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SCIENTIFIC REALISM AND SCIENTIFIC CHANGE

By John Worrall

On the face of it, science tells us that man has directly witnessed an almost insignificant part of Nature: civilised man has existed for only a millionth part of even the earth's history; he occupies an infinitesimally small corner of the universe, and his puny senses allow him direct access to only a tiny fraction of what is really happening even in his immediate neighbourhood. On the other hand, the science that tells us all this is itself a human invention and must surely in the end be in some way justified by this limited human sense-experience.

This is, of course, the source of the continuing tension between scientific realism and philosophical empiricism. Science taken at face value transcends its "empirical basis". Not only does science have consequences which are, practically speaking, beyond any direct empirical check, for example, consequences about periods when it itself entails that human life was in principle impossible, but it "transcends" experience in still more fundamental ways. For example, theories in mathematical physics are invariably ultra-precise, involving, perhaps, instantaneous velocities having real number values, whilst observation can at best guarantee imprecise statements, in this case about average velocities specified only within a certain rational interval. Moreover, theories involve talk of entities — like forces and particles of various kinds — which have directly observable *effects* but which are themselves far beyond the reach of direct observation.

According to scientific realism, scientific theories *are* to be taken at face value: despite their "transcendence" of the empirical data, they are to be taken as attempted descriptions of the universe which are true or false in the usual, correspondence sense. Whilst this is no doubt the common sense view — one that we would, other things being equal, like to take — many philosophers have argued that other things are not equal. They have argued that this realist view runs into intractable epistemological difficulties — difficulties that can be escaped only by the adoption of an *instrumentalist* view of scientific theories. According to this second view, theory does have an important and irreducible role, but *not* a straightforwardly descriptive one. Theories as a whole are, on this view, codification schemes or scaffolding for the only really descriptive parts of science, namely, those statements which are directly checkable by observation. Theories, then, are either empirically adequate or empirically inadequate, either simple and efficient or complex and inefficient; they are not, however, either true or false descriptions of the world. Instrumentalism also, of course, carries a distinctive view of theoretical terms: these are not to be regarded as having (nor even as intended to have) real reference in the world; instead, the "entities" they "refer" to are fictions — useful fictions which bring order into our systematisation of reality, but fictions nonetheless.

The problems which realism faces, especially those concerning "underdetermination" and the impact of revolutions in science, have attracted a good deal of attention in the recent literature.[1] I intend to begin, however, by taking a step back from this contemporary debate to examine the contributions made to the realism-instrumentalism dispute over many years by Karl Popper. After all, no one has insisted more emphatically than Popper that theories transcend the empirical evidence, that revolutions have played a vital and irreducible role in scientific development and, at the same time, that theories should be interpreted realistically. So if indeed transcendence and revolutions pose problems for realism, we should expect to find some sort of attempted solution of those problems within the Popperian approach. Moreover, all the anti-realist arguments developed by recent instrumentalists are already to be found in earlier writers, whom Popper explicitly attacks. This backward step should not, then, lose sight of any essential feature of the contemporary debate.

Popper has made two contributions to the realism-instrumentalism issue. First, in various papers, and most directly in his famous "Three Views Concerning Human Knowledge",[2] he developed certain arguments against the instrumentalist view of scientific theories — arguments which he himself saw as entailing the "collapse" of this view (*loc. cit.*, p. 111). Secondly, he has developed his own positive view of the status of scientific theories, a view which might be called "conjectural realism". As we shall see, he presents this view as rescuing as much of old-fashioned realism as survives the instrumentalist attack.

In the first part of the present paper I shall, then, examine Popper's arguments against instrumentalism. I conclude that none goes through against better versions of the doctrine. In the second part of my paper I consider Popper's positive doctrine, to see whether it can indeed provide a realism which solves the problems posed by instrumentalists.

I. Popper's Criticisms of Instrumentalism

In his "Three Views" paper, Popper cites explicitly quite a number of thinkers as instrumentalists, and therefore as subject to his attack. The list includes Osiander, Cardinal Bellarmino, Bishop Berkeley, Mach, Kirchoff, Hertz, Duhem, Poincaré, Schlick, Wittgenstein, Bridgman, Eddington, Bohr and Heisenberg. This is quite a motley crew. Everyone on Popper's list is

[1]The most influential recent work has been that of Quine. For recent accounts of the difficulties posed for realism by "underdetermination" and scientific revolutions, respectively, see W. Newton-Smith, "The Underdetermination of Theory by Data", in *Rationality in Science*, ed. Risto Hilpinen (Dordrecht, 1980), and Larry Laudan, "A Confutation of Convergent Realism", *Philosophy of Science*, 48 (1981), 19-49.

[2]Reprinted in his *Conjectures and Refutations* (London, 1963), Ch. 3.

fairly describable as an empiricist or positivist — but these terms, of course, cover multitudes of sins. Certainly there are important differences between the views on scientific theories of, say, Berkeley and Poincaré. With such a large target, some of Popper's arrows are almost bound to score. At the same time, Popper gives some general characterisations of the instrumentalist view, which by no means everyone on his list would subscribe to. For example, he sometimes takes instrumentalism as the view that equates scientific theories with technological "computation rules" (*ibid.*). But this is a view for which Duhem, at any rate, had nothing but scorn and contempt.[3]

In order to avoid confusion, it seems best to proceed by first giving a very broad characterisation of "instrumentalism" and then trying to follow the advice which Popper himself has many times given in other contexts, namely, to identify as the object of critical attack, the best, hardiest *specific version* of this general doctrine. The general view which unites most of the thinkers on Popper's list (as well as others who might be termed "instrumentalists" like the American pragmatists Peirce, James, Dewey and Quine) is that the highest level scientific theories, in so far as they transcend all empirical data, have no straightforward descriptive import. As for the most sophisticated and least vulnerable version of this general view, I have no doubt that this is to be found in the work of the turn-of-the-century French conventionalists — principally Pierre Duhem and Henri Poincaré. There are important differences between the views on scientific theory of even these two philosophers (some of which I shall touch on) but there is enough common ground to extract a coherent, and reasonably complete position.

Both Duhem and Poincaré were definitely instrumentalists on my terms. Duhem's view was that:

> A physical theory . . . is an abstract system whose aim is to *summarize* and *classify logically* a group of experimental laws without claiming to explain them.

It follows that

> the logician who is concerned about the strict meaning of words will have to answer anyone who asks whether physics is true or false 'I do not understand your question'. (*op. cit.*, pp. 7, 168).

Poincaré held a similar view:

> The object of mathematical theories is not to reveal to us the real nature of things, that would be an unreasonable demand. Their only object is to co-ordinate the physical laws with which experiment makes us acquainted . . . .[4]

And Poincaré famously compared the theories of mathematical physics to a library catalogue (a library in which the books, the library's real riches, are the experimental laws). One can ask whether a catalogue is efficient, simple and so on, but one obviously cannot sensibly ask whether it is true.

[3]See, e.g., P. Duhem, *The Aim and Structure of Physical Theory* (Princeton, 1954).

[4]H. Poincaré, *Science and Hypothesis* (New York, 1952), p. 211. (I should immediately add that Poincaré is not always consistent — many of his remarks are more realist in tone. His overall position could arguably be better described as "structural realist".)

I shall argue that none of Popper's arguments strikes home against the positions which Duhem and Poincaré built around this general view. (A defender of Popper might reply that I show only that Duhem and Poincaré were not instrumentalists on Popper's terms. My counter would be that, if so, Popper leaves entirely out of account an important rival position to his own.)

Underlying all of Popper's criticisms of instrumentalism is a thorough dislike of the doctrine, a dislike which stems from the belief that it threatens the dignity and importance of science. According to Popper, science is "man's most exciting intellectual adventure". He presents a dramatic and noble picture of the man of science striving imaginatively to uncover the hidden secrets of the universe, never knowing when he has succeeded but only when he has failed: pursuing a quest that is inevitably unended. Instrumentalism, on the other hand, is a

> narrow and defensive creed according to which we cannot and need not learn or understand more about the world than we know already. A creed, moreover, which is incompatible with the appreciation of science as one of the greatest achievements of the human spirit.[5]

By insisting that the role of scientific theory is *not* to attempt to describe the hidden structure of the universe but merely to codify, and hence to aid the efficient use of, data, the instrumentalist, Popper believes, supplies ammunition to those who regard science as little more than "glorified plumbing".

Popper is, of course, not alone in holding the achievements of theoretical science in high regard. Here are two characteristic passages from other authors:

> It is impossible to follow the march of one of the great theories of physics, to see it unroll majestically its regular deductions starting from initial hypotheses, to see its consequences represent a multitude of experimental laws down to the smallest detail, without being charmed by the beauty of such a construction, without feeling keenly that such a creation of the human mind is truly a work of art.

