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THE SCIENTIFIC REALISM DEBATE

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1. Introduction

A question in the philosophy of science that has engrossed the minds of many eminent thinkers is the epistemological one of what kind of knowledge, if any, science reveals of the physical world. Answers to this question are typically classified as either realist or anti-realist.[1] Structural Realism, as part of its name suggests, is a position on the realist side of the divide. In very simple terms, its advocates hold that our epistemic access to the world, so far as its non-observable part is concerned, is restricted to its structural features. The position can be traced back at least to the beginning of the twentieth century and has recently been attracting renewed interest.[2]

My main aim in this dissertation is to evaluate the structural realist answer to the aforementioned question. It seems only prudent then to devote the first chapter to an examination of the scientific realism debate. In what follows, I will delineate the boundaries of the debate, articulate the various positions and identify the protagonists. I will also sketch the main arguments and the corresponding objections and counter-objections. Finally, I will set out the main obstacles for realism. In doing so, I hope to set the stage for structural realism, explain its role in this debate as well as reveal more about the conditions of its inception and its reincarnation about a decade and a half ago.

2. The Origins and Boundaries of the Debate[3]

Arguably, the scientific realism debate did not really come into its own, i.e. was not independent from general debates about realism, until the twentieth century. The first quarter of the century was marked by a somewhat unsophisticated general realism, most memorably the critical realism of Roy Wood Sellars, formed in reaction to the rampant idealism of the nineteenth century. The logical positivists came to dominate the second quarter of the century. In view of the quantum and relativistic revolutions in physics, they found much support for their instrumentalist version of anti-realism. It was not until the 1960s, after a multifaceted attack on logical positivism, that realism was revived under the guidance of such figures as Karl Popper, Grover Maxwell, and J.J.C. Smart. At around the same time, the historically motivated work of Thomas Kuhn and Paul Feyerabend inspired new converts to, and new versions of, anti-realism. Realist voices were not kept at bay, however, with Hilary Putnam and Richard Boyd, among others, keeping the debate alive in the seventies. In the early eighties, the independent but equally powerful critiques by Bas van Fraassen and Larry Laudan shaped old problems into new challenges for the scientific realist. The

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[1] Unless otherwise noted, the terms 'realism' and 'anti-realism' will denote the more specific viewpoints of scientific realism and scientific anti-realism respectively.

[2] This widely held impression is confirmed by the recent increase in the number of publications dealing with structural realism. Note also that in the latest conference of the American *Philosophy of Science Association* (PSA 2002), structural realism was central to three out of four papers in the realism section.

[3] Detailed overviews of the debate can be found in Boyd (1984: 41-82; 2002) and Psillos (2000b).

debate as it is carried out today owes much to these developments, especially those that emerged after 1960.

The twentieth century gave birth and rebirth to a plethora of realisms and anti-realisms. The current debate is so wonderfully varied that I would be unable to justly review in one chapter, or indeed pursue in the rest of the dissertation. For this reason I will concentrate on one particular corner of the debate, something that will make my task more manageable. Three threads common to the central realist and anti-realist positions in this corner of the current debate are the following:

(CD1) There exists a mind-independent world.
(CD2) Scientific claims/sentences/statements have truth-values.
(CD3) Their truth or falsity is determinable by recourse to the mind-independent world.

These threads help circumscribe the debate. The first thread, CD1, endorses ontological realism thereby excluding positions such as traditional forms of idealism, phenomenalism, and solipsism that deny this view. Idealism holds that the world consists only of minds and/or mental states. Phenomenalism, at least in one form, can be understood in a similar way: namely as the position that the world consists only of experiences/perceptions/phenomena. Solipsism offers a more extreme description, claiming that the only thing in existence is one's own mind and mental states.

The second thread, CD2, endorses 'semantic realism'. This excludes positions such as traditional instrumentalism, the verificationist-based instrumentalism of logical positivists and fictionalism. In more detail: Traditional varieties of instrumentalism view scientific theories as means for the organisation and prediction of the observable aspects of the world and deny that they can have truth-values. Similarly, the verificationist-based instrumentalism of the logical positivists holds that only observational, as opposed to theoretical, statements are meaningful and have truth-values. The later logical positivists, who rejected the verificationist principle, argued that theoretical statements are partially interpreted and can have truth-values, all in virtue of their correspondence with observational statements.[4] Fictionalism can be thought of as a version of instrumentalism, since it holds that theories do not have a truth-value but are instead valued for their reliability or usefulness. It supposedly departs from instrumentalism in that it takes scientific theories and their ontological posits to be reliable *fictions*. How the conception of a theory or posit as a fiction differs from that as a mere tool is not all that clear.

The third and final thread, CD 3, endorses the correspondence theory of truth. It understands the notion of truth as one of correspondence between the mind-independent world and language. This excludes positions such as social constructivism and conventionalism. Social constructivists typically argue that scientific knowledge is the product of theorising, not of discovering facts about the world. Conventionalists consider the claims of science as mere agreements, whose truth is guaranteed by stipulation. While some conventionalists restrict the application

[4] Psillos (2000b) calls the instrumentalist positions that deny truth-values altogether 'eliminativist instrumentalism', and the positions that allow for truth-values but claim that the truth and meaning of theoretical statements is parasitic on those of observational statements 'reductive empiricism'.

of their view to domains like logic, arithmetic, and geometry, others apply it across the board covering, among other things, scientific claims.

