Constructive Empiricism and the Argument from Underdetermination

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1   Anyone Can Read a Book

Undoubtedly, *The Scientific Image* has been one of these few books that really had a profound impact on the philosophy of science during the last decades. Exaggerating only a little, one could even say that trying to refute van Fraassen's position in that book soon became one of the standard exercises that one had to pass to qualify as a truly realist philosopher. And who didn't want to be a realist – in one of its many guises? Luckily enough for the community of professional philosophers of science, there were many theses in the book that were deemed controversial enough to be subjected to unremitting refutation.

I have no commitments to being a realist, and the position that I will take in this paper is that of the friendly commentator. Hence, my primary intention will be to uncover the arguments for constructive empiricism, rather than to criticise them. A considerable part of the paper will be devoted to showing that, contrary to the received reading, van Fraassen nowhere uses the argument from underdetermination in his arguments in *The Scientific Image*. For understandable reasons, engaging in exegetical exercises is not the most fashionable enterprise in analytic philosophy of science. As a critical commentator put it after my presentation of an earlier version of this paper: "anyone can read a book." One should not be afraid, however, to enter into such an exercise when occasion demands it. Given the many misinterpretations of van Fraassen's position, the present case undeniably does.

Understanding that the major anti-realist position does not need the argument from underdetermination might also force a reconsideration of many realist positions, since these are often fashioned just to ward off the underdetermination threat. Moreover, as van Fraassen has been developing a much broader program in empiricist philosophy since the publication of *The Scientific Image* (see especially van Fraassen [1989] and van Fraassen [2002]), a clear picture of where he started from is of the utmost importance in judging this program, and properly understanding its background.

It should be clear that I am not just interested in setting the historical record straight for its own sake (although I do believe that this has some value). I hope that this reconsideration of *The Scientific Image* might help in reconceiving the terms of the debate, for realists and anti-realists alike. I do also hope it might help make anti-realists of more of us (although I do not believe it will). Finally, I hope that it might stand as a methodological reminder for philosophers of science. We all can read books, but sometimes it is useful to read them again.

2 The Argument from Underdetermination

In this section, I will describe a basic version of the argument from underdetermination (henceforth: UD). I will also sketch the different families of rebuttals, but without going into much detail. My presentation is primarily intended to set the stage for the following sections. For more detailed discussions, one can always consult one of the many excellent textbooks (e.g. Kukla [1998], Psillos [1999], Ladyman [2002]).[1]

The argument in one of its basic forms consists of two premises, from which one can logically deduce the falsity of scientific realism. The first premise is a logico-semantic thesis, which is sometimes taken to be further justified by historical evidence:

(1) All theories have empirically equivalent rivals.

(The equivalence consists in something like the claim that the rivals have exactly the same empirical consequences – whether these consequences are delineated sententially or in model-theoretic terms does not matter for the moment.) The second premise is a presumed epistemological principle:

(2) Since empirically equivalent theories are equally supported by all possible evidence, all of them will always be equally believable.

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[1] Laudan [1990], Laudan and Leplin [1991], Hoefer and Rosenberg [1994], Stanford [2001], Devitt [2002], Okasha [2002], and Norton [200+] are recent articles on the topic, bringing forward some of the arguments surveyed in this section (especially Laudan and Leplin [1991] has generated some extended discussions). Earman [1993], and Douven and Horsten [1998] try to exploit the argument from UD in a slightly different vein to argue for anti-realist conclusions.

Taken together, (1) and (2) imply:

(UD) Belief in any theory must be arbitrary and unfounded.

Most philosophers of science tend to be critical of both premises. I will first introduce the kind of argument that is most often levelled against the first premise. This thesis is normally introduced as following from logico-semantic considerations, because it contains a universal quantifier. It is impossible to establish that *all* theories have empirical equivalent rivals on the basis of just a handful of historical examples (barring some kind of inductive rule of inference – but how should that look when the instances are taken to be theories?). Such examples can establish at most that in some particular cases it would be unreasonable to believe in a theory. Since this is not enough to sustain the grand conclusions attributed to the argument, most criticisms focus on the untenability of the logico-semantic considerations.

One line of argument for the first premise, most strongly pushed by Kukla [1998], consists in showing that it is always possible to "cook up" empirically equivalent rivals. The most straightforward example is the following: for any theory T, construct a theory T' which asserts that T is empirically adequate, but that none of its postulated theoretical entities exist. Still more extravagant cases can easily be introduced. The quick and easy answer to this strategy is that these cooked up theories are no genuine rivals. Quick and easy, and probably true. But true for the wrong reasons – if this is to be a criticism of the first premise of the argument. It seems a hopeless task to come up with a non-vacuous criterion that could serve to sever the serious candidates from the ones that are to be expelled out of hand. Well not quite, but a non-vacuous criterion will trade on epistemic notions, such as e.g. the initial plausibility of these alleged rivals, and this comes down to denying the second premise of the argument (see also Kukla [2001]). A more elaborate argument, really targeting the first premise, would have to show that isolating the empirical content of a theory is in no way feasible, independent of the question whether it would make sense to believe in a theory merely asserting that content. If this could be proven, an essential ingredient of the recipe for the cooking of empirically equivalent

rivals would be missing. To my knowledge, no such argument has yet been conclusively provided.

