*Unnatural Attitudes: Realist and Instrumentalist Attachments to Science**

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Let us assume that a physical world picture has been discovered which satisfies all claims that can be made upon it, and which, therefore, can represent completely all natural laws discovered empirically. Then it can in no way be proved that such a picture in any way represents 'actual' Nature. However, there is a converse to this . . . Exactly similarly . . . [it] . . . cannot be disproved.

. . . Here yawns an enormous vacuum, into which no science can penetrate; and the filling up of this vacuum is the work, not of pure, but of practical reason; it is the work of a healthy view of the world. (Max Planck, 'New Paths of Physical Knowledge'.)

If you tune your television set to a channel that receives no broadcast signal, and you then turn the contrast up to 'bright', you will see a changing pattern of scattered dots and hear a static-like accompaniment. To a large extent (or so we are told) what you are seeing and hearing is background radiation left over from the very creation of the universe, the 'big bang' of some fifteen billion years ago. This opportunity to eavesdrop on the big bang in our own homes is a dramatic illustration of just how thoroughly entrenched the success of science is in everyday life and culture. Wherever we turn, technological applications of fundamental branches of physics, chemistry, and biology are on display. This conspicuous success leaves little room for anything other than a common-sense acceptance of the world of science. In the little room that is left, however, the philosophical discussion of realism continues, and with a curious result; for in this philosophical discussion, it is realism that has been put on the defensive. Indeed, as we shall see, the realist programme has degenerated by now to the point where it is quite beyond salvage. A token of this degeneration is that there are altogether too many realisms. It is as though by splitting into a confusing array of types and kinds, realism has hoped that some one variety might yet escape extinction. I

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shall survey the debate, and some of these realisms, below. Here I would just point out the obvious; that in so far as the successes of science mount while realism continues to decline we must conclude that scientific success lends no support to realism. Since it is unlikely to support anti-realism, we have some reason to suspect that the philosophical debate over realism does not concern issues that can be settled by developments in the sciences, no matter how successful science may be. Further, since that success grounds a culture of acceptance for science and its entities, we have reason to believe that the existence of those entities is also not actually the issue that concerns realism. *A fortiori*, it is not the issue that concerns anti-realism either; nor, I might add, is anti-realism the winner in the philosophical debate that realism has lost.

1. *Realism*

I take realism and anti-realism, to begin with, as expressing attitudes toward science, and in particular attitudes towards the significance of science—its proper interpretation and understanding. At the most primitive level the divide between realism and anti-realism expresses the dualism of the inner and outer, and addresses a felt need for grounding, in one place or another, in this shaky universe. For realism, science is ab*out* something; something *out there*, 'external' and (largely) independent of us. The traditional conjunction of externality and independence leads to the realist picture of an objective, external world; what I shall call the *World*. According to realism, science is about *that*. Being about the *World* is what gives significance to science. That is, on the semantic side, we are to understand scientific claims as claims about the *World*. Thus, realism adopts a special interpretative stance towards the language of science, the stance traditionally assumed by a correspondence theory of truth and referential semantics, provided the referents are (in general) taken as Real; i.e. as elements of the *World*. There is also a teleological side, for the significance that realism attaches to science lends itself to the view that what science does (in being about the *World*) is exactly what it aims to do. Thus we get the realist slogan that science aims at the truth, with the realist connection between truth and the *World* being understood.

In drawing this sketch of realism I have put together various elements that some recent realist writings have tried to keep apart. Thus Devitt (1984) tries hard to separate realism from any semantic doctrine whatsoever, although he recognizes that realism and a correspondence theory of truth (via a causal theory of Real referents) make an especially well-suited pair. Ellis (1985) pushes this possibility to what is perhaps its extreme in weaving traditional realist strands around a Piercean conception of truth, as the limit of reasonable belief. As a result, Ellis himself recognizes that the position he adopts might fairly be called a sort of idealism (ibid., p. 70).

Ellis's attempt to disentangle realism from the correspondence theory of truth, however, moves right to the heart of the classical objections to realism—the obscurity of the correspondence relation and the inscrutability of realist-style reference. As Ellis says:

> We can investigate nature and develop a theoretical understanding of the world, but we cannot compare what we think we know with the truth to see how well we are doing (ibid., p. 69).

The problem is one of access. The correspondence relation would map true statements (let us say) to states of affairs (let us say). But if we want to compare a statement with its corresponding state of affairs, how do we proceed? How do we get at a state of affairs when that is to be understood, realist-style, as a feature of the *World*? A similar question comes up if we move to reference and try to establish truth conditions compositionally, for there again, what the realist needs by way of the referent for a term is some entity in the *World*. The difficulty is that whatever we observe, or, more generously, whatever we causally interact with, is certainly not independent of us. This is the problem of *reciprocity*. Moreover, whatever information we retrieve from such interaction is, prima facie, information about interacted-with things. This is the problem of *contamination*. How then, faced with reciprocity and contamination, can one get entities both independent and objective? Clearly the realist has no direct access to his *World*, and could at best hope to get at it only by means of inferences and constructions. As Kuhn puts it:

> There is, I think, no theory-independent way to reconstruct phrases like 'really true'; the notion of a match between the ontology of a theory and its 'real' counterpart in nature now seems to me illusive in principle (Kuhn, 1970, p. 206).

There is an historical irony in the realist need to move to inference and constructions, in his need to 'entheorize' the *World*. For a traditional realist charge against his idealist foes has been precisely over their need to fall back on constructions; since, given the idealist reliance on a coherence theory of truth, such constructions seemed to leave them open to the suspicion that, for them, anything (coherent) goes. This charge relies on the realist's sense that, absent determinate inputs from the *World*, our constructions and theoretically grounded inferences are fatally underdetermined. But the difficulty posed by the problems of reciprocity and contamination leaves realism no better off than idealism. For both of them require constructions and inferences based on nothing outside ourselves and the raw materials of experience. If underdetermination taints the idealist project, there is certainly no reason to think that the realist constructions of his *World* are any better off.

In rehearsing the problems of reciprocity and contamination, one might suggest that I have been trying to stick the realist with an utterly naïve view, and then blaming him for holding it. For the interactive features involved in

getting at truth or reference may be thought only to pose difficulties for the naïve idea of direct or unmediated access to the *World*, and surely nothing in the realist creed requires anything so naïve. Thus, realism may well seek to get at the idea of correspondence (or Real reference) indirectly—perhaps by reasoning along the following lines. Since one cannot directly observe the existence of correspondence relations one end of which, so to speak, touches an observer-independent realm, perhaps we can, nevertheless, have indirect evidence for such correspondence. Perhaps, for example, we could have inductive evidence. Since induction, in the narrow sense, moves from the particular to the general, the idea might be to move inductively from particular cases of correspondence with the *World* to the general realist claim that that is what science is about. But, of course, this sort of inductive approach to realism makes no sense. For the issues raised by the classical objections challenge the realist idea of correspondence in each *particular* case, thereby leaving no foothold for this sort of inductive leap.

I believe that the recent literature in support of realism has been sensitive to these considerations. That is, I think the literature acknowledges the problem of access to the *World* as central for realism, and grants that we need something more sophisticated than either direct access or narrow inductive inference to get at the problem. What the literature seems to have settled on is a form of abductive inference; namely, inference to the best explanation, which yields *the explanationist defence of realism.*[1] This defence begins by calling attention to the instrumental success of science, i.e. to the sort of practical success one can observe in our everyday lives and culture. We are then asked to account for this, and are told that only realism can give a good explanation for why the conscientious practice of science leads to such instrumental success.

Before examining the pull of this line of argument, and what it pulls us towards, I want to emphasize that it is an argument critically sensitive to scale. What I mean is that the plausibility of the explanandum (that the conscientious practice of science leads to abundant instrumental success) is an artefact of our historical perspective. If, for example, we could examine the myriad attempts in laboratories around the world just (literally) yesterday to turn basic science to the production of a useful instrument, then, I think, we would find failure on a massive scale, and certainly not any overall success. Further, if we study the application of science over time to a reasonably complex technology, then, even when success appears at the end of the road, it generally crowns a long history of frustration and failure. For the application of science involves an enormous amount of plain old trial and

[1] See Boyd (1984) for a review of the realist state of the art. (My sketch here of the explanationist defence does not place as much stress on the methodological level as does Boyd's own strategy.) The discussion below concentrates on certain metaphilosophical problems with the explanationist defence. But the conjunction of Fine (1984*a*) with Laudan (1984) and van Fraassen (1980 and 1985) shows how deeply in trouble virtually every particular realist explanatory effort is. Boyd (1985) responds to some of this.

error; hence, it always entails an enormous amount of error. I think a reasonable historical picture would be to draw each success as sitting on top of a great mountain of failures. In inviting us to explain the intrumental success of science, the realist performs a sort of conjuring trick, and directs our line of sight only along the successful tips of the mountains of failures. The scientific landscape, I would urge, is considerably more varied than that to which the realist would have us attend.

