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Brit. J. Phil. Sci. **51** (2000), 705–728

The Present State of the Scientific Realism Debate

Stathis Psillos

> The unique attraction of realism is the nice balance of feasibility and dignity that it offers to our quest of knowledge .... We want the mountain to be climbable, but we also want it to be a real mountain, not some sort of reification of aspects of ourselves (Wright [1988], p. 25).

ABSTRACT

In this survey article I try to appraise the present state of the scientific realism debate with an eye to important but hitherto unexplored suggestions and open issues that need further work. In Section **2**, I mostly focus on the relationship between scientific realism and truth. In Section **3**, I discuss the grounds for realists' epistemic optimism.

1 Introduction

There was at one time a feeling in the philosophy of science community that the scientific realism debate had run out of steam. Fine went so far as to declare that 'realism is well and truly dead' ([1986a], p. 112) and to write the obituary of the debate, *aka* the Natural Ontological Attitude. Fortunately, this line of argument has failed to persuade many philosophers, who still think that the scientific realism debate has a glorious past and a very promising future. In the last dozen years alone there have been a number of books which cast a fresh eye over the issue of scientific realism, such as those by Suppe ([1989]), Putnam ([1990]), Almeder ([1992]), Wright ([1992]), Kitcher ([1993a]), Aronson, Harré and Way ([1994]), Brown ([1994]), Laudan ([1996]), Leplin ([1997]), Kukla ([1998]), Trout ([1998]), Cartwright ([1999]), Giere ([1999]),

Niiniluoto ([1999]) and Psillos ([1999]). Although these books differ vastly in their approaches and in their substantive theses, they can all be seen as participating in a common project: that of characterising carefully the main features of the realism debate and offering new ways of either exploring old arguments or thinking in novel terms about the debate itself. It wouldn't be an exaggeration to say that the realism debate is currently going through its renaissance period.

In what follows I try to appraise the present state of the scientific realism debate with an eye to important but hitherto unexplored suggestions and open issues that need further work. In Section **2**, I mostly focus on the relationship between scientific realism and truth. In Section **3**, I discuss the grounds for realists' epistemic optimism.

2 What is scientific realism?

2.1 Modesty and presumptuousness

I offer the following three theses as constitutive of scientific realism. Each of these is meant to distinguish realism from a particular non-realist approach.

The metaphysical thesis: The world has a definite and mind-independent structure.

The semantic thesis: Scientific theories should be taken at face value. They are truth-conditioned descriptions of their intended domain, both observable and unobservable. Hence, they are capable of being true or false. The theoretical terms featuring in theories have putative factual reference. So, if scientific theories are true, the unobservable entities they posit populate the world.

The epistemic thesis: Mature and predictively successful scientific theories are well confirmed and approximately true of the world. So, the entities posited by them, or, at any rate, entities very similar to those posited, inhabit the world.

The *first* thesis—*metaphysical realism*—is intended to distinguish scientific realism from all those anti-realist accounts of science, be they traditional idealist and phenomenalist or the more modern verificationist accounts of Dummett's ([1982]), and Putnam's ([1981], [1990]) which, based on an epistemic understanding of the concept of truth, allow no divergence between what there is in the world and what is issued as existing by a suitable set of epistemic practices and conditions. The first thesis implies that if the unobservable natural kinds posited by theories exist at all, they exist independently of our ability to be in a position to know, verify, recognise etc. that they do.

The *second* thesis—*semantic realism*—makes scientific realism different from *eliminative instrumentalist* and *reductive empiricist accounts*. Eliminative instrumentalism (most notably in the form associated with Craig's Theorem) takes the 'cash value' of scientific theories to be fully captured by what theories say about the observable world. This position typically treats theoretical claims as syntactic-mathematical constructs which lack truth-conditions, and, hence, any assertoric content. Reductive empiricism treats theoretical discourse as being disguised talk about observables and their actual (and possible) behaviour. It is consistent with the claim that theoretical assertions have truth-conditions, and hence truth-values, but understands their truth-conditions *reductively*: they are fully captured in an observational vocabulary. Opposing these two positions, scientific realism is an 'ontologically inflationary' view. Understood realistically, a scientific theory admits of a literal interpretation, *viz.* an interpretation in which the world is (or, at least, can be) populated by a host of unobservable entities and processes.

The *third* thesis—*epistemic optimism*—is meant to distinguish scientific realism from *agnostic* or *sceptical* versions of empiricism (see van Fraassen [1980], [1985]). Its thrust is that science can and does deliver theoretical truth no less than it can and does deliver observational truth. It is an implicit part of the realist thesis that the ampliative–abductive methods employed by scientists to arrive at their theoretical beliefs are reliable: they tend to generate approximately true beliefs and theories.

Semantic realism is no longer contested. Theoretical discourse is taken to be irreducible and assertoric (contentful) by all sides of the debate. Making semantic realism the object of philosophical consensus was by no means an easy feat, since it involved two highly non-trivial philosophical moves: *first*, the liberalization of empiricism, and the concomitant admission that theoretical discourse has 'excess content', that is, content which cannot be fully captured by means of paraphrase into observational discourse; and *second*, a battery of indispensability arguments which suggested that theoretical terms are indispensable for any attempt to arrive, in Carnap's ([1939], p. 64) words, at 'a powerful and efficacious system of laws' and to establish an inductive systematisation of empirical laws (see Hempel [1958]).

Given this, what is really distinctive of scientific realism is that it makes two claims in tandem, one of which (to explore Wright's ([1992], pp. 1–2) terminology) is 'modest', while the other is more 'presumptuous'. The *modest* claim is that there is an independent and largely unobservable-by-means-of-the-senses world, which science tries to map. The more *presumptuous* claim is that, although this world is independent of human cognitive activity, science can nonetheless succeed in arriving at a more or less faithful representation of it, enabling us to know the truth (or at least some truth) about it.

For many philosophers, this is *ab initio* an impossible combination of views. For, they think, if the world is independent of our abilities or capacities to investigate it and to recognise the truth of our theories of it, then how can it

possibly be knowable? Two options then appear to be open to would-be realists: either to compromise the presumptuous claim or else to compromise the modest claim.

