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ARTICLE

Agnostic empiricism versus scientific realism: belief in truth matters

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Abstract *This paper aims to defend scientific realism against two versions of agnostic empiricism: a naive agnostic position, which suggests that the only rational option is to remain agnostic as to the truth of theoretical assertions, and van Fraassen's more sophisticated agnostic empiricism—which may be called "Hypercritical Empiricism". It first argues that given semantic realism, naive agnostic empiricism cannot be maintained: there is no relevant epistemic difference between theoretical assertions and observational ones. It then focuses on van Fraassen's more sophisticated alternative to scientific realism and suggests that an attitude towards science which involves less than aiming at theoretical truth and believing in theories would be, in some concrete respect that empiricists should recognize,* worse off *than the recommended realist attitude. To this end, the paper develops the so-called "conjunction argument" into a diachronic argument for scientific realism.*

1. Introduction

Scientific realism involves two main theses: first, that scientific theories should be interpreted "realistically", that is that theoretical assertions—assertions about unobservable entities (and states of affair, in general)—have irreducible truth-conditions and, hence, they are capable of being true or false. Second, that science can attain theoretical truth no less then it can attain observational truth, where by "theoretical truth" I shall refer to the truth of what scientific theories say about unobservable entities and processes, and by "observational truth" I shall refer to the truth of what theories say about observables.

The first thesis, which might be called "semantic realism", renders scientific realism different from *eliminativist instrumentalist* and *reductive empiricist* accounts. Eliminativist instrumentalism is the position that the "cash value" of scientific theories is fully captured by what the theories say about the observable world. This position typically treats theoretical claims as syntactic-mathematical constructs which lack truth-conditions, and hence devoids them of any assertoric content. Reductive empiricist accounts, on the other hand, treat theoretical discourse as being disguised talk about observables and their actual (and possible) behaviour. Reductive empiricism is consistent with the claim that theoretical assertions (henceforth t-assertions) have truth-values, but it understands their truth-conditions *reductively*: they are fully translatable into an observa-

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tional vocabulary. Insofar as reductive empiricists are committed to the existence of observable entities, and insofar as a certain theoretical statement is fully translated into a statement couched solely in an observational vocabulary, reductive empiricists allow "theoretical" assertions to have truth-values. But they would not thereby be committed to the existence of unobservable entities. Opposing these two positions, semantic realism is an "ontologically inflationary" view. Understood realistically, the theory admits of a literal interpretation, namely an interpretation in which the world is populated by a host of unobservable entities and processes.

The second thesis of scientific realism above, which might be called "epistemic optimism", becomes necessary if we want to distinguish scientific realism from agnostic versions of empiricism. Agnostic empiricists grant semantic realism, but challenge the *epistemic* status of t-assertions: they question whether one can ever be in a position to warrantedly believe that these truth-conditions obtain, and hence to warrantedly believe that these assertions are true. So, they want to motivate the view that it is rational to suspend our judgement as to the truth of t-assertions.

Two points are important to make right from the start. One is that is that agnostic empiricists do *not* deny that science can hit upon theoretical truth. They allow that this might well happen, if only by accident. What they *do* deny is that we can ever be in a position to legitimately claim (or believe) that science has achieved theoretical truth. Hence, the "epistemic optimism" of the second realist thesis should be meant to stress that it is *reasonable*, at least occasionally, to believe that science has achieved theoretical truth. In other words, realists stress that there is some kind of *justification* for the belief that theoretical assertions are true (or near true). This is precisely the role played by confirmation: confirmation of theoretical assertions—and not just of the observational consequences of the theories—provides the justification that realists need for their epistemic optimism.

Second, it is important to distinguish between two forms of agnostic empiricism: *naive* and *sophisticated*. This is necessary in order to accommodate van Fraassen's (1980; and especially 1989) Constructive Empiricism, which, I think, is subtler than naive agnostic empiricism. van Fraassen does not want to defend the thesis that an agnostic attitude is the only rational option, as naive agnostic empiricists would have it. Rather, he stresses that an agnostic position is *no less* rational than the realist attitude, even if it is not the only rationally compelling attitude. His main argument for this view is that there is an alternative *empiricist* image of science in which search for theoretical truth, and belief in the truth of theories drops put of the picture *without any loss* for the practice of science. So, Van Fraassen's claim is that in our philosophical reflection on science, we do not have to interpret science as an activity which involves search for, and belief in, theoretical truth in order to account for its salient features and for its empirical success.

This long(ish) introduction offers some background to the present paper. Making semantic realism the object of "philosophical consensus" was by no means a trivial feat,[1] but, in accordance with the current state of play, I shall just assume it. Having distinguished between two versions of agnostic empiricism, Section 2 will discuss naive agnostic empiricism and show that, given semantic realism, a selective agnosticism about theoretical truth cannot be rationally maintained. Some of the arguments I try to rebut find their source in van Fraassen's own critique of the realist epistemic optimism. (For van Fraassen, at least occasionally, lends a hand to the naive agnostic empiricist view.) Section 3 onwards will focus on the main aim of this paper, namely to discuss van Fraassen's own alternative to scientific realism. It will be suggested that an attitude towards science which involves less than aiming at theoretical truth and believing in theories would be, in some concrete respect that empiricists should recognize, *worse off* than the recommended realist attitude. To this end, Section 3.2 explores further the so-called "conjunction argument" for scientific realism.

2. Naive agnostic empiricism

Agnostic empiricists take theoretical discourse to be truth-conditioned. But they also note that when it comes to claims putatively referring to unobservables, one can never be in a position to assert that they are true (or likely to be true). Hence, they recommend suspension of judgement. But what exactly is involved in asserting that a statement is true (or likely to be true)? If one is ready to warrantedly assert the statement S, then one should also be ready to warrantedly assert that "S" is true, and conversely. This much follows from the disquotational property of the truth-predicate, irrespective of whether one wants to endorse a minimalist account of truth (that is, to add to the above: "And this is *all* there is to truth ascriptions") or to defend a more substantive "realist" account of truth (that is, to add to the above: "Truth-ascriptions require that there is a *property* that all true statements possess, a property which one might call "correspondence with reality"). An agnostic might have views as to how the concept of truth is to be understood. Van Fraassen, for instance, goes along with a "correspondence" account of truth (cf. 1980, p. 197). But whatever the details of these views are, one's concept of "truth" should satisfy the above disquotational schema.[2]

In light of this, the issue of whether one can assert that a statement is true reduces to the issue of whether one can assert the statement. Agnostics will take literally the theoretical assertions of the theory, e.g., that "The gas in the flask is carbon monoxide", or that "Neutrinos are produced during a β-decay", which, given semantic realism, have irreducible truth-conditions. Why exactly can they not assert statements such as the above (i.e. t-assertions), but they can assert statements which refer to observable entities such as "Jupiter has 8 satellites" or "Aspirin relieves headaches", or "There is a silver-grey track in the cloud-chamber" (i.e. o-assertions)? In order to sustain a sceptical attitude towards t-assertions, one should look for (and motivate) a relevant *epistemic* difference between t-assertions and o-assertions. This is exactly what the agnostic empiricist should argue for. For if there is a principled epistemic difference in the ways in which o-statements and t-statements come to be warrantedly asserted, then one's semantic ascent to the truth of such statements should be guided by different standards.