But let us play along. Then, I think, the pull of the explanationist strategy is strong and intuitive. Consider the following analogy. Suppose we had a machine, running 'on-line', that provided the premisses for valid arguments whose conclusions checked out as instrumentally correct time and time again (the 'abundant' instrumental successes of science). Puzzled and intrigued, it would certainly be appropriate to look for a rational account of what is going on here. Surely a good account (perhaps, one might slyly insinuate, the very best) would be to infer that the premisses turned out by the machine are true (or, at any rate, largely true), for only that could explain the repeated generation of correct conclusions. The explanationist defence of realism is much the same, for the realist contends that only the truth (or approximate truth) of science could explain its generation of instrumental successes over time, and feels entitled to infer from this explanation that science is true—or largely so. As we shall see, the cogency of the inference here can plausibly be challenged. Nevertheless, I hope we can also see that the form of the argument has a strong, almost primitive, pull. If we return to the analogy, however, we will find that it does not necessarily pull in the direction of realism, for, if the machine generates premisses that lead only to *instrumentally* correct conclusions, we do not need to move to the full truth of the premisses to account for the outcomes. It would certainly be sufficient just to suppose that the premisses themselves are reliable merely with regard to their instrumental consequences. Similarly, if it is the instrumental success of science that we think wants explaining, then it seems that we require nothing more than the instrumental reliability of science in order to carry the explanation off. Indeed, anything more than that would be doing no explanatory work. Thus, the explanationist strategy for defending realism does not seem to pull all the way to realism itself. What it does pull us to, ironically again, is another of the enemies of realism; namely, instrumentalism.

Have I rigged the game? In directing the arrow of explanation at the instrumental success of science have I perhaps somehow excluded full-blown realism as a decent explanatory option, and already put instrumentalism in the winner's box? Perhaps that is so, but then so much the worse for realism, since I do not think that there is any better alternative here. For what we can be called on to explain in the context of a debate over the merits of realism cannot already suppose those very elements of realism that are under debate. For instance, one could not be called upon to explain how

science enables us to get at the truth about the *World*, since whether it does that or not is part of what we are trying to establish. Hence, the explanationist defence is constrained by the requirement that the explananda must be acceptable, if not to all parties in the debate, then at least to disinterested parties. That requirement is certainly met by taking as explanandum the instrumental success of science. One could even enlarge the notion of instrumental success so as to include all the predictive and explanatory success of science in the observable realm. So enlarged, the explanationist strategy will still lead to instrumentalism, and not to realism.

I think there is a structural reason why explanationist arguments for realism, in general, pull in the direction of instrumentalism. Given instrumentalism's pragmatic roots (it is, after all, John Dewey's term), it should be no surprise to find that the structure has to do with the connection between realism and pragmatism, and in particular with the pragmatic conception of truth. That conception, to put it indelicately, confounds truth with reliability. Its varied and distinct refinements all involve the ideas of a human community, human inquiry, and the variety of life tasks that people can do more or less successfully. Thus, the pragmatic conception of truth turns from the outer to the inner, and runs counter to the realist idea of grounding truth in a correspondence with the *World*. But not completely. For the realist must at least allow that, generally speaking, truth does lead to reliability. For instance, in the context of the explanationist defence, the realist offers the truth of a theoretical story in order to explain its success at a certain range of tasks. If this offering is any good at all, the realist must then allow for some intermediate connection between the truth of the theory and success in its practice. The intermediary here is precisely the pragmatist's reliability. Let us therefore replace the realist's 'truth' with the pragmatic conception, framed appropriately in terms of reliability. Then, if the realist has given a good explanatory account to begin with, we get from this pragmatic substitution a good instrumentalist account of the same phenomena. Indeed, the instrumentalist account we obtain in this way is precisely the intermediate account that the realist has himself been using. Since no further work is done by ascending from that intermediary to the realist's 'truth', the instrumental explanation has to be counted as better than the realist one. In this way the realist argument leads to instrumentalism. One might summarize the strands of this discussion in the following way:

Metatheorem 1. If the phenomena to be explained are not realist-laden, then to every good realist explanation there corresponds a better instrumentalist one.

Proof: In the proffered realist explanation, replace the realist conception of truth by the pragmatic conception. The result, framed in terms of reliability, will be the better instrumentalist explanation. (NB, the antecedent of this theorem can be discharged, for otherwise the realist begs the question.)

We have already seen how this metatheorem applies to undo the standard explanatory defence of realism. It might be illuminating to try it out against an interesting variant of that defence.[2] This variant focuses on a particular pattern of explanation that is sometimes successful in scientific practice; namely, explanation by reference to latent structure. One might think of the caloric theory of heat as involving such a structure, or the atomic/molecular theory of matter, or the genetic theory of inheritance. It sometimes happens that for long historical periods such structures show resilience and fertility, persisting in a recognizable way through sequences of changes and growth. When this occurs, and carries through up to the present, we come to feel that there must be something right about the core structure that accounts not just for its survival value but also for its ability, so to speak, to extend beyond itself and lead us towards successful refinements. Perhaps this is what grounds our current belief in atoms and molecules and genes (although not in caloric which, after a long and successful run, just petered out). Thus, one might suggest that when we are fortunate enough to come across a resilient and fertile structure, we can only explain (or best explain) it by supposing that the structure has captured some elements of the truth about the *World*; in particular, that the working entities postulated by the structure are actually denizens of the *World*.

The central way in which this explanatory argument differs from the one over instrumental success (in the broad sense that includes predictive success) is that it adds on to the explanandum the capacity of the central structure to change and grow progressively over time. What the realist says is that this ability for progressive growth is due to the fact that the core structure is approximately true (of the *World*, of course). That is why tinkering with that structure, pruning and extending it, shows a history of instrumental success. I do not think this realist account is worth much as an explanation,[3] but, since I only want to illustrate how the metatheorem works on it, let us pretend that it is viable. In that case, the phenomenon to be explained is the capacity the structure exhibits for progressive growth. Since we can understand this without including any presuppositions of realism, we ought to be able to apply our pragmatist strategy. Where the realist says the structure is approximately true, read that instead as affirming, by and large, that the structure can be reliably employed. We could expect to rely on it, therefore, as a basis for organizing further theoretical refinements. Hence, overall reliability functions well enough, in place of truth, to explain the ability of a structure to 'extend beyond itself'. Indeed, one can see that truth would work in an explanatory way only via the intermediary of reliability. After all, it is only the belief that one can *count on* the structure that backs up the strategy of tinkering and refining it, and the 'count on' here need pull no farther than reliability, in order to support the strategy. The

[2] This variant is presented by McMullin (1984), who also traces its recent history.

[3] See Fine (1984*a*).

proof schema of the metatheorem thus applies, and we get not merely a competing instrumentalist explanation for the phenomenon, but actually a better explanation than the realist had to offer.[4]

As we shall see, the case for realism is actually in a much worse position than the metatheorem suggests. I shall return to it below (section 3), so instead of considering possible realist rejoinders at this point, let us turn instead to instrumentalism.

2. *Instrumentalism*

I shall sketch an ideal type of instrumentalism by way of contrast with realism, and then indicate some current anti-realisms that approximate to this ideal.[5] The instrumentalism I sketch here seems to me attractive and strong, unlike the ugly and frail character featured in realist polemics.

Firstly, I take both realism and instrumentalism to be basically pro-attitudes towards the scientific enterprise in general. They accept science, endorse it and think that it is grand, and act neither as general critic nor as sceptic. This is not to say that realism (or instrumentalism) will not find some blemishes here or there to be concerned over, nor that they unthinkingly endorse whatever it is that science may say or do. It is just that these 'isms' are basically *for* science, believing that much of it is acceptable—and accepting very much of it as well. The point here for instrumentalism is simply that it should not be thought of as being in any way anti-science.