> The scientist does not study nature because it is useful to do so. He studies it because he takes pleasure in it, and he takes pleasure in it because it is beautiful. . . . I am not speaking, of course, of that beauty which strikes the senses. . . I am far from despising this, but it has nothing to do with science. What I mean is that more intimate beauty which comes from the harmonious order of its parts, and which a pure intelligence can grasp. . . Intellectual beauty is self-sufficing and it is for it, more perhaps than for the future good of humanity, that the scientist condemns himself to long and painful labours.

The first of these passages is from Duhem, the second from Poincaré.[6] It is of course possible that their general views of theories are, unbeknown

[5] Popper, p. 103. (Popper actually says that this is how the issue "should" be seen.)

[6] Respectively, Duhem, p. 24 and Poincaré, *The Value of Science* (New York, 1958), p. 8. (I have slightly modified the translation of the latter passage.)

to them, incompatible with these sentiments: possible, but not actual. Both Poincaré and Duhem simply had reasons different from Popper's for arriving at the same high estimate of the importance of theoretical science. Moreover, the idea that instrumentalism means that we "cannot and need not learn . . . more about the world than we know already" is a caricature of their position. They both gave reasons why science will, and must ever push ahead.

Let us turn then from Popper's reasons for disliking instrumentalism to his main explicit argument against the doctrine:

> My reply to instrumentalism consists in showing that there are profound differences between "pure" theories and technological computation rules, and that instrumentalism can give a perfect description of these rules, but it is quite unable to account for the difference between them and the theories. Thus instrumentalism collapses. (*op. cit.*, 111)

The crucial difference between theories and rules which leads to the "collapse" of instrumentalism is that

> theories are tested by *attempts to refute them* . . . while there is nothing strictly corresponding to this in the case of technological rules of computation or calculation. (p. 112)

As I already mentioned, by no means all instrumentalists would accept the identification of theories with "technological rules", but Popper's argument suffers from a greater defect than this: it is (if I have understood it correctly) entirely circular. Obviously Popper is right that nothing in the instrumentalist view of high-level theories *strictly* corresponds to attempts to falsify them, if by 'falsify' is meant 'show (or strongly indicate) to be false'. This is because the categories of truth and falsity (as usually construed) do not, according to instrumentalists, apply to theories. But, taken literally, Popper's argument is, then, just equivalent to the assertion that instrumentalism differs from the position he himself favours. If the undoubted fact that scientists experimentally test their theories is to count against instrumentalism it must surely be shown that there is nothing in the instrumentalist view analogous to the attempt to falsify a theory.

There is, of course, such an analogue, namely, the attempt to find out the power of the theory, or to find out how comprehensive it is. "Severe tests" are as highly prized by the instrumentalist as by the falsificationist. Duhem, for example, pointed out that a scientific theory always has "some consequences which do not correspond to any of the experimental laws already known". This affords the opportunity to test the theory, since

> Among these consequences some refer to circumstances realizable in practice, and these are particularly interesting, for they can be submitted to tests by the facts. If they represent exactly the experimental laws governing the facts, the value of the theory will be augmented. . . . If, on the contrary, there is among these consequences one which is sharply in disagreement with the facts . . . the theory will have to be more or less modified, or perhaps completely rejected. (Duhem, 28)

It is true that such a theory will have been modified or rejected, not as false, but as empirically inadequate. It will nonetheless have been rejected.

Popper insists, contrary to this, that the (consistent) instrumentalist will never talk of the rejection of a theory but only of the discovery that it has a limited range of applicability. Indeed, it is precisely because the instrumentalist cannot "get beyond the assertion that *different theories have different ranges of application*" that he "cannot possibly account for scientific progress" (Popper, 113). But, of course, the instrumentalist can account for scientific progress — progress, for him, consists of the development of theories of ever greater empirical adequacy, or ever wider ranges of application.[7] He may insist that an older theory may continue to be applied in certain restricted areas — but this is no more than a reflection of scientific practice, and the realist must account for it too. In fact both the realist and the instrumentalist can say that Newton's theory, for example, although "rejected" (i.e., no longer regarded as the best available theory) continues to be applied to slowly moving bodies in the following sense: Newton's theory has indeed been rejected and replaced by the empirically more adequate Einsteinian theory; however, there is a straightforwardly provable metatheorem that, whenever velocities are small compared with that of light, the predictions of Einsteinian theory will be empirically indistinguishable from those of Newtonian theory; hence, since the mathematics of Newtonian theory is more familiar and generally easier, scientists will usually work the problem using Newton, but, in doing this, they are really applying the best available theory, namely Einstein's.

Popper's explicit argument against instrumentalism seems, then, radically flawed. The way he supports the argument, however, reveals an underlying thesis of great importance: this is the thesis that instrumentalism is *heuristically infertile* — that those scientists who adopt it will, in general, do less good science than those who adopt a realist view.

For example, although he actually states that instrumentalists cannot account for attempted refutations, Popper's real thesis turns out, I think, to be the rather different one that instrumentalism condones and encourages a methodologically stultifying reaction to refutations:

> What may appear to us at first sight as a falsification turns out [on the instrumentalist view] to be no more than a rider cautioning us about its [i.e., the theory's] limited applicability. This is why the instrumentalist view may be used *ad hoc* for rescuing a physical theory which is threatened by contradictions . . . (Popper, 113-4).

Similarly he states that "*by neglecting falsification, and stressing application, instrumentalism proves to be . . . obscurantist . . .*" (p. 113).

Popper's claim seems to be that the instrumentalist will see nothing wrong with reacting to refutations of a general theory by simply making

[7]For a recent attempt to characterise precisely the idea that scientific progress consists in the development of ever more empirically adequate theories, see van Fraassen's *The Scientific Image* (Oxford, 1980).

an *exception* of the specific circumstances in which the refutation arises. (Schematically, we start with some general theory $\forall x(Px \rightarrow Qx)$; find an individual a such that Pa but not Qa; and hence switch to the new theory $\forall x(\sim(x=a) \rightarrow (Px \rightarrow Qx))$ & Pa & $\sim Qa$.) More generally the instrumentalist will see nothing wrong with having two or more theories in some single domain which are saved from inconsistency only by being restricted to disjoint sub-domains: after all, we can still deal with all the known facts in the domain even if we must use one tool for one set of facts and a quite different tool for another set.

However, this particular charge of heuristic infertility simply fails to stick against instrumentalism of the Duhem-Poincaré kind. Neither philosopher would be happy to allow *ad hoc* "exception-barring" methods, and both could explain a preference for single theories which cover a whole range of phenomena over sets of different theories which cover the same range, but only when taken together. This is because the methodologies of both Duhem and Poincaré contain requirements of maximum *unity* and *simplicity*.

The aim of mathematical physics, for both philosophers, is not merely to provide just any codification of the empirical laws, but rather *one which is maximally unified and simple*. For Duhem, simplicity was a prime requirement right from the start:

> A physical theory ... is a system of mathematical propositions, deduced from a small number of principles, which aim to represent as simply, as completely and as exactly as possible a set of experimental laws. (Duhem, 19)

He was, on the other hand, less definite about the principle of maximum unity. For reasons which I have never fully understood, he finally placed this principle (quite unlike the principle of simplicity) outside the logic of science proper. It is not a principle which is imposed on the scientist, but it is, nonetheless, one which will guide the actions of the scientist with "finesse". But, since "finesse" is so important for Duhem, the drive for unity should, I think, be included along with the drive for simplicity as part of his overall methodology.