This is precisely where trouble lurks for the naive agnostic. I take it that there are two candidates for the relevant epistemic difference: difference in the ways of *verification* and difference in the ways of *confirmation*. Yet neither of them can produce the principled difference which the agnostic should be after. Verification is clearly too strong if it is understood to mean "proving the truth" of a t-assertion. Old empiricists saw that very clearly when they abandoned verificationism: not only can we never be in a position to verify a universal generalization which refers only to observables; singular statements too are, strictly speaking, unverifiable. If conclusive proof is required for accepting a claim as true, then one can conceive of all sorts of circumstances in which a singular o-claim (e.g. that the reader is now reading this paper) cannot be proved. So if verification (in this strong sense) is accepted, the whole process of asserting *any* statement, be it an o- or a t-statement, cannot get off the ground.

Looking for differences in the ways of confirmation is more promising, because, to say the least, there should be no doubt that o-statements are confirmable. If t-statements are somehow inherently *unconfirmable*, then agnostic empiricists might latch onto a relevant (and valuable to them) *epistemic* difference. In what follows I shall take some time to show that there cannot be a principled confirmational difference between o-assertions and t-assertions. My point will be the following: if, because of confirmability, belief in o-assertions is rational, and if there is no confirmational difference between o- and t-assertions, then belief in t-assertions is rational, too.

How could it be that t-assertions are inherently unconfirmable? One option is to say that the evidence can *never* raise their probability. But it is not clear to me that there is a theory of confirmation which can achieve this feat. On the standard Bayesian account of confirmation, for instance, a theoretical hypothesis is confirmed insofar as its posterior probability is greater than its prior. This can be achieved whenever the probability of the evidence is less than one. So, all novel predictions—those whose probability is not equal to unity—do confirm a theoretical hypothesis which entails them. (I do not want to claim that only novel predictions confirm. "Old evidence" does confirm too. But for the point at stake, it is enough to show that at least *some* evidence can confirm theoretical hypotheses.)

Well, one may adopt the radical view that theoretical hypotheses have *zero* prior probabilities. If so, no matter what the evidence is, their posterior probability cannot be raised; hence t-assertions cannot be confirmed. I can understand this claim if it is meant to be the *definition* of the point that t-assertions are not confirmable: to say that t-assertions are not confirmable is to say that, by definition, they have zero prior probabilities. But if it is meant to be a substantive claim, then it is absurd. To give, by fiat, zero prior probability to all t-assertions amounts to either claiming that they are contradictions, where clearly they are not, or adopting theoretical dogmatism. On the latter reading, no empirical fact forces empiricists to be agnostics. They simply choose a dogmatic policy which makes confirmation of theoretical hypotheses impossible. In any case, a similar dogmatism can threaten the possibility of confirmation of o-statements. For agnostic empiricists should justify why their dogmatism is one-sided: why do they choose to give non-zero priors to claims about observables, and in particular to universal generalizations?

Agnostic empiricists might try to find some solace in van Fraassen's claim that t-assertions could be seen as having *vague* prior probabilities. The claim here is that the prior probability (prob(H)) of a t-assertion H which entails evidence E is *anywhere* in the closed interval [0, prob(E)]. Van Fraassen claims that "for the most thorough agnostic H is vague on its probability from zero to the probability of its consequences, and remains so when he conditionalises on any evidence" (1989, p. 194). Two replies are available here. First, the assignment of vague priors does not show that the evidence can have no impact on the probability of t-assertions. When the evidence is learned, i.e., when $\text{prob}(E) = 1$, the vagueness interval of the posterior prob(H/E) is increased to [0,1]. But this simply means that the evidence does bear on the posterior probability of H, because now this probability can be associated with values greater than the ones before the evidence rolled in. In actual practice, such changes in vague probabilities due to new evidence may make us change our attitude towards a hypothesis. Imagine, for instance, that I am told that the probability of surviving an open-heart operation is anywhere in the interval [.1, .4]. But suppose that some new evidence comes in about the condition of my heart which changes the interval to [.1, .8]. Surely, this new information would make me re-think my original decision to avoid the operation.[3] The second reply goes as follows. Here again, as in the case of zero prior probabilities previously discussed, if vague priors were to offer any solace to an agnostic empiricist, they could prove too much. For, equally, one may give vague prior probabilities to o-assertions. So agnostic empiricists would still have to show what the relevant difference between o- and t-assertions is in virtue of which only the latter are assigned vague priors. An agnostic might be tempted to dismiss this point by saying the following: suppose, for instance, that I enter a laboratory and I see a tube which seems to contain some stuff. Before I examine the tube, I can have a definite prior degree of belief that it contains a liquid, but the prior degree of belief that this liquid is hydrochloric acid can be vague. So, the agnostic may say, it is much more plausible to give definite priors to o-assertions than to t-assertions.

This answer will not do. Epistemically, the situations in which o- and t-assertions are involved are quite similar. One's evidence for there being a liquid in the tube is certain liquid-like impressions. Given this impressions, one has two options available. The first is to assign a vague probability to the hypothesis that there is a liquid in the tube—call it H. One then simply chooses to be an agnostic. Given that the probability of the evidence (the liquid-like impressions) is prob(E), the vague probability that one gives to H is anywhere in the interval (0, prob(E)]. Alternatively, from the evidence—the liquid-like impressions—one can infer, no doubt not always explicitly, that there is a liquid in the tube. One thereby gives H a certain definite probability prob(H). From H, one can then infer further predictions, e.g. that the stuff can be poured into a different tube, that it can be drunk etc. Further testing might or might not confirm H. If it does, one can come to assert with some more confidence that there is a liquid in the tube. But these two options are equally available when it comes to theoretical assertions. One can choose to be an agnostic by assigning a vague probability to the further hypothesis that the liquid in the tube is hydrochloric acid—call it H*. Alternatively, one can assign a definite prior probability to H* and then subject H* to further testing by deriving predictions. If these predictions are fulfilled, one becomes more confident of the truth of H*. Clearly, prob(H) will be at least as great as prob(H*), but what matters here is that testing H* has led its own probability to increase. If this probability is high enough, one can assert H* with some good confidence. It is because we rely on background beliefs as to what kinds of things can be in tubes, that we assign a definite prior probability that it is liquid. But we can similarly rely on background beliefs to assign a definite (if smaller) prior probability to the claim that this liquid is hydrochloric acid. Given this symmetry in the epistemic situation, agnostics should have to justify why they should assign vague prior probabilities only to t-assertions: if they choose not to assign vague priors to o-assertions, then it is hard to show why t-assertions should be given vague priors.

So far we have not found good reasons to say that t-assertions are not confirmable. But there is still the "ultimate objection" to be dealt with. Agnostic empiricists may well say that t-assertions are not confirmable because they are *ultimately* about unobservable entities and the latter cannot be epistemically accessed. What makes this objection interesting is that it presupposes that claims about observables *are* confirmable. If agnostic empiricists denied *this*, then their agnosticism would not just concern theoretical assertions, but any empirical claim whatever. What exactly is the relevant difference between o- and t-assertions which makes the former confirmable but the latter epistemically inaccessible? Once again, lending a helping hand to naive agnosticism, van Fraassen says of typical statements about observables: "[W]e can see the truth about many things: ourselves, trees and animals, clouds and rivers in the immediacy of experience" (1989, p. 178). "My scepticism", he adds, "is with general theories and explanations constantly handed out about all this ..." (p. 178). Presumably, the "immediacy of experience" creates a big epistemic asymmetry because we do not see the truth of t-assertions (which are typically explanatory of experience) in our immediate experience. And how could we? But the metaphor is too vague, as it stands, to be evaluated. Different ways to flesh it out will yield *radically* different approaches to what can and what cannot be epistemically accessed.