Secondly, I take both realism and instrumentalism to be non-reductive enterprises. In general, they do not endorse Russell's dictum to substitute constructions for inferred entities. Nor do they want, otherwise, to analyse away or eliminate the various fruits of scientific labour. They do not seek to replace physical objects by phenomenal constructions, to make everything mental (nor, necessarily, physical either), to redefine scientific properties by means of test/response pairs, nor to reduce causes to mere regularities of experience, and so on. Thus, phenomenalism, idealism, operationalism, and naïve empiricism may all be anti-realist, but they are not otherwise aligned with instrumentalism. Unless there are good scientific reasons in particular cases, neither realism nor instrumentalism is in the business of advocating reductions.

Thirdly, realism divides from instrumentalism over the truth of theoretical statements and the reference of theoretical terms. As I have emphasized, realism goes for truth, and understands that as correspondence with the *World*. Thus, typically, theoretical terms refer, and their referents

[4] The reader can see a different realist angle on this topic, as well as why it does not work, in my discussion below in section 3 of an argument due to Michael Friedman.

[5] The scheme of comparisons below was suggested to me by the useful map employed by Blackburn (1984), p. 147.

are entities in the *World*. Instrumentalism satisfices; it goes for something less than truth. In general, instrumentalism would be satisfied with the instrumental reliability (in the broad sense) of a theoretical story, and it treats reference in this same reliabilist way. Thus, we might say that instrumentalism treats scientific stories as though they were true, just in so far as it relies on them, and their postulated entities, as useful guides for whatever practical and theoretical jobs may arise. To make the connection with pragmatism that I have exploited in the preceding section, we can say that instrumentalism treats scientific theories as true *in the pragmatic sense*, and admits truths concerning the existence of theoretical entities in that same sense as well. One sometimes hears this expressed by saying that instrumentalism treats theoretical entities as fictions. But, if genes (for example) are to be called fictions, then one must be clear that they are not *mere* fictions, nor is genetic theory to be assimilated to some amusing piece of scientific fantasy.

Fourthly (and lastly), the divide over truth extends to a division over goals. Both realism and instrumentalism agree that science has an overriding aim, although they disagree as to what that aim is. For realism, science aims at truth (about the *World*). For instrumentalism, science aims at instrumental (including observational) reliability.

In contemporary literature, the position that van Fraassen (1980) calls 'constructive empiricism' comes close to the instrumentalist ideal that I have sketched above. An important difference concerns the concept of truth, which van Fraassen takes in what he refers to as a 'literal' sense, and holds applicable to all areas of scientific investigation. He does not say much about this sense, but we may suppose that it would exclude the pragmatic conception of truth, and perhaps the realist correspondence conception as well.[6] Van Fraassen's view places a heavy, empiricist emphasis on observability (in the view of many, too heavy an emphasis) and on an epistemological criterion for warranted belief. Observability is to be naturalized. We let our scientific theories work out what, in fact, we can observe, and then we abide by that limit. Wherever it falls, however, it circumscribes the realm of belief. Outside that realm, that is, in the realm of the unobservable, we should remain agnostic. The vehicle for this epistemological criterion is an important distinction that van Fraassen draws between acceptance (of a theory) and belief. Acceptance is like a two-dimensional vector. One dimension is epistemic and involves belief, but that is only belief in the *empirical adequacy* of the theory—roughly, belief that the theory is adequate to represent faithfully all observable phenomena. The other dimension is pragmatic and, indeed, involves commitment to the entire theoretical framework, which

[6] Van Fraassen actually waffles a bit over this, sometimes adopting a very realist-sounding, correspondence language. See, for example, van Fraassen (1980), pp. 90 and 197. But here and below I shall take it that 'literal' is meant to exclude the correspondence metaphors of realism.

we are committed to using and working with (so to speak, *as though* it were true).

Thus, constructive empiricism involves an attitude of acceptance towards science, and is non-reductive. It is happy to apply the concept of truth even to the most esoteric theoretical structures, but holds that in so far as these structures go beyond the realm of the observable we are justified in remaining agnostic concerning their truth. Although we may have grounds for accepting such structures, the methodology of science and the reach of evidence only moves us to belief in truths about observables. Accordingly, constructive empiricism sets as the goal of science not truth, but theories that are empirically adequate. This restriction to empirical adequacy is the counterpart here to what my sketch of instrumentalism refers to as instrumental reliability. Van Fraassen offers a simple and elegant argument for it. He notes, '. . . we can have evidence for the truth of a theory only via evidential support for its empirical adequacy' (1985, p. 255). Hence, if evidence provides the warrant for belief, that warrant could never be stronger for belief in the truth of a theory than it would be for believing only in its empirical adequacy. The principle of parsimony, then, dictates that we limit belief to empirical adequacy alone. As van Fraassen puts it, additional belief is 'supererogatory':

> What can I do except express disdain for this appearance of greater courage in embracing additional beliefs which will *ex hypothesi* never brave a more severe test (ibid., p. 255).

Thus, van Fraassen sees empirical adequacy as the intermediary between evidence, on the one side, and the truth of the theory on the other, and notes that the credibility of the theory is not enhanced by the evidence once we get beyond that intermediate stage. This is just like the line of argument that we teased out of pragmatism, and used earlier to support instrumental reliability in the metatheorem of section 1. The background there, however, warranted the initial restriction to evidence over instrumental success as a fairness requirement on the terms of the debate. Thus, constructive empiricism owes us an account of why the evidence can only be in terms of empirical adequacy. Van Fraassen intends to pay his debts. He distinguishes several functions of theory, such as providing explanations, enhancing practical control and satisfying our factual curiosity (ibid., p. 280). To the extent to which these functions are carried out well, that is a theoretical *virtue*. It may even be a reason for acceptance, but it is not evidence warranting belief in the theory. Noting correctly, I think, that the prime candidate for the evidential status of a virtue is that of explanation, van Fraassen addresses the virtue of explanation explicitly. The idea he urges is that, to be good, an explanatory story must be informative. However, 'credibility varies inversely with informativeness' (ibid., p. 280). For the more informative an account is the more ways it has to be false and 'hence,

to be no more likely to be true and, hence, to be no more worthy of credence' (ibid., p. 294). He goes on to remark:

> I assume here that no one can coherently call one hypothesis less likely to be true than another while professing greater credence in it (ibid., p. 294).

This is why the virtue of explanatory efficacy does not warrant belief in the truth of the explanatory story, and why the evidence that does warrant belief extends only to empirical adequacy. (For an evaluation of this line, see section 4 below.) Moreover, if we take it that the virtues of a theory are indeed grounds for acceptance, then we can see that there is indeed a wedge between acceptance and belief, and hence conceptual room for the constructive empiricist to take his instrumentalist stand.

Several critics have expressed concern over the availability of this room. In particular, Simon Blackburn says this of constructive empiricism:

> It needs faith in some distinction between accepting a statement with a truth-condition, and believing it, and I see no such distinction. I would urge that the right path for instrumentalism is to deny that the commitments have a truth-condition, at least until the quasi-realist does his work (1984, p. 223).

Blackburn's reference to the *quasi-realist* here alludes to a strategy of projection that he sees as a positive legacy of empiricism, and that he uses primarily to address emotivism and the issue of moral realism. His suggestion above is that one could also construct a strong instrumentalist position this way, although he does not himself do so. I should like to try to do so on his behalf, by way of providing a back-up to constructive empiricism as a more-or-less ideal type of instrumentalism.[7]

The basic strategy of quasi-realism involves connecting an area of discourse and practice that is puzzling in certain respects with a more homely and less puzzling area. If we call the former *thick* and the latter *thin*, then the idea is to try to explain the thicker and puzzling practice in terms of the thinner reality, 'a world which contains only some lesser states of affairs to which we respond and in which we have to conduct our lives' (ibid., p. 169). Thus, we might try to explain why we think about things in the terms of the thicker discourse, and such an explanation would ground and justify our use of the thicker framework, our behaving *as if* the thicker commitments were true. (See ibid., pp. 180, 216, and 257.) So quasi-realism tries to earn us the right, on the thinner basis, to just those features of the thicker domain that tempt people to realism about it (ibid., pp. 171 and 197). It is clear that quasi-realism is a deflationist position whose main support comes from its economy, the way it may earn us the right to use a thick discourse while avoiding the inflationist metaphysical commitments of that discourse.

[7] My construal of quasi-realism is primarily drawn from Blackburn (1984). In an earlier piece, Blackburn (1980) nicely challenges various 'easy' formulations of realism by showing their compatibility with quasi-realism.