A second line of argument for the first premise trades on conventionalist insights. A theory only has its full empirical content when conjoined with auxiliary hypotheses, and this gives rise to the suspicion that a theory could always be made empirically equivalent to any other by adding to it the right kind of auxiliary hypotheses (as in the case of different physical geometries that are made indistinguishable by adding exotic force functions). It seems to me that this line of argument is harder to establish than the first one, but let us for the moment suppose that this could be proven to be generally the case (there is anyway always the first line of argument to fall back on). Again, the most attractive answer seems to consist in doubting that these possible auxiliaries would have the right kind of *epistemic* status, e.g. that they are not plausible. Arguments against the first premise again tend to be disguised arguments against the second premise. It might be suspected, however, that the move bringing in auxiliaries might give us a direct argument against the possibility of isolating a theory's empirical content, which is an essential prerequisite to give sense to the notion of empirically equivalent rivals. Alas, a basic move that is always open to a defender of premise (1) is to retreat and consider only "total theories," that is the conjunction of a theory plus auxiliaries, which *ex suppositio* do have clear-cut empirical content – and hence can be considered to have empirically equivalent rivals, if only by invoking the first line of argument. That the warrant that we have for auxiliaries will always change over time, and that what we identify as the empirical content of a theory is accordingly not invariant, need not deter from this point. Of course, we might always be mistaken in what we isolate as the empirical content of a theory, but this only means that we were mistaken in considering some theories to be empirically equivalent, not that they cease to be empirically equivalent or that there are no empirically equivalent rivals. Such rivals will immediately be constructible once we have anew delineated the empirical content of our theory.

Premise (2) is closely associated with hypothetico-deductivism (H-D), since it seems to be predicated on the idea that all that matters to count some piece of empirical

information as support for belief in a theory is the question whether it is entailed by that theory. But H-D surely is flawed as a general theory of confirmation, so premise (2) can easily be denied. It is flawed because direct entailment is neither necessary, nor sufficient for a piece of empirical information to be confirmation for a hypothesis. There is no need to go into all the details here, which I take to be well known anyway. Let me suffice by pointing out that none of the rival theories of confirmation reduce confirmation to direct entailment of the evidence (one can think of Glymour's bootstrapping, Mayo's severe testing, or Bayesianism in one of its many guises). What all these rival theories have in common, moreover, is that they agree on the fact that a richer background, against which empirical tests are conducted, must be brought in to assess confirmation.

The tendency to smuggle in a denial of the second premise while arguing against the first premise points to the fact that for most philosophers this is the main reason for denying the conclusion (UD): a methodology which only considers straightforward deductive relations is too poor to do justice to what really is going on in science. (Hence it also follows that a theory which is cooked up to respect only these relations can be no respectable scientific theory). Another way to state the dependence between the arguments against both premises is to point out that if the equivalence asserted in premise (1) only consists in entailing the same empirical consequences, then it is too weak to engender a real problem of underdetermination.

Notice that the claim that we need more than direct entailment for something to count as evidence is not predicated on the idea of inference to the best explanation (IBE). Of course, defenders of IBE will concur with this claim, but it is a more general one, having to do with the question when something can be considered to be *empirical evidence*. One does not need to have recourse to contentious relations between explanatoriness and truth to make this point.

That the argument from underdetermination seems to be built on shaky grounds, and moreover, that one can agree on this without explicitly bringing in IBE or its likes, is generally taken to be bad news for van Fraassen's constructive empiricism. Let us now see whether this assessment really holds water.