Suppose that one allows "immediate experience" to include only whatever is *actually* observed. Then the truth of assertions about observable entities is not seen in our immediate experience. What is actually observed *is* observable, but not the other way around. So there are (or can be) assertions about observable entities whose truth is not seen in immediate experience, narrowly understood. Notice that although van Fraassen asserts that: "[E]xperience is the sole source of information about the world and [its] limits are very strict" (1985, p. 253), he takes it that: "experience can give us information only about what is both observable and actual" (p. 253). So, van Fraassen allows experience to give us information not just about what is actually observed but also about what is observ*able*. Now, there may be a tension between van Fraassen's actualism and his characterization of "observable". For the latter is a modal notion, hence it implicates the notion of *possibility*. This possible tension has been discussed by Rosen (1994) in some detail, so I shall leave it to one side.[4] The point I want to press is that if we allow experience to reveal us truths about observables, then we ipso facto endorse an understanding of the limits of experience which allows epistemic access to unobservables.

Let us, then, take the limits of experience to include observables, that is entities which *can possibly* be observed. Then a lot depends on how exactly we understand what can or cannot be observed. We all agree that the mere *logical* possibility of observation is too liberal to characterize the bounds of experience. No theoretical entity (unless its very idea is contradictory) would then fall outside the limits of experience. If logical possibility is far too liberal for the empiricist, what other conceptions of possibility are available? We should certainly look to some notion of *nomological* possibility. But nomological possibility is a double-edged sword. What grounds what is nomologically possible to observe is not what we humans, that is beings with a certain biological make up, can actually observe. Surely, it is nomologically possible to observe the satellites of Saturn, even though no human being can actually be transported anywhere near Saturn to see them with their naked eye. Rather, what grounds what is nomologically possible for us humans to observe is what the *laws of nature* allow beings with our biological make-up to observe. They allow us to observe the satellites of Saturn, had we had the technology to be transported near them. And we are right in hoping that, because Saturn's satellites are supposed to be big enough relative to a human observer, some day some humans will see them with naked eye. But, surely, the laws of nature allow us to see a virus (without using microscopes, that is), had we had the technology to blow them up, or to reduce some humans to such a minuscule size that can be put into a minute capsule and injected into someone's bloodstream. Is that science fiction? If it is, so is, currently, transporting a human near Saturn. One science-fiction story might be easier to realise than the other, but should this technical (or, better, technological) problem have any bearing on epistemological issues?

If we call the satellites of Saturn "observable" because we can imagine (that is, because it consistent with the laws of nature to imagine) technological innovations which, although still unavailable, can make us directly observe the satellites, then we should allow viruses to be *observable*. The point here is not that even if viruses are, somehow, observable, there may still be some entities which we could not possibly see with naked eye no matter what. This may well be so. Rather, the point is that it is *enough* to say to agnostic empiricists that on the presently discussed liberalized understanding of the limits of experience, some *paradigmatic* cases of what they would call "unobservable" entities would fall within these limits no less than some paradigmatic cases of what they call "unobserved-but-observable" entities. So the equation "unobservable = epistemically inaccessible" is suspect: "unobservable" is simply not co-extensional with "epistemically inaccessible".

In the absence of an argument which makes t-assertions unconfirmable, I think we should be content with Hempel's dictum: "since such theories [i.e. theories that are formulated in terms of unobservable entities] are tested and confirmed in more or less the same way as hypotheses couched in terms of more or less directly observable or measurable things and events, it seems arbitrary to reject theoretically postulated entities as fictitious" (1965, p. 81).

Before I move on, I want to block an objection to the line of argument I have offered so far. An uncharitable reader might think that there might be an *equivocation* in my argument: a slide from having no reason to believe a thesis to having a reason to disbelieve it. I think this objection misses the point of the present discussion. The thesis I tried to rebut is the following: (*P*) Despite the fact that o-assertions and t-assertions are semantically on a par (they are both truth-conditioned), there is a relevant *epistemic* difference between them such that if one accepts that o-assertions are confirmable by evidence, one can use this relevant epistemic difference to *deny* that t-assertions are confirmable as well. I took *P* to characterize the naive agnostic position I attacked. If there is no reason to believe *P*—that is if my arguments have undercut the reasons for *P*—does that imply that there is reason to disbelieve *P*? I submit that it does. For if all reasons that are summoned in support of *P* are found wanting, then belief in *P* is no longer supported (or even, rational). *This* fact justifies disbelief in *P*. Well, the objector might want to remain agnostic: there are no reasons to believe in *P*, but no reasons to disbelieve it either. But I am at a loss to see how agnosticism about the central agnostic thesis could possibly help a defender of this thesis. Could the point of the objection be that the falsity of *P* has not been *proved*? This may well be so. Yet, if the arguments offered have been cogent, the rationale for accepting *P* has been drastically undermined.

At the risk of labouring the obvious, I also want to add the following point against the present objection. The fact that there is no relevant epistemic difference between t- and o-assertions does not, on its own, show that t-assertions are confirmable. But the point I wanted to make was different (and two-fold). First, if there is no relevant epistemic difference, then it simply does not make sense for the agnostics to direct their arguments solely against the confirmability of t-assertions. Second, if there is no relevant epistemic difference, the issue of how t-assertions are confirmable is no longer hard: it simply reduces to how any kind of assertion is confirmable (cf. the Hempel quote above). The (important) details are left to the theory of confirmation and need not worry us here.[5]

3. Hypercritical empiricism

So far I have tried to undercut the rationale of naive agnosticism about theoretical truth. I attempted to rebut some popular arguments which have found a cogent articulation in the writings of van Fraassen. But, as I noted in the introduction, van Fraassen's position is subtler than the naive scientific agnostic I have dealt with. Van Fraassen surely offers arguments in favour of an agnostic attitude when it comes to his attempts to undercut the epistemic optimism associated with scientific realism. But his heart, as it were, lies elsewhere. His own "Constructive Empiricism" is an attempt to bypass the issue of whether or not we *should* be agnostic about theoretical assertions, and to replace it with the issue of whether or not we *need* to be epistemic optimists, as realists would be. To this end, van Fraassen wants to motivate a (philosophical) image of science in which search for theoretical truth does not even feature as the aim of science.