I do not presume that the excluded positions are without merit, but rather choose to concentrate on a very specific, and more manageable, problem: Assuming CD1, CD2, and CD3, can science lead us to knowledge about the mind-independent world? Participants in the scientific realism debate have, by and large, sought to answer this type of question, shying away from, or at least sidelining, ontological, semantical, methodological, and ethical questions.[5] This dissertation will be almost exclusively concerned with epistemological questions.

The following two theses will help us in the formulation of realism and anti-realism:

(OT) The observable thesis: We can have knowledge of the observable aspects of the world.

(UT) The unobservable thesis: We can have knowledge of the unobservable aspects of the world.

I have left the meaning of the terms ‘observable’ and ‘unobservable’ undefined for now, since there is disagreement over this issue. In what follows, we take a closer look at each of the two opposing camps.

3. Scientific Realism

First Approximation

As a first approximation, we can represent scientific realism as the conjunction of OT and UT. More precisely, scientific realism states that we *can* have, and *actually do* have some, knowledge of the observable and unobservable aspects of the world. But what exactly do we mean by observable and unobservable? The current consensus amongst realists follows Maxwell’s landmark essay (1962), where he argues that there is a continuum from the observable to the unobservable so that no sharp distinction between them can be drawn. Maxwell also argues that what is unobservable is contingent upon factors such as the physiology of the human eye, and that for this reason we cannot demarcate the observable from the unobservable. Some of Maxwell’s arguments rely on the theory-ladenness of observation, an idea that has been advocated by Pierre Duhem ([1914] 1991), Paul Feyerabend (1962), T.S. Kuhn ([1962]1996) and N.R. Hanson (1958) among others. Though the exact meaning of this notion is contested, most agree that since observation statements are formulated in theory-specific contexts, they are to a certain degree imbued with theoretical prejudices. We shall shortly see that the theory-ladenness of observation is a double-edged sword, employed by both realists and anti-realists in their attempts to defeat one another.

Second Approximation

Another requirement of scientific realism, already pointed out under CD2, is that scientific claims have truth-values. Our rough understanding of the concept of knowledge holds that to know something is to have a justified *true* belief about it.

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[5] For a more detailed treatment of these other dimensions of the debate see Niiniluoto (1999: ch.1).

Gettier (1963) famously presented an allegedly devastating counterexample to this analysis of the concept of knowledge. In the current context, one need not get into the details of how best, if at all, to characterise the concept. All that need concern us here is the fact that having a true belief about something is a necessary condition for knowing it. To have knowledge of some aspect of the world involves the true belief that the world is in a certain state. Thus, we can express the scientific realist view that we have knowledge of (the observable and unobservable aspects of) the world by saying that scientific claims about the world are true. As a second approximation then we can represent scientific realism as the position which holds that the scientific claims about the observable and unobservable aspects of the world are true.[6]

Third Approximation

Most, if not all, scientific realists accept that the claims made by our current theories are not typically true but rather *approximately true*. In part, the realisation stems from the simple recognition that even our best theories are invariably, though to different degrees, off the mark when it comes to the production of predictions. The recent interest in this field was initiated by Popper (1963), who used the terms 'truthlikeness' and 'verisimilitude' to express the idea that one theory could stand closer to the truth than another.[7] In Popper's account theories are taken to be sets of sentences closed under deduction. According to him, the truth content of a theory *A* is the intersection between *A* and *T*, i.e. $A \cap T$, where *T* is the set of all true sentences. On the basis of this notion, he defines increased truthlikeness thus: a theory *B* is more truthlike than a theory *A* if and only if one of the following two conditions is met:[8]

(C1) $A \cap T \subseteq B \cap T$ and $B \cap F \subset A \cap F$

(C2) $A \cap T \subset B \cap T$ and $B \cap F \subseteq A \cap F$

Popper's definition of truthlikeness was short-lived, for David Miller (1974) and Pavel Tichý (1974) independently proved that under this definition a false theory could not be more truthlike than any theory whatsoever. This is an unwanted result because one of the demands for a theory of truthlikeness is to be able to compare theories that are strictly speaking false yet approximate the truth to greater or lesser extents. Since the refutation of Popper's definition a number of different accounts of the notions of truthlikeness, verisimilitude and approximate truth have appeared.[9] The most prevalent of these takes *similarity* or *likeness* as measuring distances from the truth (see, for example, Hiplinen (1976), Niiniluoto (1987), Oddie (1986)). One of the most serious problems with this approach is that comparative judgments of truthlikeness are not translation-invariant. While in one language a theory A may be more truthlike than a theory B, this relation can be reversed in another language. Various solutions to this problem have been proposed (see, for example, Tichý (1978), Oddie (1986)) but none seems to command a consensus.

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[6] For some realists this holds only of scientific claims from the most successful sciences, i.e. physics and chemistry. Others are more liberal.

[7] Niiniluoto (1999: 65) traces the etymological origin of these terms to the Latin term 'verisimilitudo', which means likeness or similarity to truth and was introduced by the ancient sceptics Carneades and Cicero.

[8] Obviously, *F* is the set of all false sentences.

[9] Note that some authors (see, for instance, Niiniluoto (1999)) assign different meanings and functions to the concepts of approximate truth and truthlikeness.