3   Underdetermination in the Scientific Image

It is hard to find a discussion of the argument from UD in which no reference is made to van Fraassen. I believe it is fair to claim that he is generally believed to have used the argument crucially in his defence of constructive empiricism. Let me suffice with giving one example from a much cited recent book in which is stated:

> Currently the argument from UTE [underdetermination of theories by evidence] is employed centrally by Bas van Fraassen. He suggests that UTE shows that there are no reasons for believing more in one than the other of a pair of empirically equivalent theoretical descriptions. (Psillos [1999], p. 162)

André Kukla seems to agree in his book on scientific realism, but he adds a caveat:

> Yet it's curiously difficult to locate the exact place in van Fraassen's writings where this argument is presented in fully general form. (Kukla [1998], p. 59)

Nonetheless, Kukla claims that it is possible to reconstruct the argument by pulling together different arguments presented separately at different places in van Fraassen's writings.[2]

My assessment of this situation will be different. The absence of an explicit statement of something like the argument from UD isn't curious at all, since (a) a careful reading of van Fraassen's writings shows that passages that could be taken as providing

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[2] It should be noted, however, that Kukla is cautious enough to add the following remark before offering the full reconstruction: "To refer as I do to "van Fraassen's argument" is to take considerable liberties. The justification for doing so is that other philosophers have interpreted the passages in question as expositions of an argument." ([1998], p. 92) This remark only confirms my claim that the argument is indeed generally ascribed to van Fraassen. I want to do more, however: to show how these passages *should* be interpreted from the perspective of constructive empiricism.

pieces of a never fully explicated argument play an altogether different role in his expositions; and (b) this is so for a good reason, as such an argument is incompatible with his position in epistemology, which he dubbed 'voluntarism;' and (c) the first traces of this epistemology are already present from *The Scientific Image* (SI) onwards, contrary to what has been claimed by Kukla. In this section I will mainly deal with part (a) of my claim, whereas (b) and (c) will be treated in the next section.

It is not hard to see how one could come to ascribe the argument from UD to van Fraassen when considering his SI. In it, he argues for an anti-realist position with respect to scientific theories, which holds that accepting a theory only implies believing in its empirical adequacy; and while arguing for this position he spends considerable time on showing that scientific theories have empirically equivalent rivals. How else could this be interpreted but as involving the argument discussed in section 2? Well, it has to. While at pains to deny that the canons of rational inference would force us to become scientific realists, van Fraassens nowhere in his book claims that it is *irrational* to be one. I will come back to this issue in the next section, but let us for the moment accept that he never endorses the conclusion (UD). This implies that the fact that he clearly and undeniably argues for premise (1) stands in need for a different rationale.

Van Fraassen presents his discussions on empirical equivalence in the third chapter of SI, after having spent the first two chapters in presenting what is at stake in the realism debate. He has mainly debunked the most important arguments for scientific realism, and he has presented in the most general terms what he takes to be the most attractive alternative: constructive empiricism. At this point, he announces that he is in need of an improved account of the structure of scientific theories, one that is capable of providing a satisfactory answer to the question: *what is the empirical content of a scientific theory?* ([1980], p. 41)

I take it that such a goal involves two components. On the one hand, it has to be shown that it is logically possible to isolate the empirical content of any theory; on the other hand, it has to be made plausible that such a demarcation is potentially relevant. The second component demands that such an account should square with actual theories:

these must have a clearly identifiable empirical content which is distinguishable from their total content (by using the account's tools). Van Fraassen's solution has become commonplace by now: he opts for the semantic approach, which avoids many of the linguistic problems associated with making a linguistic distinction between observational and theoretical terms.

Van Fraassen's way of showing the feasibility and relevance of the notion of empirical content crucially depends on the notion of empirical equivalence, and it is in this context that he introduces his much discussed examples of empirically equivalent theories. He basically gives two classes of examples: one using fictitious examples, and one using examples taken from actual scientific theories. The former are used to show "the feasibility of concepts of empirical adequacy and equivalence" ([1980], p. 50), the latter are meant to exemplify in more detail how "science itself delineates, at least to some extent, the observable parts of the world it describes" ([1980], p. 59), thus nicely accomplishing the two components I discerned in the general goal that van Fraassen set himself.

There remains to be seen why van Fraassen has recourse to the notion of empirical equivalence, while being primarily interested in the question how to isolate the empirical content of any theory. The right way to understand this tactic, I believe, is by seeing how it enables one to show that *the empirical content, thus delineated, is a truly independent candidate for belief.* In van Fraassen's own terms: through this move it can be shown that "the precise definition of empirical adequacy … does not collapse into the notion of truth." ([1980], p. 64) Let us from this perspective have a quick look at the two classes of examples introduced by van Fraassen.

The most infamous case of empirical equivalence given in SI no doubt is the case of the fictitious philosopher Leibniz*, who accepts Newton's theory as empirically adequate, but does not believe it to be true. This is the prime example of a cooked-up empirically equivalent rival, as discussed in section 2. The main message that van Fraassen deduces from this example, however, is not that therefore one should not believe in Newton's theory, but only that Leibniz*'s attitude is a possible one; that is, that it is logically possible to believe a theory to be empirically adequate, without thereby being committed to

believe in the truth of at least one theory of the class of theories empirically equivalent with Newton's. He even goes as far as to claim that this is "the only important point here" ([1980], p. 47).