2.2 Compromising presumptuousness

Here the cue is taken from Popper's ([1982]). Take realism to be a thesis about the aim of science (truth), leaving entirely open the issue of whether this aim is (or can ever be) achieved. Implicit in this strand is the idea that truth is understood realist-style (in the sense of correspondence with the world), in order not to compromise the modest claim as well. Popper is famous for claiming both that we can never say that this aim has been achieved, and that truth is somehow magically approached by the increasing verisimilitude of successive theories; 'magically' because there is nothing in Popper's account of verisimilitude which, even if it worked,[1] guarantees that there is a 'march on truth'. Musgrave ([1996], p. 23) agrees that realism is (along with a semantic thesis) an *axiological thesis*: 'science aims for true theories.' There is clear motivation for this compromise: even if all the theories we ever come up with are false, realism isn't threatened (*ibid.*, p. 21). Musgrave doesn't think that all our theories have been or will be outright false. But he does take this issue (whatever its outcome may be) to have no bearing on whether realism is a correct attitude to science.[2] There are, however, inevitable philosophical worries about the axiological characterization of realism. First, it seems rather vacuous. Realism is rendered immune to any serious criticism stemming from the empirical claim that the science we all love has a poor record in truth-tracking (see Laudan [1984]). Second, aiming at a goal (truth) whose achievability by the scientific method is left unspecified makes its supposed regulative role totally mysterious. Finally, all the excitement of the realist claim that science engages in a cognitive activity that pushes back the frontiers of ignorance and error is lost.

So the thought seems irresistible that if realists take seriously the claim that they must compromise, then their only real option is to compromise their modesty, not their presumptuousness: if the world isn't in any heavyweight way independent of us, then its knowability can be safeguarded. The modest claim can easily be compromised, by coupling realism with an epistemic notion of truth which *guarantees* that the truth (and hence what there is) does not lie outside our cognitive scope.

[1] Its problems have been shown by Miller ([1974]) and Tichy ([1974]).

[2] It's a bit ironic that Van Fraassen ([1980], p. 8) also characterizes realism as an axiological thesis together with a (non-Popperian) doxastic attitude, *viz.* that acceptance of a theory involves belief in its truth.

2.3 Compromising modesty

Here the main cue is taken from Putnam's ([1981]). Take realism to involve an epistemic concept of truth, that is a concept of truth which guarantees that there cannot be a divergence between what an ideal science will assert of the world and what happens (or what there is) in the world. This line has been exploited by Ellis ([1985]) and Jardine ([1986]). For Ellis, truth is 'what we should believe, if our knowledge were perfected, if it were based on total evidence, was internally coherent and was theoretically integrated in the best possible way' ([1985], p. 68). There are many problems with this view that I won't rehearse here (see Newton-Smith [1989b]; Psillos [1999], pp. 253–55). The only thing I will note is that it's not at all obvious whether the suggested theory of truth can deliver the goods. To use Jardine's ([1986], p. 35) words, the required concept of truth should be neither too 'secular', nor too 'theological'. It should avoid an awkward dependence of truth on the vagaries of our evolving epistemic values, whilst linking truth to *some* notion of ideal epistemic justification. But in the attempt to break away from 'secular' notions of truth and to make truth a standing and stable property, it moves towards a 'theological' notion: the justification procedures become so ideal that they lose any intended connection with humanly realizable conditions. In the end, the concept of truth becomes either 'secular', resulting in an implausible relativism, or else 'theological', and hence not so radically different from a (realist) non-epistemic understanding of truth, according to which truth *outruns* the possibility of (even ideal-limit) justification. To be sure, Putnam ([1990], p. viii) has dissociated his views on truth from the (Peircean) ideal-limit theory on the grounds that the latter is 'fantastic (or utopian)'. Still, his proposed alternative, which ties ascriptions of truth to the exemplification of 'sufficiently good epistemic situations' fares no better than the Peircean theory *vis-à-vis* the secular/theological test above. For one can always ask: what other than the truth (realist-style) of a proposition can *guarantee* that the sufficiently good conditions of justification obtain?[3]

Wright has an interesting dual thesis i) that a suitable epistemic concept of truth does *not* necessarily compromise the modesty of scientific realism and ii) that the best hope for the presumptuous claim of scientific realism rests on a broadly verificationist (epistemic) understanding of truth. For Wright, scientific realism stands mainly for a) anti-reductionism and b) the claim that theoretical discourse is apt for 'representation or fit with objective worldly states of affairs' ([1992], p. 159). So the first part of his dual thesis ([1992], pp. 158–59) stems from the thought that the anti-reductionist stance of semantic realism, which treats theoretical discourse as apt for representation, is consistent with a (suitably) 'evidentially constrained' account of truth. This

[3] For a fresh look at Putnam's ([1981]) 'internal realism', see Niiniluoto [1999], Ch. 7).

is so because, he claims, scientific realists may accept *both* that theoretical assertions faithfully represent worldly states-of-affairs *and* that these states-of-affairs are '*in principle*' detectable (and hence, in principle verifiable). In particular, these worldly states-of-affairs need not be representable in a humanly intelligible way. Hence the world ends up being independent of human representation (as scientific realism requires), yet the world's in principle detectability suggests a notion of truth which is suitably 'evidentially constrained', and hence epistemic. (The motto for Wright's verificationist scientific realists would be: there is no in principle undetectable truth.) The second part of Wright's dual thesis stems from the thought that the realists' epistemic optimism requires that 'the harvest of best methods is (likely to be) truth and may, *qua* so harvested, be reasonably so regarded ([1986], p. 262). But, he goes on, if truth is not taken to be what is 'essentially certifiable by best method' (as a verificationist realist would have it), then there is no guarantee that truth is achievable. So, Wright claims, the door is left open either to a van Fraassian sceptic, or else to a Quinean pragmatist who 'cashes out' talk of truth in terms of talk of a(n) (everlasting) set of simplicity-guided adjustments in our evolving network of beliefs in response to empirical anomalies.[4]

