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VII

STRUCTURAL REALISM: THE BEST OF BOTH WORLDS?

JOHN WORRALL

Presently accepted physical theories postulate a curved space-time structure, fundamental particles, and forces of various sorts. What we can know for sure on the basis of observation, at most, are only facts about the motions of macrosopic bodies, the tracks that appear in cloud chambers in certain circumstances, and so on. Most of the content of the basic theories in physics goes 'beyond' the 'directly observational'—no matter how liberal a conception of the 'directly observational' is adopted. What is the status of the genuinely theoretical, observation-transcendent content of our presently accepted theories? Most of us unreflectingly take it that the statements in this observation-transcendent part of the theory are attempted descriptions of a reality lying 'behind' the observable phenomena: that those theories really do straightforwardly assert that space-time is curved in the presence of matter, that electrons, neutrinos, and the rest exist and do various funny things. Furthermore, most of us unreflectingly take it that the enormous empirical success of these theories legitimizes the assumption that these descriptions of an underlying reality are accurate, or at any rate 'essentially' or 'approximately' accurate. The main problem of scientific realism, as I understand it, is that of whether or not there are, after reflection, good reasons for holding this view that most of us unreflectingly adopt.

There are, of course, several anti-realist alternatives on offer. The most widely canvassed is some version of the pragmatic or instrumentalist view that the observation-transcendent content of our theories is not in fact, and despite its apparent logical form, *descriptive* at all, but instead simply 'scaffolding' for the experimental laws. Theories are codification schemes; theoretical terms like 'electron' or 'weak force' or whatever should not be taken as even intended to refer to real entities, but instead as fictional names introduced simply to order our experimental laws into a

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system.[1] A more recent anti-realist position—that of van Fraassen—holds that theortical terms *do*, at any rate purportedly, refer to real entities (and are not, e.g., simply shorthand for complex observational terms), but that there is no reason to assume that even our best theories are true nor even 'approximately' true, nor even that the *aim* of science is to produce true theories; instead, acceptance of a theory should be taken to involve *only* the claim that the theory is 'empirically adequate', that it 'saves the phenomena'.[2]

I can find no essentially new arguments in the recent discussions (see Worrall 1982). What seem to me the two most persuasive arguments are very old—both are certainly to be found in Poincaré and in Duhem. The main interest in the problem of scientific realism lies, I think, in the fact that these two persuasive arguments appear to pull in opposite directions: one seems to speak for realism and the other against it; yet a really satisfactory position would need to have both arguments on its side. The concern of the present paper is to investigate this tension between the two arguments and to *suggest* (no more) that an old and hitherto mostly neglected position may offer the best hope of reconciling the two.

The main argument (perhaps 'consideration' would be more accurate) likely to incline someone towards realism I shall call the 'no miracles' argument (although a version of it is nowadays sometimes called the 'ultimate argument' for realism—see Musgrave 1988). Very roughly, this argument goes as follows. It would be a miracle, a coincidence on a near-cosmic scale, if a theory made as many correct empirical predictions as, say, the general theory of relativity or the photon theory of light *without* what that theory says about the fundamental structure of the universe being correct or 'essentially' or 'basically' correct. But we shouldn't accept miracles, not at any rate if there is a non-miraculous alternative. If what these theories say is going on 'behind' the phenomena is indeed true or 'approximately true', then it is no wonder that they get the phenomena right. So it is plausible to conclude that presently accepted theories are indeed 'essentially' correct. After all, quantum theory gets certain phenomena, like the Lamb shift, correct to, whatever it is, 6 or 7 decimal places; in the view of some scientists, only a philosopher, overly impressed by merely logical possibilities, could believe that this is compatible with the

[1] According to a famous remark of Quine's, for instance, the theoretical entities involved in current science (like electrons) are epistemologically on a par with the Greek gods—both are convenient fictions introduced in the attempt to order (empirical) reality (Quine 1953: 44).

[2] Van Fraassen 1980. Van Fraassen calls his position 'constructive empiricism' (for criticisms see my 1983 review of his book).

quantum theory's failing to be a fundamentally correct description of reality.

Notice, by the way, that the argument requires the empirical success of a theory to be understood in a particular way. Not every empirical consequence that a theory has and which happens to be correct will give intuitive support for the idea that the theory must somehow or other have latched on to the 'universal blueprint'. Specifically, any empirical consequence which was *written into* the theory *post hoc* must be excluded. Clearly it is no miracle if a theory gets right a fact which was already known to hold and which the theory had been engineered to yield. If the fact concerned was used in the construction of the theory—for example, to fix the value of some initially free parameter—then the theory was *bound* to get that fact right. (On the other hand, if the experimental result concerned was *not* written into the theory, then the support it lends to the idea that the theory is 'essentially correct' is surely independent of whether or not the result was already known when the theory was formulated.[3])

This intuitive 'no miracles' argument can be made more precise in various ways—all of them problematic and some of them more problematic than others. It is, for instance, often run as a form of an 'inference to the best explanation' or Peircian 'abduction'.[4] But, as Laudan (1981) and Fine (ch. I, this volume) have both pointed out, since the anti-realist is precisely in the business of denying the validity of inference to the best explanation in science, he is hardly likely to allow it in philosophy as a means of arguing for realism. Perhaps more importantly, and despite the attempts of some philosophers to claim scientific status for realism itself on the basis of its explanatory power,[5] there is surely a crucial, pragmatic difference between a good scientific explanation and the 'explanation' afforded by the thesis of realism for the success of our present theories. A requirement for a convincing *scientific* explanation is *independent* testability—Newton's explanation of the planetary orbits is such a good one because the theory yields so much else that is testable besides the orbits: the oblateness of the earth, return of Halley's comet, and so on. Yet in the

[3] I have argued for this notion of empirical support and against the idea that temporal novelty is epistemically important in my 1985 paper and especially in my 1989*a* paper, which includes a detailed historical analysis of the famous 'white spot' episode involving Fresnel and Poisson, and often taken to provide support for the 'novel facts count more' thesis.

[4] This form of the argument is strongly criticized by Larry Laudan (1981). Strong and cogent reservations about the alleged explanation that realism supplies of science's success were also expressed in Howard Stein's paper delivered to the Neuchâtel conference.

[5] This position seems to have been held by Boyd, Niiniluoto, and others. It is disowned by Putnam (1978): 'I think that realism is like an empirical hypothesis in that it could be false, and that facts are relevant to its support (or to criticizing it); but that doesn't mean that realism is scientific (in any standard sense of "scientific"), or that realism is a hypothesis.'

case of realism's 'explanation' of the success of our current theories there can of course be no question of any independent tests. Scientific realism can surely not be *inferred* in any interesting sense from science's success. The 'no miracles' argument cannot *establish* scientific realism; the claim is only that, other things being equal, a theory's predictive success supplies a prima-facie plausibility argument in favour of its somehow or other having latched on to the truth.