When discussing examples of actual scientific theories to make his general point of the distinctness of empirical adequacy and truth, van Fraassen does so under the heading of 'underdetermination' (the quotation marks are his). However, it becomes clear rather quickly that he does not intend the argument discussed in section 2. All his examples show one theory (respectively classical mechanics, quantum mechanics, and general relativity) that has truly different models that save the same phenomena. The conclusion is that all these theories indeed do have extra structure which, on the theories' own account, does not represent observable events. Hence, what a theory says about what is observable is not all that it says: its empirical structures are really *sub*-structures. In van Fraassen's own words: "In this section I have tried to give examples of very basic and general sort of how, in the description of the world by a physical theory, we can see a division between that description taken as a whole, and the part that pertains to what is observationally determined." ([1980], p. 63) Notice that van Fraassen is only interested in the internal structure of *one* specific theory.

No epistemological considerations enter at all during these discussions of empirical equivalence. At this point van Fraassen is clearly interested only in semantic issues, and he uses empirical equivalence as a means for making his general point, not as an end in itself. The last paragraph of the chapter begins with drawing the main moral to be learned from these considerations:

> With this new picture of theories in mind, we can distinguish between two epistemic attitudes we can take up toward a theory. We can assert it to be true …, and call for belief; or we can simply assert its empirical adequacy, calling for acceptance as such. ([1980], p. 69)

Constructive empiricism is thus shown to be a possible position in philosophy of science. Hereupon follows a much-cited epistemological remark, the only one in the whole chapter:

> In either case we stick our neck out: empirical adequacy goes far beyond what we can know at any given time. … Nevertheless there is a difference: the assertion of empirical adequacy is a great deal weaker than the assertion of truth, and the restraint to acceptance delivers us from metaphysics. (Ibid.)

With which the chapter ends.

4 Van Fraassen's (early) voluntarism

Notwithstanding the fact that one finds suspiciously little on epistemological matters in SI,[3] van Fraassen was generally taken to argue that it is irrational to be a scientific realist; i.e. the conclusion (UD) was ascribed to him. It is now generally accepted that he cannot hold such a position anymore, since that would be incoherent given the voluntarist position in epistemology that he developed since then. In its most succinct (and incomplete) formulation, voluntarism implies that "rationality is only bridled irrationality" (van Fraassen [1989], p. 172): any behaviour that does not transgress the boundaries of logic – that does not make one incoherent – is not irrational. Rationality is all about permission, not about obligation. While one is not obligated to become a scientific realist (so much is clearly proven by the arguments in SI), the voluntarist will have to admit that it is nevertheless permissible to be one. Believing in the truth of scientific theories, even if compelling reasons are lacking, does not necessarily make

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[3] The only section really concerned with epistemological questions is section 4.1 (pp. 71-73), and in it the reader is warned by van Fraassen that "I must postpone to another occasion a treatise on epistemology" ([1980], p. 71). In a recent look-back, van Fraassen writes: "In *The Scientific Image* it was hard to stay clear of epistemology, though I tried." ([2001], 164)

someone incoherent. By adopting voluntarism the argument from UD can be of no more avail to an anti-realist. Hence, the general impression is one of an attenuation of van Fraassen's position. André Kukla sums up the situation as follows:

> In 1980, constructive empiricism is presented as a conclusion that follows from arguments that ought to persuade any rational person to abandon realism. In a 1985 reply to his critics, van Fraassen equivocates between the relatively strong claims of 1980 and the permissive turn in his epistemology that is to come. … By 1989, van Fraassen explicitly concedes that it isn't irrational to be a realist. His claim is only that it isn't irrational to be an antirealist. ([Kukla 1998], p. 151)

I strongly believe this view of the matter to be untenable. Nowhere in SI does one find an explicit ascription of irrationality to realists; it follows that Kukla's claim about the arguments in SI must be based on an implicit reconstruction of something like the argument from UD. In the course of my further assessment of such a reconstruction it will emerge that if one wants to impute an epistemological position to van Fraassen in 1980, based on the scattered remarks on epistemology in SI, it has to be an embryonic form of something like his later voluntarism.