As a form of instrumentalism, quasi-realism (I take it) would identify the thin discourse with the domain of observation and experimental practice (or the like). On this basis, and in terms of human capacities and interests, it would seek to explain how we come to employ the highly theoretical language of science in just the way that we do employ it. If successful, this explanation would justify our commitment to theory, and in a non-reductive way. Thus quasi-realism is set up so as to conform to the first two ideals of instrumentalism; it is a non-reductive, pro-attitude towards science. When it comes to the issue of truth, quasi-realism is, in typical instrumentalist style, prepared to settle for something less.[8] For the justification of scientific practice that it would provide only extends to treating theories as if they were true, and their entities as if they were real. Again, this is no trivial kind of fictionalism, but precisely the robust version embodied in the third ideal of instrumentalism. Finally, since the whole thrust of the projective strategy is to look for the point of adopting the thick discourse so as to be able to explain its use, the fourth, teleological ideal of instrumentalism seems to fit in nicely with the quasi-realist program as well. Of course, the aim of science cannot outstrip the attainment of those virtues whose pursuit justifies its practice. Hence, quasi-realism will settle for virtues other than truth when it formulates a teleology for science; perhaps empirical adequacy would do.

In order to maintain its deflationist profile, quasi-realism has to adopt the position about explanation that van Fraassen defends; namely, the idea that explanatory efficacy is a virtue distinct from truth. For otherwise the explanatory success of science would seem to count as evidence for its truth, and quasi-realism would thus run the risk of degenerating into realism *tout court*. Presumably this feature of the inferential structure of science would fall out of the justification that quasi-realism hopes to provide for the explanatory role of scientific theories, just as for constructive empiricism this feature falls out of an epistemological side argument over the warrant for belief. We shall see in Section 4 why these instrumentalisms fail. But first let us turn to further shortcomings of realism.

3. *The end of the* World

We saw in the last section that the central point at which realism branches off from instrumentalism concerns the realist truth claims for a theory. Looking at exactly the same historical patterns of scientific practice and success as the realist, it seems to the instrumentalist that we can do justice to all that with something less than truth, and that in this area less is better. Metatheorem 1 encompasses one version of that anti-realist perception. But there is a complementary and deeper result here as well. For the general requirement of fairness entails that in the debate with instrumentalism the

[8] Alternatively one could say that quasi-realism settles for truth, but only when that is construed in a special, quasi-realist way. I explore this alternative in section 4.

realist is not free to assume the validity of any principle whose validity is itself under debate. I would suppose that this is a straightforward application of the rules against vicious circularity, or begging the question. Thus, as we saw in the background to metatheorem 1, the realist is constrained, in seeking explananda, not to enter any alleged theoretical 'truths' into the explanation competition. But remember that one central issue in the divide between instrumentalism and realism over theoretical truth is a principle of abductive inference. At dispute is the principle according to which explanatory success provides grounds for belief in the truth of the successful explanatory story (or the existence of the entities figuring in that story). For example, we have seen van Fraassen's explicit argument against this principle (and there are others).[9] Hence, the rule of non-circularity constrains the realist as follows: he must not offer as grounds for belief in realism its role in successful explanatory stories, on pain of begging the question. If we go all the way back to the beginning of our discussion of realism we can see the good sense of this constraint, for the explanationist defence was introduced precisely in order to provide evidence for a correspondence with the *World* that realism requires but that seems not directly accessible. The idea was that in so far as realism might function in successful explanations of scientific practice, that success would give us grounds for believing in realism's central theoretical entities—correspondence, or real-World reference. Thus we treat 'correspondence' analogously, say, to 'electron', and count the explanatory success of theories that employ it as evidence for its 'reality'. But, of course, since this is precisely the pattern of inference whose validity instrumentalism directly challenges at the level of ordinary scientific practice, one could hardly hope to get away with using the same inference pattern at the meta-level. (Well, I suppose the realist could hope!)

Let us take up van Fraassen's terminology and distinguish between the virtues of a theory and its truth. Since instrumentalism is a pro-attitude, it recognizes many theoretical virtues, which it embraces. These include explanatory power, breadth of application, practical utility, the capacity to unify disparate-seeming fields of practice, precision and computability of many numerical parameters, testability, organicity (i.e. the extent to which the theory forms an organic whole), simplicity (in various respects), and so on. According to the instrumentalist these are all terrific things, devoutly to be wished for, but they none of them bear on the truth of the theory. That is, the instrumentalist counts the virtues among the grounds one *might* have, in one circumstance or another, for adopting a theory. But the instrumentalist affirms that these are not grounds for belief. Hence, the canons of argument suffice to establish the following:

[9] For example, see Cartwright (1983), Hacking (1983), and Friedman (1983), chap. 3.

Metatheorem 2. No support accrues to realism from any argument simply to the effect that realism possesses one (or more) of the virtues.

I have phrased the theorem cautiously in order to leave open the possibility that one could connect the virtues with some further non-question-begging considerations and thereby try legitimately to support realism. I shall offer an example of this possibility below. But I should urge that, as I read the literature, virtually every contemporary defence of realism falls prey to metatheorem 2.

One of the most able practitioners of the explanationist defence of realism is Richard Boyd. It is, therefore, instructive to study Boyd's reaction to the preceding metatheorem, in the case where the virtue in question is explanatory power.[10] Boyd acknowledges that the issue between realism and instrumentalism is joined precisely over the legitimacy of abductive inference, so that the metatheorem is on target, but also thinks that it is a double-edged sword. For Boyd holds that ordinary inductive inference is theory-dependent in a way that involves abduction on the theoretical side, and hence urges that if abduction is suspect, then so too is induction. In particular, therefore, he suggests that the induction from scientific success to empirical adequacy (or instrumental reliability) comes out every bit as suspect as does the abduction from scientific success to truth. *Tu quoque.* Perhaps this is so, although I think van Fraassen is unhappy with Boyd's conception of induction here, and would probably reject the suggestion that belief in empirical adequacy is based on inductive inference.[11] For my part, I am quite happy if Boyd's response provides a challenge for at least those instrumentalisms wedded to a certain programme of induction. But I must point out that even so, Boyd's response so far does nothing whatsoever to get realism off the hook.

Boyd acknowledges this (1984, p. 72), and, of course, has more to say. Conceding the circularity of giving an abductive defence of realism, Boyd makes the following suggestion:

> I suggest that an assessment of the import of the circularity in question should focus not on the legitimacy of the realist's abductive inference considered in isolation, but rather on the relative merits of the overall accounts of scientific knowledge which the empiricist and the realist defend (1984, p. 73).

Boyd goes on to cite some of the merits of realism over empiricism, as he sees them. There are explanatory merits, which include explaining the mistakes of empiricism, accounting for the causal role of the senses in acquiring information, and explaining the success of experimental science. Realism also has the merit of naturalism, meshing nicely with a causal theory of

[10] I draw on Boyd (1984), pp. 65–75, and Boyd (1985). Boyd is responding to Fine (1984*a*).

[11] McMichael (1985) further challenges the capacity of constructive empiricism actually to support its belief in empirical adequacy. For van Fraassen on induction, see van Fraassen (1985) and references there.

perception and naturalized epistemology. Finally, realism has the merit of encompassing the best part of empiricism, the idea that all factual knowledge rests on the senses. (And, of course, realism avoids empiricism's sundry mistakes.)

It is to be doubted whether the empiricist would agree that only realism has all these merits nor, and more to the point, whether these are indeed 'merits' at all. But one thing seems clear enough: explaining, meshing, and encompassing are at best, in van Fraassen's terms, *virtues*. Thus, Boyd's suggestion for how to assess the admitted circularity of grounding belief on one virtue (explanation) is to ground belief on several! I submit that this does not avoid circularity, but flaunts it. Indeed, in responding to van Fraassen's comical comparison of the realist arguments with Aquinas's 'five ways', Boyd (1985, p. 32) turns to Celestine theology and recommends that we view his defence of realism as an instance of the theological principle of believing 'what is necessary to "rationally reconstruct" liturgical practice,' with the substitution of 'scientific' for 'liturgical'.

What drives Boyd to theology in his defence of realism, I suspect, is his pessimism at finding any way to support realism that does not run foul of metatheorem 2. The strategy that he adopts, therefore, is to change the canons of rational debate by ignoring an admitted circularity, and boldly to pursue the course of begging the question. This is possible. The usual canons are not sacrosanct, and one can always go the Poincaré–Duhem way to save any opinion. However, when that route involves changing the canons of rational discourse, then it seems to me well beyond the bounds of the good sense that Duhem refers us to in these matters, and that seems to me a clear signal that the realist programme is in a thoroughly degenerate state. In a less loaded way, perhaps one should just conclude that there is, in general, no rational defence of realism.