Conversely, Poincaré had no doubts at all about the requirement that theories must be *unified*. Indeed, for Poincaré, this requirement stems from the fact that Nature itself is unified:

> If the different parts of the universe were not as the organs of the same body, they would not react one upon the other, and we in particular should only know one part. We need not, therefore, ask if Nature is one, but how she is one. (*Science and Hypothesis*, 145)

On the other hand, he was somewhat more wary than Duhem about the simplicity requirement, and his views here are rather subtle. He saw that the requirements of unity and (pragmatic) simplicity often pull in opposite directions; and he argued that the facts sometimes force science into complexity, and that sometimes apparent simplicity (such as the interdependence of pressure, volume and temperature of a given mass of gas) is explained by science as due to an averaging out of a very large number of extremely complex phenomena. Nonetheless, it is only if simplicity wins out in the end that science is possible (see p. 149); and, certainly,

> in most cases every law is held to be simple until the contrary is proved. (p. 146)

Neither Poincaré nor Duhem would, then, in fact condone the sort of response that Popper claims any instrumentalist *must* condone, namely, responding to a particular refutation of some more general theory by making particular exceptions or by parcelling out the domain of the original theory to be dealt with by different hypotheses. Such manoeuvres automatically detract from simplicity and unity. Of course, as Poincaré especially insists, scientists may sometimes see no option to such manoeuvres and should not give up the game entirely on this account. The following assertion of Poincaré's might *seem* to play straight into Popper's hands:

> Two contradictory theories, provided they are kept from overlapping, and that we do not look to find in them the explanation of things, may, in fact, both be very useful instruments of research. (p. 163)

However, taken with Poincaré's other remarks about always seeking the simplest theory, this is perhaps just an expression of the sensible "realistic" attitude that we should not scorn the best we can do at present just because it is not maximally satisfying. If Popper's conjectural realism is inconsistent with this attitude, then, as Paul Feyerabend has often pointed out, it is definitely too demanding.[8]

It might be objected that the instrumentalist's requirements of unity and simplicity are extremely, perhaps hopelessly, vague; that the two notions are difficult to distinguish (this is why some of Duhem's and Poincaré's remarks are very confusing); and that, although we seem able to recognise well enough *particular cases* of "unified" and "simple" theories (and, perhaps still better, particular cases of disunified and complex theories), neither Duhem nor Poincaré nor any of their successors has succeeded in giving anything remotely resembling an acceptable *general* account of these notions. I accept all this.[9] However, it can count as an argument for realism against instrumentalism only if the realist can avoid appealing to these notions. Surely, however, the realist needs them too.

Popper's methodological rules certainly condemn *ad hoc* responses to falsification. It would not have been good scientific practice for nineteenth-century astronomers to react to the difficulties with Mercury's orbit by resorting to the "theory" that all bodies except Mercury obey Newton's laws, whilst in Mercury's case such and such happens. But what if God decided — for the sake of variety and in order to discomfit presumptuous mankind

[8]See especially Feyerabend's paper in *The Critical Approach to Science and Philosophy*, ed. M. Bunge (London, 1965).

[9]With one caveat: both Duhem and Poincaré did give very clear examples of how initially simple and unified ideas could become complicated by the accretion of *ad hoc* machinery, designed to accommodate observations which had refuted the original idea.

— to create a world that is generally Newtonian, but with one or two exceptions? In that case, this Popperian methodological rule and the Popperian realist idea that science aims at a true description of the universe would pull in opposite directions. Now a Popperian might respond that he does indeed allow that it is quite possible that by applying his methodological rules we shall be led away from the truth; but practically speaking, there is no doubt that he discounts this possibility. In other words, he implicitly assumes that Nature itself is "simple" and "unified". (As will become clear, I do not believe that his adoption of this assumption should be held against the realist. My only point here is that since both the instrumentalist and the realist need the notions of simplicity and unity, their vagueness is a disadvantage of both positions equally.)

So the charge of condoning methodologically obscurantist reactions to refutations does not stick against better versions of instrumentalism. But it seems hard to believe that there is not some substance to Popper's charge of comparative heuristic infertility. It has, after all, often been suggested that the scientist who believes that his theories are attempted descriptions of the world is likely to see conceptual difficulties in his theories when his instrumentalist colleague can see no cause for concern; and that it is precisely through attempting to solve these sorts of problems that scientists have often in the past achieved what both the realist and the instrumentalist would regard as scientific progress. Perhaps this was what Popper primarily had in mind when characterising instrumentalism as a "narrow and defensive creed".

This heuristic argument can never, of course, be conclusive. Even if it turned out that most scientific breakthroughs were achieved in a way which involved interpreting theories in a realistic fashion, the instrumentalist could still point out, along with Duhem, that

> chimerical hopes may have incited admirable discoveries without those discoveries embodying the chimeras that gave birth to them. (Duhem, 31)[10]

But this reply would surely seem weaker the more it was found that the realist attitude towards theories had played a major role in scientific discovery. No philosophical view is totally compelling, but if it could be shown that belief in the view had actually done some work in creating what was on all sides agreed to be valuable, then this would surely be a strong argument for it.

Does realism, then, outscore instrumentalism in heuristic power? It might seem in our post-positivist age that the answer is unambiguously and rather obviously positive. There is, for example, the case of the atomic-kinetic theory whose development was surely guided by the belief in real

[10]See also p. 95: "Does this mean that no discovery has ever been suggested to any physicist by this [realist] method? Such an assertion would be a ridiculous exaggeration. Discovery is not subject to any fixed rule. There is no idea so foolish that it may not some day be able to give birth to a new and happy idea."

atoms. It is well known that Duhem was a vigorous opponent of the atomic programme and that Poincaré's attitude towards it was less than whole-hearted. Yet the atomic programme led to theories which extended even our empirical "instrumental" knowledge. The realist can, it seems, happily rest his case. Or can he?

Perhaps the realist can establish that his position has greater heuristic power, but I believe that he has not yet done so. There are two main reasons why Popper's claim that realism is superior from the heuristic point of view is, at any rate, unproven.

First, consider somewhat more carefully how the realist is likely to see high-level problems with his theories, to which, however, his instrumentalist counterpart will be blind. These problems are, by definition, not constituted by clashes with empirical results, nor are they outright *logical inconsistencies* — for the instrumentalist can spot and condemn those just as readily as the realist. They must instead be "incoherencies". Presumably no logically consistent and empirically adequate theory can appear incoherent unless it clashes with some previously held general metaphysical view of the world. Any extra heuristic force in realism must come from taking such clashes seriously and looking to modify or replace the theories concerned: from a belief that the world just cannot be the way the latest scientific theory seems to tell us it is.

This is clearly no necessary part of scientific realism, as I have so far characterised it. There have indeed been various disputes in which the "realists" were concerned to defend some metaphysical view or other. But the arch scientific realist, in the sense that I have understood him, simply insists that his present best bet is that the world *is* the way his present best scientific theories tell him it is, and if this clashes with previously held general metaphysical views, then this may indicate the need to revise those views. Of course, those critics of logical positivism who showed that some "meaningless" metaphysics had played an important role in scientific advance were correct. But to land the realist with the additional thesis that clashes between metaphysics and scientific theories are always to be taken as indicating the need for new theories would surely be to make his position absurd. Our new found, post-positivist respect for metaphysics and its power to influence and change science should not blind us to the fact that science has shown at least as much power to influence and change metaphysics. The cases in which clashes between science and metaphysics have been scientifically fertile are counterbalanced by other cases in which metaphysical views have bred a discontent with scientific theory which has absorbed a good deal of effort but which has had no positive results. One clear-cut example is the discontent with, and frequent misunderstanding of, Newton's theory of universal gravitation caused by its clash with Cartesian mechanistic metaphysics. The only outcome of this discontent was a good deal of effort, which, if not entirely wasted, certainly left science unchanged whilst leading to a radical revision of what counted as a mechanistic explanation. It seems that quantum mechanics is turning out to be another example where attempts to reconcile a scientific theory with previously held general views about the world have proved scientifically unfruitful. "Incoherencies" in our theories may be, not unfortunate features which we should seek to eliminate, but instead reflections of real aspects of the world which appear anomalous only because of ingrained metaphysical prejudices.

This shows that the idea that realism is the heuristically more fertile view of theories needs careful handling. But there is a further, and more important, reason why this is so. Duhem and Poincaré did not defend a "narrow and defensive creed" with no heuristic power at all, but rather a creed which involves an alternative account of the main heuristic driving force of science. According to Duhem and Poincaré, breakthroughs in physics come about, more often than not, through considerations purely internal to physics. These are principally to do with mathematical symmetry, coherence and elegance. For example, Duhem contended that many advances are made simply by trying out in new areas laws expressed in mathematical equations of the same form as ones which have already proved successful in other areas:

> The history of physics shows that the search for analogies between two distinct categories of phenomena has perhaps been the surest and most fruitful method of all procedures put in play in the construction of physical theories. (Duhem, 95-6)

Duhem insisted that such a search cannot, or at any rate cannot always, be prompted by general metaphysical ideas about real similarities between the two groups of phenomena: the method has often been applied in cases where these groups are physically quite distinct. For example,

> The laws which govern the distribution of stationary temperatures in a group of good conductors of heat, and the laws which fix the state of electrical equilibrium in a group of good conductors of electricity pertain to absolutely different physical objects. However, the two theories whose object is to classify these laws are expressed in two groups of equations which the algebraist cannot distinguish from each other. (pp. 96-7)

Poincaré emphasised another role that is played by purely mathematical considerations: a scientist might very well arrive at a new theory by spotting an asymmetry in the mathematical equations of the old theory and restoring symmetry by adding a new term. This leads to new equations which, let us say, gain extra confirmation. The new term might well then be given a realist interpretation — but only later, once all the real work of discovery has been done. For example, according to Poincaré, we owe the great breakthrough in electrodynamics to the fact that Maxwell was "steeped in the sense of mathematical symmetry"; hence he looked at the current electrodynamical laws "under a new bias" and "saw that the equations became more symmetrical when a [new] term was added" (*The Value of Science*, 78). Attempts were subsequently made to give a realist interpretation to this term (*via* various ether models), but none of these, insisted Poincaré, bore any fruit in terms of modifying the equations.