In order to cast some light on (and to criticize) Wright's thesis, I note the following. In his ground-breaking ([1992]), Wright presents a 'minimalist' concept of truth (not to be confused with Horwich's ([1990]) account) which is characterized by some 'syntactic and semantic platitudes' (e.g. Tarski's T-schema, good behaviour with respect to negation, a 'thin' correspondence intuition, stability *et al.*). These platitudes (on this proposal) guarantee that a certain discourse with a truth-predicate in it is assertoric (apt for truth and falsity), but leave open the question whether the concept of truth has a more robust substance. Now, some realists believe that the concept of truth has this more robust (non-epistemic) substance which is captured by a 'thick' notion of correspondence with reality, *viz.* that the *source* of the truth of theoretical assertions is worldly states-of-affairs. This notion is taken by realists to be epistemically unconstrained. Wright juxtaposes this realist notion of truth with an epistemically constrained one: 'superassertibility' ([1992], p. 48). He takes it to be the case that this epistemic notion of truth meets the minimalist requirements for a truth-predicate stated above, and then asks whether there are features of a discourse that somehow dictate that this discourse needs or implies a concept of truth stronger than superassertibility. He proposes four criteria for judging whether a discourse involves an epistemic or a non-epistemic conception of truth: extensional comparison, convergence (or Cognitive Command), the Euthyphro Contrast, and the width of cosmological role. Put in a tiny nutshell, Wright's insight is the following. It may be ([1992], p. 143) that truth (realist-style) and superassertibility are extensionally divergent notions (that is there are truths which are not superassertible and/or conversely). It may be that truth (realist-style) features in best explanations of why there is convergence-of-opinion in a discourse (pp. 146ff and 176ff). It may be (pp. 77–82) that the direction of dependence between truth (realist-style) and superassertibility is one-way only: it's because certain statements are true (realist-style) that they are superassertible and not conversely. And it may be (p. 196) that statements in a discourse play a wide cosmological role, in the sense that their truth (realist-style) contributes to the explanation of assertions and attitudes in other spheres or discourses. This (extremely compressed presentation of Wright's seminal idea) leads me to the following conjecture. Even if Wright is right in pointing out that, prima facie, scientific realists need not compromise their modesty by adopting an epistemically constrained conception of truth, the very features of the truth-predicate implicated in the assertoric theoretical discourse in science are such that it satisfies all the criteria that Wright himself has suggested as pointing towards the operation (or implication) of a (realist-style) concept of truth in a discourse. If this conjecture is right (and it needs a lot of careful thought to be substantiated), then the realist aspiration to modesty *ipso facto* suggests a substantive non-epistemic conception of truth.

[4] Quine, however, is a professed realist (see [1981], p. 92). For an exploration of Quine's views on realism, see Hylton ([1994]).

What about the second part of Wright's thesis, *viz.* that scientific realists had better adopt an epistemic conception of truth if they are to retain their epistemic optimism? The problem, I think, with this suggestion (which Wright masterly recognises and tries to meet) is that a verificationist version of scientific realism brings with it all of the problems that made verificationism discredited as a philosophical theory of meaning (and truth). In particular, its viability depends on two premises: first, that radical underdetermination of theories by evidence is a priori impossible; second, that we can make sense of an observation language which is theory-free and which is used to 'cash out' the suitable notion of verifiability. As for the first premise, it seems obvious that the very logical possibility of two or more mutually incompatible theories being empirically equivalent entails that truth doesn't *necessarily* lie within our cognitive capacities and practices. As for the second premise, if observation is theory-laden in such a way that we cannot segregate a set of theory-neutral 'observation reports', then we cannot even begin to formulate the thesis that theoretical assertions are true in the sense that they are verifiable (or even that their truth is detectable) by means of 'observation reports'.

Some realists (e.g. Devitt and Horwich) take scientific realism to be a metaphysical doctrine that asserts the existence of unobservable entities, but argue that 'no doctrine of truth is constitutive of realism: there is no entailment from one doctrine to the other' (Devitt [1984], p. 5). Here company is parted, however. Devitt ([1984], Ch. 4) argues that, in so far as a concept of truth is

involved in the *defence* of realism, it should be a correspondence account, in order to safeguard the claim that the world is independent in its existence and nature from what we believe. Horwich ([1997]), on the other hand, after declaring that the scientific realism debate is about the independence and accessibility of facts about unobservable entities, takes the view that a 'deflationary' conception of truth (which is itself lightweight and metaphysically neutral) is all that is needed for the defence of scientific realism. His core thought is that the truth-predicate doesn't stand for any complex property, but is a quasi-logical device for forming generalisations over propositions. One can of course pit Devitt's defence of correspondence truth against Horwich's deflationism. But the serious philosophical issue that nonetheless remains is Horwich's ([1997]) thesis that the scientific realism debate can be fully stated and explained without any substantive (i.e. non deflationary) concept of truth. In particular, Horwich (*ibid.*, p. 36) claims that, even when the concept of truth is explicitly used in a realist (or anti-realist) thesis, e.g. when realists say that science achieves theoretical truth, or when instrumentalists say that theoretical hypotheses are truth-valueless, or when verificationists say that all truths are verifiable, even then it can be captured by a deflationist understanding of truth. But I doubt that this is so easily established. When realists say, for instance, that theoretical discourse should be understood literally, they imply that theoretical assertions shouldn't be taken as translatable into a vocabulary that is committed only to observable states-of-affairs. The notion of translatability (or its lack) may inevitably involve reference to sameness (difference) of truth-conditions, which, arguably, are not part of the resources available to the deflationist (see Field [1992], pp. 324–25). An important issue at stake, which is quite independent of the scientific realism debate, is whether a deflationist story is explanatorily complete as an account of truth, *viz.* whether it explains everything that there is to know about the concept of truth in a discourse. Dissenting voices (e.g. Field [1992]; Papineau [1993]) have made the point that a deflationist account cannot adequately explain successful action, which at least intuitively, is taken to require that the *truth-conditions* of the beliefs that guide this action have been realised. In any case, this is a key area in which further research and argument are necessary.[5]

2.4 Conceptual independence and epistemic luck

Despite all these attempts to force a compromise on scientific realists, neither modesty nor presumptuousness has to go. It doesn't follow from the claim that

[5] Jennings ([1989]) has tried to articulate 'scientific quasi-realism' in the spirit of Blackburn ([1984]). The main thought is that quasi-realists can 'earn the right' to talk about the truth-or-falsity of theories, without the concomitant commitments to a realist ontology: the posited theoretical entities inhabit a 'projected' world. Whether this is a genuine middle way has been challenged by Fine ([1986b]) and Musgrave ([1996]).