For that reason, realism has begun to splinter, first dividing theoretical truth from individual referents, and then becoming very particulate about the individuals. Ian Hacking (1983, p. 27) speaks of 'two realisms', realism about theories and realism about entities.[12] He allows the former to drift away from the latter, hopefully to be lost at sea. As for theories, realism is the doctrine of their truth that we have already explored. Perhaps the best direct arguments against that are presented by Nancy Cartwright (1983). She looks at physics, where laws ought to be true if ever they are, and argues from the details of practice in particular cases that the use of non-phenomenological laws is instrumental, and that the question of their truth is simply not a serious issue. Both she and Hacking agree that the question of their truth comes up only in certain special cases, predominantly concerning causal laws and mechanisms. Thus, the scientific question is whether certain causes are actually operative. When we subscribe to the applicability of the causal law, we answer in the affirmative. This (obviously!) amounts to belief

[12] I think the term 'entity realism' is due to Ellis (1979).

in the causes and, hence, to belief in the entities acting as causes (the electrons that carry the charge, or the genes whose jumping from place to place results in a mutation). Hacking pursues this idea of causal mechanisms and low-level rules of practice to offer a criterion sufficient for a judgement that an entity is real; namely, when we can successfully build a scientific instrument using the entity, then (surely) it exists.

I think this criterion (perhaps deliberately, see note 22) misses the point. When electrons are already taken for granted, we talk of them without notice and readily describe their deployment in all manner of affairs, including scientific instrumentation, if they figure in that. There is no special reality-making power involved in such instrumentation, nor in any other act of intervening in nature. Nor does ease of discourse about these acts, or others, have the power to make the objects of discourse real. The behaviour and discourse simply show what is already taken for granted. The realist question here is whether the entities exist. The behaviour and discourse are evidence that the entities, and the low-level and causal generalizations in which they figure, constitute an excellent way to organize and investigate our environment. This is what we take for granted, but is this evidence for their existence, as opposed to their instrumental reliability? In van Fraassen's terms, does the practice here support belief or merely acceptance? We are back to the sting of metatheorem 1, and there is no apparent way to overcome that which also gets around the circularity formulated in metatheorem 2. Despite having thrown over half of its realist companions, entity realism seems no better off than realism as a whole.

The dialectic of the debate with instrumentalism seems to set the following task for realism: to try to identify very special kinds of tests, the passing of which would yield more than just evidence for instrumental reliability, actually providing legitimate grounds for belief. This would overcome both metatheorems in a way that, I believe, adoption of a causal idiom and practice as above does not. In the course of his discussion of space-time theories, Michael Friedman (1983, chapter 3) has taken up this task. He wants to understand why some central structures in successful explanatory theories are counted as real and others (for example absolute space) are not. His answer is 'unifying power'. If the structure exhibits such power over time, then we count it as real. We do this, according to Friedman (ibid., p. 243) not out of 'aesthetic' considerations (here, perhaps, he jibes at van Fraassen's 'virtues') but because unifying power generates extra confirmation, and that actually gives us the required ground for belief.

Consider belief in the reality of molecules. According to Friedman, the story goes like this. We begin with the phenomena of the behaviour of gases under changes of temperature (phenomenological thermodynamics) and introduce a molecular model from which that behaviour can be derived. The question is whether we should believe that these molecules are real, as opposed to taking them as representative fictions merely useful for getting

at phenomenological thermodynamics. If we take the molecular model literally, then, when we come to treat chemical phenomena, we will want to adumbrate our molecular structure so as to be able to encompass, for example, the laws of chemical combination. If we succeed in doing this, then the phenomena of chemistry will provide evidence for our molecular model over and above the evidence from the behaviour of gases. Thus, the power of the molecular structure to unify phenomenological chemistry along with thermodynamics generates that much additional confirmation for the structure. As time goes on, each such episode of unification boosts the confirmation of the structure, and gives further grounds for our belief in molecules. Or does it?

The question is whether the hypothesis that molecules are real (i.e. that there are molecules) receives any more confirmation from the history of unification than does the hypothesis that molecules are useful, representative fictions. In short, does Friedman's scenario get around metatheorem 1? How could it, when the successful restructuring of the initial model for gases into a model both for gases and for chemical phenomena only shows that the general idea of building molecular models is a pretty good one; i.e. pretty useful. Thus, further unification will further confirm the utility of such molecular representations. Friedman seems to anticipate this rejoinder, for he points out that if we take the history as confirming different molecular representations in each separate episode, then no one model gets repeated boosts. Moreover, Friedman thinks that only the realist can take the history otherwise; that is, take the confirmation of each episode of unification as directed at one and the same model. No doubt Friedman has a particular anti-realism in mind, for his discussion is directed at the history of relationism over space and time, and perhaps there the relationists have the problem of distinct models for distinct episodes. Perhaps. But I do not think that is a problem for instrumentalism, as I understand it, in general. According to instrumentalism, science is non-reductive and aims at instrumental reliability. So, when the initial molecular model is introduced in the context of the study of gases, the domain of the model and the family of relations on that domain have to be left open-ended in order to leave room for further not-yet-anticipated applications. The model is not a mere reductive translation of phenomenological thermodynamics, but is rather an attempt from the start to use thermodynamics in the search for generally reliable structures. From this perspective, what both realist and instrumentalist hope to do is to make successive molecular representations whose successful applications will all ring to the credit of the initial underspecified model. From this perspective, however, a history of unification generates no more credit (or credibility) for the realist hypothesis about molecules than it does for the instrumentalist one.

To earn such credit, the realist would have to be in a position to predict something about the history of unification that the instrumentalist is not

able to predict. For example, could the realist say that because he took molecules as real to begin with, he was then in a position to anticipate that the molecular model would succeed in chemistry, and hence that the actual unification with chemistry counts for the reality of molecules? Perhaps this is what Friedman is after in suggesting that realism is in a special position to reap the profits of each unification? But what kind of 'anticipation' is this from the reality of molecules to their successful role in chemistry? Inferences of the form 'Ms are real, hence they will be useful in treating the subject of C' are not only invalid; they are generally absurd. (Try monkey/clouds, or maypole/cholera.) When absurd prophecies happen to turn out we do not usually think that lends credibility to the prophet. Of course, historical retrospect may blunt our sense of the absurd. One *can* come to feel that what is is what had to be. This is the fallacy of Whig history. For realism to avoid it, it had best look again at what predictions it can actually support. Something like this sounds better: if molecules are real and chemical phenomena really involve them, then the molecular gas model can be refined to apply to chemistry. Even this is not quite right, however, since the initial gas model might not have captured enough of the truth about molecules to be a good place to start from. But we can ignore such fine tuning, for the instrumentalist has a clear counterpart in any case, namely that if molecular models (like the gas model) are really useful for treating chemical phenomena, then the initial molecular gas model can be refined to apply to chemistry. The point again is that there is no evidence for realism beyond what there is for instrumental reliability.

Friedman was not trying to find an argument for realism in general. Rather, he was looking for a way to divide off real theoretical entities from the others. He offers unification as the crucial test. What we have seen, however, is that the strategies of the metatheorems play themselves out not only in the general case, but also for each category or individual that seeks the realist banner. No doubt the idea of realism has a strong appeal. What I have been trying to show, however, is that one is not actually moved to realism if one only heeds the call of reason. The instrumentalist, of course, is no better off.

4. *Inflationism in disguise*

According to the quasi-realist, electron talk and practice is the projection of some thinner discourse which, presumably, includes the total body of electrical evidence and experience. That projection earns me the right to engage in electron practice and to speak the electron vernacular. According to constructive empiricism, that same evidence and experience at best grounds my *acceptance* of electron theory (although I am not sure exactly which theory that is), and that acceptance involves not only belief in the empirical adequacy of electron theory, but also commitment to the electron world view, its discourse and practice. Either way we come to the same

place, justified in saying, for example, that Millikan accurately determined the charge on an electron (quite a small number), that in the Davisson-Germer experiment electrons were diffracted by a nickel crystal, that in the Compton experiment the electrons scatter but conserve their total energy, that the ball used in the Fairbank attempt to detect free quarks had its charge lowered by spraying it with electrons, that the electrons can be filtered out from hydrogen to obtain a source for proton-proton scattering, etc. That is, we are justified in saying that electrons have certain definite properties, behave in certain regular ways under identifiable conditions, and that we can manipulate electrons to do certain (more or less) practical things. Moreover, we are justified not only in saying all of this, but in behaving just as though it were true. We can make the electron measurements, induce the electron behaviour, and construct the electron tools. How could we be justified in engaging in this robust discourse and practice without actually believing that there *are* any electrons? (Just imagine how *that* absurdity would come out in the hands of Pinter, or Beckett!) Of course the instrumentalist has his response—deflationism.