Let me motivate this empiricist alternative by a tale, a variant of which was told by van Fraassen (1975) about abstract entities. Once upon a time there were two possible worlds, Oz and Id. These worlds were very similar to one another, and very similar to the actual world—call this @. Both worlds enjoyed "the paradise that Boyle, Newton, Mendel, Rutherford, Bohr and the rest have created" in @ (cf. van Fraassen, 1994, p. 192). But there was one difference. In Id, the aim of science was to achieve theoretical truth, and when theories were accepted, they were believed to be true. In Oz, the aim of science was to achieve empirical adequacy, and when theories were accepted, they were believed to be empirically adequate. No presumption about the truth or falsity of whatever these theories said of the unobservable world was made. But, now, the tale goes on, imagine a philosopher who reflects on the actual science, i.e. "the paradise that Boyle, Newton, Mendel, Rutherford, Bohr and the rest have created" in @. Is there anything in the "paradise" of @ which dictates that, when it comes to the philosopher's account of the epistemic and aim-theoretic characterization of science in @, the philosopher should think that Oz is not possible or well-founded? In other words, does our philosophical reflection on science dictate that we (philosophers) should view science *as an activity which involves search for and belief in theoretical truth in order to account for its practice and for its success?* Do we *have to* view @ as Id, as realists suggest, or can we make sense of science if we take @ to be like Oz?

The moral of the fairytale that van Fraassen would like us to draw is that there is an alternative theoretical–philosophical image of science: one can see science as an activity or practice which is possible, intelligible and successful, without also accepting that science aims at, and succeeds in, delivering theoretical truth. He suggests that it is precisely *this* image that modern empiricism should juxtapose to scientific realism. His "constructive empiricism"—what I prefer to call *hypercritical empiricism*—is an attempt to show that Oz is well-founded and that therefore one need not go for Id—as realists do. If this is right, then whether or not @-science can attain theoretical truth becomes irrelevant. What matters is that there is an image of science which makes a realist understanding of @-science *optional.*

Before I "fill in" the details of Oz-science, I want to make a couple of points that will pre-empt possible misunderstandings and facilitate my exposition. In his exchange with Rosen (1994) and myself (1996), van Fraassen (1994, van Fraassen *et al.*, 1997) has insisted that his general epistemic and aim-theoretic account of science should not be seen as a summary-statement of the epistemic and aim-theoretic attitudes of individual scientists, or of the "abstract noun" *the* scientist. Rosen (1994) has conclusively shown that if van Fraassen's view was taken to describe the epistemic and aim-theoretic practices of actual scientists, then it could be empirically tested and, probably, be shown to be false. That is to say, if Oz and Id above were taken to be two alternative *descriptions* of the epistemic and aim-theoretic views of actual scientists in @, then it could be empirically tested whether or not Oz or Id offers a more accurate description of @-scientists. So, van Fraassen has insisted that Constructive Empiricism is a philosophical view about *science*—a view that empiricist philosophers should consider and accept—and not about scientists and their (conscious or unconscious) behaviour.[6] Constructive Empiricism offers an alternative *philosophical* characterization of science. In particular, and this is going to connect us with the previous section, it is an image which agnostic empiricists should accept, in place of naive agnosticism. Quoting with approval Rosen's quote, van Fraassen says: "'The aim" of attempting to carry through the constructive empiricist interpretation of science is "to show that even though he sees no reason to believe what they say, the [scientific agnostic] need not be driven out from the paradise that Boyle, Newton, Mendel, Rutherford, Bohr and the rest have created" (1994, pp. 191–192).

The other point of clarification concerns what exactly needs to be accounted for by a philosophical theory of science. Realists and their opponents would agree that it is not the behaviour of actual scientists. But then, what is it? In a certain sense, it is the *phenomenology* of scientific activity. This phenomenology should not include the intentions and doxastic attitudes of scientists, but it should include the salient features of the activity they are engaged with; most importantly, it should include some central features of its practice and its empirical success. van Fraassen is in agreement here (cf. 1994, p. 191). His aim is to offer an interpretation of "what we all come to agree on classifying as science", in particular an interpretation which makes Oz possible and well-founded.

Let me now move on by highlighting some of the details of Oz-science. The criterion of success in Oz is not truth in every respect, but empirical adequacy. When a scientific theory is accepted, it is accepted as empirically adequate and not as true. Acceptance in Oz involves the belief that the theory has reached its aim, i.e. that it meets its criterion of success, i.e. that it is empirically adequate. Hence acceptance involves *some* belief, but it is belief in empirical adequacy as opposed to (theoretical) truth. But acceptance in Oz involves more than belief. It involves what van Fraassen has called "commitment": "a commitment to the further confrontation of the new phenomena within the framework of that theory, a commitment to a research programme, and a wager that all relevant phenomena can be accounted for without giving up the theory" (1980, p. 88). What is very important to stress is that Oz-science is said to incorporate, in one form or another, every element of scientific practice which, realists would argue, speaks in favour of belief in theoretical truth (e.g. that theories are essentially employed in the interpretation of the phenomena, that they are used as the basis for explanation and prediction, that theoretical virtues are relied upon in theory-choice etc.). Yet in Oz believing in the truth of the t-assertions, or even aiming at theoretical truth, are simply not part of the picture.

With all this in mind, the question before us is the following: are the conceptual resources present in Oz enough to account for all salient features of actual science? In the sequel, I shall try to show two things. First, an Oz-attitude towards theories makes sense only if we assume some form of verificationism. Second, there is a central aspect of scientific practice, the "conjunctive practice", which does not make good sense in Oz.

3.1 Commitment as potential belief

In an attempt to rebut van Fraassen's distinction between belief and acceptance, Paul Horwich (1991) has argued that, *qua* psychological states, belief and acceptance are one and the same. Belief, he notes, is a psychological state with a certain causal role. "This" says Horwich, "would consist in such features as generating certain predictions, prompting certain utterances, being caused by certain observations, entering in characteristic ways into inferential relations, playing a certain part in deliberation, and so on" (1991, p. 3). One could also add that beliefs stand, typically, as causal intermediaries between one's desires and one's actions to satisfy them. But, Horwich also notes, acceptance of a statement with assertoric content is individuated (as a psychological state) by exactly the same causal role. Hence, he concludes, acceptance can be no different from belief: it *is* just belief.

I think this argument is sound as far as it goes. But it does not go very far against van Fraassen, the reason being that van Fraassen *defines* Oz-acceptance in such a way that there is a property of belief which is *not* a property of acceptance. To be sure, Oz-acceptance is tantamount to belief when it comes to assertions about observables. But there is a *divergence* when it comes to theories (or individual assertions) which make reference to unobservables. In this case, Oz-acceptance involves belief that the theory is empirically adequate *and* commitment to the theory. But although Oz-commitment to the theory is pretty much like belief in the theory, it does not involve belief in the *truth* of the theory. So, unlike belief, Oz-acceptance does not entail belief in truth, when the accepted statement make reference to unobservables. (I can simplify that by saying that Oz-acceptance does not entail belief in theoretical truth.)