The most important clue for ascribing to van Fraassen an analogue principle to the epistemological premise (2), which is needed to get the argument from UD from the ground, is to be taken from the last sentence of chapter 3 of SI (which I already quoted at the end of my section 3). In it van Fraassen claims that the important distinction between believing in the empirical adequacy of a theory and in its truth is that the former is a weaker attitude. Coupled with the presumed conclusion that any rational person should be a constructive empiricist, this gives an epistemological principle that states that one should only believe the weaker claim of two claims compatible with all empirical information. (The threat of arbitrarily imposing a border immediately looms large, for why should one then still believe in the empirical adequacy with respect to all observable events, rather then restricting belief to the set of observed events…) Some people

apparently have taken the following much-cited remark of van Fraassen as corroborating the ascription of this principle to him:[4]

> There does remain the fact that even in endorsing a simple perceptual judgement, and certainly in accepting any theory as empirically adequate, I am sticking my neck out. There is no argument there for belief in the truth of the accepted theories, since it is not an epistemological principle that one might as well hang for a sheep as for a lamb. ([1980], p. 72)

Obviously, the claim here is weaker than the presumed epistemological principle (although it is consistent with it); even more importantly, the passage quoted continues as follows:

> A complete epistemology must carefully investigate the conditions of rationality for acceptance of conclusions that go beyond one's evidence. What it cannot provide, I think (and to that extent I am a sceptic), is rationally compelling forces upon these epistemic decisions. ([1980], pp. 72-73)

Here we find van Fraassen explicitly denying that there can be epistemic rules that force (dis)belief on us! The paragraph from which this quote is taken is dedicated to dispelling the suspicion that he is arbitrarily, maybe even incoherently, endorsing a rule that in the right situation (e.g. with all relevant evidence in) would compel one to belief in empirical adequacy, while at the same time denying such a rule the force to compel full belief in the theory. I take his answer to be that since belief in empirical adequacy is never *forced* on us, neither can full belief be forced on an agnostic.

Note how far away we have moved from "arguments that ought to persuade any rational person to abandon realism." On the contrary, we find van Fraassen claiming that

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[4] André Kukla, at least, locates this quote as the source for ascriptions of such a principle to van Fraassen ([1998], p. 94).

no epistemology can be governed by rules that *compel* assent, hereby giving us a succinct preview of the epistemological position to be further explicated in his later writings. However, these latter developments have been taken to imply that van Fraassen has come to see the realism debate as irresolvable (cf. e.g. Kukla [1998]), and one might wonder whether ascribing this position to him in 1980 wouldn't make the debate a non-starter from the beginning – apparently denying SI much of its perceived impact. (One important reason why the argument from UD has always been attributed to van Fraassen, is undoubtedly that many philosophers thought it *should* have been there: granted that acceptance without full belief is possible, this attitude would only be defensible if (UD) would have been established.) However, since SI van Fraassen has been stressing that constructive empiricism should be seen as a view on science, not as an epistemological position: it doesn't tell us what we should (dis)believe, but it gives an answer to the question "what is science?" by indicating the criteria that determine what counts as success in science. Moreover, this view of what the debate on scientific realism is about is not a retraction on van Fraassen's part, but clearly lies at the heart of SI. When discussing different possible formulations of scientific realism, van Fraassen explicitly favours formulations that focus "on the understanding of the theories without reference to reasons for belief" ([1980], p. 7), a preference clearly reflected in his final formulations of scientific realism and constructive empiricism. Of course, this immediately brings up the further question why one would adopt one view rather than another, and van Fraassen clearly thinks that SI does establish that constructive empiricism is the best view. It is therefore time to round up my discussion of the arguments in SI by providing what I take to be the most interesting way to see what they establish when seen in their complete argumentative context.

I take it that the positive argument for constructive empiricism, as expounded in SI, consists of the following four components.

(1) One can distinguish between two different attitudes one can take up towards a theory: accepting it and believing it. This is established by the arguments showing that it is always

possible to isolate the empirical content of a theory. *Constructive empiricism is a possible position.*

(2) There are never compelling reasons to opt for full belief in scientific theories. This is established by van Fraassen's critical discussions (in chapter 2 of SI) of rules that could be taken to provide such reasons (such as inference to the best explanation), coupled with his more general denial of the possibility of epistemologically compelling rules (which would threaten to make the decision to accept without full belief arbitrary or even incoherent). (Incidentally, when summarizing the result of his discussions in the second chapter of SI, van Fraassen states: "I resisted such inference [to the truth of a theory], arguing in effect that when the theory has implications about what is not observable, the evidence does not warrant the conclusion that it is true." ([1980], p. 71) This could be taken as throwing doubts on my ascription of a proto-voluntarism to van Fraassen in 1980, and might be given a strong reading as claiming that it is irrational to believe in the truth of theories. However, nowhere does van Fraassen indicate how he intends the term "warrant" to be understood, whereas his claim that there can be no compelling forces upon epistemic decisions about claims going beyond one's evidence is unambiguous and inconsistent with such a strong reading. Moreover, when we look at the places where he discusses such purported compelling reasons, he always opts for a cautious reading of what his discussions establish.[5]) *Constructive empiricism thus wouldn't make science – as conceived by it – an eminently arbitrary or even irrational practice.*