> . . . there is also a positive argument for constructive empiricism—it makes better sense of science, and of scientific activity, than realism does and does so without inflationary metaphysics (van Fraassen, 1980, p. 73).

Apparently, deflationism lies behind our moving all the way to commitment but not to belief, all the way to treating electrons just as though they were real, but not beyond. Like a pubescent child receiving parental advice, we are admonished not to go all the way (although, provided we save appearances, virtually anything less will do!). But this behaviour will neither preserve innocence, nor avoid sin—and sensible parents know better.

The instrumentalist considers realism a sin, and he seeks to avoid it by deploying the strategy of always going for a little less. This is the strategy of metatheorem 1, and I have argued that it can indeed be used successfully to avoid realism. But there are other sins in the book. Consider epistemology.

Constructive empiricism relies on empiricist epistemology. This is the conjunction of two philosophical ideas: the central idea of epistemology proper, that belief requires a warrant; and the central application of that idea in empiricism, that only experience can legitimate belief. Constructive empiricism is, if you like, a programme for understanding science in such a way as to save this philosophical creed. Thus, this brand of empiricism can follow the usual lattice of inferences and reasons that issues in scientific beliefs only until it reaches the border of the observable, at which point the shift is made from belief to acceptance. But the inferential network that winds back and forth across this border is in no way different from that on the observable side alone. Nor does constructive empiricism impute any difference to it. For example, it is not as though van Fraassen thinks 'inference to the best explanation' is all right if it gets one to observables,

but not right otherwise. Not at all. According to van Fraassen this form of 'inference' never yields a good reason for belief (a conclusion that realists like Cartwright, Friedman, and Hacking largely share; see note 9). Given the admitted uniformity of scientific inferential practice, however, it cannot actually be deflationism that keeps us shy of belief (for example in electrons). For the deflationist policy that recommends against the needless multiplication of entities includes the recommendation not to multiply the significance of practices when there is no difference among the practices themselves. Despite the uniformity of practice, however, constructive empiricism does feel a need to multiply its interpretation of that practice (going here for acceptance, and only there for belief). That need goes against its deflationist promise. It is generated only by the prior commitment to empiricist epistemology.

There is no other argument for it. In support of a general distinction between belief and acceptance, constructive empiricism can point to circumstances where reasons for commitment to a theory (for example its informativeness or audacity; van Fraassen, 1985, p. 281) are not reasons for belief in its truth. But such considerations fail to support the policy of *always* going for acceptance and *never* for belief, when it comes to unobservables. Why must it be the case, say for electrons, that the complex history of evidence, successful use, and reasoning *at the very best* supports belief in the observational reliability of electrons and supports our commitment to behave just as though they exist but nevertheless fails to support the belief that they do exist? How could one know this prior to examining the actual history of the case? Recall the argument from section 2. Support for the truth of a theory, we were told, only comes by way of support for its empirical adequacy. When pressed for why this is so, truth is distinguished from the virtues. The virtues, it is argued, do not lend credence to a theory. Even if we accept this, it hardly proves what is required. For what we require is some positive argument for why nothing other than empirical adequacy bears on truth. To show that there are some other things (the virtues) that do *not* bear on truth is beside the point (although it certainly challenges the realist's reliance on the virtues). Here is another try. Constructive empiricism wants us to restrict belief to empirical adequacy because (we are told) the only way experience could count against any theory is (logically speaking) by first counting against its empirical adequacy (ibid., p. 254). But why is this Popperian twist to the point? Why should the fact that empirical adequacy is first in the line of vulnerability to experience issue in a blanket policy of restricting belief exactly there? What positive arguments or reasons connect the two, providing the grounds to multiply interpretations of the inferential practice? The answer is that constructive empiricism has no argument. It goes its inflationist way in order to prop up empiricist epistemology. There is no other (or better) reason that supports its chosen path.

If epistemological inflation is the sin of constructive empiricism, then semantics is the sin of quasi-realism. For how is the quasi-realist going to answer the challenge of why one should follow the 'thick' scientific discourse all the way up to the truth (of existence claims, for example) but not to include that truth? If I understand the remark in Blackburn's critique of van Fraassen, that he sees no distinction between acceptance and belief, then his response ought to be that there really is no room to go right up to truth, without actually going all the way and including truth itself. Thus, among the practices that quasi-realism seeks to ground would be the belief-generating procedures involved in the scientific give-and-take. So quasi-realism, I think, (unlike constructive empiricism) does not intend to fall short of grounding actual belief in the truth of a theory (or hypothesis), when that belief is actually grounded in the relevant scientific practice. Similarly, I think quasi-realism does not want to fall short of giving scientific statements a truth value, when they have earned it. In particular, then, quasi-realism will (presumably) sometimes count existence claims as true; perhaps, for instance, it would so honour the claim about electrons. If this is correct, however, how can quasi-realism fail to be counted as just another selective realism—thus erasing the hedging 'quasi'?

To protect its hedge, quasi-realism needs to distance itself from realism somewhere. Since, if successful, it would actually justify our common scientific beliefs and truth claims, then that distance can only be found in the way in which quasi-realism interprets (or understands) either belief or truth. Given its rejection of constructive empiricism's attempt to reinterpret belief, we might expect quasi-realism to go for a non-correspondence account of truth. Thus, quasi-realism will justify truth claims—the claim that electrons exist, for example—but only if we understand those claims in a way different from how the realist understands them. This semantic manœuvre, then, would yield only the *appearance* of realism, i.e. mere 'quasi-realism'.

When it comes to truth, quasi-realism tries to adopt a consistent policy. It tries to earn the right to truth talk by constructing a quasi-realist account of truth. Remember that this is, in the first instance, an explanatory account (not an analysis or the like), although it does give a particular construal to what truth talk is all about, its *significance* (so to speak). The explanation must begin with a 'thin' discourse from which it will try to project 'truth'. For Blackburn (1984, chapter 7), this takes the form of sketching a coherence account of truth, one sensitive to the classical objections to coherence and one which tries to do some justice to the intuitions of a correspondence theory. I shall not discuss the details for there is a general reason to be wary of any such proposal.

The reason is this. The quasi-realist strategy requires a thin, unproblematic discourse on which to base its projections. To project out 'truth', the thin discourse must be truth free. But where are we to find any such thin

discourse? The redundancy property of truth makes truth a part of any discourse that merits the name. In particular, if we go for acceptance or coherence theories of truth we try to build to truth from certain systems of judgements (or the like). But the judgement *that P*, and the judgement *it is true that P* are the same. You cannot have one without the other. That is to say, you cannot include one in the system without thereby including the other. Thus, one cannot build up to truth. One can only move from certain truths to others, by means of (hopefully) valid (i.e. *truth*-preserving) principles. Thus there is no thin discourse from which the quasi-realist explanations of truth talk could begin.

Of course Tarski built a theory of truth. Surely he began with a discourse that did not contain the truth predicate and showed, recursively, how one could introduce it? No. Tarski did not start with a discourse, as the quasi-realist requires it, at all. He started with a recursively formalized language. The quasi-realist needs much more, for not even the vocabulary of science is recursive (nor even recursively enumerable). Much worse is the fact that quasi-realism is not actually about discourse, but is concerned rather with discourse-cum-practice, i.e. with 'forms of life'. But there is no form of life, however stripped down, that does not trade in truth. Certainly no such barren form could hope to explicate science. Thus, the reference to formalized language is quite beside the point.

More to the point would be to realize that truth is involved not just in the thin discourse itself, but also in the explanatory principles that the quasi-realist would seek in order to carry out his projection. For how could one hope to justify scientific truth-judging except by using explanatory principles involving our interests and capacities for getting at the truth. Blackburn actually illustrates this feature himself, in showing that a coherence theory must somehow incorporate the 'correspondence' idea that true believers respond to the facts,[13] although it is not at all clear how it can do so given the explanatory restriction to coherence (and comprehension and control). Blackburn is right; one could never hope to justify our judgements as judgements of truth on such a slender basis. That is, there is actually no coherent basis on which to build a quasi-realist account of truth.

Let me summarize my reflections on instrumentalism, in the two versions I have studied, by means of the following conservation principle.

Metatheorem 3. Instrumentalism avoids the inflationary metaphysics of realism only by pumping up either epistemology or semantics.