How could we restore the thrust of Horwich's argument against van Fraassen's *intended* position? We should start by noting that Oz-acceptance involves two elements, one being cognitive, the other said to be non-cognitive. The cognitive element is *belief*: belief in empirical adequacy. As van Fraassen has noted: "If you accept a theory, you must at least be saying that it reaches its aim, i.e., meets the criterion of success in science (whatever that is)" (1983, p. 327). Given that in Oz the criterion of success is empirical adequacy, Oz-acceptance commits at least to the belief (or to the assertion) that the theory has "latched onto" some truths, namely truths about observables. The supposed non-cognitive element of Oz-acceptance is *commitment*. We have already quoted van Fraassen on "commitment". Here is another relevant quote: "In addition, the acceptance involves a commitment to maintain the theory as part of the body of science. That means that new phenomena are confronted within the conceptual frame of the theory, and new models of the phenomena are expected to be constructed so as to be emdeddable in some models of that theory. It should go without saying that, even when acceptance is unqualified, it need not be dogmatic; fervent and total commitment need not be blind or fanatical" (1985, p. 281). So, commitment is what Oz-acceptance involves, on top of belief in empirical adequacy. Elsewhere, van Fraassen has compared "commitment" to "taking a stand" (1989, p. 179). Take, for instance, one of his own examples: "It seems likely to me that we have evolved from lesser organisms" (p. 179). This is a statement which involves reference to unobservable entities (lesser organisms) and processes (evolution). Van Fraassen suggests that in making this judgement, one (he?) need not report a belief; one just takes a stand. He notes that judgements as the above do not "state or describe, but avow": they express "propositional attitudes" (p. 179).

One may well try to drive a *definitional* wedge between ordinary acceptance and Oz-acceptance, by adding a non-cognitive element to the latter. But the difficulty with this move is that it is not clear at all in what sense a commitment is a non-cognitive attitude. The phenomenology of commitment (described in the quotes cited above) is identical to the phenomenology of belief. Besides, if commitments express propositional attitudes, they are no less truth-evaluable than beliefs and other such attitudes. Calling the attitude expressed "avowal" is no help. It is hard to see what exactly is involved in belief which is not involved in "avowal", and conversely. If someone told me that they doubted that we have evolved from lesser organisms, I could reply to them that I avow that we do. And if I *avow* that *we have evolved from lesser organisms*, I am ready to act on this avowal in no different way than if I *believe* that *we have evolved from lesser* *organisms*. But perhaps, the key to the issue is the "as if" operator: to say that "I avow that p" *should* be understood as "I believe that things are *as if* p". To believe *that* p is not, arguably, the same as to believe *that* things are *as if* p. But what *exactly* is the difference? Here is a possible suggestion. The "as if" operator "brackets" the truth-value, in particular the truth, of the belief: an "as-if" belief has all the characteristics of belief except that the truth of the belief is "bracketed". If this suggestion captures what van Fraassen intends to say, it seems to me very tempting to point out the following. One can characterize Oz-acceptance as *potential belief*, since Oz-acceptance involves—via belief in empirical adequacy *and* commitment—whatever is involved in belief *minus* holding the theoretical assertions of the theory to be true. So, Oz-acceptance is to be contrasted with *actual* belief in that the latter involves, in addition, holding the theoretical assertions of the theory to be true. If this characterization is right, then the following question suggests itself: once the theory is Oz-accepted, what else would it be required to hold the theory true? In other words, once all the elements of *potential* belief—via commitment and belief in empirical adequacy—are present, what would or could turn potential belief into actual belief?

An immediate reply to my question could be that being in the state of potential belief is *enough* for explaining scientific practice. But this would be too quick. If all we are concerned with is finding the minimal explanation of scientific practice, then one could restrict the cognitive aspect of Oz-acceptance even further, relegating even more to "mere" non-cognitive commitment. Oz-acceptance could incorporate in its cognitive dimension solely the belief that the theory saves the phenomena only on the working days—let's call that "working empirical adequacy". This attitude is certainly weaker than belief in empirical adequacy *simpliciter*, hence we might just assume that belief in "working empirical adequacy" in order to explain the practice of science. Alternatively, Oz-acceptance may merely involve the belief that the theory is unrefuted, and relegate the stronger belief that the theory is empirically adequate to a mere non-cognitive commitment. What this suggests to me is that if the cognitive dimension of Oz-acceptance can be further restricted, then we simply have to accept that what is minimally required for the explanation of scientific practice does not and should not dictate one's philosophical reconstruction of science.

The rejoinder here might be that if the cognitive dimension of Oz-acceptance was further restricted, then the account offered would not be in accord with some salient feature of scientific practice in @, namely that acceptance involves belief in empirical adequacy and not mere belief in the theory being unrefuted, or being "working empirically adequate". Such accounts of acceptance would be "revisionary", as Rosen (1994, p. 155) has put it. And certainly, it is part of the rationale of a philosophical account of @-science that it should not be revisionary: it should not aim to reform salient aspects of science, but instead it should account for them. Yet, what exactly should Oz-acceptance involve in order not to be revisionary? Should it involve *just* belief in empirical adequacy, or should it involve *at least* such belief? If it is the former, why is that a non-revisionary claim? To say that anything more or less than belief in empirical adequacy would be revisionary would not do, unless one had already established that the non-revisionary cognitive dimension of acceptance should implicate *just* belief in empirical adequacy. If it is the latter (that is if Oz-acceptance should involve *at least* belief in empirical adequacy), then it is left open that Oz-acceptance might involve actual belief as well. (Note here that I am not talking about the epistemic attitudes of the scientists, but rather, in the spirit of van Fraassen's demand, about philosophical accounts of what acceptance should involve.) Why then should we take it for granted that Oz-acceptance should involve *just* belief in empirical adequacy and *nothing* more? To say that anything more is explanatory redundant will not do. For so can be belief in empirical adequacy, as opposed to belief in the theory being unrefuted etc. I shall leave this point as a challenge, for the time being. In the next section, I shall offer a more sustained argument why belief solely in empirical adequacy will not do.

So, let me return to my main point: what would or could turn potential belief—exemplified in Oz-acceptance—into actual belief—exemplified in Id-acceptance? Frankly, I see no other candidate than the following: that what is required to turn a potential belief into an actual belief is a *proof* that the theory is true. Then, potential belief could not possibly turn into actual belief. For no proof in the truth of the theory is forthcoming. But this answer would commit van Fraassen to verificationism. That is certainly unwelcome. For, if what is required for believing the theory is a proof that the theory is true, then—by the very same token—proof should be the requirement for the belief that the theory is empirically adequate. Since no such proof is forthcoming, belief in truth is no more precarious than belief in empirical adequacy.[7]

3.2 Belief in truth is better

What the Oz tale shows, van Fraassen might say, is that belief in the truth of t-assertions is "supererogatory". On the one hand, he might note, it is unnecessary for the functioning of science. Oz-science proceeds with just belief in the empirical adequacy of theories and does exactly as well. On the other hand, he says, belief in truth fosters the illusion that one takes an extra risk by believing the theory to be true (cf. 1985, p. 255). But there is no such extra risk involved, he notes, because we can have evidence for the truth of the theory only via evidence for its empirical adequacy. What van Fraassen asserts is that we can never have more reasons to believe in the truth of a theory than to believe in its empirical adequacy. Since the truth of the theory entails its empirical adequacy, it follows from the probability calculus that the probability that a theory is true is less than or equal to the probability that it is empirically adequate. In line with the anti-metaphysical Occam's razor, van Fraassen suggests, belief in the truth of the theory is redundant (cf. 1985, p. 255).[8]

Let us try to evaluate the above argument. All that the probability calculus entails is that if we are to assign probabilities to a theory's being true and a theory's being empirically adequate, then the latter probability should be at least as high as the former. However, the probability calculus does not dictate how high or how low the probability of a theory's being true might be. In particular, it does *not* dictate that the probability that the theory is true is not (or cannot be) high, or at any rate, high enough to warrant the belief that the theory is true. This is a crucial issue. Realists do not deny that the observational consequences of a theory are at least as likely to be true as the theory. As we have already seen, what they emphatically deny is that the theoretical assertions of theories are somehow inherently insupportable; that they can never be likely (or more likely to be true than false). Agnostics should then need precisely to show that the likelihood of a theory can never be high (or higher than 0.5). This, as we noted in Section 2, is something that has not been shown. Nor are there good prospects that it *can* be done.

