility of preferring some theories over others.

7. Why the Contradiction is Apparent Only

Let us very briefly summarize some basics from the epistemological view defended in Batens [1985], [1992a] and [1992b]. A knowledge system is not a coherent whole, but is composed rather loosely of elements and of coherent subsystems. Each of these elements and coherent subsystems is 'indexed' by its function (see below) with respect to the solution of specific problems or kinds of problems; they function as data, as methodological instructions, or as values. There may be certain relations (of two different kinds) between coherent subsystems. The unification of a knowledge system is a more complex matter than is usually maintained, in view of the (different) functions that elements and coherent subsystems should play with respect to (different) problems or sets of problems. Where it is possible, unification is important (and fruitful). But it is not desirable at any cost–for example, not at the cost of decreasing the problem solving capability. A world-view is a coherent subsystem of a knowledge system (and by no means coincides with the latter). It may be related to several other such subsystems (e.g., some scientific theories, the set of rules applied for handling objects in everyday situations, etc.)

Problems are solved within 'contexts' (short for problem-solving situations). Apart from the problem itself and the participants, the main elements of a context are a set of certainties, a set of relevant statements and a set of methodological instructions. These three are the functions referred to in connection with elements and coherent subsystems of knowledge systems. An essential feature is that these sets are not constituted by our full knowledge system, and are not arrived at by selecting from the knowledge system what-

ever is relevant to the problem. Rather this selection depends on the indices that are attached to elements and coherent subsystems. As the knowledge system needs not be (and in general is not) monolithic, it follows that something (say, a statement) may constitute the problem in one context, but may function as a certainty or as a relevant statement in a second context. It is even possible that the negation of the very same element functions as a certainty in a third context.

The basic idea about progress is that the improvement of our views is fundamentally an internal matter. The existence of an external world, that is the source of our experiences, does not preclude that only *our* (theory-laden) experiences are available to us. Whenever we try to improve (part of) our knowledge system, we always rely, for lack of any independent criterion, upon (specific parts of) our knowledge system.

The ambiguity in (1a) should now be apparent. There are (at least) two clearly distinct sets of problems for which the answer to (1a) is relevant. Let us call it the in-world set and the reflective set respectively. (1a) should be answered in the positive with respect to the problems in the in-world set, but in the negative with respect to problems in the reflective set.

Questions (2a)-(2c) and (3a)-(3c) typically concern the convictions of scientists (and others) with respect to in-world problems–the elements of their knowledge systems that function in contexts in which in-world problems are attacked. That there is an external world (ontological realism) is a (usually implicit) certainty in such contexts. And many parts of contemporary scientific theories will (justly) function as (explicit) certainties in such contexts. Roughly, these problems concern the structure of the world, including humans as subsystems of the world. They are related to situations (contexts) in which we try to improve our theories about the world–typically by relying on some of them–or in which we apply them for acting or for understanding. Building, modifying, and applying our world-view gives rise to problems that belong to this set. But many other members of the set are not in any direct way related to world-views, and some are even fully internal to some scientific theory. For some knowledge systems, value-judgements and norms are included amongst those problems. We take this to be their most appropriate locus, but many philosophers will object to that.

With respect to in-world problems we have good reasons to consider the theoretical parts of some scientific theories as true. On reflection, it seems foolish to deny this. It is hard to see how we should think and talk without relying on some theoretical parts of scientific theories.

There are also reflective problems. We may ponder upon the reliability of a world-view, of a scientific theory, of its interpretation(s), etc. It is with respect to some such problems that there seem to be good reasons to answer (1a) in the negative; it is such problems that are affected by the arguments adduced in section 3. With respect to such problems, it seems naive to consider the theoretical parts of scientific theories as true.

Phrased without any reference to other questions, the answer to (1a) appears (in its most natural interpretation) as relevant to some reflective problems. This is why the positivist insight seems so compelling if we just look at (1a) itself.

All this may sound convincing enough if you agree to the epistemological view that was briefly sketched before. Still, let us try to phrase the distinction in terms that might persuade a more general audience. There are some questions that directly concern our image of the world. We claimed that humans are justified in approaching these, where needed, by means of the theoretical machinery of some scientific theories. Suppose that someone asks you in which way plants produce oxygen. Then the correct thing to do is to give a story about cells, chemical processes, light rays, etc. And of course you believe that this is how the thing works; you have excellent reasons to use large parts of biological and chemical theories as certainties or as relevant statements in contexts of this kind. Next, consider the question whether we may be really certain that the theoretical statements of present-day chemistry and biology describe the world as it actually is. Is it impossible or unlikely that new data or new theoretical developments will force us to replace them? Apparently, 'no' is the only wise answer to the last question. There is no foolproof guarantee that the theoretical framework of those theories will not, for good reasons, be changed drastically. In a context in which you attack such problems, no part of chemical or biological theories will function either as a certainty or even as a relevant statement. The relevant statements will comprise, for example, descriptions of the historical adventures of theories and conceptual schemes. Of course, the negative answer to this question is no reason to change your story about the plants. It does not make sense to add, in front of each sentence 'according to the present theory, but it may be fundamentally wrong'. It might not actually be too harmful to do so for the plant story, but it would put us in an untenable situation for decisions and explanations that are highly value-laden or connected to aims that we consider central.