Both Poincaré and Duhem explicitly allowed that realist (or "explanatory") considerations certainly *seem* to play a ubiquitous role in scientific development. They claimed, however, that detailed investigation of particular cases would reveal that, *more often than not*, the "explanatory" considerations had tagged along behind, once all the real work had been done by internal, primarily mathematical, considerations. As Duhem vigorously expressed it:

> The descriptive part has developed on its own by the proper and autonomous methods of theoretical physics; the explanatory part has come to this fully formed organism and attached itself like a parasite. (p. 32)

Duhem undoubtedly overstated the case. While there are episodes in the history of science in which scientists have been guided (temporarily at any rate) by largely formal considerations, there certainly are other episodes in which progress has been achieved by insisting on a realist interpretation of terms in equations which had hitherto received none. Moreover, some of Duhem's and Poincaré's accounts of particular episodes are certainly debatable.[11] But if Duhem overstates his case, so does Popper: the choice is not the straightforward one between a heuristically powerful realism and a heuristically sterile instrumentalism; rather these two philosophical positions carry conflicting accounts of the main driving force of science. If realism really is superior on heuristic grounds, this remains to be proved.

II. Popper's "Third View": Realism and Revolutions Reconciled?

Popper's criticisms, then, do not lead to the collapse of instrumentalism. Nonetheless his own positive proposal, his "third view" of scientific theories might, of course, be preferable even to an uncollapsed instrumentalism. Popper himself presents his view as containing everything that can be rescued from older realist positions "after allowance has been made for what was justified in the instrumentalist attack".[12] As we have seen, however, Popper does not allow instrumentalism its full weight. In the present section, therefore, I repeat and revise Popper's exercise. I take three main arguments from Duhem and Poincaré (all of which, especially the second

[11]In particular, Poincaré's account of Maxwell's breakthrough is far from uncontroversial. For an alternative account (which favours the realist view) see Elie Zahar, "Why did Einstein's Programme Supersede Lorentz's?", *The British Journal for the Philosophy of Science*, 24 (1973).

[12]Popper, *op. cit.*, p. 103. The extreme realist position from which Popper starts (his "first" view) is essentialism: the idea that "truly scientific theories describe the "essences" or the "essential natures" of things . . ." (p. 104), and, more importantly from our present point of view, the idea that the "scientist can succeed in finally establishing the truth of such theories beyond all reasonable doubt" (p. 103). I, of course, agree with Popper that the "first" view is entirely untenable and so have concentrated in the present paper on the debate between the "second" and "third" views.

and third, have been developed by more recent philosophers[13]). I try to see whether the realist can successfully counter these arguments and, if not, what allowances he has to make to accommodate them. I then try to assess whether what remains of realism is a worthwhile and defensible position.

(i) *Duhem's argument from idealisation*

Each of the three anti-realist arguments revolves around the fact that theories transcend the observational data. Duhem argued that this transcendence is, in part, a reflection of an inevitable mismatch between theory and reality. He argued that reality must be taken to be as it is revealed to us through observation: fuzzy and imprecise. Theories, on the other hand, are totally precise and hence spruce up or idealise reality. This is one of the reasons why infinitely many logically incompatible but experimentally indistinguishable theories can always be given of the same range of phenomena.[14] And it is also why theories cannot be literally true descriptions of reality:

> The mathematical symbol forged by theory applies to reality as armor to the body of a knight clad in iron: the more complicated the armor, the more supple will be the rigid metal seem to be; . . . but no matter how numerous the fragments composing it the armor will never be exactly wedded to the human body being modelled. (Duhem, 175)

Whether or not Duhem is correct that such a mismatch is inevitable, there certainly are cases where it occurs. For example, whether or not there are any "point-particles" in Nature, Newtonian particle mechanics was certainly successfully applied to objects (like apples and planets) which are clearly not point-particles. These applications involve abstraction or idealisation in a direction indicated by mathematics rather than Nature. There are even laws of which we know that there are, strictly speaking, no real but only idealised instances: the "ideal gas laws" are obvious examples.

The realist asserts that scientific theories are true-or-false attempted descriptions of Nature. But a Newtonian account of planetary motion which treats planets as point-particles (or, equivalently as it turns out, as perfect spheres) cannot be literally true. On the other hand, many would baulk at insisting that any such account must be false. The instrumentalist would be happy to allow that this account is neither true nor false, but simply "empirically adequate". Similarly the ideal gas laws are clearly not true; but many would baulk at regarding them as false. The realist seems embarrassed, but for the Duhemian instrumentalist this is just an especially marked case of a general rule.

[13]I make no claims to do anything like full justice to the recent literature on the subject. I try to give what I see as the bare bones of the anti-realist argument and then the bare bones of a (conjectural) realist response. In the process many important issues are inevitably skated over, and many important papers ignored.

[14]This is true, according to Duhem, even at the level of statements about individual facts: see his distinction between "practical" and "theoretical facts" in *The Aim and Structure of Physical Theory*, Pt. II, Chs. III and IV.

(ii) *Poincaré and the argument from "underdetermination"*

A second argument, one stemming from Poincaré, has proved very influential. Poincaré argued that certain important theories can be shown directly to lack a truth-value in the usual correspondence sense. Physical geometries are his favourite, though by no means only, examples:

> The question: Is Euclidean geometry true? . . . has no meaning. We might as well ask if the metric system is true, and if the old weights and measures are false. . . One geometry cannot be more true than another, it can only be more convenient. (*Science and Hypothesis*, 50)

Certainly a physical geometry is not testable in isolation. In order to test it, we should first have to identify the geometrical notion of a straight line with some physical process — the path of a perfectly unperturbed light ray, perhaps, or the path of a perfectly unaccelerated particle. As the word 'perfectly' indicates, we could always, in the event of the empirical refutation of a geometry thus interpreted, blame the refutation on some physical imperfection — on the existence of some hitherto unsuspected refracting medium or external force. But even in the presence of systematic deviations from the predictions of the interpreted physical geometry, scientists could always hold onto the formal geometry and modify the "co-ordinating definition", that is, give up the identification of 'physical straight line' with, say, 'path of an unperturbed light ray'.

Suppose, for example, that we have made any number of measurements using "perfectly rigid rods". (Of course, no real rod is perfectly rigid, but the known forces which affect length are "differential", i.e., they affect differently rods of different physical and chemical constitutions; and so these imperfections can, at any rate in principle, be identified and corrected away.) Any such set of measurements could be accommodated within any number of different theoretical systems: taking any physical geometry as basic, a characterisation of the "congruence relation" for the rods can then be read off from the empirical results. Suppose, for example, that the angles of triangles marked out by suitably oriented rigid rods are found consistently to sum to something other than 180°. This could be explained either by assuming that the rods remain self-congruent throughout the investigated region (that is, they mark out the same length irrespective of their spatial position and orientation) and that the geometry of the region is non-Euclidean; or it could be explained by assuming that the geometry is Euclidean but the distance marked out by a single rigid rod is not constant but instead varies with spatial position and orientation.

Poincaré argued — as Reichenbach's account made especially clear — that the above means that there is no truth of the matter about which geometry applies to space. The two apparently conflicting accounts are in fact equivalent: no matter of fact is at issue between them. It makes no sense to ask which geometry "really" applies. It makes sense only to ask which geometry it is more *convenient* to apply. (Indeed Poincaré himself held the very strong thesis that it would always be more convenient for us to clothe the facts in the language of Euclidean geometry.[15])

The important point in this, as in Poincaré's other famous examples, is not just that any set of data can be accommodated within, but also that there is a straightforward proposition-by-proposition translation between, the two "different" theoretical systems. To take a second example, Poincaré held that there is no truth about the velocity of the solar system through absolute space, not just because all the facts of planetary and terrestial motions can be accommodated to any assumption about this velocity, but also because this accommodation is so straightforward. (In fact, in so far as the motion of uncharged bodies is concerned, the required accommodation is, of course, nil: we can attribute any uniform absolute velocity to the centre of gravity of the solar system without affecting the (necessarily relative) motions we observe. And Poincaré argued that matters become only moderately more complicated when the motions of electrically charged bodies are taken into account.[16])

If two theoretical systems have exactly the same empirical consequences and if, moreover, there is a straightforward way of translating the two accounts of any given facts, then the two theoretical systems are, according to Poincaré, entirely equivalent, despite any apparent syntactic inconsistencies between them. These disagreements are merely apparent: there is no truth of the matter over which they clash. Moreover, there are several important cases where the possibility of constructing such alternative, but equally factually adequate, systems demonstrably holds.