Certainly the psychological force of the argument was sharply felt even by the philosophers who are usually (though, as we shall see, mistakenly) regarded as the great champions of anti-realism or instrumentalism: Pierre Duhem and Henri Poincaré. Here, for example, is Duhem:

> The highest test, therefore of [a theory] is to ask it to indicate in advance things which the future alone will reveal. And when the experiment is made and confirms the predictions obtained from our theory, we feel strengthened in our conviction that the relations established by our reason among abstract notions only correspond to the relations among things. (1906: 28)

And here Poincaré:

> Have we any right, for instance, to enunciate Newton's law? No doubt numerous observations are in agreement with it, but is not that a simple fact of chance? And how do we know besides, that this law which has been true for so many generations, will not be untrue in the next? To this objection the only answer you can give is: It is very improbable. (1905: 186)

So the 'no miracles' argument is likely, I think, to incline a common-sensical sort of person towards some sort of scientific realist view. But he is likely to feel those realist sentiments evaporating if he takes a close look at the *history* of science and particularly at the phenomenon of *scientific revolutions*.

Newton's theory of gravitation had a stunning range of predictive success: the perturbations of the planetary orbits away from strict Keplerian ellipses, the variation of gravity over the earth's surface, the return of Halley's comet, precession of the equinoxes, and so on. Newtonians even turned empirical difficulties (like the initially anomalous motion of Uranus) into major successes (in this case the prediction of a hitherto unknown trans-Uranian planet subsequently christened Neptune). Physicists were wont to bemoan their fate at having been born after Newton—there was only one truth to be discovered about the 'system of the world', and Newton had discovered it. Certainly an apparently hugely convincing 'no miracles' argument could be—and was—constructed on behalf of Newton's theory. It would be a miracle if Newton's theory got the planetary motions so precisely right, that it should be right about Neptune and about Halley's comet, that the motion of incredibly distant objects like

some binary stars should be in accordance with the theory—it would be a miracle if this were true but the theory is not. However, as we all know, Newton's theory was rejected in favour of Einstein's in the early twentieth century.

This would pose no problem if Einstein's theory were simply an extension of Newton's; that is, if it simply incorporated Newton's theory as a special case, and then went on to say more. In general, if the development of science were cumulative, then scientific change would pose no problem either for the realist or for his 'no miracles' argument. The reason why Newton's theory got so many of the phenomena correct could still be that it was true, just not the whole truth.

Unfortunately Einstein's theory is not simply an extension of Newton's. The two theories are logically inconsistent: if Einstein's theory is true, then Newton's has to be false.[6] This is of course accepted by all present-day realists. The recognition that scientific progress, even in the 'successful', 'mature' sciences, is not strictly cumulative at the theoretical level, but instead involves at least an element of modification and revision is the reason why no present-day realist would claim that we have grounds for holding that presently accepted theories are *true*. Instead, the claim is only that we have grounds for holding that those theories are 'approximately' or 'essentially' true. This last claim might be called 'modified realism'. I shall, for convenience, drop the 'modified' in what follows, but it should be understood that my realists claim only that we have grounds for holding that our present theories in *mature* science are *approximately* true.

[6] Professor Agazzi in his paper at Neuchâtel took the view that Newtonian physics remains true of objects in its intended domain and that quantum and relativistic physics are true of objects in quite different domains. But this position is surely untenable. Newton's theory was not about (its 'intended referent' was not) macroscopic objects moving with velocities small compared with that of light. It was about *all* material objects moving with *any* velocity you like. And that theory is *wrong* (or so we now think), gloriously wrong, of course, but wrong. Moreover, it isn't even, strictly speaking, right about certain bodies and certain motions and 'only' wrong when we are dealing with microscopic objects or bodies moving at very high velocities. If relativity and quantum theory are correct, then Newton's theory's predictions about the motion of *any* body, even the most macroscopic and slowest-moving, are *strictly* false. It's just that their falsity lies well within experimental error. That is, what is true is that Newton's theory is an *empirically* faultless approximation for a whole range of cases. It's also true, as Agazzi claimed, that scientists and engineers still often see themselves as applying classical physics in a whole range of areas. But the only clear-sighted account of what they are doing is, I think, that they are *in fact* applying the best-supported theories available to them—viz. quantum mechanics and relativity theory. It's just that they know that these theories themselves entail the meta-result that, for their purposes (of sending rockets to the moon or whatever), it will make no practical difference to act *as if* they were applying classical physics, and indeed that it would be from the empirical point of view a waste of effort to apply the mathematically more demanding newer theories only for that sophistication to become entirely irrelevant when it comes to *empirical* application.

This realist claim involves two terms which are notoriously difficult to clarify. I shall propose my own rough characterization of the 'mature' sciences shortly. As for 'approximately true', well-known and major difficulties stand in the way of any attempt at precise analysis. Indeed, various attempted characterizations (such as Popper's in terms of 'increasing verisimilitude') have turned out to be formally deeply flawed.[7] Although we do often operate quite happily at the intuitive level with the notion of approximate truth, it is surely not the sort of notion which can happily be left as a primitive. For one thing: if the notion is going to do the work that realists need it to do, it is going to have to be transitive. Realists need to claim that although some presently accepted theory may subsequently be modified and replaced, it will still look 'approximately true' in the light not just of the next theory which supersedes it, but also in the light of the theory (if any) which supersedes the theory which supersedes it, etc. But is transitivity a property that the notion of approximate truth possesses even intuitively?

But there is anyway an important *prior* question here: that of whether or not, talking intuitively, in advance of formal analysis, the history of science (or some selected part of it) speaks in favour of successive scientific theories being increasingly good 'approximations to the truth'. This clearly depends on just *how radical* theory change has standardly been in science. Again, of course, we are dealing in unfortunately vague terms. But surely the realist claim—that we have grounds for holding that our present theories are approximately true—is plausible only to the extent that it seems reasonable to say that Newton's theory, for example, 'approximates' Einstein's, and that, in general, the development of science (at any rate the development of successful, 'mature' science) has been 'essentially' cumulative, that the deposed theories themselves, *and not just their successful empirical consequences*, have generally lived on, albeit in 'modified form', after the 'revolution'. If, on the contrary, theory change in science has often involved 'radical' shifts—something like the complete rejection of the genuinely theoretical assumptions (though combined of course with retention of the successful empirical content)—then realism is in dire straits. Before going further, let's be clear on the dependence of realism on the claim that theory change has been 'essentially cumulative'.

Assume, first, that the realist has convinced us that the development of theoretical science has indeed been 'essentially cumulative'. He could then argue for his realism roughly as follows. The development of the 'mature'

[7] See Tichy 1974 and Miller 1974.

sciences has so far been 'essentially' cumulative at all levels—theoretical as well as observational. It seems reasonable, therefore, to infer inductively that that development will continue to be 'essentially cumulative' in the future. This presumably means that, even should our present theories be replaced, they will continue to appear 'approximately' correct in the light of the successor theories. Such a development is, of course, logically compatible with the genuinely theoretical assumptions, both of presently accepted theories and of those destined to be accepted in the future, being entirely untrue. However, this is highly implausible, since it would make the empirical success of all these theories entirely mysterious; while, on the other hand, the assumption that our present theories are approximately true is enough to explain the empirical success as non-miraculous.