the world is independent of human cognitive activity that either human inquirers are cognitively closed to this world or else when they come to know it, they somehow *constitute* it as the object of their investigation. Fine ([1986b], p. 151) has tried to make a meal out of this purported implication of the mind-independence claim. But I think he is mistaken. It all depends on how exactly we understand the realist claim of mind-independence. It should be taken to assert the logical-conceptual independence of the world, in the sense that there is no conceptual or logical link between the truth of a statement and our ability to recognise it, assert it, superassert it or the like. The entities that science studies and finds truths about are deemed independent of us (or of mind in general) not in any causal sense, but only in a logical sense: they are not the outcome (whatever that means) of our conceptualisations and theorizing. All this is consistent with the claim that science and its methodology are causally dependent on the world. Indeed this *causal contact* with the world is presupposed by the realists' claim that our methods of interaction with the world can be such that, at least in favourable circumstances, they can lead to the formation of warranted beliefs about the 'deep structure' of the world. Despite several pages of philosophical argumentation that this contact with the independent world is impossible because it would amount to 'getting out of our skin' (see Rorty [1991], pp. 46ff), or because it's 'conceptually contaminated' (Fine [1986b], p. 151), it's a simple truth that our (inevitably) conceptual give-and-take with the world need not lead to the neo-idealist (or neo-Kantian) thought that the causal structure of the world is a reflection (or projection) of our concepts and theories. The independence of the world needn't be compromised. And it cannot be compromised unless one adopts the implausible view that worldly entities are *causally constituted* as entities by our conceptual and epistemic capacities and practices.[6] To be sure, realists must grant that their 'epistemic optimism' that science has succeeded in tracking truth presupposes a certain *epistemic luck*: it's not a priori true that science has been or has to be successful in truth-tracking. If science does succeed in truth-tracking, then this is a *radically contingent fact* about the way the world is and the way scientific method and theories have managed to 'latch onto' it (see Boyd [1981]). So the presumptuousness of realism is a contingent thesis that needs to (and can) be supported and explained by argument that shows that the ampliative–abductive methods of science can produce theoretical truths about the world and deliver theoretical knowledge.

If neither modesty nor presumptuousness need compromising, isn't there still an issue as to how presumptuous scientific realism should be? I think we should reflect a little on what exactly the *philosophical* problem is. I take it to be the following: is there any strong reason to believe that science cannot

[6] For the defence of all this, see my ([1999], pp. 245–46) and Niiniluoto ([1999], pp. 94–95).

achieve theoretical truth? That is, is there any reason to believe that, after we have understood the theoretical statements of scientific theories as expressing genuine propositions, we can never be warranted in claiming that they are true (or at least, more likely to be true than false), where truth is understood realist-style? There are some subtle issues here (to which we shall return below), but once we see the problem in this way, it seems obvious that what realism should imply by its presumptuousness is not the implausible thought that we philosophers should decide which scientific assertions we should accept. We should leave that to our best science. Rather, presumptuousness implies that theoretical truth is achievable (and knowable) no less than is observational truth. This claim, however, may have a *thin* and a *thick* version. The thin version is defended by Leplin ([1997]). His 'Minimal Epistemic Realism' is the thesis that 'there are possible empirical conditions that would warrant attributing some measure of truth to theories—not merely to their observable consequences, but to theories themselves' (*ibid.*, 102). As Leplin is aware (p. 121), many realists opt for a thicker version. This version should imply (and be engaged in the defence of the claim) that the ampliative–abductive methods of science are reliable and can confer justification on theoretical assertions. This thick version, I think, is the enduring result of Boyd's contribution to the defence of realism.[7] But why do we need it? We need it because a 'thin' account cannot allow for rational or warranted belief in the unobservable entities posited by science (and the assertions made about them). The thin claim merely asserts a counterfactual connection between some possible empirical conditions and the truth of some theoretical assertions. This cannot be the litmus test for scientific realism because, suitably understood, it's universally acknowledged that this connection is possible. Not only does the thin claim fail to guarantee that this possible connection may be actual (a condition required for the belief in the truth of a theoretical assertion), but any attempt to give such a guarantee would have to engage the reliability of the method which connects some empirical condition with the truth of a theoretical assertion. Hence, the defence of the rationality and reliability of these methods cannot be eschewed.

To me this means that the presumptuous strand in realist thought should be thick. But others may disagree. Here there is definitely space for more discussion. One issue that needs to be explored—as hinted at by Suppe ([1989], pp. 340–6) and developed by Kitcher ([1993a], Ch. 3)—is how standard epistemological theories of justification, reliability and belief formation can be employed in the realism debate. It may turn out, as I ([1999], pp. 83–86) and Suppe ([1989], p. 352) believe it will, that the debate on scientific realism is best conducted in the context of broader externalist epistemological theories about the nature of knowledge, justification, etc.

[7] For a defence, see Trout ([1998], Ch. 3) and my ([1999], Ch. 4).

So far, we have resisted the claim that the concept of truth implicated in scientific realism should be something less than a 'correspondence with reality'. The relevant pressures have led some realists to back down (e.g. Giere [1988], p. 6). Others, however, have tried to explicate the notion of correspondence in such a way as to remove from it any sense in which it is 'metaphysically mysterious'. Of these attempts, Kitcher's ([1993b], pp. 167–69) stands out because he shows that this notion a) need not commit us to an implausible view that we should (or need to) compare our assertions with the world and b) arises out of the idea that a fit between representations and reality *explains* patterns of successful action and intervention. A correspondence account of truth is just another theory that can be judged for (and accepted on the basis of) its explanatory merits.