The argument from "underdetermination of theory by data" has been much canvassed by more recent writers.[17] Various different notions of underdetermination — most of them weaker than the one implicit in Poincaré — tend to be confused. In thc weakest characterisation of the notion, the condition of ready intertranslatability is entirely dropped and underdetermination is considered demonstrated if it can be shown that, given any theory there is a different one with the same (past, present *and future*) empirical consequences. If any set of statements closed under logical deduction is regarded as a theory, then the general existence of underdetermination in this weak sense is a completely trivial consequence of the transcendent nature of scientific theory. (Simply pare down the given theory to its set of empirical consequences and then consider any conservative extension of that set back into the original theoretical language.)[18]

[15]"Euclidean geometry is, and will remain, the most convenient . .", *op. cit.*, p. 50. (The important reference for Reichenbach's account is, of course, *The Philosophy of Space and Time* (New York, 1958). For an exceptionally clear exposition see also Wesley Salmon, *Space, Time and Motion* (Minneapolis, 1980).)

[16]See especially his *Electricité et Optique* (Paris, 1901).

[17]See especially Newton-Smith, *op. cit.*

[18]It was presumably this sense (or something like it) that Quine had in mind when he stated that he "expected wide agreement" on the thesis that there are theories which are "logically incompatible and empirically equivalent": "On the Reasons for Indeterminacy of Translation", *The Journal of Philosophy*, 67 (1970), 179.

The anti-realist argument which underdetermination threatens to supply goes roughly as follows. The realist is caught on the horns of a dilemma. Either he regards any set of underdetermined theories as entirely equivalent — in which case he accepts a positivistic reduction of his theories and essentially abandons his realism;[19] or he continues to regard his presently accepted theory as a true or false description of reality, both hidden and revealed, and indeed as his present best guess as to the truth, whilst at the same time allowing that other theories could readily be constructed which receive equal warrant from all the data (future as well as past) and yet which tell a different story about "hidden reality".

This certainly seems an untenable position. Whether or not the realist is indeed forced into it by the phenomenon of underdetermination is a question to which I shall soon turn. (We shall see that the distinction between different notions of underdetermination is crucial to the realist response.)

(iii) *The Argument from Scientific Revolutions*

There is a third argument whose persuasive force has, historically speaking, proved greatest of all. The apparent fact that there have been revolutions in science — radical discontinuities at the theoretical level — points to a different type of underdetermination: to the non-negligible possibility that the observational and experimental results we know at present and take to support some theory T will turn out to be equally well, or better, explained (along with some others, perhaps so far unknown) by some quite different theory T' — a theory which entails a quite different account of the basic structure and contents of the universe. If we take seriously the highly theoretical parts of science as intended descriptions of the world, then we shall have to admit that there have indeed been many radical revolutions in science. For example, science long ago told us that light is some sort of "effluvium", later that it is a discrete material particle, then a continuous wave in an all-pervading medium, then a sort of particle-wave hybrid; at one stage science told us heat is some sort of fluid, at another that it is molecular motion; and so on.

The argument that this supplies against realism can be put, very roughly, in the form of a question: why we should have any confidence in what present-day science might seem to tell us about some aspect of the basic structure of the universe when science has changed its mind so often about this basic structure in the past? The history of science gives us no reason to believe that our high-level theories and the entities they introduce will survive indefinitely. On the contrary, history gives us, if anything, reason to believe that the "entities" through which theories presently explain certain observable phenomena will be swept aside by some future revolution

[19]Dummett's strict positivism leads him to regard Quine's claim (previous footnote) as "absurd" since "there could be nothing to prevent our attributing the apparent incompatibility [between the theories] to equivocation" (*Frege*: *Philosophy of Language*, p. 167n).

(just like phlogiston, caloric, the aether and the rest). On the other hand, the empirically successful consequences of our theories do seem to be preserved somehow or other in superseding theories. Better then to stick to this empirically successful part of science and to regard the rest as temporary scaffolding.

There is no doubt that this discontinuity argument was a strong motivation behind the adoption of instrumentalism by both Poincaré and Duhem. The former wrote:

> 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 another; 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*. (*Science and Hypothesis*, 160)

Fortunately this despairing conclusion does not follow from the correct view of scientific theories. The contradictions reside only in the parts of theories which claim to describe underlying realities and in fact "merely name . . . the images which we substituted for the real objects which Nature will hide forever from our eyes" (*ibid.*, 163). In all scientific revolutions, the real "representative" descriptive part of the older theory will have been incorporated into the newer theory: although we may change the framework within which we codify scientific results, the accumulation of these results is essentially continuous.

Duhem expressed an almost identical view. All contradictions in science reside in the "explanatory parts" of theories:

> When the progress of experimental physics goes counter to a theory and compels it to be modified or transformed, the purely representative part enters nearly whole in the new theory, bringing to it the inheritance of all the valuable possessions of the old theory, whereas the explanatory part falls out in order to give way to another [entirely different] explanation. (Duhem, 32)

But this (alleged) "explanatory part" is chimerical on Duhem's view. Hence, again when viewed properly, there is a real continuity in science which, however, "is not visible to the superficial observer due to the constant breaking out of explanations which arise only to be quelled" (p. 33). The progress of science is in fact like a mounting tide:

> Whoever casts a brief glance at the waves striking a beach does not see the tide mount, he sees a wave rise, run, uncurl itself, and cover a narrow strip of sand, then withdraw leaving dry the terrain which it had seemed to conquer . . . . But under this superficial to-and-fro motion, another movement is produced, deeper, slower, imperceptible to the casual observer; it is a progressive movement continuing steadily in the same direction and by virtue of it the sea constantly rises. (pp. 38-9)

(iv) *The Realist's Response to these Arguments*

I now turn to consider how the realist might respond to these arguments, and to assess the acceptability of any shifts which the realist must make in the process.

In response to Duhem's claim about an inevitable mismatch between (precise) theory and (imprecise) reality, the realist will surely point out that Duhem makes a metaphysical assumption, and one which there seems no very good reason to accept. The undoubted fact that our observational procedures, no matter how delicate, are bound to be imprecise to some degree does not, of course, entail that reality is like that. Duhem makes an assumption, and the realist simply makes a counter-assumption: that reality is precisely delineated and hence that there is no reason in principle why our precise mathematical theories should not describe it accurately, even if our clumsy observations never test a precise point prediction of such a theory. It should be noted, then, that metaphysical assumptions underlie the positions of both the realist and the instrumentalist.

As for the point about applying particle mechanics to non-particles and ideal gas laws to real gases, the realist has two optional responses. He might allow that these idealised theories are indeed neither-true-nor-false, claiming that he can allow a few exceptions so long as the general rule is that theories have definite truth-values. However, this would surely be an unwise response in view of the ubiquity of idealised or simplifying assumptions in science. Better to grasp the nettle firmly and insist that such simplifying assumptions are best regarded as false.

There is no real difficulty in adopting this line in the case of the ideal gas laws. These are, indeed, best regarded not as true laws of shadowy idealised gases, but as false theories about real gases: false theories which, however, yield predictions that are, in a wide range of circumstances, close enough to the truth for all practical purposes. In fact, when scientists apply the ideal gas laws, they are best understood as really applying much more realistic laws. There is, however, a general proof that the prediction made by these realistic laws will, in the circumstances considered, be almost undetectably different from that achieved — with a great deal less analytical effort — from the false idealised law.

In the Newtonian case, matters are slightly more difficult since there it could be argued that every real world application will involve some idealisation. No theoretical astronomer would, I take it, even attempt a Newtonian account of the motion of, say, the moon which took into consideration its precise topographical features — every mountain and crater, every nook and cranny. This is not merely because we do not know these exact details, but because we know that the craters and mountains are small enough to affect the prediction hardly at all. So taking them into account would only enormously (perhaps impossibly) complicate the mathematics. The theoretical astronomer also has to make an assumption about the density distribution within the moon: in the usual account the assumption is of spherical symmetry. Here we have (or had until recently) little direct knowledge, but the symmetry assumption is of course hardly likely to be strictly correct. Again the realist can surely admit that this idealising assumption is false, although good enough for practical purposes (i.e., it yields observational consequences which are not noticeably false). On the realist account, the Newtonian theory of gravitation — that every material particle attracts every other in a certain way — is an attempt truly to describe the universe. However, in applying the theory it is convenient (perhaps even necessary in the present state of mathematical knowledge) to make certain false assumptions which are known (or assumed) to be close approximations to the truth.[20] These assumptions together with the theory therefore yield predictions which (so solid-sounding continuity arguments imply) are themselves close to the truth — perhaps experimentally indistinguishable from it. The Newtonian theory of the moon's motion is not to be identified with the standard text-book account, which, because it assumes the moon is a perfect sphere (and for other reasons), fails fully to fit the real world. The real Newtonian theory is a much more general one: that each "moon particle" is subject to certain gravitational forces. This is certainly untestable as it stands but it was intended nonetheless to fit the world precisely.