No one, I take it (reiterating the point made earlier), would claim that this argument is completely watertight. The inductive 'inference' from 'essential cumulativity' in the past to 'essential cumulativity' in the future could of course be questioned. Moreover, there is still the problem of what exactly is involved in approximate truth; and indeed the problem of whether or not the assumption of the approximate truth of our present theories really would explain their empirical success. It might seem plausible, intuitively speaking, to suppose that if a theory is 'approximately' or 'essentially' true, then it is likely that most of its consequences will themselves be 'essentially' correct. To take a straightforwardly empirical example, say that I make a slight arithmetical error in totting up my bank balance and come to the strictly mistaken view that my total worldly fortune is £100, when the truth is that it is £103. Will it seem 'miraculous' if this strictly false theory none the less supplies a quite reliable guide to life? After all, it might be claimed, most of the consequences that I am likely to be interested in—for example, that I can't afford a month's holiday in Switzerland—will in fact be consequences both of the false theory, that I hold, and of the truth. None the less, plausible or not, there are formidable formal difficulties here.[8] Every false theory, of course, has infinitely many false consequences (as well as infinitely many true ones), and there are things that my 'nearly true' theory gets totally wrong. For example, the truth is that my total fortune expressed in pounds sterling is a prime number, whereas the 'nearly true' theory I hold says—entirely incorrectly—that it's composite. Moreover, the argument seems committed to the claim that if theory T 'approximates' theory T', which in turn

[8] Two recent attempts to overcome these difficulties are Oddie 1986 and Niiniluoto 1987—though both attempts involve substantive, non-logical, and therefore challengeable assumptions.

'approximates' T'', then T 'approximates' T''. (The theories which eventually supersede our presently accepted ones, might themselves—presumably *will*—eventually be superseded by still further theories. The realist needs to be assured that any presently accepted theory will continue to look approximately correct, even in the light of the further theories in the sequence, not just in the light of its immediate successor.) But is this transitivity assumption correct? After all, if we took a series of photographs at one-second intervals, say, of a developing tadpole, each photograph in the sequence would presumably 'approximate' its predecessor, and yet we start with a tadpole and finish with a frog. Does a frog 'approximate' a tadpole? I propose, however, that, for present purposes, we put all these difficulties into abeyance. If he can sustain the claim that the development of the 'mature' sciences has been 'essentially cumulative', then the realist has at least some sort of argument for his claim.

If, on the contrary, the realist is forced to concede that there has been *radical* change at the theoretical level in the history of even the mature sciences, then he surely is in deep trouble. Suppose that there are cases of mature theories which were once accepted, were predictively successful, and whose underlying theoretical assumptions none the less now seem unequivocally entirely false. The realist would have encouraged the earlier theorist to regard his theory's empirical success as giving him grounds for regarding the theory itself as approximately true. He now encourages scientists to regard their newer theory's empirical success as giving them grounds for regarding that newer theory as approximately true. The older and newer theories are radically at odds with one another at the theoretical level. Presumably, if we have good grounds for thinking a theory T approximately true, we equally have good grounds for thinking that any theory T' radically at odds with T is false (plain false, not 'approximately true'). So the realist would be in the unenviable position of telling us that we now have good grounds to regard as false a theory which he earlier would have told us we had good grounds to believe approximately true. Why should not his proposed judgement about presently accepted theories turn out to be similarly mistaken?

Assuming, then, that the realist is not talking about 'good grounds' in some defeasible, conjectural sense,[9] realism is not compatible with the existence of radical theoretical changes in science (or at any rate in mature science). The chief argument against realism—the argument from scientific revolutions—is based precisely on the claim that revolutionary changes have occurred in accepted scientific theories, changes in which the

[9] See below, pp. 150–151.

old theory could be said to 'approximate' the new only by stretching the admittedly vague and therefore elastic notion of 'approximation' beyond breaking-point.

At first glance, this claim appears to be correct. Consider, for example, the history of optics. Even if we restrict this history to the modern era, there have been fundamental shifts in our theory about the basic constitution of light. The theory that a beam of light consists of a shower of tiny material particles was widely held in the eighteenth century. Some of its empirical consequences—such as those about simple reflection, refraction, and prismatic dispersion—were correct. The theory was, however, rejected in favour of the idea that light consists, not of matter, but of certain vibratory motions set up by luminous bodies and carried by an all-pervading medium, the 'luminiferous aether'. It would clearly be difficult to argue that the theory that light is a wave in a mechanical medium is an 'extension', or even an 'extension with slight modifications', of the idea that light consists of material particles: waves in a mechanical medium and particles travelling through empty space seem more like chalk and cheese than do chalk and cheese themselves. Nor was that all: Fresnel's wave theory was itself soon replaced by Maxwell's electromagnetic theory. Maxwell, as is well known, strove manfully to give an account of the electromagnetic field in terms of some underlying mechanical medium; but his attempts and those of others failed, and it came to be accepted that the electromagnetic field is a primitive. So again, a fundamental change in the accepted account of the basic structure of light seems to have occurred—instead of vibrations carried through an elastic medium, it becomes a series of wave-like changes in a disembodied electromagnetic field. A mechanical vibration and an electric ('displacement') current are surely radically different sorts of thing. Finally, the acceptance of the photon theory had light consisting again of discrete entities, but ones which obey an entirely new mechanics.

In the meanwhile, as *theories* were changing light from chalk to cheese and then to superchalk, there was a steady, basically cumulative development in the captured and systematized empirical content of optics.[10] The material particle theory dealt satisfactorily with simple reflection and re-

[10] Genuine examples of 'Kuhn loss' of captured *empirical* content are remarkably thin on the ground—*provided*, that is, that *empirical* content is properly understood. Feyerabend and Kuhn both use examples of 'lost' content which are either clearly highly theoretical (Feyerabend even uses 'The Brownian particle is a perpetual motion machine of the second kind' as an example of an empirical statement!) or highly vague (Kuhn claims, e.g. that while phlogiston theory could explain why metalas are 'similar' to one another, the superseding oxygen theory could not). For a criticism of Feyerabend on facts see Worrall 1991; for a criticism of Kuhn see Worrall 1989*b*.

fraction and little else; the classical wave theory added interference and diffraction and eventually polarization effects too; the electromagnetic theory added various results connecting light with electrical and magnetic effects; the photon theory added the photoelectric effect and much else besides. The process at the empirical level (properly construed) was essentially cumulative. There were *temporary* problems (e.g. over whether or not the classical wave theory could deal with the phenomena which had previously been taken to support the ('essentially') rectilinear propagation of light), but these were invariably settled quickly and positively.[11]

Or take the Newton–Einstein case again. At the *empirical* level it does seem intuitively reasonable to say that Einstein's theory is a sort of 'extension with modifications' of Newton's. It is true that, even at this level, if we take the maximally precise consequences about the motion of a given body yielded by the two theories, they will always strictly speaking contradict one another. But for a whole range of cases (those cases, of course, in which the velocities involved are fairly small compared to the velocity of light), the predictions of the two theories will be strictly different but observationally indistinguishable. It is also true, of course, that Newton's equations are limiting cases of corresponding relativistic equations. However, there is much more to Newton's theory than the laws of motion and the principle of universal gravitation considered simply as mathematical equations. These equations were interpreted within a set of very general theoretical assumptions which involved amongst other things the assumption that space is infinite, that time is absolute, so that two events simultaneous for one observer are simultaneous for all, and that the inertial mass of a body is constant. Einstein's theory entails, on the contrary, that space is finite (though unbounded), that time is not absolute in the Newtonian sense, and that the mass of a body increases with its velocity. All these are surely out-and-out contradictions.