Many realists have abandoned the task of trying to give formal treatments to these notions and have instead focused on more informal accounts (see, for example, Aronson, Harré and Way (1994), Newton-Smith (1981), Smith (1998) and Psillos (1999)). Whether any such informal account delivers the goods is a contentious issue. At any rate, it is sufficient for the current purposes to note, as a third approximation, that scientific realism can be represented as the position that the scientific claims about the observable and unobservable aspects of the world are at least approximately true.[10]

General Formulation

Before we present a general formulation, we must consider one more element, namely the *aim* that scientific realism ascribes to science. According to the first part of van Fraassen's definition of scientific realism "*Science aims to give us, in its theories, a literally true story of what the world is like*" (1980: 8) [original emphasis]. Most realists are happy with this characterisation. Given the traits we have attributed to scientific realism so far, it seems hardly necessary to state that at least one of the main aims of science is to give us true/ approximately true claims about the world. It is nonetheless worth making this feature explicit in our general formulation of scientific realism:

(SCR) Scientific Realism: Science aims to produce, and has succeeded in producing, true/approximately true claims about both the observable and the unobservable aspects of the world.

This formulation captures the spirit of scientific realism. To present a more complete picture, however, we need to look at the main claims that often accompany scientific realism. In 'A Confutation of Convergent Realism', Laudan provides a list of the central claims advocated by scientific realists, correctly acknowledging that "there is probably no realist who subscribes to all of them [though] most of them have been defended by some self-avowed realist or other" (1981: 20). Here is a no-frills version of that list:

(RC1) Scientific theories in mature sciences are typically approximately true.
(RC2) More recent theories are closer to the truth than earlier ones.
(RC3) All the terms, i.e. observational and theoretical, of theories in mature science genuinely refer.
(RC4) Successive theories in mature science 'preserve' the theoretical relations and referents of earlier theories.
(RC5) New theories (do and should) explain the success of their predecessors.
(RC6) Claims (RC1)-(RC5) constitute the best, if not the only, explanation for the success of science, and this success provides empirical confirmation for realism. (1981: 20-21).

Laudan calls the conjunction of all these claims 'convergent epistemological realism', the idea being that successive scientific theories steadily converge to an ultimate and final theory that faithfully reflects reality.

[10] It might be objected that this statement needs to be restricted to mature scientific claims. Indeed, most, if not all, scientific realists adopt this restriction. This point is correct and is taken on board in the next few paragraphs. For more on the concept of mature scientific claims, I ask the reader to look at the last few paragraphs of section six of this chapter.

Having presented a general formulation of scientific realism plus a list of central accompanying claims, it would now be useful to say a few things about the main varieties of realism. Given the numerous, and usually subtle, disagreements over the claims on the above list, it would prove cumbersome to use the list as a point of departure.[11] However, we can make a rough and ready distinction between *total realism* and *partial realism*.[12] Contra total realism, partial realism imposes a distinction between those kinds of theoretical components that can represent some aspect of the world and those that cannot.[13] By 'kinds' I here mean the general classificatory schemes employed to systematise science, i.e. entities, laws, etc. Under the banner of total realism we can place philosophers such as Richard Boyd, Philip Kitcher, Jarrett Leplin, W.H. Newton-Smith, Ilkka Niiniluoto, and Stathis Psillos.[14] Under the banner of partial realism we can cite Nancy Cartwright, Ronald Giere, Ian Hacking, Rom Harré, Ernan McMullin, John Worrall, and Elie Zahar.[15]

We can cut deeper into partial realism by asking the question, '*What* is it that the partial realist claims we have knowledge of?' Ian Hacking and Nancy Cartwright, for example, are realists about entities, claiming agnosticism about theories. According to Hacking's influential account, hypothetical entities become real "[w]hen we use them to investigate something else" (1982: 1165). His prime example concerns PEGGY II, a polarising electron gun, built according to our knowledge of the causal properties of electrons. When the gun was successfully used to discover the first known example of parity-violation in a weak neutral current interaction, Hacking maintains, we gained further evidence to believe in the reality of electrons.

Similarly, Cartwright (1980) has argued for a realist attitude towards the causes of phenomena, which, at least in this case, involves realism about the entities that feature in causal accounts. It is the fundamental laws of physics, according to her, that we should be wary about since "to the extent that they are true, [they] don't explain much" (867). In Cartwright's view, the fundamental laws of physics work well, and are considered approximately true, in controlled laboratory experiments. But they do not, and according to her cannot, be taken to be true of or explain what goes on in the world outside the laboratory. Outside, the laws need to be augmented by additional assumptions and auxiliaries to be able to model anything; and even then they under-perform in their predictive and explanatory power when compared to what they can achieve in a laboratory. Worse still, they are often completely inapplicable. Having painted this bleak picture, Cartwight argues against fundamental physical laws and in

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[11] Leplin (1984: 1-7) attempts to go down this path with a similar list but the result, though somewhat informative, is rather convoluted.

[12] Similar distinctions have been put forward by others. Ilkka Niiniluoto, for example, distinguishes between critical realism and critical half-realism (1999: 12). Arthur Fine (1998) identifies piecemeal realism in a manner similar to Niiniluoto's critical half-realism.

[13] Some total realists, like Philip Kitcher and Stathis Psillos, draw their own distinctions between those theoretical components that we should believe in and those that we should not. Their distinction does not make them partial realists in the sense explained above, for it does not discriminate between *kinds* of theoretical components. For example, they do not advocate belief only in laws but not entities, or vice-versa, like partial realists do.

[14] For further reference see Boyd (1990), Kitcher (1993), Leplin (1984), Newton-Smith (1989), Niiniluoto (1999), and Psillos (1999).