(3) One can understand all aspects of scientific methodology perfectly well from the viewpoint that the main criterion for scientific success is empirical adequacy (this is mainly established in chapters 4 and 5 of SI, although some of the discussions in chapter 2 are relevant as well). The distinction between acceptance and belief can be put to good use in making sense of scientific practice. *Hence, constructive empiricism is an attractive*

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[5] "[W]e can still say that there is no need to believe good theories to be true" ([1980], pp. 11-12); "I shall just conclude that it is, on the face of it, not irrational to commit oneself only to a search for theories that are empirically adequate" ([ibid.], p. 19); "Merely following the ordinary patterns of inference in science does not obviously and automatically makes realists of us all." ([ibid.], p. 23); "The [realist's] decision to leap is subject to rational scrutiny, but not *dictated* by reason and evidence." ([ibid.], p. 37)

position, which "makes better sense of science, and of scientific activity, than realism does" ([1980], p. 73)*.*

(4) *For an empiricist, constructive empiricism is also the best view of science:* it not only makes sense of science, "it does so without inflationary metaphysics." (Ibid.) Of course, a realist will not see the metaphysics accrued to believing scientific theories as inflationary, and van Fraassen is here stating what it means for him to be an empiricist, rather than providing us with an argument to become one. As he is an empiricist, and as he accordingly can think that everybody should be one, he can proclaim that the best view on science is constructive empiricism. In the end, this comes down to a deeply value-laden judgement. I will come back to this point in Section 5.2.

It is clear that the distinction between accepting and believing a theory is central to the constructive empiricist view on science. As I explained, van Fraassen's discussions of empirical equivalence are used to show this to be a viable distinction. Some could argue, however, that the only argument *why* one would want to make such a distinction seems to lie in the fact that it makes possible constructive empiricism. Accordingly, it can be claimed (as has been done by Horwich [1991]) that such a distinction remains utterly artificial if no independent argument for the relevance of this distinction can be provided. It is important to take up this challenge since Horwich also suggests that the only reason for distinguishing between pragmatic and epistemic virtues lies in the argument from underdetermination ([1991], pp. 1,11).

5   Acceptance vs. Belief

In a number of publications following upon SI van Fraassen develops a simple, but at first sight effective line of argument to show that we *need* to distinguish between the attitudes of accepting and believing a theory ([1983a,1983b,1983c,1985]). The position of SI thereby seems to be considerably strengthened. It will be seen, however, that the argument fails to establish its intended goal. Nevertheless, this analysis will provide us with a better

understanding of how acceptance and belief are intimately related for a constructive empiricist.

5.1   The Simple Argument

Van Fraassen's simple argument goes as follows.

(1) Scientific theories are accepted because they have certain virtues. Nothing is presupposed about what such acceptance implies; this will depend on the nature of these virtues.

(2) We can distinguish at least two classes of virtues, based on a quick glance of how theories are assessed. Theories are praised because they provide us with information (e.g. they enable explanations), and they are praised because there is a considerable chance that they are true. Hence theories can have informational and confirmational virtues.

(3) Although up to this point we did not prejudge the matter concerning the relationship between these two classes of virtues, we will have to conclude that informational virtues cannot be confirmational virtues. Consider a theory T and a part of it, T': T will always score at least as good on informational virtues. However, a part of a theory can never be less likely to be true than the theory itself (this is basic logical point: since T is logically stronger it has less models, implying that it has more ways to be false). Hence, T cannot score better on confirmational virtues. Informational virtues are not always at the same time confirmational virtues.

(4) Since informational virtues are reasons for accepting a theory, reasons for acceptance are not always reasons for belief.

(5) Acceptance is not belief, since both notions are governed by a different logic.

Since it is a basic logical point that strength and probability of being true pull in different directions, it follows that reasons for accepting a theory cannot always be reasons for believing it. Before pointing out why this argument fails, let me quickly dispel the doubt that it might implicitly be based on an argument from underdetermination. We are only comparing the semantic relations between one theory and some part of it. In no way does

this show how we should compare different theories with respect to the credence they can rationally be accorded; neither does it show that we cannot or need not believe in more than the empirical adequacy of a theory – notice that empirical adequacy is even nowhere mentioned in the argument. We are only investigating the logic of the notions acceptance and belief, not the relation between belief and possible evidential grounds.