3 Epistemic optimism

It's hard to exaggerate the role that Sellars played in the realist turn during the 1960s. His attack on the 'myth of the given' and his endorsement of the 'scientific image', according to which what is real is what successful scientific theories posit, prioritised scientific theories over folk theories of the 'manifest image' as our guide to what there is (see Churchland ([1979]). Smart ([1963], p. 39) and Maxwell ([1962], p. 18) followed suit by offering arguments for realism based on the explanation of the success of science. If all these unobservable entities don't exist, if theoretical assertions are not well-confirmed and true descriptions of an unobservable world, then it isn't possible to explain the empirical success of science and the observation of predicted correlations among observable entities. Putnam ([1975], p. 73) turned all this into a famous slogan: realism 'is the only philosophy of science that does not make the success of science a miracle'. Hence, the well-known 'no miracle' argument for realism. To be sure, the central thought in this argument is that realist assertions offer not the only but the *best* explanation of the success of science. Be that as it may, the point of the argument is that the success of scientific theories lends credence to the following two theses: a) that scientific theories should be interpreted realistically and b) that, so interpreted, these theories are well confirmed because they entail well-confirmed predictions. The original authors of the argument, however, didn't put an extra stress on *novel* predictions, which are, presumably, the litmus test for the ability of alternative approaches to science to explain the success of science. For it is only on a realist understanding that novel predictions about the phenomena *come as no surprise*. Yet there has been notorious disagreement as to how exactly the conditions of novelty should be understood. A novel prediction has been taken to be the prediction of a phenomenon whose existence is ascertained only after a theory has predicted it. This, however, cannot be the whole

story since theories also get support from explaining already known phenomena. So it's been suggested (e.g. Worrall [1985]) that the 'temporal view' of novelty should be replaced by a 'novelty-in-use' view: a prediction of an already known phenomenon can be use-novel with respect to some theory provided that information about this phenomenon was not *used* in the construction of the theory. Yet it's been notoriously difficult to make precise the intuitive idea of 'use novelty'. A fresh analysis comes from Leplin ([1997], p. 77) who analyses 'novelty' by reference to two conditions: 'independence' and 'uniqueness'. The thrust is that a prediction of a phenomenon O is novel for a theory T if no information about O is necessary for the prediction of O by T, and if at the time T explains and predicts O, no other theory 'provides any viable reason to expect' O. If these requirements are satisfied, it's hard to see what other than the relevant truth of the theory T could best explain the novel predictions.[8]

Why, then, has the realist turn come under so much pressure? The main target of the non-realist onslaught has been realism's epistemic optimism. Note that the original Smart–Maxwell formulation of the 'no miracle' argument rested on the assumption that once semantic realism is established, belief in the truth of genuinely successful scientific theories is (almost inevitably) rationally compelling. Van Fraassen's ([1980]) reaction to this was that the abductive–ampliative methodology of science fails to connect robustly empirical success and truth: two or more mutually incompatible theories can nonetheless be empirically congruent and hence equally empirically successful. Given that at most one of them can be true, semantic realism can be retained but be accompanied by a sceptical attitude towards the truth of scientific theories. Now, realists face a dilemma. As Newton-Smith ([1978], p. 88) pointed out, realists can cling on an 'Ignorance Response' or an 'Arrogance Response'. On the first horn, realists choose to hang on to a realist metaphysics of an independent world, but sacrifice their epistemic optimism. On the second horn, they try to secure some epistemic optimism, but sacrifice the independence of the world by endorsing a view that denies that there are 'inaccessible facts' which can make one of the many rival theories true. In a way, van Fraassen's own attitude amounts to the 'Ignorance Response'.[9] As for the 'Arrogance Response', it's hard to see how one can be a realist and still

[8] Two reviewers of Leplin's book (Sarkar [1998]; Ladyman [1999]) argue that the Uniqueness Condition (UC) is too strong: given that some other theory T′ may predict O soon after T has first predicted it, why should we accept that the (accidental) historical precedence of T makes O novel for T but not for T′? In fairness to Leplin, it is crucial that T′ satisfies the Independence Condition (IC). If not, T′ is not a contender. If yes, O can be said to be prima facie novel for *both* T and T′. Then, naturally, the failure of UC cannot make O shift the epistemic balance in favour of either T or T′ and more evidence should be sought after.

[9] Van Fraassen challenges the realists' epistemic optimism. But, unlike Wright and like scientific realists, he takes scientific statements 'to have truth-conditions entirely independently of human activity or knowledge' ([1980], p. 38).

endorse it. For, to say the least, 'trimming down' the content of the world so that it contains no inaccessible facts leaves three options available (all of which should be repugnant to realists). First, to reinterpret the empirically equivalent theories so that they are not understood literally and the apparent conflict among them doesn't even arise (an option taken by some Logical Empiricists). Second, to adopt an epistemic notion of truth that makes it the case that only one of the empirically equivalent theories passes the truth-test (see Jardine [1986]). Third, to argue that all these theories are true, thereby relativizing the concept of truth (see some time-slice of Quine [1975], pp. 327–28).

Can realists eschew the 'Ignorance Response'? The gist of van Fraassen's challenge is that the explanatory virtues that are part and parcel of the abductive–ampliative methodology of science need not (and perhaps cannot) be taken to be truth-tropic. Hence, any realist hope of forgoing the 'Ignorance Response' by grounding their epistemic optimism on explanatory considerations seems to vanish. Not so fast, though. Putnam's enduring contribution to the realist cause is his thought that the defence of realism cannot be a piece of a priori epistemology, but must rather be part and parcel of an empirical-naturalistic programme which claims that realism is the best empirical hypothesis of the success of science ([1978], p. 19). Capitalizing on this thought, Boyd ([1981], [1984]) embarked on an attempt to establish the accessibility of (and rationality of belief in) theoretical truth by trying to defend the reliability of abductive–ampliative inferences. This well-known *abductive defence of realism* starts from the fact that the heavily theory-laden scientific methodology is instrumentally reliable (i.e. it yields correct predictions and is empirically successful) and argues that the best explanation of this instrumental reliability is that the background theories (which inform and dictate the methods used by scientists) are approximately true. This is a philosophical (second order) Inference to the Best Explanation (IBE) which suggests that there is a contingent (a posteriori) link between abductive–ampliative methodology (and the concomitant notion of 'best explanation') and truth. If successful, this argument grounds realists' epistemic optimism. It also removes the sting from the rival argument from the underdetermination of theories by evidence (UTE). For two empirically equivalent theories may not (as a matter of contingent fact) be equally good in their explanatory virtues. Hence one of them may well be the best explanation of the evidence and command rational belief.[10]

In any case, UTE rests on two questionable premises. (I): for any theory T there is at least another one incompatible theory T′ which is empirically congruent with T.[11] (II): if two theories are empirically equivalent, then they

[10] For a proper formulation and defence of this argument, see my ([1999], pp. 78–81).