[11] The case of rectilinear propagation of light provides an illustrative example both of the essential empirical continuity of 'mature' science and of what it is about this process that leads Feyerabend and Kuhn to misrepresent it. Certain *theories* become so firmly entrenched at certain stages of the development of science, so much parts of 'background knowledge', that they, or at any rate particular experimental situations *interpreted in their light*, are readily talked of as 'facts'. This was certainly true of the 'fact' that light, if left to itself, is rectilinearly propagated. Here then is surely a 'fact' which was 'lost' in the wave revolution, since Fresnel's theory entails that light is *always* diffracted—it's just that in most circumstances the difference between the diffraction pattern and the predictions of geometrical optics is well below the observational level. But this last remark gives the game away. The idea that light is (*rigidly*) rectilinearly propagated was never an empirical result (not a 'crude fact' in Poincaré's terminology). The real empirical results—certain 'ray tracings', inability to see round corners or through bent opaque tubes, etc.—were not 'lost' but simply re-explained as a result of the shift to the wave theory.

The picture of the development of science certainly seems, then, to be one of essential cumulativity at the empirical level, accompanied by sharp changes of an entirely non-cumulative kind at the top theoretical levels.[12] This picture of theory change in the past would seem to supply good inductive grounds for holding that those theories presently accepted in science will, within a reasonably brief period, themselves be replaced by theories which retain (and extend) the empirical success of present theories, but do so on the basis of underlying theoretical assumptions entirely at odds with those presently accepted. This is, of course, the so-called *pessimistic induction*—usually regarded as a recent methodological discovery, but in fact already stated clearly by Poincaré.[13] How can there be good grounds for holding our present theories to be 'approximately' or 'essentially' true, and at the same time seemingly strong historical-inductive grounds for regarding those theories as (probably) ontologically false?

Unless this picture of theory change is shown to be inaccurate, then realism is surely untenable, and basically only two (very different) possibilities open. The first can be motivated as follows. Science is the field in which rationality reigns. There can be no rational acceptance of claims of a kind which history gives us grounds to think are likely later to be rejected. The successful empirical content of a once accepted theory *is* in general carried over to the new theory, but its basic theoretical claims are not. Theories, then, are best construed as making no real claims beyond their directly empirical consequences; or, if they *are* so construed, acceptance of these theoretical claims as true or approximately true is no part of the rational procedures of science. We are thus led into some sort of either pragmatic or 'constructive' anti-realism.

Such a position restores a pleasing, cumulative (or quasi-cumulative) development to science (i.e. to the 'real part' of science); but it does so at the expense of sacrificing the 'no miracles' argument entirely. After all, the theoretical science which the pragmatist alleges to be insubstantial and to play a purely codificatory role has, as a matter of fact, often proved *fruitful*. That is, interpreted literally and therefore treated as claims about the structure of the world, theories have yielded testable consequences over and above those they were introduced to codify, and those consequences have turned out to be correct when checked empirically. Why? The pragmatist asserts that there is no answer.

[12] That this is the intuitive picture was fully emphasized by Poincaré and Duhem, rather lost sight of by the logical positivists, and re-emphasized by Popper and those influenced by him (such as John Watkins and Paul Feyerabend).

[13] See Putnam 1978: 25; and Poincaré 1905: 160 (quoted below, p. 157).

The other alternative for someone who accepts the empirically cumulative, theoretically non-cumulative picture of scientific change, but who wishes to avoid pragmatism is pure, Popperian *conjectural realism*. This is Popper's view stripped of all the verisimilitude ideas, which always sat rather uncomfortably with the main theses. On this conjectural realist view, the genuinely theoretical, observation-transcendent parts of scientific theories are not just codificatory schemes, they are *attempted* descriptions of the reality hidden behind the phenomena. And our present best theories are our present best shots at the truth. We certainly have reason to think that our presently best theories are our present best shots at the truth (they stand up to the present evidence better than any known rival), but we have no real reason to think that those present theories are true or even closer to the truth than their rejected predecessors. Indeed, it can be accepted that the history of science makes it very unlikely that our present theories are even 'approximately' true. They do, of course, standardly capture more empirical results than any of their predecessors, but this is no indication at all that they are any closer to capturing 'God's blueprint of the universe'. The fully methodologically aware theoretical scientist nobly pursues his unended quest for the truth knowing that he will almost certainly fail and that, even if he succeeds, he will never know, nor even have any real indication, that he has succeeded.

Conjectural realism is certainly a modest, unassuming position. It can be formulated as a version of realism in the senses we have so far discussed—as saying in fact that we do have the best possible grounds for holding our present best theories to be true (they are best confirmed or best 'corroborated' by the present evidence); we should not even ask for better grounds than these; but since the best corroborated theory tomorrow may fundamentally contradict the best corroborated theory of today, the grounds that we have for thinking the theories true are inevitably conjectural and (practically, not just in principle) defeasible. I defended this conjectural realist view myself in an earlier paper: presentations of the view frequently (almost invariably) met with the response that there is little, if any, difference of substance between it and anti-realism.[14] The main problem, I take it, is again that conjectural realism makes no concessions to the 'no miracles' argument. On the conjectural realist view, Newton's theory does

[14] For my defence of conjectural realism see Worrall 1982. The response of 'no real difference' between conjectural and anti-realism was made many times in seminars and private discussions (by van Fraassen amongst others). See also, e.g., Newton-Smith 1981, where realism is *defined* as including an 'epistemological ingredient' foreign to this conjecturalist approach. I should add that I am of course giving up the conjectural realist position in the present paper only in the sense that I am now inclined to think that a *stronger* position can be defended.

assert that space and time are absolute, that there are action-at-a-distance forces of gravity, and that inertial mass is constant; all this was entirely wrong, and *yet* the theory based on these assumptions was highly empirically adequate. This just has to be recorded as a fact. And if you happen to find it a rather surprising fact, then that's your own business--perhaps due to failure to internalize the elementary logical fact that all false theories have true consequences (in fact, infinitely many of them).

Both the pragmatist and the conjectural realist can point out that we can't, on pain of infinite regress, account for everything, and one of the things we can't account for is why this stuff that allegedly does no more than streamline the machinery of scientific proof or that turns out to be radically false should have turned out to be fruitful. There obviously can be no question of any 'knockdown refutation' of either view. None the less, if a position could be developed which accommodated some of the intuitions underlying the 'no miracles' argument and yet which, at the same time, cohered with the historical facts about theory change in science, then it would arguably be more plausible than either pragmatism or conjectural realism.