What lies behind this conservation rule is not just an induction-by-enumeration, from constructive empiricism and quasi-realism. Rather, the general argument derives from reflecting on how we could go almost all the way with science and yet fall shy of its beliefs and truth claims except by

[13] See the discussion of 'correspondence conditionals' in Blackburn (1984), pp. 244 ff.

imposing extra-scientific epistemological strictures on *belief*, or by building a special, extra-scientific semantics for *truth*. The point of the theorem is to unmask instrumentalism, so we can see that it is just more inflationism (in deflationist clothing).

5. *The natural ontological attitude*

I have been trying to set up the debate between realism and instrumentalism so as to emphasize what they share, since I believe that what they hold in common is what defeats them. In metatheorem 3, this comes out as inflationism. But that is a consequence of more basic attitudes, for neither realism nor instrumentalism wants to be inflationist. What each does want, however, is to interpret science in accordance with a set of prior, extra-scientific commitments. For realism, the significance of science *must* derive from its being about the *World*. For instrumentalism, the significance derives from how science grows from and relates to us (observability, or the thin discourse that *we* project outwards). These extra-scientific orientations to science preserve some cherished elements of recognizable philosophical schools. In each case, inflationism is the consequence of trying to reconcile science with the special interpretative stance of a particular school. But behind the commitment to philosophical schools, realism is bound to instrumentalism by something even more basic—their common presupposition that science is the sort of enterprise that requires and/or permits of a general interpretation. In this sense, realism and instrumentalism are basically hermeneutic attitudes. That is their undoing.

In one way or another, realism and instrumentalism are devoted to the global enterprise of 'making sense of science'. As I suggested at the outset, this is part of a search for authority. When the need is felt for an outside authority we get the characteristics of realism: the invention of the *World* and the idea of truth as correspondence to the *World*, or the idea of reference as actually reaching out to touch the *World*. Then realism moves to 'authenticate' science, firstly by means of an explanationist strategy for making its methods and practices intelligible ones in a search for truth, and secondly by postulating truth as what science is searching for. As Boyd says, realists believe what they have to believe to make this reconstruction viable. Where realism looks outside for authority, instrumentalism looks inward. When instrumentalism takes a specifically empiricist cast, the range of knowledge is predetermined by the realm of the observable. That in turn dictates a fairly radical reinterpretation of scientific practice, surgically grafting altered significance on to the practice at precisely the point where science moves beyond the observable. Postulating as its aim simply knowledge of observables (empirical adequacy), this too 'makes sense' of science, providing an authenticating package for it as an enterprise rationally pursuing its goals. Quasi-realism also introspects in order to

'explain' scientific practice as a projection outwards from us, our interests, habits, and capacities. In order to prevent its validating explanations from extending too far, however, quasi-realism allies itself with an idealist theory of truth.[14] The result is supposed to authenticate science, which is described in terms of truth (and all), but a science whose significance is kept subtly tethered to its human origins.

I think there is an important lesson to be learned by contrasting realism with quasi-realism; namely, that there is a quite definite middle ground. Realism would agree with quasi-realism in almost every particular of their respective descriptions of scientific practice. Where they differ is over the significance of these descriptions; realism driving that significance outwards to the *World*, and quasi-realism recognizing the outward pull but seeking somehow to anchor it in us. Of course what I refer to here as 'significance' is not so much a question of individual meaning, but rather the kind of significance a practice acquires when it is properly set. No doubt this is why realism and instrumentalism have to do with attributing goals to science, for the end is an important ingredient of the setting. But suppose we give up this hermeneutic orientation. Suppose we were no longer to seek for authority, or authentication. Suppose we undo the idea that science needs to be made sense of, or that science requires a setting. Then what realism and quasi-realism would still share would be a description of scientific practice in terms of structures of evidence and inferences sometimes generating beliefs about what is true, and sometimes generating reasonable pragmatic commitments that fall short of belief.

This undoing of the hermeneutic orientation, while embracing the common ground that it leaves, is what I call NOA (pronounced as in 'Noah'), the *natural ontological attitude*.[15] It counsels us to resist the impulse to ask 'What does it all mean?' NOA urges us not to undertake the construction of teleological frameworks in which to set science. It suggests the subversive idea that perhaps there is no need for authority (inner or outer), nor for general authenticating. NOA whispers the thought that maybe we can actually get along without extra attachments to science at all, and NOA certainly proposes no additions that go beyond the history and practice of science itself. NOA is thoroughly deflationist, puncturing all three balloons of metatheorem 3.

NOA thinks of science as an historical entity, growing and changing under various internal and external pressures. Such an entity can be usefully studied in a variety of ways, sociological, historical, economic, moral, and methodological—to name a few. One can ask a variety of questions about particular developments in particular historical periods. Sometimes there will be a basis in the practice itself for answering such questions. Sometimes

[14] There are other recent anti-realist alliances, for example those with acceptance theories of truth (as in Putnam's (1981) 'internal realism'). See my critique of these 'truthmongers' in Fine (1984*b*).

[15] NOA was introduced in Fine (1984*a*). I expand on it in Fine (1986).

that basis will support several plausible answers. Sometimes it will be clear that there is no basis in science itself for addressing the question, and then one must judge for oneself whether it is worth adding attachments on to science so as to make a place for the question, or whether one should just let the issue drop. Good sense and the dialectic of reasons tutor such judgements, nothing forces them. Let me offer two examples to illustrate NOA's approach.

Does science aim at truth, or does science merely aim at empirical adequacy? This is the springboard for the realism/instrumentalism controversy. NOA wants to pull back a bit from the question to ask, more fundamentally, whether science 'aims' at all. Of course, there is a point to particular investigations, and certainly particular research groups have aims and goals (to do a better experiment, to solve an outstanding problem, to build a better instrument, etc.). But only a fallacy in quantifier logic would lead one from 'They all have aims' to 'There is an aim they all have'. Nietzsche understood this logic in *The Will to Power* where he wrote (§ 55):

> Can we remove the idea of a goal from the process and then affirm the process in spite of this?—This would be the case if something were attained at every moment within the process.[16]

What, then, other than faulty reasoning, would support the idea that the whole scientific enterprise has an 'aim'? I can think of two lines of thought here. First is the concern for authority and authentication. The concern is to establish the rationality of science, and the thought is that we could carry this off if we could show that scientific practice is an appropriate means for achieving its ends. So, there *must* be ends! But what if there were no aim of science, would we then have to invent one in order to feel secure in our conviction that science is rational? (Here, not surprisingly, we broach Kantian themes: the invention of the noumena and the argument for God.) But why should we feel insecure? What actually frightens us here? The method that NOA follows is to search for the roots of our concerns, to try to trace the motivational pattern both historically and personally. With regard to the rationality issue, I suspect the historical link is to various foundational programmes and their attempts to overcome scepticism and relativism. But, foundationalism and its problems aside, why 'overcome'? Is the threat of scepticism or relativism so enormous that we have to invent over-arching goals in order to demonstrate the rationality of our scientific practice? Even if we do proceed with the invention and demonstration, will that really help us? The moral of the realism/instrumentalism debate suggests that no reasoned satisfaction is to be had from such a project. Moreover, scepticism or relativism notwithstanding, the rationality of science-in-the-small is

[16] I found this passage in Blackburn (1980), quoted on p. 359. Further parallels between NOA and Nietzsche are drawn by Rorty (1986*b*), whose developing neo-pragmatism (see Rorty, 1982 and 1986*a*) seems quite NOAish.

frequently apparent to its students and practitioners. So do we really need to worry about a Big Aim for science overall? NOA suggests that in fact we manage perfectly well with less. Since the actual implies the possible, then indeed we *can* do very well, thank you, with less. There is, however, a second way that the question of aims might come up. For sometimes it is certainly quite in order to observe a practice (say, a tribal rite) and to wonder what it is about, what it is in aid of, or what it means. Sometimes the answers to these questions are not apparent, not even to the actors themselves. Sometimes we can try out different answers, test them against the on-going practice, and even elicit the response of the practitioners to them as well. This is an extremely difficult area; that is, an area where it is hard to be disciplined and to exert reasoned control. Nevertheless, sometimes we come to know that the hermeneutic attitude is simply out of control. This can happen in the small. For example, if we overhear viewers at the local art gallery debating the artist's aim in putting a little red dot in the upper right corner of a painting, then we know things have got out of hand; for the convention is for the dealer to put a dot there when the painting has been sold. It can also happen in the large, as when we ask, 'What does it all mean?', knowing full well that the mood will pass (or, anyway, hoping it will). I would suggest that science is like life. It shows its multiple- and mini-aims daily. But the quest for a general aim, like the quest for the meaning of life, is just hermeneuticism run amok. What NOA points out, again, is how we thrive with less. The result of this first illustration of NOA is not that it holds science to be pointless. What NOA holds is that nothing seems to accrue to our understanding of science if we go looking for general aims or goals, although pursuit of the quest may tell us something about the knights errant.