Is the risk involved in believing the theory to be true illusory? That is a bit quick. Realists argue there is something to be gained by believing the theory to be true. Envisage two theories T_1 and T_2 which are accepted as true. One can form their conjunction $T_1\&T_2$ and claim that since T_1 and T_2 are true, so is $T_1\&T_2$. One then comes to believe $T_1\&T_2$ and starts applying it to the explanation of the phenomena. Is it only explanation that can be gained, though? In general $T_1\&T_2$ will entail more observational consequences than T_1 and T_2 taken individually. So there is certainly something extra to be gained by believing the theories: extra observational consequences that would not have become available if the theories had been taken in isolation. Besides, as Michael Friedman (1983, pp. 244–247) has persuasively argued, these extra consequences boost the confirmation of T_1 and T_2 taken individually. Over time, T_1 and T_2 have received two confirmational boosts: one on their own and another as parts of $T_1\&T_2$. This argument has become known as the "conjunction argument".

There is a crucial difference between truth and empirical adequacy. Although, truth is preserved under conjunction, empirical adequacy is not, at least not necessarily. So although "T_1 is true" and "T_2 is true" entail that "$T_1\&$ T_2 is true", "T_1 is empirically adequate" and "T_2 is empirically adequate" do *not* entail that "$T_1\&T_2$ is empirically adequate". The conjunction of two empirically adequate theories might even be inconsistent. The model theoretic explication of empirical adequacy makes this apparent. To say that T_1 is empirically adequate is to say that there is a model ϕ of T_1 such that all phenomena of type P are embedded in ϕ—let us abbreviate this as $\exists\phi(P)$. Similarly, to say that T_2 is empirically adequate is to say that there is a model ψ of T_2 such that all phenomena of type Q are embedded in ψ—i.e. $\exists\psi$ (Q). However, $\exists\phi$ (P) and $\exists\psi$ (Q) do not entail $\exists\chi(P\&Q)$, i.e. that there is a model χ of $T_1\&T_2$ such that all phenomena of types P and Q are embedded in χ. There should be no doubt that, after the conjunction of the two theories has been effected, the constructive empiricist can always accept $T_1\&T_2$ as empirically adequate (as opposed to believing that $T_1\&T_2$ is true). But the whole point is that the eventual decision to accept $T_1\&T_2$ as empirically adequate is *parasitic* on the following process: accepting T_1 to be true and T_2 to be true, and then forming their conjunction $T_1\&T_2$.[9]

An immediate reaction to the "conjunction argument" is to say that, although it is sound, it is irrelevant. For the conjunctive practice involved in the argument is not a salient feature of scientific practice, and hence it need *not* be accounted for. This much seems to be implied by van Fraassen's (1980, pp. 83–84) initial reaction. He has claimed that in actual practice, theories are not conjoined in a straightforward manner, but they are corrected. Even if true, this point is exaggerated. First of all, when theories are put together with other auxiliary theories and hypotheses to derive predictions, there is no correction process involved—mere conjunction. For instance, when some optical theory is conjoined with elementary fluid mechanics in order to test a prediction about the velocity of light in a medium, no process of correction is involved. Second, although it is certainly true that *some* processes of conjunction involve the prior correction of one of the theories, the conjunction will now involve the new corrected theory $T_1^\star$ and the theory T_2. Hence, the original argument still goes through (cf. Hooker, 1985).

So, the conjunctive practice is a feature of science that needs to be accounted for. If belief in truth is involved in accepting a theory, then there is no problem. So, there is no problem to account for this practice if we go for Id. Let us compare this with what happens in Oz. As we have already seen, science in Oz aims only at empirical adequacy; and Oz-acceptance implicates only the belief that the theory is empirically adequate. Hence, the conjunctive practice cannot be immediately "rationalized" in Oz, unless it is parasitic on belief in truth.

What is important to note is that the "conjunction argument" can be developed into a *diachronic argument* for belief in (the truth of) theories. Before I state it, let me facilitate subsequent discussion by concentrating on a set of ideal practitioners of science. I call this set "ideal" simply because I do not want to risk confusing between the aim-theoretic and epistemic aspects of science and the relevant attitudes of scientists. So, a set of ideal scientists will be a set of persons who impersonate, as it were, the aim-theoretic and epistemic aspects of science in Oz and in Id, respectively. With this in mind, the argument goes like this. Because they hold their theories to be true, Id-scientists (i.e. ideal practitioners of science in Id) are diachronically better off than their Oz-counterparts. They can routinely conjoin these theories with *whatever* auxiliary assumptions (or other theories) are available or become so in the future and be able to derive extra observational consequences which their Oz-counterparts would have missed had they only accepted theories as empirically adequate. This argument does not merely refer to currently available auxiliary assumptions (or theories), but also to those that will become available in the future. The claim is that belief in theories is a better way to guarantee that scientists will not miss out on hitherto unknown observational consequences which their theories will yield when they will be conjoined with hitherto unavailable auxiliary assumptions or theories. What this argument implies is that, in a sense, "the paradise that Boyle, Newton, Mendel, Rutherford, Bohr and the rest have created" cannot be fully re-created in Oz, unless conjunctive scientific practice in Oz is parasitic on belief in truth of theories.

Precisely because the conjunctive practice cannot be dismissed as a non-existing practice, I want to consider carefully a possible reply to this diachronic argument for realism. This goes as follows. Although Oz-acceptance implicates only belief in empirical adequacy, Oz-science has developed the "conjunctive practice" no less. However, the *justification* offered is different. Upon philosophical reflection on this practice, it is noted (or, rather, the constructive empiricist spokesman of Oz notes) that the justification is inductive (of the second-order): when theories T_i and T_j were conjoined in the past, the resulting new theories Ti&Tj yielded more predictions than their individual predecessors, and were more likely to be empirically adequate. Oz-scientists, therefore, have endorsed this practice because, on (second-order) inductive grounds, it is more likely that this past successful practice will yield empirically adequate theories if it is followed persistently in the future, than if it is not.

The inductive argument under consideration relies on the premise that, in the past, conjoined theories T_i&T_j have tended to be more empirically adequate than their individual predecessors T_i and T_j. This invites the following objection. The actual (second-order) inductive argument should be much more complicated. Given the finite amount of evidence available at *any* time, the argument should proceed in *two* steps. It should *first* rely on a first-order induction in order to move from the claim that the conjoined theory T_i&T_j is unrefuted to the claim that T_i&T_j is empirically adequate. It is only *then* that one can perform the second-order induction in order to move from the claim that conjoined theories have been empirically adequate in the past, to the claim that the practice of conjoining theories tends to generate more empirically adequate theories. The difficulty lies mainly with the first step, i.e. with the first-order induction. For, as Boyd (1985) has tellingly argued, ordinary inductive projections from a theory's being unrefuted to a theory's being empirically adequate depend on theory-generated *judgements of projectibility*. Among the many theories that are unrefuted at any given moment of time only a few are projected to be empirically adequate. The selection of those which are projectable cannot just be based on observational evidence, since clearly all unrefuted theories tally equally well with the observational evidence available. Those that are selected are precisely those which are considered theoretically plausible by their proponents, e.g. those that are licensed by other background theories and relevant background beliefs. If, however, first-order inductions are theory-led and theory-informed, then they carry with them several theoretical commitments which cannot be simply brushed aside. Ideal practitioners of Oz-science who need to perform these first-order inductions in order to move on to the second-order induction about the practice of conjoining theories end up being no less committed to theories than their Id-counterparts.