Is it possible to have the best of both worlds, to account (no matter how tentatively) for the empirical success of theoretical science without running foul of the historical facts about theory change? Richard Boyd and occasionally Hilary Putnam have claimed that realism is itself already the best of both worlds. They have claimed, more or less explicitly, that the picture of scientific change that I have painted is inaccurate, and so the argument from scientific revolutions is based on a false premiss: the history of science is *not* in fact marked by radical theoretical revolutions (at any rate, not the history of 'mature' science). On the contrary, claims Boyd:

> The historical progress of the mature sciences is largely a matter of successively more accurate approximations to the truth about both observable and unobservable phenomena. Later theories typically build upon the (observational and theoretical) knowledge embodied in previous theories. (1984: 41–2)[15]

Elsewhere he asserts that scientists generally adopt the (realist) principle that 'new theories should . . . resemble current theories with respect to their accounts of causal relations among theoretical entities' (Boyd 1973:

[15] In discussion Richard Boyd acknowledged that he made no claim of approximate continuity for the 'metaphysical' components of accepted scientific theories. But I had thought that was what the debate is all about: does the empirical success of theories give us grounds to think that their basic ('metaphysical', observation-transcendent) description of the reality underlying the phenomena is at any rate approximately correct? Several of Richard Boyd's comments suggested to me, at least, that he defends not a full-blown realism, but something like the structural realism that I try to formulate below.

8). Similarly, Putnam once claimed (1978: 20) that many historical cases of theory change show that 'what scientists try to do' is to preserve 'as often as possible' the 'mechanisms of the earlier theory' or 'to show that they are "limiting cases" of new mechanisms'. I want first to explain why I think that these claims are wrong as they stand. I shall then argue that valid intuitions underlie the claims, but these intuitions are better captured in a rather different position which might be called *structural* or *syntactic realism*.

Larry Laudan has objected to Boyd and Putnam's claims by citing a whole list of theoretical entities, like phlogiston, caloric, and a range of ethers, which, he insists, once figured in successful theories but have now been *totally* rejected (1982: 231). How, Laudan wants to know, can newer theories resemble older theories 'with respect to their accounts of causal relations among theoretical entities' if the newer theories entirely reject the theoretical entities of the old? How can relativistic physics be said to preserve 'the mechanisms' of, say, Fresnel's account of the transmission of light, when, according to Fresnel's account, transmission occurs via periodic disturbances in an all-pervading elastic medium, while, according to relativity theory, no such medium exists at all? How can later scientists be said to have applied to Fresnel's theory the principle that 'new theories should ... resemble current theories with respect to their accounts of causal relations among theoretical entities' when these later theories entirely deny the existence of the core theoretical entity in Fresnel's theory? Boyd alleges that the mechanisms of classical physics reappear as limiting cases of mechanisms in relativistic physics. Laudan replies that, although it is of course true that *some* classical laws are limiting cases of relativistic ones,

> there are other laws and general assertions made by the classical theory (e.g., claims about the density and fine structure of the ether, general laws about the character of the interaction between ether and matter, models and mechanisms detailing the compressibility of the ether) which could not conceivably be limiting cases of modern mechanics. The reason is a simple one: a theory cannot assign values to a variable that does not occur in that theory's language ... Classical ether physics contained a number of postulated mechanisms for dealing inter alia with the transmission of light through the ether. Such mechanisms could not possibly appear in a successor theory like the special theory of relativity which denies the very existence of an etherial medium and which accomplishes the explanatory tasks performed by the ether via very different mechanisms. (Laudan 1982: 237–8)

Does the realist have any legitimate come-back to Laudan's criticisms? Certainly *some* of Laudan's examples can be dealt with fairly straightforwardly. Boyd and Putnam have been careful to restrict their claim of 'essential' cumulativity to 'mature' science only. Pre-Lavoisierian chemistry is their chief example of an immature science, so they would be happy to concede that phlogiston has been entirely rejected by later science.[16] Presumably, some of the other items on Laudan's list of once scientifically accepted but now non-existent entities would receive similar treatment.

The cogency of this reply clearly depends to a large extent on whether or not some reasonably precise account can be given of what it takes for a science to achieve 'maturity'. Neither Boyd nor Putnam has anything very precise to say on this score, and this has naturally engendered the suspicion that the realist has supplied himself with a very useful *ad hoc* device: whenever it seems clear that the basic claims of some previously accepted theory have now been totally rejected, the science to which that theory belonged is automatically counted as 'immature' at the time that theory was accepted.

What is needed is a reasonably precise and *independent* criterion of maturity. And this can, it seems to me, in fact be 'read off' the chief sustaining argument for realism—the 'no miracles' argument. This argument, as I indicated before, applies only to theories which have enjoyed genuine predictive success. This must mean more than simply having correct empirical consequences—for these could have been forced into the framework of the theory concerned after the effects they describe had already been observed to occur. The undoubted fact that various chemical experimental results could be incorporated into the phlogiston theory does not on its own found any argument, even of the intuitive kind we are considering, to the likely truth of the phlogiston theory. Similarly, the fact that creationist biology can be made empirically adequate with respect to, say, the fossil record clearly founds no argument for the likely truth of the Genesis account of creation. Such empirical adequacy can of course easily be achieved—for example, by simply making Gosse's assumption that God created the rocks with the 'fossils' there already, just as they are found to be. (Perhaps God's purpose in doing this was to test our faith.) But the fact that this elaborated version of creationism is then bound to imply the empirical details of the fossil record is, of course, neither a miracle nor an indication that the theory 'is on the right track'. The explanation for this predictive 'success' is, of course, just that it is often easy to incorporate already known results *ad hoc* into a given framework. Nor is the success of a theory in predicting *particular* events of an *already known kind* enough on its own to sustain a 'no miracles' argument in favour of a theory. Even the most *ad hoc*, 'cobbled up' theory will standardly be predictive in the

[16] '[W]e do not carry [the principle of the benefit of the doubt] so far as to say that "phlogiston" referred' (Putnam 1978: 25).

sense that it will entail that the various results it has been made to absorb will continue to hold in the future. (For example, the heavily epicyclic corpuscular theory of light developed in the early nineteenth century by Biot, having had various parameters fixed on the basis of certain results in crystal optics, implied, of course, that the 'natural' generalizations of those results would continue to hold in the future.) Theories will standardly exhibit this *weak* predictiveness because, Popper or no, scientists do instinctively inductively generalize on the results of well-controlled experiments which have so far always yielded the same results. But the success of such inductive manœuvres, though no doubt miraculous enough in itself, does not speak in favour of the likely truthlikeness of any particular explanatory theory. The sort of predictive success which seems to elicit the intuitions underlying the 'no miracles' argument is a much stronger, more striking form of predictive success. In the stronger case, not just a new instance of an old empirical generalization, but an entirely new empirical generalization follows from some theory, and turns out to be experimentally confirmed. Instances of this are the prediction of the existence and orbit of a hitherto unknown planet by Newton's theory and the prediction of the white spot at the centre of the shadow of an opaque disc and of the hitherto entirely unsuspected phenomenon of conical refraction by Fresnel's wave theory of light. So my suggestion is that, instead of leaving the notion of maturity as conveniently undefined, a realist should take it that a science counts as mature once it has theories within it which are predictive in this latter demanding sense—predictive of general *types* of phenomena, without these phenomena having been 'written into' the theory.