[11] For a defence of (I), see Kukla ([1998], Ch. 5).

are epistemically equivalent too (that is, equally confirmed or supported by the evidence). Both premises have been forcefully challenged by realists. Some (e.g. Newton-Smith [1987]) have challenged (I) on the grounds that the thesis it encapsulates is not proven. Note, in passing, that realists should be happy with a local scepticism. It may turn out that some domains of inquiry (e.g. the deep structure of space-time) are beyond our ken. Others (e.g. Glymour [1980]; Boyd [1981]; Laudan and Leplin [1991]; Laudan [1996]) have objected to (II). Here there are, on the face of it, two strategies available. One (IIa) is to argue that even if we take only empirical evidence in the strictest sense of the word as bearing on the epistemic support of the theory, it does not follow that the class of the observational consequences of the theory is co-extensional with the class of empirical facts that can lend support to the theory. An obvious counter-example to the claim of co-extensionality is that a theory can get indirect support from evidence it does not directly entail. The other strategy (IIb) is to note that theoretical virtues are epistemic in character and hence can bear on the support of the theory. Here again there are two options available to realists: (IIb.1) to argue (rather implausibly in my view) that some theoretical virtues are constitutive marks of truth (e.g. McMullin [1987]); or (IIb.2) to argue for a broad conception of evidence which takes the theoretical virtues to be broadly empirical and contingent marks of truth (see Boyd [1981]; Churchland [1985]; Lycan [1988]). (IIb.2) is an attractive strategy for two reasons: a) it challenges the strictly empiricist conception of evidence and its relation to rational belief; b) it removes, if successful, the apparent tension between modesty and presumptuousness, without also forging an a priori link between theoretical virtues and truth. (IIb.2) is perhaps the most difficult position to defend, but on closer inspection it may well turn out that (IIa) and (IIb.2) are, at root, the very same strategy (see Psillos [1999], pp. 171–76).[12]

Not all defenders of realism take the abductive defence of IBE to be central in the defence of realism. There are a few specific problems here and one broadly philosophical problem. The specific problems regard the notion of explanation and the conditions under which it can be called 'best'. Some realists countenance specific forms of causal explanation (e.g. Salmon [1984] for the so-called 'common cause principle', or Cartwright [1983] for 'inference to the most probable cause') but deny that these can generalise to engender a blanket notion of IBE. Others (e.g. Lipton [1991]) try to provide (descriptively) an account of when a (potential) explanation is best and then to tell a story as to when this explanation licences inference. In the same boat, Niiniluoto ([1999], pp. 185–92) sketches a formal model of IBE in which the 'best explanation' is linked to the 'best confirmed' hypothesis, given the evidence. Finally, there are those (e.g. Miller [1987]) who argue that there

[12] For a critique of some social constructivists' reaction to UTE, see Laudan ([1996], pp. 50–53).

cannot be a general mode of inference called IBE, but instead that local ampliative inferences in science are licensed only when they are backed up by 'topic-specific truisms', that is principles which are so entrenched that no-one in the specific domain can seriously deny them. This last position, however, is sensitive to the issue of what renders these principles 'truisms' if not the fact that they have been arrived at by a legitimate application of IBE.

What I called the 'philosophical problem' of the abductive defence of realism has caused a heated discussion. It has been widely argued (see Laudan [1984], p. 134; van Fraassen [1985], p. 255; Fine [1986a], [1986b]) that the realists' *use* of (a second-order) IBE in defence of realism is circular and question-begging. For, the thought is, it takes for granted the reliability of a mode of inference which is doubted by non-realists. This challenge has led some realists to question the viability of the abductive strategy. Newton-Smith ([1989a], p. 179) for instance has called the realism associated with this strategy 'faded'. And Harré ([1988]) left behind 'truth realism' and its 'deeply flawed' abductive defence in favour of a methodological strategy that he called 'policy realism' (see also Hendry [1995]).

This issue is the focal point of the debate at present. A proper appreciation of what is at stake presupposes a better understanding of the broader epistemological agendas of the participants. As is explained in detail in my ([1999], Ch. 4), the abductive defence of realism can only proceed within a broad naturalistic framework in which the charge of circularity loses its bite, because what is sought is not justification of inferential methods and practices (at least in the neo-Cartesian internalist sense) but their explanation and defence (in the epistemological externalist sense). It's not as if the abductive defence of realism should persuade a committed opponent of realism to change sides. Strict empiricists, for instance, are not likely to be moved by any defence of IBE, be it circular or straight, precisely because as McMullin ([1994], p. 100) has noted, they simply choose to tolerate unexplained regularities and phenomena. (One such regularity is that science has been instrumentally reliable and successful.) Van Fraassen's insistence that the explanatory virtues are merely pragmatic is just a further twist to this tolerance of the unexplained. So, strict empiricists reject the abductive defence of realism not so much because it's circular (they would reject a defence of IBE even if it were straight), but mainly because they refrain from accepting the existence of unobservable entities on any grounds that transcend those which can be derived from naked-eye observations (see Salmon [1985]). But unless this attitude is itself the most reasonable to adopt (and I doubt whether it is), it doesn't follow that IBE is unreasonable.[13] Nor does it follow that the employment of IBE in an abductive defence of the reliability of IBE is question-begging and unpersuasive. Many

[13] Van Fraassen ([1989], pp. 160–70) has suggested that IBE—conceived as a rule—is incoherent. Harman ([1996]) and Douven ([1999]) have rebutted this claim.

(if not all) use *modus ponens* unreflectively as a sound inferential rule and yet an establishment of the soundness of *modus ponens* at the meta-level by an argument which effectively uses *modus ponens* can still explain to them why and in virtue of what features deductive reasoning is sound. In any case, realists vary in the extent to which they adopt an abductive defence of the reliability of IBE. There are those brazen realists, like Boyd [(1981)], Trout ([1998]) and Psillos [(1999)]who take the charge of circularity seriously and try to meet it within a naturalistic perspective. One central thought in this camp is that abduction is the only general mode of ampliative reasoning and if this fails, then no ampliative reasoning (and hence no learning from experience) is possible. Then there are the temperate realists (see Leplin [1997], p. 116) who stress that abduction and induction are distinct modes of reasoning and try to argue that IBE is no worse than ordinary inductions which are OK for non-realists. Finally, there are realists (like Brown [1994], Ch. 1) who side-step the charge of circularity and argue that the explanatory story told by realism is just more adventurous and enlightening than alternative stories.