As a second illustration, consider the project for a science of science. Our 'natural' attitude could easily be assimilated to Quine's naturalized epistemology, and then even to a 'naturalized' science of science. But the description of science as an historical entity was intended precisely to undercut at least one version of that idea, the idea that science has an essence. If that were our picture, then indeed one could imagine a sort of chemistry of science which seeks for regularities in the phenomena, the laws covering that, and then looks for even deeper structures that may lie behind those—the very molecules and atoms of science! If science is an historical entity, however, then no such grand enterprise should tempt us, for its essence or nature is just its contingent, historical existence. But then, perhaps, a different picture is tempting; for the science of evolution deals with historical entities, and so we might be tempted to a sort of biology of science. Some have been so tempted. In its most sensible version,[17] one would look for models of scientific practice in limited historical periods and

[17] See Giere (1985).

domains, and then try to find informative generalizations that cut across the various models. What one would study in this way would be topics like theory choice and theory construction, much as biological models study natural selection and mutation. But there, so far as I can see, the analogy ends. For, after all, as an historical entity science is an individual, like a particular species—the horse, for example.[18] Many sciences contribute to our understanding of the horse, but there is no 'science of the horse'. From an evolutionary point of view, there is only a natural history. I believe the same is true of science itself. If we index conceptions of what counts as a theory and what counts as criteria of choice, for time, place, and topic, then what inclines one to think that any overlap between distinctly indexed models of 'theory choice' will signify anything beyond our penchant for finding similarities (and perhaps the very ones we put in the models!)? The inclination to think there will be projectible traits can only reside in essentialism. Moreover, the prospects for building a useful science on the empirics of correlational data are not very strong, especially in the light of our experience in sociology, political science, and quantitative history. NOA inclines us to be wary of such projects, although it certainly offers no knockdown, *a priori* argument against them.

The anti-essentialist orientation of NOA comes out not only in its conception of science as historical and open-ended, but also in its similar conception of truth and truth claims. For NOA allies itself with what Blackburn (1984, p. 229) dismissively calls 'quietism'. Less pejoratively, NOA holds a 'no-theory' conception of truth. This is what I think van Fraassen usually means by taking truth 'literally'; i.e. as an unanalysed term whose use is basic and well understood.[19] This 'no-theory' accepts the usual logic and grammar of truth, including its redundancy property (at least in the non-problematic cases). It also accepts what Rorty (1986*a*) nicely identifies as 'the cautionary use', as in, 'Yes, you are justified in holding that, but I wonder whether it is true.' This cautionary use signals that the concept of truth is not adequately captured in any justificationist scheme (acceptance theories, coherence theories, etc.). The general idea is to accept entrenched uses but to refrain from the project of seeing those uses as grounded in 'the nature of truth', some deep truth-making properties, or the like. Thus, NOA does not think that truth is an explanatory concept, or that there is some general thing that makes truths true.[20]

This no-theory attitude towards truth separates NOA from realism, since realism is committed to a special interpretative stance. I think that realists themselves sometimes fail to appreciate their own commitment, so let me try to drive it home this way. Consider idealism (truth as coherence) or

[18] Hull (1980) argues the case for treating biological species as individuals and as historical entities.

[19] See n. 6 above.

[20] Williams (forthcoming) is a good defence of the idea that truth is not an explanatory concept, and hence not a suitable object for theory construction.

pragmatism (truth as utility). These anti-realisms could well subscribe, for example, to the reality of electrons; i.e. hold it true that electrons exist. Indeed, I suppose that sensible idealists and pragmatists do so. But that does not represent a conversion to realism on their part, nor even to realism over electrons. Thus, the question of what entities one believes in does not adequately demarcate realism from anti-realism. This point is well understood in the philosophy of mathematics. For example, we all believe that there is one and only one even prime number (the number 2). That belief, however, scarcely makes number-realists (much less Platonists) of us all. The point is that realism requires two distinct elements. It requires belief and it also requires a particular interpretation of that belief. Thus anti-realism, in particular instrumentalism, pursues the following strategy. If it does not withhold belief, then it offers instead a non-realist interpretation of that belief. In constructive empiricism and quasi-realism, respectively, we have witnessed the alternatives here. But the reader will no doubt notice that there is an interesting third way.[21] For one can go along with belief, but then simply not add on any special interpretation of it—neither realist nor anti-realist. That is the way of NOA.[22]

This way, then, separates NOA not only from realism but from the various truth-mongering anti-realisms as well. What about empiricism? The argument of the preceding section is that the empiricist *a priori* commitment to observables puts it at odds with its otherwise deflationist self. NOA opts for deflationism over such *a priori* commitments. Indeed, from the perspective of NOA, it becomes very clear that the central philosophical concern of constructive empiricism is not so much over realism but centres instead on the topic of evidence, and its reach. Constructive empiricism addresses this concern by offering a sort of proto-theory of evidence. NOA's attitude makes it wonder whether any *theory* of evidence is called for. The result is to open up the question of whether in particular contexts the evidence can reasonably be held to support belief (regardless of the character of the objects of belief). Thus NOA, as such, has no specific ontological commitments. It has only an attitude to recommend: namely, to look and see as openly as one can what it is reasonable to believe in, and then to go with the belief and commitment that emerges. Different NOAers could, therefore, disagree about what exists, just as different, knowledgeable scientists disagree. One NOAer might even find specific grounds in certain

[21] There are some other options too. I take constructive empiricism to withhold belief but to go for a no-theory of truth. It could, however, go for a realist correspondence idea of truth and still withhold belief in the truth where unobservables are concerned. As I mentioned in n. 6 above, I think van Fraassen sometimes leans this way. Finally, a more Machian empiricism presumably withholds belief and also gives a non-realist account of truth. So the other options do not lead to fresh philosophical stances.

[22] It may well be that 'entity realism' is just a special version of NOA. That depends on how the truth of existence claims is to be understood (or not). I suspect that Hacking's (1983) version is like NOA, and that the realist label is just his way of trying to redirect philosophical discussion by giving the new direction an old and honoured name. I do not know about the other 'entity realists'.

cases for bracketing belief in favour of commitment, for instance, while another might go for some measure of belief. These could both be reasonable attitudes, and there may even be no good way to choose between them. We all need to believe. The beauty of empiricism is that it challenges this need by making us scrutinize the bases for belief. In this respect, empiricism helps to keep us honest, and I count it a good thing. NOA would like to hang on to that virtue, but thinks we can do so without having to swear philosophical allegiance only to things seen. As Einstein remarked about Mach's empiricism:

> It cannot give birth to anything living; it can only exterminate harmful vermin (Speziali, 1972, p. 114).

If we examine NOA in terms of the categories used to contrast realism with instrumentalism, then NOA comes out like this. It is a pro-attitude towards science, basically accepting although ready to challenge too-easy conclusions in specific cases. It is non-reductive, finding grounds for that in an historical and anti-essentialist attitude that it would extend to the practice of science itself. It goes for truth and the idea that science can involve belief in the truth (even, for example, about unobservables). But its way with truth is a 'no-theory' that rejects all realist and anti-realist construals. As for 'the aim of science', NOA thinks this is a chimera, conjured up in response to misplaced hermeneuticism and fear of the irrational. NOA thinks we can do better.

NOA involves a trusting attitude. It trusts the overall good sense of science, and it trusts our overall good sense as well. In particular, NOA encourages us to take seriously the idea that what the scientific enterprise has to offer is actually sufficient to satisfy our philosophical needs. It urges us to explore what happens philosophically when we approach science with trust, and openly; i.e. without rigid attachments to philosophical schools and ideas, and without intentions for attaching science to some ready-made philosophical engine. This approach automatically avoids the inflationary philosophies of realism and anti-realism, and it does so without running into the clutches of relativism or irrationalism. In short, our worst fears are not realized. Instead, NOA opens up a 'third way'. The hallmark of this way is a radical deflationism that eschews additions and attachments to science. Thus the 'naturalness' of NOA, if you like, is the 'California natural'—no additives, please!

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[Footnotes]

[17] **Philosophy of Science Naturalized**
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