With this somewhat more precise characterization of maturity, Laudan's list of difficult cases for the modified realist can indeed be pared down considerably further. Laudan must be operating with some much weaker notion of empirical success than the idea of *predictive* success just explained when he cites the gravitational ether theories of Hartley and LeSage as examples of 'once successful' theories.[17] Presumably he means simply that these theories were able successfully to accommodate various already known observational results. But if we require *predictive* success of the strong kind indicated above, then surely neither Hartley's nor LeSage's speculative hypothesis scored any such success.

However there is no doubt that, no matter how hard-headed one is about predictive success, some of Laudan's examples remain to challenge

[17] I have criticized Laudan on this point in Worrall 1988*b*.

the realist. Let's concentrate on what seems to me (and to others[18]) the sharpest such challenge: the ether of classical physics. Indeed, we can make the challenge still sharper by concentrating on the elastic solid ether involved in the classical wave theory of light proposed by Fresnel.

Fresnel's theory was based on the assumption that light consists in periodic disturbances originating in a source and transmitted by an all-pervading, mechanical medium. There can be no doubt that Fresnel himself believed in the 'real existence' of this medium—a highly attenuated and rare medium all right, but essentially an ordinary mechanical medium which generates elastic restoring forces on any of its 'parts' that are disturbed from their positions of equilibrium.[19] There is equally no doubt that Fresnel's theory enjoyed genuine predictive success—not least, of course, with the famous prediction of the white spot at the centre of the shadow of an opaque disc held in light diverging from a single slit. If Fresnel's theory does not count as 'mature' science, then it is difficult to see what does.[20]

Was Fresnel's elastic solid ether retained or 'approximately retained' in later physical theories? Of course, as I have repeatedly said and as realists would admit, the notion of one theoretical entity approximating another or of one causal mechanism being a limiting case of another is extremely vague, and therefore enormously elastic. But if the notion is stretched *too* far, then the realist position surely becomes empty. If black 'approximates' white, if a particle 'approximates' a wave, if a space-time curvature 'ap-

[18] See e.g. Hardin and Rosenberg 1982, which tackles this challenge on behalf of the realist (see below, pp. 156–7).

[19] This is not to deny, of course, that Fresnel was also guided by what was already known empirically about light. It is also true that at the time of Fresnel's work, much remained to be discovered about the dynamical properties of elastic solids. As a result, Fresnel's theory was dynamically deficient in certain respects (especially when viewed in hindsight). But the fact that he failed to construct a fully dynamically adequate theory of light as a disturbance in an elastic solid medium (or better: the fact that his theory ran into certain fundamental dynamical problems) does *not* mean that Fresnel did not even aim at such a theory, nor that he did not intend the theory he produced to be interpreted in this way. He clearly thought of light as a disturbance in an elastic medium, and dynamical and mechanical considerations (often of an abstract, mathematical sort) certainly guided his research, along with the empirical data on light.

There is no doubt that, as Whittaker pointed out (1951: 116), some aspects of Fresnel's theory—in particular the discontinuity of the normal component of the displacement across the interface between two media—cohere rather better with Maxwell's notion of a displacement current than they do with the idea of an ordinary dynamical displacement. But, *contra* Hardin and Rosenberg (who cite Whittaker), this doesn't mean that Fresnel was talking about displacement currents all along; instead, he was talking—in a flawed and problematic way—about elastic displacements.

[20] Cf. Laudan 1982: 225 (also p. 115, this volume): 'If that [Fresnel's prediction of the "white spot"] does not count as empirical success, nothing does!'

proximates' an action-at-a-distance force, then no doubt the realist is right that we can be confident that future theories will be approximately like the ones we presently hold. This won't, however, be telling us very much. It does seem to me that the only clear-sighted judgement is that Fresnel's elastic solid ether was entirely overthrown in the course of later science. Indeed, this occurred, long before the advent of relativity theory, when Maxwell's theory was accepted in its stead. It is true that Maxwell himself continued to hold out the hope that his electromagnetic field would one day be 'reduced' to an underlying mechanical substratum—essentially the ether as Fresnel had conceived it. But in view of the failure of a whole series of attempts at such a 'reduction', the field was eventually accepted as a primitive entity. Light became viewed as a periodic disturbance, *not* in an elastic medium, but in the 'disembodied' electromagnetic field. One would be hard pressed to cite two things more different than a displacement current, which is what this electromagnetic view makes light, and an elastic vibration through a medium, which is what Fresnel's theory had made it.

Hardin and Rosenberg (1982), replying to Laudan, suggest that, rather than trying to claim that Fresnel's elastic solid ether was 'approximately preserved' in Maxwell's theory, the realist can 'reasonably' regard Fresnel as having been talking about the electromagnetic field all along. This is certainly a striking suggestion! As someone influenced by Lakatos, I certainly would not want entirely to deny a role to rational reconstruction of history. Indeed, it does seem reasonable for a historian to reserve the option of holding that a scientist did not *fully* understand his own theory; but to allow that he may have totally *mis*understood it and, indeed, that it could not really be understood until some 50 years after his death, to hold that Fresnel was 'really' talking about something of which we know he had not the slightest inkling, all this is surely taking 'rational reconstruction' too far. Even 'charity' can be overdone.[21] Fresnel was *obviously* claiming that the light-carrying 'luminiferous aether' is an elastic solid, obeying, in essence, the ordinary laws of the mechanics of such bodies: the ether has 'parts'; restoring elastic forces are brought into play when a part is disturbed out of its equilibrium position. He was obviously claiming this, and it turned out that, if later science is right, Fresnel was wrong. Hardin and Rosenberg's claim has a definite air of desperation about it.

[21] Putnam has a well-known (and notoriously vague) 'principle of charity' (or 'benefit of the doubt') which says that 'when speakers specify a referent for a term they use by a *description* and, because of mistaken factual beliefs that those speakers have, that description fails to refer, we should assume that they would accept reasonable reformulations of their descriptions' (1978: 23–4).

None the less, there is *something* right about what they, and Boyd, say. There *was* an important element of continuity in the shift from Fresnel to Maxwell—and this was much more than a simple question of carrying over the successful *empirical* content into the new theory. At the same time, it was rather less than a carrying over of the full theoretical content or full theoretical mechanisms (even in 'approximate' form). And what was carried over can be captured without making the very far-fetched assumption of Hardin and Rosenberg that Fresnel's theory was 'really' about the electromagnetic field all along. There was continuity or accumulation in the shift, but the continuity is one of *form* or *structure*, not of content. In fact, this claim was already made and defended by Poincaré. And Poincaré used the example of the switch from Fresnel to Maxwell to argue for a general sort of *syntactic* or *structural realism* quite different from the anti-realist instrumentalism which is often attributed to him.[22] This largely forgotten thesis of Poincaré's seems to me to offer the only hopeful way of *both* underwriting the 'no miracles' argument *and* accepting an accurate account of the extent of theory change in science. Roughly speaking, it seems right to say that Fresnel completely misidentified the *nature* of light; but, none the less, it is no miracle that his theory enjoyed the empirical predictive success that it did; it is no miracle because Fresnel's theory, as science later saw it, attributed to light the right *structure*.