The quick way to see that the argument must fail to establish its conclusion that acceptance and belief are genuinely distinct, is by noticing that the same tension holds for any belief. We want our beliefs to be true, but we can only be sure that we shun error at the expense of foregoing all beliefs in non-tautological propositions. But we don't want to believe only tautologies, we do want (true) beliefs that give us information that we value. This means, however, that we will always have reasons for believing a proposition – reasons having to do with the kind of information the proposition contains, and reasons having to do with the "risk quotient" we set for these kind of beliefs – that cannot be *ipso facto* reasons for believing it to be true. The Gordian knot can only be cut by a value-driven decision. This is of course a classical pragmatist point, with which van Fraassen completely agrees, since it lies at the core of his voluntarism. But then, what becomes of the argument that belief and acceptance should be distinguished because they are governed by a different logic? When confronted with a similar point made by Paul Teller, van Fraassen responds as follows (the "ulterior motives" referred to are the reasons for holding a belief that cannot be reasons for believing it to be true):

> A belief held for ulterior motives is still a belief. It does not become acceptance instead of belief that way. The distinction between what a person believes and what s/he merely accepts is not made on the basis of why s/he has that attitude but on the basis of what that attitude is. (van Fraassen [2001], p.)

This is clearly a retreat on van Fraassen's part. All that he claims here is that acceptance and belief are distinct attitudes because they *are*, no longer because we can show that they have to be. I will not further go into the question whether and how this distinction could

be further fleshed out, but I would like to submit without further argument that to deny the reality of any such distinction would impoverish our epistemic life to such an extent as to become totally uninteresting from a philosophical point of view. To spell out the exact nature of such a distinction within our epistemic attitudes, however remains one of the open projects for constructive empiricism.

5.2   Belief Reconsidered

For the moment, let us without further argument accept with van Fraassen that belief and acceptance are genuinely distinct attitudes, and investigate what we can learn from the failure of his simple argument. Remember that the argument failed because it had to be conceded that belief in any proposition or theory is subject to the same tension which van Fraassen wanted to ascribe to acceptance. Obviously this has consequences for how we can understand the nature of epistemic virtues and their relation with pragmatic virtues.

Let me begin with making a crude distinction between both kinds of virtues, as van Fraassen wants to understand them. When scientists want to assess whether the aim of science has been achieved (to some extent) for a particular theory, they will look at how it scores on epistemic virtues. When they assess whether that theory has other valuable characteristics they will have a look at its pragmatic virtues.

How can scientists ever come to have a belief about a scientific theory? As the above analysis showed, this will always raise the question which beliefs they value – if there were no valuable non-tautological beliefs, the only sensible thing to do would be to believe only tautologies. This pragmatic value-driven decision then sets the stakes for the next question: do they have enough evidence for these beliefs – i.e. how does the theory score on virtues relevant for the epistemic goal set by that decision. [6]

The assessment whether a theory has other valuable characteristics is *prima facie* not susceptible to the same tension: having a low logical strength is not valued in itself – for the decision made on the ground of pragmatic virtues, there is not the same risk of

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[6] The answer to the question: "when do we consider the evidence in hand to be good enough"? then of course requires a further value-driven decision.

being in error as is the case with purely epistemic decisions. Whereas belief about a theory will always be the result of both a value-driven decision and the theory's epistemic virtues, acceptance of the theory will be the result of this belief and the theory's pragmatic virtues. We can see that acceptance is only indirectly subject to the tension that van Fraassen ascribed to it in his simple argument. This argument apparently had things upside down.

The insight that it is primarily belief that is subject to this tension can help us to better discern the exact nature of van Fraassen's constructive empiricism. Any scientific decision to accept a theory, and hence to believe that it achieves (to a certain extent) its epistemic goal, involves a prior decision as to what beliefs are valuable. The constructive empiricist view on science implies that while individual scientists may value many kinds of beliefs, as scientists – i.e. as persons engaged in a common practice – they *must* value belief in the empirical adequacy of theories as an overriding goal. By subscribing to the correctness of the constructive empiricist view on science, one adopts the view that science is driven by empiricist sentiments.[7] This also implies that the epistemic virtues will have to be empirical virtues – and empirical virtues only.

By now it should be clear why empiricists can take such a delight in the constructive empiricist view on science. It enables them to portray scientific activity as the paradigm of what they take to be a sensible epistemic enterprise. They can see their own values as underlying science. Of course, any individual, be it a scientist or a philosopher, can always opt for a stronger belief than the belief that a theory is empirically adequate, but the empiricist will have "disdain"[8] for this decision. These beliefs are "not additionally vulnerable,"[9] and hence not valuable from his perspective. A

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[7] We can extract what van Fraassen takes the core nature of these sentiments to be from various places in his writings, e.g.: "All our factual beliefs are to be given over as hostages to fortune, to the fortunes of future empirical evidence, and given up when they fail…" (van Fraassen [2002] , p. 63). The context in which this is proclaimed is important. This is not an empiricist teaching to scientists how they should behave; this is an empiricist trying to learn from science what his proper epistemic attitudes should be.

[8] van Fraassen [1985], p. 252.