The realist response to this first argument seems to me to carry a good deal of plausibility. But it should be noted that the response already presupposed that scientific theories may be true-or-false even though radically underdetermined by the data: for it must, of course, be admitted that, in consistently substituting mathematically precise statements for observationally imprecise statements, scientists are choosing one of infinitely many possible precise statements which are compatible with observation.[21] I turn now then to a fuller consideration of the second and apparently much more threatening anti-realist argument — the one from underdetermination.

Suppose that T is the presently best available theory in some field. The realist enjoins that we take T's "transcendent" part as an attempt to describe the reality hidden behind the phenomena. The underdetermination argument threatens to demonstrate that there are always rival theories, T', T'', . . . which stand on a par with T so far as the evidence is concerned but which tell quite a different story about "hidden reality". Two questions arise. Would this threatened demonstration, if it materialised, make realism untenable? And, if so, does the demonstration in fact go through?

My answer to the first question is, yes. Such a demonstration would force the realist into simply insisting that theories have truth-values whilst allowing that we are never in a position even to say what is the present best

[20]This means "close to the truth in certain well-defined respects" — relative to the basic, primitive functions and predicates specified by the theory itself. David Miller has shown in "The Accuracy of Prediction", *Synthese*, 30 (1975) that no false theory is closer to the truth in all respects than any other.

[21]See Duhem's treatment of the relationship between "theoretical" and "practical facts", *op. cit.*, Pt. II Ch. IV.

guess as to the truth in any field. Of course someone whose realism is, in the jargon, "purely semantic" would be happy to occupy even this position. For him no obstacle in the way of deciding what truth-value is possessed by a theory has any effect on the question of whether or not the theory has a truth-value. I am not sure that this view altogether deserves the belittling qualifier 'purely': it is often important to insist, against positivists of various kinds, that it does make perfect sense to separate sharply the questions of how we decide the truth-value of a sentence and of whether or not it has one. Nonetheless, if the scientific realist did admit that we are never in the position of knowing even what our present best guess about the truth is, then one would surely not need to be anything like a strict positivist to regard his realism as amounting to very little.

The interesting question, then, is whether theories are underdetermined to the extent required to force this admission from the realist. I shall argue that there are three notions of "underdetermination" which should be kept quite distinct but which have often been conflated. There are senses in which there is a general, but uncontroversial, guarantee of the existence of underdetermined theories, and there is a sense in which underdetermination would, if general, really trouble the realist. However, there is no sense in which underdetermination can be both guaranteed to exist generally and shown to trouble the realist.

It is surely true that there is always more than one set of theoretical assumptions which imply *all the empirical evidence known to date.* Indeed this is a straightforward consequence of Duhem's important point that "single" scientific hypotheses have no directly testable empirical consequences. Instead, only rather large (though of course finite) groups of "single" hypotheses have such consequences. This means that, in the event of an empirical refutation, purely logical considerations leave a good deal of leeway as to which particular hypothesis to replace. Moreover, as Duhem emphasised and as recent case-studies have confirmed, scientists have exploited this leeway a good deal more heavily than nutshell histories of science tend to suggest. Various early nineteenth-century interference and diffraction experiments are often cited as having established the superiority of the classical wave theory of light by crucially refuting the corpuscular theory. In fact there were well-established ways of incorporating such phenomena within the corpuscular-theoretic framework.[22] The Michelson-Morley experiment used sometimes to be cited as a crucial refutation of classical physics but, as is now widely recognised, the null-result can in fact be explained classically *via* the Lorentz-Fitzgerald contraction hypothesis. This indicates that, by piecemeal adjustments to one of them, two theoretical systems which certainly clash at the top can be made each to incorporate all the

[22]For some of the details see my "Thomas Young and the "Refutation" of Newtonian Optics", in *Method and Appraisal in the Physical Sciences*, ed. Colin Howson (Cambridge, 1976).

known empirical data. The work of Lorentz and Poincaré indicated how this could be done in the case of classical physics and special relativity theory. In the case of the wave-corpuscular rivalry in optics, no system of corpuscular optics was ever fully worked out which was empirically equivalent to the wave theory of Young and Fresnel, but Biot showed how it could be done for the phenomena of polarised light and Newton, Brougham and others had earlier indicated how it could be done for interference and diffraction effects (which they explained as due to "inflexion"). The task was never completed, not because it was logically impossible, but because it did not seem scientifically worthwhile.

This is, of course, the important point. No one (least of all Duhem) suggests that the facts may not continue to favour one theoretical system even when they have been incorporated within a rival system. The rear-guard actions in defence of some favoured hypothesis, to whose logical possibility Duhem pointed, present scientists with no difficult problems of appraisal. Such actions are invariably both unsuccessful and short-lived. Even where some empirical result newly predicted by theoretical system S is incorporated *post hoc* into a rival system S', the result is generally still regarded as lending support only to S. Eventually the defenders of S' surrender. (The historical fact that from this point on S will outstrip S' in terms of simple empirical content should not cloud the logical fact that S' will still be modifiable into a system having equal known empirical content.) In sum, the fact that two different theories each yield a given observationally accepted consequence does not mean that scientists will regard that consequence as lending the same support to both theories. Hence two theories may each yield all the known empirical results and yet one of them be taken as much better supported by the data than the other.

This sort of intuitive scientific judgment is, of course, reflected in most presently canvassed accounts of empirical support. For Bayesians, support depends not just on the probability of the evidence given the hypothesis but also on the prior probability of the hypothesis. For Popperians, a directly empirically testable statement may follow from each of two theories and yet present the opportunity for a genuine test of only one. Although it is not always explicitly admitted, this Popperian thesis is closely connected with considerations of the relative simplicity of the two theories. Similarly the arch-realist Einstein asserted that there are two separate criteria for acceptable theories — an "external" criterion of simple agreement with the empirical data, and an "internal" criterion of "harmony" and "simplicity".[23] The realist can simply claim that this extra factor in empirical support, over and above merely yielding the correct empirical results, is somehow or other connected with the likelihood of the theory's being true of the world. The sort of complexity and disunity which scientists invariably find repug-

[23]See especially his "Autobiographical Notes" in *Albert Einstein, Philosopher-Scientist*, ed. P. A. Schillp (Evanston, 1953).

nant invariably arises from elaborate modifications of a theory made in an effort to accommodate *post hoc* empirical results which had fallen naturally out of some rival theory. It is precisely in such cases that scientists generally will not regard the results as supporting the patched-up theory. So the fact that, given any theory, we can construct another, different, theory with the same empirical consequences will not impress the scientist nor, if his formal criterion of empirical support reflects the scientist's intuitive judgments, need it affect the realist.[24]

But then the admitted fact that, given the accepted theory in any field, scientists could, with sufficient ingenuity, create a theory which clashes with it but which equally well implies all the known data, does not entail that we may not reasonably continue to prefer the accepted theory on empirical grounds. Duhem's point shows that empirical support is not just a question of yielding the right empirical data; it does not establish a sense of underdetermination which will trouble the realist.

Two theories which each entail all *known* empirical results may, of course, turn out to clash over some new result. But can we not guarantee the existence of theories which are fully observationally equivalent to some given one, i.e., which have exactly the same known *and unknown* observational consequences? Again we surely can supply this guarantee — at any rate, in principle. Take the given theory, strip it down to its set of observational consequences and consider any conservative extension of this set back into the original theoretical language. Or take the given theory T and conjoin to it any purely theoretical statement s, that is, one which has no observational consequences of its own, and one which, when either it or its negation is conjoined with T, produces no extra observational consequences. Then $T \mathbin{\&} s$ and $T \mathbin{\&} \sim s$ are inconsistent theories with exactly the same observational consequences.[25]

But, as before, none of this entails that, given any theory we can produce a rival which stands on a par with it *vis-à-vis* the empirical data. There are, of course, all sorts of reasons for objecting to the rival theories arrived at in the ways suggested. For example, the first method does not even guarantee that the theory will be finitely axiomatisable; whilst in the second case both $T \mathbin{\&} s$ and $T \mathbin{\&} \sim s$ would clearly be regarded as unacceptable. The realist regards the extra virtues of the accepted theory as not merely pragmatic but as somehow indicating greater likelihood to be true.[26] This second sort

[24]For more details of the account of empirical support implicit in these remarks see my contributions to *Progress and Rationality in Science*, ed. G. Radnitzky and G. Andersson (Dordrecht, 1979).