Poincaré's view is summarized in the following passage from *Science and Hypothesis*, which begins by clearly anticipating the currently fashionable 'pessimistic induction':

> The ephemeral nature of scientific theories takes by surprise the man of the world. Their brief period of prosperity ended, he sees them abandoned one after the other; he sees ruins piled upon ruins; he predicts that the theories in fashion today will in a short time succumb in their turn, and he concludes that they are absolutely in vain. This is what he calls the *bankruptcy of science*. (1905: 160)

But this passage continues:

> His scepticism is superficial; he does not take into account the object of scientific theories and the part they play, or he would understand that the ruins may still be good for something. No theory seemed established on firmer ground than Fresnel's, which attributed light to the movements of the ether. Then if Maxwell's theory is preferred today, does it mean that Fresnel's work was in vain? No; for Fresnel's object was not to know whether there really is an ether, if it is or is not formed of

[22] One critic who explicitly does not classify Poincaré as an instrumentalist is Zahar (see his 1983*b*). The term 'structural realism' was also used by Grover Maxwell for a position which he derived from Russell's later philosophy (see Maxwell 1970*a*,*b*). Maxwell's position grows out of different (more 'philosophical') concerns, though it is clearly related to that of Poincaré (one of the points for further research is to clarify this relationship).

atoms, if these atoms really move in this way or that; his object was to predict optical phenomena.[23]

This Fresnel's theory enables us to do today as well as it did before Maxwell's time. The differential equations are always true, they may be always integrated by the same methods, and the results of this integration still preserve their value.

So far, of course, this might seem a perfect statement of positivistic instrumentalism: Fresnel's theory is really just its empirical content, and this is preserved in later theories. However, Poincaré goes on to make it quite explicit that this is *not* his position.

It cannot be said that this is reducing physical theories to simple practical recipes; these equations express relations, and if the equations remain true, it is because the relations preserve their reality. They teach us now, as they did then, that there is such and such a relation between this thing and that; only the something which we then called *motion*, we now call *electric current*. But these are merely names of the images we substituted for the real objects which Nature will hide for ever from our eyes. The true relations between these real objects are the only reality we can attain. (1905: 162)

Poincaré is claiming that, although from the point of view of Maxwell's theory, Fresnel entirely misidentified the *nature* of light, his theory accurately described not just light's observable effects but its *structure*. There is no elastic solid ether. There is, however, from the later point of view, a (disembodied) electromagnetic field. The field in no clear sense approximates the ether, but disturbances in it do obey *formally* similar laws to those obeyed by elastic disturbances in a mechanical medium. Although Fresnel was quite wrong about *what* oscillates, he was, from this later point of view, right, not just about the optical phenomena, but right also that these phenomena depend on the oscillations of something or other at right angles to the light.

Thus, if we restrict ourselves to the level of mathematical equations—*not*, notice, the phenomenal level—there is in fact complete continuity between Fresnel's and Maxwell's theories. Fresnel developed a famous set of equations for the relative intensities of the reflected and refracted light beams in various circumstances. Ordinary unpolarized light can be analysed into two components: one polarized in the plane of incidence, the other polarized at right angles to it. Let I^2, R^2, and X^2 be the intensities of the components polarized in the plane of incidence of the incident, reflected, and refracted beams respectively; while I'^2, R'^2, and X'^2 are the components polarized at right angles to the plane of incidence. Finally, let

[23] Poincaré is quite wrong about Fresnel's 'object' (see above, n. 19). However, the normative philosophical question of how a theory *ought* to be interpreted is, of course, logically independent of the historical, psychological question of what its creator in fact believed.

i and r be the angles made by the incident and refracted beams with the normal to a plane reflecting surface. Fresnel's equations then state

$$R/I = \tan(i-r)/\tan(i+r)$$
$$R'/I' = \sin(i-r)/\sin(i+r)$$
$$X/I = (2\sin r.\cos i)/(\sin(i+r)\cos(i-r))$$
$$X'/I' = 2\sin r.\cos i/\sin(i+r)$$

Fresnel developed these equations on the basis of the following picture of light. Light consists of vibrations transmitted through a mechanical medium. These vibrations occur at right angles to the direction of the transmission of light through the medium. In an unpolarized beam, vibrations occur in all planes at right angles to the direction of transmission—but the overall beam can be described by regarding it as the composition of two vibrations: one occurring in the plane of incidence and one occurring in the plane at right angles to it. The bigger the vibrations, that is, the larger the maximum distance the particles are forced from their equilibrium positions by the vibration, the more intense the light. I, R, X, etc. in fact measure the amplitudes of these vibrations, and the intensities of the light are given by the squares of these amplitudes.

From the vantage-point of Maxwell's theory as eventually accepted, this account, to repeat, is entirely wrong. How could it be anything else when there is no elastic ether to do any vibrating? None the less, from this vantage-point, Fresnel's theory has exactly the right structure—it's 'just' that what vibrates according to Maxwell's theory are the electric and magnetic field strengths. And in fact, if we interpret I, R, X, etc. as the amplitudes of the 'vibration' of the relevant electric vectors, then Fresnel's equations are directly and fully entailed by Maxwell's theory. It wasn't, then, just that Fresnel's theory *happened* to make certain correct predictions; it made them because it had accurately identified certain relations between optical phenomena. From the standpoint of this superseding theory, Fresnel was quite wrong about the nature of light; the theoretical mechanisms he postulated are *not* approximations to, or limiting cases of, the theoretical mechanisms of the newer theory. None the less, Fresnel was quite right not just about a whole range of optical phenomena, but right that these phenomena depend on something or other that undergoes periodic change at right angles to the light.

But then, Poincaré argued, his contemporaries had no more justification for regarding Maxwell as having definitively discovered the nature of light, as having discovered that it *really* consists in vibrations of the electromagnetic field, than Fresnel's contemporaries had had for regarding Fresnel as having discovered the nature of light. At any rate, this attitude towards Maxwell would be mistaken if it meant any more than that Maxwell built on the relations revealed by Fresnel and showed that further relations existed between phenomena hitherto regarded as purely optical on the one hand and electric and magnetic phenomena on the other.

This example of an important theory change in science certainly appears, then, to exhibit cumulative growth at the structural level combined with radical replacement of the previous ontological ideas. It speaks, then, in favour of a *structural* realism. Is this simply a feature of this particular example, or is preservation of structure a general feature of theory change in mature (i.e. successfully predictive) science?

This particular example is in fact unrepresentative in at least one important respect: Fresnel's equations are taken over completely intact into the superseding theory—reappearing there newly interpreted but, as mathematical equations, entirely unchanged. The much more common pattern is that the old equations reappear as *limiting cases* of the new—that is, the old and new equations are strictly inconsistent, but the new tend to the old as some quantity tends to some limit.