[15] See, for example, Cartwright (1983), Giere (1988), Hacking (1982), Harré (1988), McMullin (1984), Worrall (1989), and Zahar (2001).

favour of the reality of entities that feature in more localised causal interactions. Her best-known counter-example to the explanatory power of fundamental laws of physics is the intractable dynamics of a thousand dollar-bill floating around in St. Stephen's Square in Vienna.[16]

The other major type of partial realism is structural realism. John Worrall and Elie Zahar, for instance, are realists about structures, i.e. typically laws of nature represented by mathematical equations, claiming that theoretical posits and non-structural parts of theories alike are suspicious. Since the next chapter is devoted to an explanation and elaboration of structural realism, I will restrict my comments here to the prima facie incompatibility between entity realism and structural realism. If entity realists remain agnostic with regard to theories/fundamental laws, which presumably includes structures, and structural realists remain agnostic with regard to theoretical posits, then obviously the two positions can hardly disagree more. Niiniluoto (1999: 139) goes as far as to call them 'diametrically opposite'. I think that his ruling may be a bit premature. Despite their professed aversion towards theory, entity realists make allowances for some, low-level, theory. Hacking, for example, appeals to 'low-level causal properties', which, no matter how much glazing he puts on them, are simply theoretical properties. Similarly, as we shall see in the next chapter, structural realism does not reject knowledge of entities but rather restricts such knowledge to their structural features.[17]

4. Arguments in Support of Realism

Over the years many arguments have been proposed in favour of realism. Of these, few have carried as much weight as the *No Miracle* and *Inference to the Best Explanation* arguments. The following is a brief exposition of the principal claims involved in these arguments as well as objections raised against them.

The argument that came to be known as the 'no miracle argument' (NMA) was independently proposed by J.J.C. Smart (1963) and Hilary Putnam (1975). According to the NMA, scientific realism is the *only* view that does not make the success of science a miracle. Given the empirical success of scientific theories, it would be a coincidence of almost cosmic proportions or a miracle if they were not at least approximately true. The tacit assumption underlying the NMA is that most of us are unwilling to accept that the success of science is a miracle. We thus opt for the purportedly only alternative, scientific realism.

It could be objected that the NMA poses an unfair dilemma: either uphold scientific realism or consider the success of science a miracle. The second disjunct is generally accepted as not really an option. Indeed, van Fraassen (1980: 39-40) concedes that we might need to account for the success of science, but denies that the only or best account is scientific realism. To support his point he makes an analogy between the practice of science and the theory of evolution. Scientific theories also struggle for survival with only the 'fittest', i.e. most successful, surviving. These, van Fraassen

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[16] See her (1998: 28). Cartwright correctly credits Otto Neurath with the example.

[17] Chakravartty (1998) goes as far as to say that the two views, when properly construed, entail each other. Though I do not find this claim convincing, I agree with the general idea that entity and structural realism can be harmonised with one another without much difficulty. For more on this see my review (2003a) of Niiniluoto's book.

says, need not be true or approximately true but they need to be *empirically* successful.[18]

In order to strengthen arguments like the NMA, realists oftentimes emphasise the importance of novel predictions.[19] It is argued, for example, that scientific realism best accounts for the *novel* success of science. A prediction is novel, according to the most basic notion of novelty, if the phenomenon predicted was not known to have existed prior to the theory's prediction of it. This is often called *temporal novelty*. More sophisticated notions have been proposed over the years. Elie Zahar (1973), for example, has proposed the notion of *heuristic novelty*, also called *design novelty*, to convey the idea even if a phenomenon *P* is known prior to the inception of a theory *X*, its prediction by *X* will be novel provided that *P* was not used in the construction of *X*. More generally, so long as a body of evidence was not used in a theory's construction it counts as heuristic-novel.[20] Newton's gravitational theory is a case in point. Although the precession of the equinoxes was known to Newton, his theory was not constructed using this phenomenon. According to the notion of heuristic novelty, the subsequent prediction of the phenomenon using Newton's theory counts as novel.

Many philosophers believe that the concept of *inference to the best explanation* (IBE) is due to C.S. Peirce, who introduced it under the name of 'abduction'. What certainly is uncontestable is that a century later Gilbert Harman (1965) branded this type of reasoning 'inference to the best explanation'.[21] The idea behind IBE is simple and intuitive, its use abundant in scientific practice. If a theory X *explains* some evidence better than any of its rivals, then it is reasonable to choose X over the others. IBE is thus essentially comparative in nature, with explanatory merits as the adjudicating force.[22] This much seems trivial. More contentiously, many realist supporters of IBE have argued that we should not merely choose X over its rivals but that we should believe in the *truth* or *approximate truth* of X.

It is not hard to see how this largely methodological concern has been hijacked for the epistemological concerns of the scientific realism debate. Boyd and Putnam, in particular, are credited with developing an IBE-based explanationist defence of realism that has come to dominate the realists' arsenal.[23] Their argument is that the empirical success of science, not just a body of evidence, requires explaining. The best, indeed the only, explanation for this success, according to them, is realism.[24]

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[18] To understand this argument properly one needs to know more about van Fraassen's take on the epistemological status of scientific theories. This task will be taken up in the coming sections. In the meantime it is important to note that van Fraassen's evolution analogy is criticised in, among other places, Brown (1994: 6-7).

[19] There is a thriving literature on this topic. Some notable articles include Worrall (1985), Mayo (1991), and Achinstein (1994).

[20] John Worrall offers a notion of *use-novelty* that is a development of Zahar's notion (see the former's (1985) and especially his (2002)).

[21] In Peirce's work, abduction is more general than inference to the *best* explanation; it is inference to *some* explanation.

[22] Having said that, I don't think that proponents of this view would be alarmed if someone pointed out the fact that a lot of theories have no extant rivals. In reply, they would probably say that if it is a theory of a mature science and it explains the data, it should still be considered as true or at least approximately true. For them, being the sole contestant just means that it is the only one that explains the data, and in that respect the best available theory.