[25]The problems posed by examples of this sort for the notion of empirical support are discussed in an important book by Clark Glymour, *Theory and Evidence* (Princeton, 1980). See also my review of Glymour's book in *Erkenntnis*, forthcoming.

[26]This undoubtedly involves the realist in extra metaphysical assumptions about the world. But without some such assumptions no sense can, I believe, be made of the development of science. The view, still prevalent amongst philosophers, that the fewer such assumptions the better, is not an assumption that I share.

of underdetermination is, like the first, admittedly general; it points, again like the first, to the fact that empirical support is not just a question of entailing the right empirical results; but it does not threaten realism.

The claim that there always is, in the Platonic heaven, a fully empirically equivalent, but rival theory to any given one is unlikely to trouble scientists. But what if the rival can actually be brought down to earth? What if it can actually be constructed, not in a piecemeal, but rather in a systematic way? This brings us to the third and strongest sense of underdetermination and back to Poincaré's famous examples. There are pairs of theories which are apparently contradictory but between which there is a straightforward translation procedure. This procedure allows any account of some phenomenon in terms of one of the theories to be turned into an account of that phenomenon in terms of the other. This translatability, of course, guarantees empirical equivalence of the two theories not just with respect to known results but with respect to all possible results.

Underdetermination of this sort certainly appears to pose a much greater threat to realism. It seems quite reasonable for early nineteenth-century scientists to have continued to hold that the various known polarisation effects supported only the wave theory of light and not Biot's highly modified corpuscular theory, because of the number of highly ingenious, complex and arbitrary hypotheses Biot was clearly forced to adopt precisely so as to accommodate these facts. But to translate one Newtonian account of observed planetary motions based on one supposition about the absolute velocity of the centre of gravity of the solar system into an account based on a different such supposition is a straightforward, mechanical exercise. There are surely much stronger grounds in the latter case for regarding the rival accounts as equally supported by the facts they both yield.

If this claim of equal empirical support in such cases were accepted and if this sort of underdetermination were demonstrably general, then the realist would indeed be forced into a position which I have already admitted is untenable. Fortunately for the realist, there is — so far as I know — no general proof of underdetermination in this stronger sense. All that exist are treatments of particular cases in which two or more syntactically different theories arguably stand on a par with respect to all possible empirical data. The ready intertranslatability of the theories in such cases has convinced some philosophers (notably Reichenbach) that the "different" theories ought in fact to be regarded as, not just empirically, but fully equivalent. A precise account of synonymy is required to decide this issue and I have none to offer. Happily this seems not to matter so far as the realism-instrumentalism debate is concerned: the realist can successfully accommodate these cases whether or not the various theories involved are regarded as at bottom synonymous.

If, in, say, the Poincaré-Reichenbach example, the two different geometries with two different, but compensating, congruence relations are regarded as synonymous, then the realist will presumably say that we have simply learned, in this case with some difficulty, what to have a realist attitude towards: what makes an assertion about the world is a set of geometrical axioms interpreted *via* a congruence relation. True, if the realist generally regarded any two empirically equivalent theories as synonymous then he would, of course, have surrendered his grounds for disagreeing with the instrumentalist. But, so long as this admission is restricted to these particular and exceptional cases of apparently conflicting but readily intertranslatable theories, then realism remains distinct and plausible.

On the other hand, if the two physical geometries are regarded as definitely non-equivalent then the realist has two options. He might argue that, despite the intertranslatability, one theory is better empirically supported than the other: the theory based on Euclidean geometry, even though empirically adequate, requires the unacceptable postulation of a series of systematic coincidences which obscure the true nature of space. This seems to reflect scientific practice in this and certain similar cases: the general theory of relativity, incorporating Riemannian geometry, is regarded as a systematically better account of the phenomena than possible classical (and Euclidean) rivals. But even in examples where the data do seem to be neutral between conflicting accounts such as in the case of the various Newtonian accounts of planetary motion, the realist remains undefeated. He can simply allow that, in the domain of these particular theories, there is no present best guess as to the truth. Again, if this admission were a general one the realist position would become empty, but so long as the admission is restricted to particular and exceptional cases, realism's plausibility remains intact.[27] Especially is this so, since, in such cases, scientists will generally be discontented with all the available theories and will look to replace them, at any rate in the long term, with a theory which does not share this degree of underdetermination. When it became clear that, within classical physics, *any* absolute velocity could be given to the centre of mass of the solar system and the "appearances" still saved, then there was a good deal of discontent with the absolute space hypotheses. The realist interprets this discontent as indicating, *not* that the absolute space hypothesis is neither true-nor-false, but rather that the hypothesis has turned out not to be sufficiently closely integrated with the rest of the system of classical physics to share in the latter's overall empirical confirmation.

Finally, what reply can the realist make to the argument from scientific revolutions? This argument is surely still the greatest threat he faces. The problem again concerns the issue of how much "epistemological ingredient"

[27]I disagree, then, with Newton-Smith's assertion (*op. cit.*, p. 105) that "Given that there can be cases of the underdetermination of theory by data, realism . . . has to be rejected". What is needed to show that realism is untenable is a demonstration that underdetermination is ubiquitous.

the realist can legitimately add to his position.[28] Had science's development been continuous and cumulative, had later theories simply extended earlier theories instead of radically revising them (at any rate at the highest theoretical levels), then the situation would of course be unproblematic. No doubt the view that present theories are true (perhaps only parts of the truth, but nonetheless true) would, even in that case, be a conjecture, but at least it would be a conjecture unchallenged by the history of science. Things being as they actually are, if realism were held to carry with it the claim that the presently accepted theory in any field is actually a true description of reality, then it would surely be absurdly presumptuous. After all, Newton's theory was firmly believed to be true for over a century, but is now regarded as false.

Or is it? Is it not rather "approximately true"? Does not a deeper analysis of scientific change reveal an essentially continuous development underlying the apparently dramatic changes involved in the Einsteinian, and other, revolutions? After all, no serious philosopher, even in the early nineteenth century when Newton's theory was still regarded as absolute truth, really believed that the development of dynamics and astronomy up to that point had been strictly cumulative; rather the talk was of later theories incorporating and (slightly) correcting their predecessors.[29] If the history of science were a history of essential continuity across revolutions then it would be quite consistent with at any rate a watered-down epistemological ingredient in realism. This ingredient would make realism say, roughly, that we have good reason to hold that our presently accepted theories are at any rate approximations to the truth.

This is precisely the line taken by recent realists like Boyd and Putnam who add that this epistemological ingredient carries with it the further assumption that the theoretical terms involved in presently accepted theories refer, at any rate approximately, to real world entities.[30]

Popper's somewhat earlier development of the notion of *verisimilitude* also seems to have been aimed at supplying realism with a watered-down but still active "epistemological ingredient". The notion was intended to make sense of the idea that "we can, and often do, approach more and more closely to the truth".[31] Of course, Popper insists that the claim that one theory is more verisimilar than another can never be more than a conjecture

[28]'Epistemological ingredient' is Newton-Smith's phrase (*op. cit.*).

[29]Thus, although Whewell, for example, speaks of Newton's laws assuming Kepler's as *facts*, he also stresses that Newton's theory revealed some earlier observations to be only approximations (W. Whewell, *History of the Inductive Sciences*, ed. G. Buchdahl and L. Laudan (London, 1967), vol. 2, p. 136). And when it comes to Fresnel's wave theory of light and its treatment of double refraction, Whewell remarks, "Thus this beautiful theory *corrected, while it explained* the best of the observations which had previously been made . . ." (*ibid.*, 335; emphasis supplied).

[30]Putnam, citing Boyd, asserts that "(1) Terms in a mature science typically *refer*. (2) The laws of a theory belonging to a mature science are typically approximately true." "What is Realism?", *Proceedings of the Aristotelian Society*, 76 (1975-6), 179.

[31]*Conjectures and Refutations*, 231 (original italics omitted).

based on (but never justified by) the finite sample of already tested observational consequences of the two theories. But at least post-verisimilitude Popperians may, and indeed are encouraged to, conjecture that successive theories in a given field (say Aristotle's, Galileo's, Newton's and Einstein's theories in mechanics), though probably all false and though invariably logically inconsistent with one another, may nonetheless have increasing verisimilitude — that, by proceeding through the sequence of successive theories, science may have approached more and more closely to the truth.