The rule in the history of physics seems to be that, whenever a theory replaces a predecessor, which has however itself enjoyed genuine predictive success, the 'correspondence principle' applies. This requires the *mathematical equations* of the old theory to re-emerge as limiting cases of the mathematical equations of the new. As is being increasingly realized,[24] the principle operates, not just as an after-the-event requirement on a new theory if it is to count as better than the current theory, but often also as a heuristic tool in the actual development of the new theory. Boyd (1984) in fact cites the general applicability of the correspondence principle as evidence for his realism. But the principle applies *purely* at the mathematical level, and hence is quite compatible with the new theory's basic theoretical assumptions (which *interpret* the terms in the equations) being entirely at odds with those of the old. I can see no clear sense in which an action-at-a-distance force of gravity is a 'limiting case' of, or 'approximates', a space-time curvature. Or in which the 'theoretical mechanisms' of action-at-a-distance gravitational theory are 'carried over' into general relativity theory. Yet Einstein's equations undeniably go over to Newton's in certain limiting special cases. In this sense, there is 'approximate continuity' of *structure* in this case. As Boyd points out, a new theory could capture its predecessor's successful empirical content in ways other than yielding the

[24] See e.g. Zahar 1983*b* and Worrall 1985, as well as Boyd 1984.

equations of that predecessor as special cases of its own equations.[25] But the general applicability of the correspondence principle certainly is not evidence for full-blown realism—but, instead, only for structural realism.

Much clarificatory work needs to be done on this position, especially concerning the notion of one theory's structure approximating that of another. But I hope that what I have said is enough to show that Poincaré's is the only available account of the status of scientific theories which holds out realistic promise of delivering the best of both worlds: of underwriting the 'no miracles' argument, while accepting the full impact of the historical facts about theory change in science. It captures what is right about Boyd's realism (there is 'essential accumulation' in 'mature' science at levels higher than the purely empirical) and at the same time what is right about Laudan's criticism of realism (the accumulation does not extend to the fully interpreted top theoretical levels).

As one step towards clarifying the position further, let me end by suggesting that one criticism which, rightly or wrongly, has been levelled at scientific realism does not affect the structural version. Arthur Fine has strikingly claimed that

> Realism is dead . . . Its death was hastened by the debates over the interpretation of quantum theory where Bohr's non-realist philosophy was seen to win out over Einstein's passionate realism. (p. 21, this volume)

But realism has been pronounced dead before. Some eighteenth-century scientists believed (implicitly, of course; they would not have expressed it in this way) that realism's death had been hastened by debates over the foundations of the theory of universal gravitation. But it is now surely clear that in this case realism was 'killed' by first saddling it with an extra claim which then proved a convenient target for the assassin's bullet. This extra claim was that a scientific theory could not invoke 'unintelligible' notions, such as that of action-at-a-distance, as primitives. A realist interpretation required intelligibility, and intelligibility required

[25] Putnam gives this account of Boyd's position in his 1978, adding that applying the correspondence principle 'is often the *hardest* way to get a theory that keeps the old observational predictions'. I find this last remark very difficult to understand. How exactly could it be done otherwise? (I am assuming that what comes out is required to be a theory in some recognizable sense rather than simply any old collection of empirical statements.) Zahar has shown (see n. 35) how the correspondence principle can be used as a definite heuristic principle supplying the scientist with real guidance. But suppose a scientist set out to obtain a theory which shares the successful empirical consequences of its predecessor in some other way than by yielding it predecessor's equations as limiting cases—surely he would be operating completely in the dark without any clear idea of how to go about the task. (I am assuming that various logical 'tricks' are excluded on the grounds that they would fail to produce anything that anyone (including the anti-realist) would regard as a theory.)

interpretation of the basic theoretical notions in terms of some antecedently accepted (and *allegedly* antecedently 'understood') metaphysical framework (in the Newtonian case of course this was the framework of Cartesian action-by-contact mechanics). Without claiming to be an expert in the foundations of quantum mechanics (and with all due respect for the peculiarities of that theory), it does seem to me that, by identifying the realist position on quantum mechanics with Einstein's position, Fine is similarly saddling realism with a claim it in fact has no need to make. The realist is forced to claim that quantum-mechanical states cannot be taken as primitive, but must somehow be understood or reduced to or defined in classical terms.

But the structural realist at least is committed to no such claim—indeed, he explicitly disowns it. He insists that it is a mistake to think that we can ever 'understand' the *nature* of the basic furniture of the universe. He applauds what eventually happened in the Newtonian case. There the theory proved so persistently successful empirically and so persistently resistant to 'mechanistic reduction' that gravity (understood as a genuine action-at-a-distance force) became accepted as a primitive irreducible notion. (And action-at-a-distance forces became perfectly acceptable, and realistically interpreted, components of other scientific theories, such as electrostatics.) On the structural realist view, what Newton really discovered are the relationships between phenomena expressed in the mathematical equations of his theory, the theoretical terms of which should be understood as genuine primitives.[26]

Is there any reason why a similar structural realist attitude cannot be adopted towards quantum mechanics? This view would be explicitly divorced from the 'classical' metaphysical prejudices of Einstein: that dynamical variables must always have sharp values and that all physical events are fully determined by antecedent conditions. Instead, the view would simply be that quantum mechanics does seem to have latched on to the real structure of the universe, that all sorts of phenomena exhibited by micro-systems really do depend on the system's quantum state, which really does evolve and change in the way quantum mechanics describes. It is, of course, true that this state changes discontinuously in a way which the theory does not further explain when the system interacts with a 'macroscopic system'—but then Newton's theory does not *explain* gravitational interaction, but simply postulates that it occurs. (Indeed, no theory, of course, can explain everything on pain of infinite regress.) If such

[26] See, in particular, Poincaré's discussion of the notion of force (1905: 89–139).

discontinuous changes of state seem to cry out for explanation, this is because of the deeply ingrained nature of certain classical metaphysical assumptions (just as the idea that action-at-a-distance 'cried out' for explanation was a reflection of a deeply ingrained prejudice for Cartesian-style mechanics).

The structural realist simply asserts, in other words, that, in view of the theory's enormous empirical success, the structure of the universe is (probably) something like quantum-mechanical. It is a mistake to think that we need to understand the nature of the quantum state at all, and, *a fortiori*, a mistake to think that we need to understand it in classical terms. (Of course, this is not to assert that hidden variables programmes were obvious non-starters, that working on them was somehow obviously mistaken—no more than the structural realist needed to assert that the attempts at a Cartesian reduction of gravity were doomed from the start. The only claim is that ultimately evidence leads the way: if, despite all efforts, no scientific theory can be constructed which incorporates our favourite metaphysical assumptions, then no matter how firmly entrenched those principles might be, and no matter how fruitful they may have proved in the past, they must ultimately be given up.)

It seems to me, then, that, so long as we are talking about *structural* realism, the reports of realism's death at the hands of quantum mechanics are greatly exaggerated.[27]

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[27] It is not in fact clear to me that Fine's NOA (the natural ontological attitude) is substantially different from structural realism. Structural realism perhaps supplies a banner under which *both* those who regard themselves as realists *and* those who regard themselves as anti-realists of various sorts can unite.

Similar remarks about the 'anti-realist' consequences of quantum mechanics are made—though without reference to Fine—by McMullin (1984: 13). In allegedly defending realism, McMullin *also* seems to me in fact to defend structural realism. See my review of the Leplin volume (Worrall 1988*a*) in which McMullin's article appears.

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