[9] van Fraassen [1985], p. 255.

realist of course will retort that these beliefs are valuable, since they allow us to have a more satisfying world picture, one that is more unified etc. It is precisely at this point that the debates become irresolvable. Irresolvable, but not senseless, I would urge – unless one would want to forego all debates concerning values. At this point, however, we are entering a different debate, one concerning the question what we should believe, rather than what the nature of science is. [10]

5.3   Epistemic vs. Pragmatic Virtues

Let me now answer a suspicion that might have arisen with some readers, and that was explicitly raised by Horwich ([1991], p. 1), who claims that "the only possible rationale" for distinguishing epistemic and pragmatic virtues "is based on underdetermination of theory by data". Is the distinction between acceptance and belief hence not predicated upon the acceptance of the argument from UD? Have a look at the following typical statement by van Fraassen:

> … pragmatic virtues do not give us any reason over and above the evidence of the empirical data, for thinking that a theory is true. ([1980], p. 4)

If this were indeed accepted by van Fraassen on the ground of UD, this would lead to one of these regresses that are so typical for the debates on scientific realism. To reach the conclusion (UD) from the premises (1) and (2) (see section 2), one would need to have this distinction between pragmatic and epistemic virtues already in place (to argue that non-

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[10] Is it possible to come up with one right view on the aim of science? Ironically, van Fraassen, the arch-nominalist, seems to be trapped in an essentialist position with respect to science. (For the centrality of nominalism to van Fraassen's thinking, witness the discussion between Ladyman [2000], and Monton and van Fraassen [2003].) This issue hangs closely together with the separation that van Fraassen has to make between the intentionality of a scientist participating in the enterprise of science, and the intention of any individual scientist (see Rosen [1994], and van Fraassen [1994]). One could also question the unitary view on science that van Fraassen seems to endorse.

empirical virtues do not break the epistemic ties between empirically equivalent theories). But one cannot use the conclusion UD in defence of one of its premises! If this were how things stand, van Fraassen would be guilty of the same kind of question begging that he so skilfully laid bare in the realist's arguments.

In SI, van Fraassen mainly concentrates on arguing that explanatoriness is a pragmatic virtue, in the sense explained in the quote above. As I see it, he uses two tactics to this end, one mainly rhetorical, the other substantial. The rhetorical tactic, no doubt very effective, consists in placing the onus on the realist. Why would the fact that a theory offers (very) good explanations give us reason, over and above the evidence of the empirical data, for thinking that a theory is true (as the realist would have it)? What would that connection precisely look like (which kind of explanations would qualify, etc.)? Besides these scattered remarks, van Fraassen also develops a positive argument. This consists in developing an alternative account of explanation which supposedly does full justice to scientific practice. This account implies that explanation is not some irreducible goal, separated from considerations of empirical adequacy and strength. It is a goal, but not one overriding the demand of empirical adequacy. The success of an explanation is always the success of an empirically adequate and informative description ([1980], p. 157). This account of explanation is presented as descriptive of good scientific practice. On van Fraassen's view, it is thus science itself that shows us that pragmatic virtues are distinct from epistemic ones, in the sense that the latter are primary. Of course, one can quarrel about the adequacy of van Fraassen's account of explanation, but since here I am mainly interested in laying bare his argumentative strategy, I will not enter into these debates.

6   In Conclusion

It has already been stressed sufficiently that constructive empiricism is not presented as an epistemological position. It does not tell us under what conditions we would be justified in believing certain scientific claims. There is, however, also a truly constructive part

about the position. It tries to formulate answers to questions such as: how do scientists construct their theories, why do they impose certain demands on these theories, which role does the demand for explanations play in their activity…? In conclusion, constructive empiricism should primarily be seen as a view on methodology. Hence, it should come as no surprise that the argument from UD nowhere figures among van Fraassen's arguments for his position.

The reason why so many authors have nevertheless succumbed to the temptation of ascribing the argument to van Fraassen, should have been clarified by the reconstruction offered here. The argument from UD and van Fraassen's arguments in SI do share a common premise, the main difference being that van Fraassen does not directly build an ethics of belief on that premise. It is connected with such an ethics, but, as has become clear, in a much more subtle way.

It follows that laying bare problems with the argument from UD turns out to be a less promising tactic to use for realists. After concluding that the argument fails to deliver its promised goods, Samir Okasha, e.g., claims "a victory by default for the scientific realist" (Okasha [2002], p. 306). This default is clearly undercut, but that is not necessarily all. My reconstruction of van Fraassen's argument also points the way to other possible anti-realist positions. It should be remarked, for instance, that no heavy weight was put on the precise nature of the distinction between sub-structures and "theoretical" super-structures. One can thus imagine someone proposing an anti-realist position more or less along the lines presented here, but without accepting van Fraassen's demarcation of the observable. There is thus a broader class of anti-realisms that does not need the argument from UD. Anti-realists should rejoice over this freedom.

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