[23] Not all scientific realists in fact accept the explanationist defence (see Newton-Smith (1989)).

[24] Psillos (1999: 71) argues that realism is the *best* rather than the *only* explanation of science.

They thus see the NMA as an instance of IBE. That is, it is inferred that the success of science is not due to a miracle but rather to the truth/approximate truth of the theories employed. In fact, Boyd, Putnam, and more recently Psillos, treat scientific realism as a scientific hypothesis, whose support comes from the view that it is the only viable explanation of the methodological success of science.[25]

The most thorough study of IBE thus far has been that of Peter Lipton (1991). He compares IBE to various traditional inferential devices, such as the 'instantial model of inductive confirmation' and the hypothetico-deductive model, arguing that IBE overcomes some of their shortfalls. Not only is IBE better than the competition, according to Lipton, but it also "gives a natural description of familiar aspects of our inferential procedures" and "has a number of distinctively philosophical applications" (66,70). IBE is not a monolithic concept. Lipton identifies a range of IBEs, of which he singles out inference to the loveliest potential explanation (see ch.4). This is contrasted to inference to the likeliest potential explanation, the loveliest explanation offering the most understanding while the likeliest being best supported by the evidence. Lipton prefers inference to the loveliest potential explanation because he thinks that explanatory loveliness can be a guide to likeliness and that our inference making becomes less interesting the more we restrict the role of explanatory virtues.

Both IBE and the explanationist defence of realism have been criticised on numerous grounds. The most common objection is that using IBE to choose one theory over existing rivals guarantees neither the theory's truth nor its approximate truth. After all, the pool may contain only false theories. This objection has, in turn, been used to argue that the explanationist defence of realism is question-begging (see, for example, van Fraassen (1985)). Given that non-realists do not accept IBE in science, the argument goes, there is no basis to accept the (meta-level) explanationist defence of realism. Van Fraassen, in particular, offers an alternative account according to which "we are always willing to believe that the theory which best explains the evidence, is *empirically adequate*" (1980: 20). He thus uses IBE, originally brought into the debate to support the realist, to make an anti-realist inference, namely that an explanatory better theory is empirically adequate.

5. Scientific Anti-Realism

All anti-realists, not surprisingly, share a distrust of, or scepticism towards, realist claims. Just like realism, anti-realism can be found in various forms and guises. With regard to scientific knowledge, the general anti-realist intuition is that we cannot know whether any of the claims made by scientific theories about the mind-independent world are true or approximately true. As a consequence, anti-realists consider the realist claims RC1-RC6 unwarranted. In particular, they denounce the realists' principal claims that theories are increasingly approximating the truth and that the theoretical terms in currently successful theories refer, i.e. the entities alleged to exist by these theories really do exist.

As previously indicated, given the assumptions that I set out in the beginning of this chapter, i.e. CD1, CD2 and CD3, only one anti-realist position qualifies as an alternative to realism. I am referring to constructive empiricism, the position that is

[25] Boyd calls this the 'abductive strategy' which he contrasts with a similar approach that he calls 'local explanationism'. For more see his (2002: 7-9). Notice that the so-called abductive strategy is similar to Laudan's claim RC6.

widely thought of as the main anti-realist competitor in this debate. In what follows I take a look at the main tenets of constructive empiricism.

Constructive Empiricism

The view identified as 'constructive empiricism' is the brainchild of Bas van Fraassen. It shares some features of the older instrumentalism, but it diverges from it in at least one important respect. As van Fraassen is at pains to point out, constructive empiricism insists on a literal construal of the language of science. In short, theoretical statements are understood as having truth-values. The catch, however, is that we cannot find out what truth-values theoretical statements have. We can only assign truth-values to observational statements. That, according to him, is enough to present science as a rational process.

In line with logical positivists, but against realists, van Fraassen supports a distinction between observables and unobservables. To be precise, he lambasts the use of expressions such as 'observational vs theoretical dichotomy' and 'theoretical entity', saying that these are examples of category errors. Entities are observable or unobservable, while terms and concepts are theoretical. This clarification, argues van Fraassen, leads to two important questions: 1) Is language divisible into theoretical and non-theoretical parts? 2) Are objects and events divisible into observable and unobservable ones? He answers the first negatively by appeal to the idea that our language is thoroughly theory-laden. He answers the second affirmatively in saying that though the term 'observable' is a vague predicate, just like most predicates in natural language, "it is usable provided it has clear cases and clear counter-cases" (1980: 16). He goes on to say that seeing with the naked, i.e. unaided, eye is a clear case of observation whereas 'seeing' particles in a cloud chamber is a clear counter-case.

Constructive empiricism is offered as an epistemologically frugal view that can nonetheless make sense of science. More precisely, constructive empiricism is the view that "*science aims to give us theories which are empirically adequate; and acceptance of a theory involves a belief only that it is empirically adequate*" (12) [original emphasis]. One evident difference between van Fraassen's position and realism is the replacement of the criterion of *truth* with that of *empirical adequacy*. What exactly is empirical adequacy and why should we prefer it to truth? The answer to the first part of the question is that a theory is empirically adequate when everything it asserts about the *observable* world is true. Echoing Duhem's phrase 'saving the phenomena', van Fraassen argues that a theory is empirically adequate if it saves the phenomena. The answer to the second part of the question is that the criterion of empirical adequacy is less demanding (and presumably more warranted) than the criterion of truth, for it requires theories to make true assertions *only* about the observable aspects of the world. In other words, Van Fraassen rejects UT.