It seems to us that the refusal to consider both kinds of questions as different (and as not *directly* relevant to each other) involves a rejectable form of one-dimensionality. A comparison with ideological stands may help. Positivists are like those people that have no ideological views, or having them, refuse any serious commitment to them, because they realize, with respect to reflective problems, that they might be mistaken. They would not stop a Hitler, would not interfere with aggression or oppression, because they have no absolute warrant that their perception of the situation is correct. Realists, in turn, resemble bigots who, even if they quarrel among them about details, display a reckless commitment for the holy cause and fail to even consider that they might be wrong. The comparison is somewhat rough, but we hope it helps.

8. Some Remarks in Conclusion

The ambiguity of (1a) does not entail that arguments for the answers to its two senses are fully disconnected. In this respect we agree with Laudan's [1984] reticulated model, or rather would like something more extreme–see Batens [1992a]. For example, historical facts are quite relevant for the answer to the interpretation of (1a) with respect to reflective questions; another example follows in the next to last paragraph. But let us move on.

We already pointed out that the negative answer to (1a), in its interpretation that is relevant to reflective questions, does not reduce to some general form of scepticism. Both interpretations of (1a) are meaningful and clearly relevant to specific questions. As we see it, the situation is completely different for (1b). Even in its interpretation that is relevant to in-world questions, it should be answered in the negative. At least until Laudan's [1981] is rebutted in a serious way. (Again, we are not trying to make all parties happy.) Similarly, we have heavy doubts about measuring the truth-likeness of scientific theories. The basic modifications that might occur to present-day theories will involve rather deep conceptual changes, and we never saw a measure that is able to account for those.

Still, all this should not refrain us from aiming at the truth and for considering some parts of some scientific theories as true with respect to in-world problems. We consider them as true because (i) they are the best amongst their competitors according to our standards of theory appraisal, and (ii) moreover fulfil some supplementary criteria. We admit that we feel unable to specify the set of supplementary criteria, but we can give some examples. Some of these criteria are synchronic, others diachronic. The (specific part of the) theory should not be affected by fundamental empirical problems. Its conceptual scheme (or the subschemes that go with the specific part of the theory) should be coherent internally as well as with the conceptual schemes from the best theories in other domains. In other words, the (part of the) theory should not generate important internal or external conceptual problems.[9] But this conceptual scheme should also exhibit a certain persistence: it should have been the underlying scheme of the best theory, or of the successive best theories, in the domain during a period of some length in which novel sets of data were integrated, or some impressive conceptual problems were solved. Actually, the situation is not extremely different for observational statements. Most of what was said in this paragraph may be repeated with respect to these, even if some of it may sound somewhat artificial. The most striking similarity concerns the reliability of the conceptual scheme.

Relying on criteria as those from the previous paragraph, we may have reasons to believe that parts of our present theories are approximately true.

9. Note that this criterion, as the preceding and following ones, are absolute and not comparative. It is not sufficient that (the specific part of) the theory generates less conceptual problems than (the corresponding parts of) its competitors.

But the fact that we rely on such criteria does by no means force us to consider successive theories as converging towards the truth. Of course, if we consider a contemporary theory as true, we necessarily consider it as more true than its predecessors. But it does not follow that older predecessors should be less true than younger ones. And if, in 2050, our present theory is superseded by a successor, there is no reason why, by the 2050 standards, our present theory should still come out as more true than its predecessors.

We have three warnings left. The first is that we by no means maintain that all contemporary scientific theories are true. It is a concrete matter to decide which parts of which scientific theories are to be considered as true and which such parts have to be integrated in our world-views (or have to be removed from them).

The second warning is more serious. With respect to scientific theories, especially with respect to creative episodes connected to their generation or to their applications, hypothesized truth should not be taken as too monolithic. It is possible that a scientist tries out an interpretation without seriously believing it beforehand. It is possible that she tries out two different interpretations more or less simultaneously.[10] We by no means wanted to say that scientists arrive at theories by relying on their firm and constant beliefs about the relevant aspects of reality. Quite to the contrary, the degree of belief in the correctness of an interpretation may fluctuate with the success of its application in a creative process. World-views are more stable than those (separate) interpretations. Whereas interpretations are often tentative, we saw that theoretical parts of theories are only considered as true if they fulfil rather demanding criteria; only these will be integrated in world-views as stable elements of high quality. But even world-views are to a large extent hypothetical and dynamic, at least the world-views of active and creative people. This is especially so because they cannot be fully built up from high quality elements.

Even if it may be sensible to answer (1a) differently according to the interpretation, does this not ruin the notion of truth? Remark that we never said that theoretical statements are false with respect to the reflective problems. Neither did we say that they have no truth value (as instrumentalists and other positivists do). But we maintain that there are overwhelming reasons to accept that we neither have nor ever shall have a genuine warrant for their truth. And we claim that this insight is relevant for the solution of some problems. We also said that we have good reasons to accept some theoretical statements as true with respect to in-world problems, just as much as to accept some observational statements as true. The notion of truth is left fully intact by the combination of these claims. It is good old correspondence. Only, there are statements that we justifiedly consider as true with respect to

10. But we decidedly deny that those scientists are merely playing a game. Such a view would contradict the subsequent statements.

some contexts, but cannot justifiedly consider as true with respect to others– and this does not even apply to theoretical statements only. It is important that we have a world-view, which pertains to in-world problems. It is equally important that we may take a reflective distance from it, and be aware of the possibility of (even undiscovered) failure. The fact that we (may) have both is, it seems to us, a rather deep aspect of the human condition that has far reaching implications. And this very fact itself should be a part of our world-view. This is by no means a contradiction.

Maybe we should have proposed a new terminology: in-world realism and reflective realism. Words sometimes help.

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