One might however argue that in Oz these so-called theoretical commitments are merely "pragmatic virtues". Hence, commitment to the truth of the theory which guides and informs projectibility judgements may still be avoided. To which I reply as follows. The ultimate problem with the attempt to justify the conjunction practice by a second-order induction is that it leaves the original point of the diachronic conjunction argument untouched. Even if one conceded that after a while it became apparent that conjoining theories pays off in terms of empirical predictions, one should still want to know what exactly was involved in conjoining the first few theories. For prior to learning from experience that conjoined theories tend to be more empirically supported, increased empirical adequacy could not have been a good enough motive. This can be shown by the following consideration.

Take any two theories T_i and T_j. Suppose that they are unrefuted and that one has fixed probabilities prob(T_i) and prob(T_j) that each of them is empirically adequate. This information on its own implies *nothing at all* about the crucial probability prob(T_i&T_j is empirically adequate). There is no definite probabilistic relation that obtains between prob(T_i is empirically adequate), prob(T_j is empirically adequate) and prob(T_i&T_j is empirically adequate). Hence there is not even a lower bound for prob(T_i&T_j is empirically adequate). Prob(T_i&T_j is empirically adequate) might be *anywhere* in the interval [0,1]. So, if it is expected that prob(T_i&T_j is empirically adequate) has a definite value at all, let alone that it is greater than the probability of each of the two theories being empirically adequate, this judgement must be based on something other than estimations of probabilities of theories" being empirically adequate. This judgement is, in fact, parasitic on ascribing *truth* to the theories *before* the conjunction takes place. The relevant judgement should then be something like that: T_i is true; T_j is true; hence T_i&T_j is true; if T_i&T_j is true, T_i&T_j is going to be empirically adequate; hence prob(T_i&T_j is empirically adequate) is now anywhere in the interval [prob(T_i&T_j), 1]. We may still lack a definite connection among prob(T_i is empirically adequate), prob(T_j is empirically adequate) and prob(T_i&T_j is empirically adequate) but this should no longer worry us. Once we switch to taking theories as true what matters is that there is a definite motive to conjoin: that the conjoined theory will, as a rule, yield more observational consequences and that, therefore, the scientists will be in a better position to test whether or not it is empirically adequate.

Recently, André Kukla (1994) has suggested that there might be a way to account for the conjunctive practice from an empiricist point of view. He concedes that the "conjunction argument" is telling against constructive empiricism, but notes that there is a form of empiricism, what he calls "*conjunctive empiricism*", which is immune to this argument. When a conjunctive empiricist accepts a theory, he accepts its T# version, where T# says that the empirical consequences of T, *in conjunction with whatever auxiliary theories are accepted*, are true. Belief in T# is stronger than belief that "T is empirically adequate", but Kukla notes, it is also weaker than belief in T itself. Hence, belief in T# might be the right way to reconstruct Oz-attitude towards theories.

Is conjunctive empiricism any better than constructive empiricism? One plausible thought is that the ability of T# to yield correct observational consequences makes sense only if it is accepted that T is true. For simply there is nothing other than the truth of

T which can guarantee that T will yield correct observational consequences when it is conjoined with any auxiliaries which might become available in the future. To take seriously the possibility that T might be characteristically false and but that it yields correct predictions and that it will *keep yielding* them when conjoined with hitherto unavailable—and God-knows-what—auxiliaries, is no more credible than to believe that a coin heavily biased in favour of tails will *fail* to systematically yield tails when the tosses are made under God-knows-what hitherto unspecified circumstances. This is possible, but *very* unlikely.

As Kukla has noted, a stubborn empiricist would find the essence of the above argument question-begging. For, in effect, it suggests that the best—if not the only—explanation of T#'s ability to keep yielding correct predictions is that T is true. This need for explanation, Kukla insists, is what a stubborn empiricist would deny. So let us leave this argument to the one side. What I shall show is that, despite its promise, *conjunctive empiricism* falls foul of the original argument against constructive empiricism. Moreover, the conjunctive empiricists' belief in T# is *doubly parasitic* on belief in the truth of T and belief in the truth of all auxiliaries that might become available in the future. Let us see how this is so.

There are two defining moments of conjunctive empiricism. First, that when one accepts T#, one accepts that the "phenomena will also confirm all the empirical consequences that follow from the conjunction of T with other accepted theories" (Kukla, 1994, p. 959). Second, that once the switch to (T_i)#s is effected, the inference of $(T_1\&T_2)\#$ from $(T_1)\#$ and $(T_2)\#$ is "unobjectionable" (p. 959). Let us now examine carefully these (T_i)#s. In order to get them, the conjunctive empiricist has to conjoin T with other accepted theories A. He thereby has to form T&A. It is only *then* that he can withdraw to believing only T#, that is that the phenomena will confirm all the empirical consequences that follow from the conjunction of T with A. But, given the *original* "conjunction argument", the process of forming the subject of the conjunctive empiricist's belief (i.e. T#) is *parasitic* on believing in the truth of T and A taken separately. Suppose otherwise. That is, suppose that the conjunctive empiricist's premises are that (i) T has true observational consequences and (ii) that A has true observational consequences. From these two premises it does *not* follow that all of the observational consequences of T&A are true. It might simply be the case that T&A entail an extra observational consequence which is false. So, T# does not follow from the conjunctive empiricist's premises, unless T and A are taken to be true.

But let us suppose that the conjunctive empiricist has come up with his (T_i)#s. Can he conjoin them freely, as Kukla says, even though they are not taken to be true? Is the inference of $(T_1\&T_2)\#$ from $(T_1)\#$ and $(T_2)\#$ "unobjectionable"? As I shall show, this inference is guaranteed to be valid only if it is parasitic on belief in truth. Let "Cn" stand, as usual, for the set of the logical consequences of a set of axioms. Recall that Kukla's $(T_i)\#$ is short for the set of the observational consequences of T_i &A_i, where T_i is a theory and A_i is a set (any set) of auxiliary assumptions. The inference that Kukla calls "unobjectionable" rests on the assumption that $Cn((T_1)\#\&(T_2)\#) = Cn(T_1\&T_2)\#$. But it should be by now clear that the formula $Cn((T_1)\#\&(T_2)\#) = Cn(T_1\&T_2)\#$ may fail. This is, to repeat, because the consequences of conjoining the observational consequences of $(T_1\&A_1)$ with the observational consequences of $(T_2\&A_2)$ are a proper subset of the consequences of the conjunction $[(T_1\&A_1)\ \&\ (T_2\&A_2)]$. The latter conjunction entails extra consequences, some of which might be observational. The only way in which it is guaranteed that the above formula will not fail is to take the (T_i)#s to be true.