Discussions of the merits and drawbacks of constructive empiricism can be found in abundance.[26] Many of the objections raised against it are directed at the notion of empirical adequacy. John Worrall (1984) and Alan Musgrave (1985), for example, have independently argued that if a theory is to be empirically adequate in van

[26] Churchland and Hooker (1985) contains a collection of essays on constructive empiricism including a reply from van Fraassen.

Fraassen's sense, then it must save all the phenomena, not just those actually observed so far. But since we can never have access to all the phenomena, we will never be warranted in accepting a theory as empirically adequate. Many other objections are directed at the observable-unobservable distinction. It has been argued, for example, that the selective scepticism that van Fraassen advocates cannot really be upheld since it is based on an arbitrarily drawn distinction (see Paul Churchland (1982) and Gary Gutting (1983)).

6. Arguments in Support of Anti-Realism

Two arguments that have a venerable history supporting anti-realism are: 1) the underdetermination of theory by evidence and 2) the damning historical record of science.

Underdetermination of Theory by Evidence

Though currently found in various formulations, the main idea behind the underdetermination of theories by evidence (UTE) is, roughly speaking, that for any given body of evidence there are infinitely many competing theories that can 'accommodate' it, so that the evidence cannot uniquely determine a scientific theory.

That the inference from the evidence to the theory is not deductively valid is an age-old idea. One prominent advocate is David Hume. Hume famously argued that no matter how many occurrences of an event we observe, we cannot derive a universal statement from them. This has come to be known as the problem of induction.[27] A similar idea that has been around since (at least) the late nineteenth century concerns the fitting of curves. It is a matter of fact that infinitely many curves pass through any finite number of points. The analogy with UTE should be obvious, i.e. infinitely many theories can accommodate the same (inevitably finite) body of evidence.

A related, though distinct, idea was put forth by Pierre Duhem ([1914] 1991). He argues that confirmation is a holistic affair. More precisely, he argues that a hypothesis can never be tested in isolation, since it cannot produce testable predictions without auxiliary assumptions. Put differently, a counterinstance falsifies the whole conjunction (i.e. hypothesis plus auxiliaries), leaving us uncertain about which of the conjuncts are to blame. Duhem's thesis was subsequently revived, though arguably in a different guise, by W.V. Quine (1951). He has proposed the stronger argument that any hypothesis in our web of beliefs can always be saved by adjusting the web to accommodate evidence that was previously thought of as negative.[28]

UTE supports anti-realist accounts in that it holds that no matter how much evidence we amass we will always have infinitely many theories to choose from, i.e. we will never be able to uphold any one theory as the true one. We can formulate a constructive empiricist version of UTE:

[27] Nelson Goodman (1965) presents concrete examples of how induction can fail to pick the right theory. See Colin Howson (2001) for one of many alleged solutions of the problem.

[28] Donald Gillies (1993) argues that Duhem's thesis differs from Quine's thesis. Carl Hoefer and Alexander Rosenberg (1994) point out the differences between underdetermination and what has come to be known as the 'Duhem-Quine thesis'.

(UTE-CE): For any given body of observational evidence there are infinitely many *empirically equivalent* theories that diverge on their theoretical claims.

Though it is not uniquely associated with constructive empiricism, the concept of empirical equivalence features centrally in it. We say that two or more theories are empirically equivalent when they entail the same observational consequences.[29] To remind the reader, constructive empiricism urges belief in a theory's empirical adequacy, i.e. roughly speaking belief that only the observational consequences of the theory are true. UTE-CE supports constructive empiricism for it holds that no observational evidence will ever allow us to find out which theoretical claims are true or approximately true. Consequently, UTE-CE upholds the belief that only the observational consequences of the theory can be shown to be true.

Given the gravity of these allegations, it is not surprising that the many UTE variants have come under heavy fire (see, for example, Clark Glymour (1980)). In a landmark article, Laudan and Leplin (1991) have objected, among other things, that the notion of empirical equivalence is not well defined. Even if we ignore this, they argue, we can still choose between empirically equivalent theories because: (1) a theory is not necessarily supported by the empirical consequences it entails and (2) a theory can be supported by evidence that it does not itself entail. The second point can be interpreted in one of two ways: (2a) a theory can be supported by empirical evidence over and above the evidence it entails and (2b) a theory can be supported by extra-empirical evidence, namely by considerations of economy, simplicity, unity, explanatory worth, etc. Whether such considerations are epistemically relevant is the object of debate. Moreover, what counts as evidence for a theory can have a tremendous impact on the efficaciousness of the above claims and, by extension, on UTE and the debate as a whole. In all, the realists hope to show that there are justifiable methods through which we can choose between empirically equivalent theories.

The Damning Historical Record of Science

At the beginning of the twentieth century, Pierre Duhem and Henri Poincaré made a compelling case that the history of science is punctuated by the overthrow of hitherto successful theories.[30] The logical positivists, who inherited much from both Duhem and Poincaré, largely ignored historical considerations. The result was a pervasive, though tacit, assumption that scientific knowledge was at once both cumulative and progressive.

It was not until the 1960s that this assumption was genuinely brought into question. Thomas Kuhn ([1962]1996), Paul Feyerabend (1962; 1965), and many others reinstated the point made earlier by Duhem and Poincaré and reinforced it with historical case studies. Kuhn, in particular, argued that defining moments such as the Copernican, Newtonian and Einsteinian revolutions, bring about a shift in paradigm that replaces old concepts and theories by radically new ones.[31] The meanings of theoretical concepts belonging to competing paradigms are so radically different, Kuhn argues, that it is impossible to compare either the paradigms or the concepts, let

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[29] For a somewhat different notion of empirical equivalence see Quine (1975).
[30] See the next chapter for details.
[31] Put simply, a paradigm consists of one or more theories, auxiliary hypotheses, heuristic models, ontological assumptions and methodological principles.

alone support the view that there is some continuity between them.[32] This has come to be known as the 'incommensurability thesis'.