There is also a deep empirical challenge to realism and its abductive defence: the Pessimistic Induction. As Laudan ([1984]) has pointed out, the history of science is replete with theories that were once considered to be empirically successful and fruitful, but which turned out to be false and were abandoned. If the history of science is the wasteland of aborted 'best theoretical explanations' of the evidence, then it might well be that current best explanatory theories might take the route to the wasteland in due course. Not all realists find this argument threatening. Some (e.g. Devitt [1984]) find it simply (and, I think, correctly) overstated. Others (e.g. Almeder [1992]) take a 'blind realist' stance: at any given stage of inquiry some of our theoretical beliefs are true, yet we can never tell which these are because 'we have no reliable way of determining which of our currently completely authorised beliefs will suffer truth-value revision in the future' (p. 178).[14] Yet others (e.g. Hacking ([1984])) answer the pessimistic induction by choosing to be realist about entities which can be manipulated by well-designed experiments and *not* about theories. This last view—known as *entity realism*—is criticised in my ([1999]), pp. 225–8). What about those of us who think that we should take seriously the Pessimistic Induction and try to meet it?

Although other strategies may be available, I think the best defence of realism is to try to reconcile the historical record with some form of realism. In order to do that, realists should be more selective in what they are realists

[14] Apart from noting that persistent retention at the theoretical level may be a reliable (but fallible) way to single out the theoretical assertions that won't suffer truth-value revision, one can claim that Almeder's stance falls pray to the 'preface paradox'. He ([1992], p. 180) tries to counter this claim. But a related problem still remains. A 'blind realist' asserts *both* that for each and every theoretical assertion P we can't know whether it is true *and* that we know that some Ps are true. Even if this joint assertion is consistent, the first part removes any basis for accepting the second.

about. A claim that now emerges with some force is that theory-change is not as radical and discontinuous as the opponents of scientific realism have suggested. Realists have aimed to show that there are ways to identify which theoretical constituents of abandoned scientific theories essentially contributed to their successes, separate them from others that were 'idle'—or as Kitcher has put it, merely 'presuppositional posits'—and demonstrate that those components which made essential contributions to the theory's empirical success were those that were retained in subsequent theories of the same domain (see Kitcher [1993a]; Psillos [1999]). If the relevant realist arguments are sound, then the fact that our current best theories may well be replaced by others does not, necessarily, undermine scientific realism. All it shows is that a) we cannot get at the truth all at once; and b) our inferences from empirical support to approximate truth should be more refined and cautious in that they should commit us only to the theoretical constituents that do enjoy evidential support and contribute to the empirical successes of the theory. Realists ground their epistemic optimism on the fact that newer theories incorporate many theoretical constituents of their superseded predecessors, especially those constituents that have led to empirical successes. The substantive continuity in theory-change suggests that a rather stable network of theoretical principles and explanatory hypotheses has emerged, which has survived revolutionary changes, and which has become part and parcel of our evolving scientific image of the world.

This reaction to the Pessimistic Induction was initiated by Worrall's seminal ([1989]). What he called 'structural realism' is an attempt to capitalise on the fact that despite the radical changes at the theoretical level, successor theories have tended to retain the mathematical structure of their predecessors. Worrall's thought is that theories can successfully represent the structure of the world, although they tend to be wrong in their claims about the entities they posit. Despite its initial appeal, it turns out that this particular position is very difficult to defend (see Ladyman [1998]; Psillos [1999], Ch. 7; Psillos [forthcoming]). Cartwright ([1999], p. 4) has taken a different path. She is happy to go from the 'impressive empirical successes of our best physics theories' to 'the truth of these theories', but she denies that the assertions made by these theories are universal in scope. Rather, she goes for a 'local realism about a variety of different kinds of knowledge in a variety of different domains across a range of highly differentiated situations' (*ibid.*, p. 23) which tallies with her view that the world is best seen as disunified, with no laws or principles holding across the board and across different domains of inquiry. This is certainly an issue that needs to be investigated further. Partly because it seems to be an open issue whether realism is inherently linked with the aim of unification (but see Kitcher [1999]). But mainly because, arguing as Cartwright does, for local truths which may vary from one model to another and from one domain to

another, may well involve a perspectival notion of truth which has characteristics not suitable for realism.

As is well known, realists talk of approximate truth and take science and its methods to issue in (at least most typically) approximately true beliefs. How much of a substantive concession this is is a matter or dispute. Laudan ([1984]) claims that the realist cause is doomed unless a formal semantic for approximate truth is in the offing. Giere ([1988]) concedes this but claims that realists can do well with a notion of similarity between the theoretical model and the domain to which it applies. Aronson, Harré and Way ([1994]) try to make good on the notion of similarity by devising an informal account of approximate truth that rests on the view that theories are type-hierarchies of natural kinds. Others (e.g. Niiniluoto [1999]) still think that there are good prospects for a formal (and consistent) explication of approximate truth. My own view (see [1999], Ch. 11) is that we shouldn't be deterred in our philosophical disputes by formal issues if the operative notions are intuitively clear and do not lead to paradoxes. As Smith ([1998]) has suggested, the intuitive notion of 'approximate truth' can be explicated sufficiently well to be usable along the following lines: for a statement P, 'P' is approximately true iff approximately P. This relegates much to the concept of approximation, but there is no reason to think that a domain-specific understanding of approximation is not robust enough to warrant ascription of approximate truth in statements about each domain.

Although, as we have seen, there have been extremely important and profound challenges to realism, the only articulated rival philosophical position that has emerged is van Fraassen's *Constructive Empiricism* ([1980]).[15] This view is already familiar to everyone and has been thoroughly debated in Churchland and Hooker ([1985]). Its core point is that committed empiricists cannot be forced to be scientific realists because a) they can offer an alternative account of science which takes science to aim at empirical adequacy and which involves only belief in the empirical adequacy of theories; and b) this account of science is complete in the sense that there are no features of science and its practice which cannot be accounted for (or explained away) from this empiricist perspective. Given that it is impossible to do justice to the massive literature on this subject in the present space (but see Rosen [1994] and my [1999], Ch. 9), I shall only make a general comment about the spirit of van Fraassen's approach.[16] As Miller ([1987], p. 369) nicely put it, van Fraassen's stance is a kind of modern 'principle of

[15] And, of course, Fine's ([1986a], [1986b]) quietist dismissal of the philosophical debate altogether. Fine's views are criticized in detail in Musgrave ([1989]) and my ([1999], Ch. 10).