But perhaps I have been unfair to Kukla. For there is, after all, a way in which the formula $Cn((T_1)\#\&(T_2)\#) = Cn(T_1\&T_2)\#$ will hold good, even though the (Ti)#s are not true. This will happen if, as Earman (1978) has implied, $(T_1)\#$ and $(T_2)\#$ are *complete* with respect to observational consequences, that is if for *any* sentence S in the language of $(T_1)\#$ that draws only on the observational vocabulary, either $(T_1)\#$ entails S, or else it entails its negation (and similarly for $(T_2)\#$). Now, Kukla might just have assumed that his $(T_i)\#$s are, by definition, *complete* in the above sense. But Earman has rightly dismissed this option, because, as he says "it cannot be expected to hold for interesting scientific theories". The reason is simply that the observational consequences of scientific theories will typically be conditionals of the form $S_1 \rightarrow S_2$, and "the theory by itself will not decide the truth of either S_1 or S_2" (1978, p. 198). The same goes, I should add, for auxiliaries and other concomitant theories A. I see no justification for the expectation that there are *always* going to be auxiliaries which, being parts of Kukla's $(T_i)\#$s, will decide either all the antecedents or all the consequents of the observational conditionals $S_i \rightarrow S_j$ implied by scientific theories. That the $(T_i)\#$s are complete in the Earman sense is at best a "promissory note" with no hope that it can be cashed. I then conclude that insofar as the conjunctive empiricists" inference is "unobjectionable", it is because it is doubly parasitic on belief in truth. Only predication of truth *guarantees* that the inference from $(T_1)\#$ and $(T_2)\#$ to $(T_1\&T_2)\#$ is valid.

So, what has gone wrong? I think all that Kukla has noted is that belief in $T_i\#$s is the "fall back" position that empiricists should accept in the face of the "conjunction argument". But belief in $(T_i)\#$s, although stronger than belief in empirical adequacy, does not solve the original "conjunction problem": it accentuates it. There should be no doubt that *after* T&A is accepted, its probability is going to be less than, or equal, to the probability of T#. But this does not show that we need not believe in T&A before we choose to believe T# instead. Whether or not the degree of belief in T&A is high enough to warrant belief is an open (empirical) issue. But there is no argument which says it will never be high, or high enough. Whether or not, after one forms T#, one throws away the degree of belief in T&A and stick to the belief in T#, the fact remains that the latter degree of belief was made possible *because of* the former.

What can we conclude from this? If my arguments are right, then, from a diachronic point of view, belief in truth is a *more rational* attitude towards theories than mere belief in empirical adequacy. Even if, in the end of the day, the aim for which one develops and conjoins theories is increased empirical adequacy, *this* aim is better achieved via believing in the truth of theories. Oz-science shoots itself in the foot.

4. Conclusion

Van Fraassen (1994, p. 191) has pondered on the possibility that his interpretation of science might fail, that is that there may be salient aspects of science which are not accountable in an Oz-based approach. What would he then do? He notes that there are two options available to him. The first is to say that "science does not exist in [his] culture"; the other being that "[he] had had a very wrong idea about what science is". What is rather astonishing is what van Fraassen goes on to say: "It would be a hard choice. For saying the former, I would cut myself off from the discussion of this enterprise in which we all have much practical interest. Saying the latter, on the other hand, I would disconnect the reference of "science" from the object of my admiration. Empiricists admire science, but of course they admire science as they conceive it—how else?"

I am not sure how to interpret all this. But it is indeed hard to say that if constructive empiricism turns out to be an inadequate account of science, so much the worse for the intellectual enterprise we have come to call "science". Empiricists and realists had better not disagree about what constitutes the object of their admiration. If they just disagree on what science is, then the danger is that the discussion between them will be closed off very quickly. What I think the arguments of this paper have shown is that (a) naive versions of agnosticism about theoretical truth cannot be rationally maintained; and (b) the more sophisticated constructive empiricism has still some work to do if it is to show that its account of @-science (the object of its admiration) can plausibly be other than the realist story that Id-science is the right way to interpret @-science. Belief in truth does pay off, and hence it matters, in a diachronic sort of way. Not only does the realist story accounts for the "conjunctive practice" of @-science. More importantly, the realist account of this practice makes the realist views more rational than those of the sophisticated empiricists. Do we then have to be realists about the aim of science and the epistemic and doxastic aspects of science? Whatever else one may think about this question, sophisticated agnostic empiricism is not as rational an attitude to science and its practice as scientific realism.

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Notes

1. The interested reader should look at Feigl's excellent, but relatively unknown paper (1950).
2. I leave aside some important issues pertaining to the nature of truth-ascriptions in science. But nothing hangs on this for the development of the argument of this paper. For more on this cf. chapter 10 of my forthcoming book (1999).
3. For a similar point cf. Sober (1993).
4. I think Rosen (1994, pp. 171H174) has identified a real problem with van Fraassen's position. It is the following. It is reasonable to think that van Fraassen is an agnostic about modal facts. (They are no less unobservable than electrons etc.) But to say of an entity that it is observable is to report a modal fact: it is possible that this entity can be observed. If someone is an agnostic about modal facts, then they should be agnostic about what entities are observable. (More precisely, the point is that, although if they know that an entity is observable, then they can trivially derive the modal fact that it is possible to observe it, agnosticism about modal facts deprives them of a general characterization of what entities are observable.) If this argument is sound, van Fraassen's position cannot even get off the ground. For it requires that theories should be accepted as empirically adequate, where a theory is empirically adequate if and only if whatever it says about the observable entities (and phenomena) is true. Yet, if there is no way to characterize what entities are observable, there is no way to give any content to the belief that a theory is empirically adequate: it is totally unspecified for which entities the theory must be right about.
5. The familiar objection of the underdetermination of theories by evidence is dealt with in my paper (1997b) and in Laudan's (1996).
6. Van Fraassen dissociates his Constructive Empiricism from both of the following theses: (1) All (or most) scientists aim to construct empirically adequate theories, and believe the theories they accept to be empirically adequate; (2) the conscious understanding of (all or most) scientists is that the aim of science is to produce empirically adequate theories (cf. 1994, pp. 181, 187, 188).
7. Some similar themes have been explored in my papers (1996) and (1997a).

8. This argument of van Fraassen's might be taken to imply that we should not believe in the truth of theories. If so, van Fraassen would subscribe to what I characterized as naive agnosticism. Yet, even by this argument, van Fraassen wants to make the point that belief in truth is optional, and hence not rationally compelling.
9. Couldn't one just re-state the empirically adequate analogues of T_1 and T_2 as follows: $\exists\phi$(P) and $\exists\phi$(Q)? Still, it does not logically follow that $\exists\phi$(P&Q). For as Friedman (1983, pp.245–246) has pointed out, from the facts that (a) there is mapping M of the phenomena P into structure ϕ and (b) there is mapping M′ of phenomena Q into structure ϕ, it does not follow that there is a common mapping M″ that maps phenomena P&Q into ϕ. So, for instance, "there is a mapping M that maps the gas phenomena into a molecular structure" and "there is a mapping M′ that maps chemical phenomena into a molecular structure" do not entail that "there is a mapping M″ that maps the gas phenomena and the chemical phenomena into a molecular structure".

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