Indeed, Kuhn avoids the notions of truth and approximate truth altogether, opting instead for an account of progress that views science as a problem-solving endeavour. Given incommensurability, the argument goes, there is no common ground from which to judge the goals of the competing theories and, therefore, scientific theories cannot be said to be increasingly approaching the truth. The notion of incommensurability is often intertwined with that of the theory-ladenness of observation. Since observation is theory-laden, the anti-realist argues, it cannot serve as independent ground upon which rival theories can be judged. In sum, Kuhn claims that theory change involves radical shifts in which essential theoretical components including central theoretical terms are thrown away and thus that scientific knowledge is neither cumulative nor progressive towards the truth.

Even though arguments based on the historical record of science were originally launched against logical positivist instrumentalism, an anti-realist position, they have since become the staple of anti-realists in their attempts to bring down realism. At stake are the realist claims on the above list. RC4, for example, is in direct conflict with the historical arguments, for the latter undermine the claim that successive theories in mature sciences preserve at least some of the theoretical relations and referents of earlier theories – notably the central ones.

The realist reaction to these early historical arguments has followed one of two strategies. On one strategy the realists have launched an offensive against the notions of scientific revolution, paradigm, and incommensurability, claiming that they suffer from vagueness (see, for example, Dudley Shapere (1964) and Lakatos (1970)). Lakatos' 'methodology of scientific research programmes', in particular, replaced the concept of paradigm with that of scientific research programme, characterising the latter in ways that would support a more rationalist outlook towards theory change in the history of science.[33] On the other strategy realists have contested the anti-realist points on historical grounds (see, for example, Richard Purtill (1967)).

A more sophisticated version of the historical argument has been put forward by Laudan (1977; 1981). Laudan criticises the use of connections between reference, approximate truth, and success in support of the explanationist defence of realism as tenuous. More precisely, he argues that the predictive and explanatory success of a theory guarantees neither its approximate truth nor that its central theoretical terms genuinely refer. The available historical evidence, according to him, clearly shows a repeated overthrow of scientific theories as false and their referents as not genuinely referential, despite explanatory and predictive success. Laudan cites the following long list of theories as evidence for his claim: the crystalline spheres of ancient and medieval astronomy, the humoral theory of medicine, the effluvial theory of static electricity, the 'catastrophist geology', the phlogiston theory of combustion, the caloric theory of heat, the vital forces theories of physiology, the electromagnetic ether, the optical ether, the theory of circular inertia, and the theories of spontaneous

[32] The implicit assumption here is a descriptive theory of reference according to which a theoretical term *t* refers to an entity *a* if and only if *a* satisfies the theoretical (i.e. descriptive) claims made by the scientific theory employing *t*.

[33] Lakatos presented his work as a synthesis of some of Kuhn's and Popper's ideas.

generation (1981: 33). This argument, thus, challenges the realist claims RC3 and RC4.[34]

Implicit in Laudan's argument is the so-called 'pessimistic induction' (PI).[35] Laudan argues that, given the historical evidence, the inference from explanatory and predictive success to approximate truth and successful reference is unwarranted. Thus construed, the argument is a modus tollens, not an induction (see Lyons (2002)). However, one can read this argument as an induction. That is, given the historical evidence that past successful theories were abandoned as false and referentially unsuccessful, we can inductively argue that current or even future theories will also succumb to the same fate. This reasoning employs historical evidence to argue, inductively, for pessimism with regard to the approximate truth and referential success of our theories.

Though some realists have largely ignored the pessimistic induction, many more have taken it seriously. Some of these have attacked the argument itself (see, for example, Hardin and Rosenberg (1982), Psillos (1996), and Devitt (1984: 143-9)). Others have engaged in historical case studies in an attempt to show that the historical record can be reconciled with scientific realism (see, for example, Worrall (1989; 1994), Kitcher (1993), Psillos (1999: ch.6)). This last move usually involves showing that abandoned theoretical components are not essential for the explanatory and predictive success enjoyed by the theories they were embedded in. In other words, the theoretical components that survive theory change are those that are responsible for the abandoned theories' successes.

In their fight against historical arguments the realists have appealed to the notion of *mature science*. By categorising those theories that have been abandoned in their entirety as belonging to an insufficiently developed or immature science, the realists hope to restore the cumulativity of scientific knowledge. The distinction between mature and immature science is appealing on independent grounds because many would like to draw a line between the early primitive/undeveloped stages of a given science and the latter stages where the science presumably begins to blossom.[36] Many, for instance, would find Aristotelian physics or the Ptolemaic systems of astronomy unworthy of even being called proto-science.[37] Boyd (1984) and Putnam (1978) cite

[34] Laudan does not stop there. Like many others, he accuses the realists of failing to provide a semantical and epistemological characterisation of the notion of approximate truth, holding that this makes RC1 and RC2 'so much mumbo jumbo' (1981: 32). He also questions RC5 saying that a theory might be better supported than its rivals yet not be able to explain why its rivals were successful (47). Given his distrust of all these claims, i.e. RC1-RC5, he thinks that RC6, which relies upon them, cannot be upheld.