[16] Van Fraassen ([1985], p. 255) implies that since the probability of a theory's being empirically adequate is less than or equal to its probability of being true, belief in truth is 'supererogatory'. But the above probabilistic relation between truth and empirical adequacy doesn't imply that the probability that the theory is true is not (or cannot be) high enough to warrant belief. What realists emphatically deny is that theoretical assertions are inherently insupportable. A variant of this thought is explored in Dorling ([1992], pp. 367–68).

tolerance'. Although in his ([1980]) van Fraassen can easily be interpreted as trying to show that scientific realism is an irrational attitude (and hence that constructive empiricism is the only rational attitude to science), in his later work ([1989], [1994], [2000]) he emphasizes a new conception of rationality according to which constructive empiricism is *no less* rational than scientific realism. This new conception of rationality suggests that 'what is rational to believe includes anything that one is not rationally compelled to disbelieve' ([1989], pp. 171–72). Hence, van Fraassen says, since scientific realism is not rationally compelling, and since disbelief in constructive empiricism is not rationally compelling either, constructive empiricism is an equally rational option. All this may suggest that the door to scepticism is open, since from the fact that one is not rationally compelled to disbelieve P, it doesn't follow that one has (or could possibly have) good reasons to believe P. But van Fraassen feels no threat here for he *denies* the 'sceptical' claim that 'it is irrational to maintain unjustified opinion' ([1989], p. 178). This new aspect of van Fraassen's philosophy, and his post-1990 attempt to articulate empiricism, have not yet received the attention they deserve. As an attempt to initiate this discussion, it might be possible to argue that there are tensions in van Fraassen's account of rationality. In particular, one could argue that from the fact that scientific realism is not rationally compelling it doesn't follow that constructive empiricism is no less rational an option. (Compare: from the fact that it's not rationally compelling to believe in Darwinism it does not follow that belief in Creationism is equally rational.)[17] In order, however, to substantiate this tension, we need to show at least one of the following things. Either that there are aspects of the phenomenology of science which do not make good sense under Constructive Empiricism—e.g. Friedman ([1983] pp. 236–50) and I think ([1999], p. 204) that the practice of diachronic conjunction of theories offers such a test-case. Or, that joint belief in the existence of observable middle-sized material objects and unobservables is more rational than a combination of belief in middle-sized objects and agnosticism about unobservables. This last thought has been explored by Forrest ([1994]). It's motivated by the claim that belief in the existence of unobservable entities (as opposed to agnosticism about them) rests on the same grounds as belief in the existence of middle-sized material objects (as opposed to agnosticism about them). This last claim, however, presupposes that there is no principled difference between having reasons to believe in the existence of observables and having reasons to

[17] Van Fraassen agrees with the Bayesians that the concept of rationality applies to belief-change rather than to the content of the belief. But, although he accepts that belief-revision based on conditionalization is a rational option, he thinks that 'rationality doesn't require conditionalization' ([1989], pp. 174 and 175). For a critique of van Fraassen's account, see O'Leary-Hawthorne ([1994], pp. 137–42). Kukla ([1998]) has tried to improve on van Fraassen's 'New Epistemology'. For a criticism of Kukla's 'Epistemology X', which, I think, verges on incoherence, see my ([forthcoming b]).

believe in the existence of unobservables. Despite van Fraassen's insistence on the contrary, there is a lot of sound philosophical argument that the equation of the unobservable with the epistemically inaccessible is bankrupt (see Churchland [1985]; Salmon [1985]).

4 Conclusion

In light of the preceding, I would identify the following issues as central for the years to come. First, the connection between scientific realism and the concept of truth. Second, the connection between the scientific realism debate and more mainstream epistemological theories of knowledge and justification. Third, the role of theories of explanation in a) the explication of IBE and b) in the abductive defence of realism. Fourth, the advancement and defence of a principled theory of substantive continuity in theory-change. Fifth, the assessment of van Fraassen's liberal conception of rationality. A sixth issue which I didn't touch in this survey is the claim made by van Fraassen ([1989]), Suppe ([1989]), da Costa and French ([1990]) and Giere ([1999]) that the realism debate is best conducted within the new 'semantic conception of theories'.[18]

Acknowledgements

Many thanks to Theodore Arabatzis, Hasok Chang, Chris Daly, Henk de Rogt, Igor Douven, Steven French, Barry Gower, James Ladyman, Robert Nola, David Papineau and Mauricio Suarez for their encouragement, stimulating correspondence and comments. Unfortunately, I couldn't incorporate all of their suggestions.

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[18] For a discussion of Giere's ([1999]) see my ([forthcoming c]).

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Brit. J. Phil. Sci. **51** (2000), 729–742

Interpreting Theories: The Case of Statistical Mechanics

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1

Theories are conceived, hypothesized, tested, accepted or rejected, and applied. But when they are foundational theories they are also interpreted. What on earth is interpretation? And why is it needed at all? Is it a part of ongoing scientific practice, or, rather, is it some sort of running commentary on ongoing science from the outside—the work of the philosopher rather than of the scientist? Or is it some strange hybrid of science and philosophy that doesn't fit easily into the standard goings-on of either discipline?

If we take a comparative look at what goes on when a fundamental theory is said to be interpreted, I think there is little doubt that we will agree that 'interpretation' is a family-resemblance term. There may be some chain of relevant pairwise similarities that connect all the standard cases of interpretation into a chain, but there are plainly pairs of interpretations of theories that have few interesting common elements. A very wide range of theories have been subjected to interpretive programs. But in order to focus our attention it will be best to look just at those interpretations directed toward foundational theories in physics. Even with this restriction, we shall see, there is quite a diversity of things that goes on when one is said to be offering an interpretation of one of these fundamental physical theories.

One might think of interpretation of theories as a project whose sole, or at least primary, motivation comes downward from general philosophical considerations. General considerations of semantics and epistemology lead us to disputes between 'realist' and 'instrumentalist' (or 'positivist' or 'fictionalist') interpretations of theories. Isn't the dispute about the legitimacy of positing absolute space in Newton's formulation of classical dynamics just an application to a specific instance of a far more general philosophical dispute about the justifiability, or indeed intelligibility, of propositions that make reference to the in-principle empirically inaccessible? Or, again, general questions of 'interpretation' from outside of physics lead us to puzzle over the meaning of assertions that advert to probabilities. Aren't the disputes about the interpretation of statistical mechanics just applied special cases of the general