[35] Though Laudan's (1981) argument is now widely known as the 'pessimistic induction' argument, it has been pointed out (see Timothy D. Lyons (2002)), that this argument is only present in his (1977). The argument has also been put forward, independently on the face of it, by Putnam, who says that "…eventually the meta-induction becomes compelling: *just as no term used in the science of more than fifty* (or whatever) *years ago referred, so it will turn out that no term used now* (except maybe observation terms, if there are such) *refers*" (1978: 25) [original emphasis]. It is worth noting that the argument is also called 'pessimistic meta-induction'. Obviously the 'meta' refers to the fact that it is *about* science and its inductive methods, rather than *within* science.

[36] A similar distinction is utilized to demarcate science from religion.

[37] Michael Friedman, however, suggests that even Aristotelian physics has handed down some approximately correct theoretical components (see his (2001)).

the phlogiston theory of combustion as another example of an immature science – in this case chemistry.

The concept of maturity is notoriously elusive. Laudan complains that the vagueness besetting the concept risks making the realist claims RC4 and RC5 vacuously true because theories that have not bequeathed anything to their successors can always be branded 'immature'. One way to anchor the concept is by attaching a condition of genuine predictive success to it. That is, unless a theory is explanatorily and predictively successful, it will not count as mature. Yet, even this is not enough to save the realists from the clutches of history. This is made obvious by Laudan's list, which specifically targets theories with genuine empirical success that were subsequently abandoned nonetheless.

Worrall has pressed for a more refined notion of mature science arguing that "[t]his must mean more than simply having correct empirical consequences" (1989: 153). His suggestion is that a science reaches maturity only when its theories can predict entirely novel types of phenomena. Chief amongst his examples is Fresnel's theory of light. The theory unexpectedly and correctly predicted a bright spot at the centre of the shadow of an opaque disc that was lit from a single slit. Though this theory appears on Laudan's list, Worrall argues, the essential part of the theory, namely Fresnel's equations, were preserved through theory change.

Whether Worrall's notion of maturity saves the realist from the allegedly embarrassing historical record is an issue that has yet to be taken up. Prima facie, it seems to me that his criterion is too strict in that it could eliminate approximately true theories that do not make any predictions of novel types of phenomena. One need only consider that a successor to a given theory may be closer to the truth simply on account of accuracy, and not by predicting new types of phenomena.

7. The Main Realist Obstacles

Given the current state of the debate, we can easily identify the main obstacles realists have to overcome if they are to make any progress. With little or no doubt, the following four are the most talked about and presumably most important obstacles for the realist in the current debate:

(RP1) We must be able to choose between empirically equivalent theories. That is, we must be able to show that from a pool of empirically equivalent theories at least some are more epistemically warranted than others.

(RP2) The historical record of science must be accounted for somehow. It must be shown that at least some components of theories, other than observational consequences, survive theory change, and that only those that survive were responsible for the success of a given theory.

(RP3) It must be shown, or at least it is preferable to show, why the success of science needs explaining and, furthermore, why scientific realism provides a better explanation than any alternative position.[38]

[38] Not all realists think that the success of science needs explaining. Worrall is one such realist.

(RP4) The notions of approximate truth, truthlikeness and verisimilitude need to be given rigorous characterisations. If no adequate formal treatments can be given, as indeed conceded by some realists, more robust informal accounts as well as the reasons why such accounts would work need to be clearly explained.

This dissertation will investigate whether structural realism can overcome RP1 and RP2. RP3 and RP4 are thus purposely bracketed. To include RP3 and RP4 into my investigation would mean either to excessively expand the dissertation or to cover one or more of the four obstacles only superficially. I think that RP1 and RP2 are sufficiently independent to be able to be addressed without first addressing RP3 and RP4. Regarding RP3, I will assume that the success of science needs some explanation, or, at least, some accounting for. Though I do not aim to provide a thorough answer to the question why scientific realism and in particular structural realism offers a better explanation/account of this success than anti-realism, some of my arguments will be supportive of this view. Regarding RP4, I will rely, like so many other philosophers I mentioned earlier, on some informal understanding of the notion of approximate truth. I do not assume that this informal understanding is sufficient for a realist programme. That is an issue that needs to be investigated thoroughly but not in this dissertation.

8. Conclusion

Arthur Fine (1984) has suggested an alternative to both realism and anti-realism, which he has called the 'natural ontological attitude' (NOA) and which he classifies as non-realism. According to him, NOA is a deflationary attitude that does not seek to impose a 'general interpretive scheme' on science. Unlike realism and anti-realism, for example, NOA does not set any aims, like truth or empirical adequacy, for science. So much for what NOA is not. What about its positive dimensions? Fine claims that NOA is to be equated with what he calls the 'core position', i.e. that which is common to both realists and anti-realists. The 'core position' is simply the view that the results of scientific enterprise are true. The realist and anti-realist positions are 'unnatural', according to Fine, because they add metaphysical theses about the character of truth and reality to the core position.

On the basis of the above, Fine has called for a dismissal of the whole debate pronouncing realism dead. His call has not been heeded however. What is more, NOA has been rightly criticised for its failure to adequately distinguish itself from realism. It has been argued, for example, that NOA is just realism in disguise, for it accepts something that anti-realists like van Fraassen reject, namely the truth of scientific claims about the unobservable world. That is, against Fine's claims, NOA cannot be equated with the common core.

Despite Fine's dismissal, many philosophers believe that at least some headway can be made in the debate. What seems evident from the elaboration of the debate offered in this chapter is that the central arguments are now more sophisticated than fifty or a hundred years ago. That, of course, does not necessarily mean that we are progressing towards a resolution of the debate. Rather, it, at least, means that a lot of interesting tools have been discovered or invented in the process. Indeed, some of these contributions have been made by structuralists, and, in particular, structural realists. It is to the historical and conceptual development of structural realism that I turn to next.