These ideas are clearly predicated on the assumption that, despite apparent revolutions, the development of science can be shown to have been essentially continuous. Is this assumption really tenable? There is no doubt that some recent philosophers have exaggerated the extent of discontinuity in scientific development and that the picture at the empirical level is indeed of essential continuity. It has proved more difficult than might have been expected to spell out exactly what "essential continuity" involves, even at this level. The problems are first that, while a new theory will indeed typically explain the empirical success of its predecessor, it will not generally do so by yielding the same empirical consequences. Instead it generally yields consequences which are strictly inconsistent with, but only slightly (perhaps imperceptibly) different from, the empirical consequences of its predecessors. A second problem is that occasionally a temporary loss of content even at the empirical level is involved in switching to a new theory (although it should be said immediately that always in such cases failing to make the switch would lead to even greater loss, and, more importantly, scientists developing the new theory will attempt to restore continuity by making good the loss).

These problems explain why the idea of "essential continuity" at the empirical level is difficult to characterise formally, but this idea surely remains intuitively correct. Successive theories in optics, for example, have brought within their compass ever more empirical results about light: earlier theories dealt with simple reflection and refraction, subsequent theories with these plus the phenomena of interference and diffraction, then polarisation effects, the connections between light and magnetism, and so on. It is true that, looked at more closely, this was no case of straightforward accumulation — the empirical content of the n-1th theory is not a proper subset of the empirical content of the nth. For example, theories prior to Fresnel's wave theory had included the simple law of reflection (angle of reflection = angle of incidence). Fresnel's theory certainly explains this part of the empirical success of its predecessors, but not by yielding this simple law; indeed it strictly speaking contradicts it. Instead Fresnel's theory explains the law as an approximate, large-scale effect, empirically indistinguishable from the truth, except in special circumstances. It is also true that this series of theories involved one or two hiccups — examples of temporary "Kuhn loss" of even low-level empirical content. One example concerns the phenomenon of prismatic dispersion. The phenomenon received from the Newtonian corpuscular theory an explanation which, as far as this one effect was concerned, was entirely straightforward; but it remained without a satisfactory explanation within Fresnel's wave theory for around fifty years. For the most part, however, the empirical success of older theories is somehow or other explained by the new theory and even where "Kuhn loss" occurs the scientists who have accepted the new theory work hard to make good the loss: the continuity idea plays the role here of a regulative principle.

Several philosophers have pointed out that this "essential continuity" is not restricted to the purely empirical level but often extends to the level of the mathematical equations of the theory.[32] There are exceptional cases in which the equations of the older theory are taken over wholesale and unchanged by the new theory, despite the dramatic changes in theoretical interpretation of the terms of the equations brought about by the switch. (A famous example is again supplied by the history of optics. Several of Fresnel's equations reappear entirely unchanged as special cases of Maxwell's equations, although the latter completely reinterprets light as an electromagnetic phenomenon.) Usually, however, the equations involved in successive theories are logically inconsistent, but there is a straightforward sense in which the new equations reduce to the old as some mathematical quantity becomes smaller and smaller. Hence there can be, at the same time, logical discontinuity and mathematical continuity. Indeed, the requirement that the new theory yield the equations of the old as limiting cases has figured in science, not only as an adequacy requirement (or, rather, as one particular way of fulfilling the general adequacy requirement that the new theory explain the empirical success of the old), but also, and even more importantly, as a heuristic principle guiding the construction of new theories. (The clearest account of this role of "the correspondence principle" has been given recently by Elie Zahar.[33])

But this is surely as far as continuity extends: if there is essential continuity at the empirical level in science, and even at the level of mathematical equations, there appears to be no continuity whatsoever at the highest theoretical levels. Taking the history of optics again and looking this time at what successive theories have said about the basic constitution of light, we find enormous and seemingly unbridgeable discontinuities: effluvia gave way to material particles, these were superseded by disturbances in an all pervading medium, which in turn gave way to currents, changes in the electromagnetic field, and then to spatially localised photons obeying an entirely new and indeterministic quantum mechanics. There is simply no sign here of any convergence on one unique picture of reality, no sign of our approximating closer and closer to the truth.

[32]See, e.g., Heinz Post, "Correspondence, Invariance and Heuristics: In Praise of Conservative Induction", *Studies in the History and Philosophy of Science*, 2 (1971), and W. Krajewski, *Correspondence Principle and Growth of Science* (Dordrecht, 1977).

[33]"Logic of Discovery or Psychology of Invention?", *The British Journal for the Philosophy of Science*, 33 (1982).

Both the approach of Boyd and Putnam and that of Popper have run into special difficulties, but, in view of sequences like the one presented by the history of optics, both approaches seem quite generally at odds with a genuinely realistic construal of scientific theories.

All Popper's attempts to characterise formally the notion of verisimilitude have turned out to be unsound.[34] It seems to me nonetheless likely that a reasonable characterisation of *empirical verisimilitude* (of which successive theories may have had ever more) might be rescued from these difficulties. But the idea that science may present us with a series of theories which have increasing overall verisimilitude seems to me not merely hard to characterise but generally and intuitively unsound. Suppose Einstein's theory is true. We should then certainly want to allow that Newton's theory is a good approximation empirically speaking. But would we want to say that Newton's theory, interpreted realistically, is close to the truth? The natural judgement is surely, on the contrary, that it is plain false: it involves the assumptions that space is absolute, that two events simultaneous for one observer are simultaneous for all, and that the mass of a body is a constant independent of velocity — all of which are just wrong. Or, more clearly still, suppose the truth is that heat is molecular motion. Would we then want to say that the theory that heat is a sort of fluid is approximately true, or close to the truth?

As for the Boyd-Putnam approach, special difficulties have attended the attempt to provide clear accounts of "approximate truth" and "approximate reference". But again the whole approach seems at odds with the facts of scientific development; unless, speaking loosely, "empirical verisimilitude" and "overall verisimilitude" are to be identified: unless, that is, it can be successfully argued that the radical discontinuities, even at the highest theoretical level, disappear when properly analysed.[35] It might be argued, for example, that Einsteinian photons are not, after all, so very different from classical waves of light. Photons, when observed, may be spatially discrete, but they nonetheless have the property of exhibiting, when in bulk, various wave-like properties, for example, the capacity to produce interference and diffraction patterns. Or, to take a second example, it might be argued that the caloric theory should not be interpreted as having involved the assumption that heat is a real, substantial fluid, but only that

[34]The faults in Popper's original (and basic) definition were first pointed out independently by David Miller and Pavel Tichy, *The British Journal for the Philosophy of Science*, 26 (1975).

[35]Another possibility, again actually employed by Putnam and Boyd, is to restrict the continuity claim to "mature" science: there is indeed an unbridgeable discontinuity between the caloric and kinetic theories of heat but the science of heat had not yet attained maturity when the caloric theory was accepted. In the absence of any serious account of scientific maturity, this is a very convenient device. The problem of course is that it will have to be employed much more often than its defenders would wish. Was nineteenth century "classical" physics really "immature"? Certainly the relativity and quantum revolutions have completely overturned its central highest-level theoretical assumptions. (See also Laudan's remarks on this topic, *op. cit.*).

it was a "something or other" which "flowed" from hotter to colder bodies (but never *vice versa*), of which different materials need more to increase their temperature by a given amount, and so on. All of which would tend to eliminate the clash with the later theory.

This approach (which seems actually to be the one taken by Boyd and Putnam) can be made to work to some extent: precisely because it is nothing more than a lightly veiled restatement of the claim that the development of science has been "essentially" continuous at the empirical level and even at the level of mathematical formulae, a claim which I have just admitted seems "essentially" correct. However, this success is clearly bought by in effect abandoning realism, as usually understood, and instead espousing a positivist approach which regards theoretical entities as essentially characterised by their observable properties. This sort of move is likely to make realists of even arch-instrumentalists like Duhem and Poincaré. It was, after all, precisely so that continuity in science could be restored that Duhem and Poincaré recommended that the highly theoretical and highly revisable parts of science not be interpreted in a realistic, descriptive fashion. Of course, if we first cut science down to size *via* some empiricist re-interpretation, then most people will be happy to hold a realist attitude toward to result. But a genuine realist will surely, contrary to all this, have to insist on a sharp difference between photons and waves, and between fluids and motions (differences which may be important heuristically) even though the elements in these pairs may share many observable properties.

Of course, Popper, who has always emphasised the importance and irreducibility of scientific revolutions, would never take this positivist line. In his case the attempt to supply realism with an "epistemological ingredient" *via* the notion of verisimilitude should, perhaps, be regarded as an aberration. After all, despite his references to our aiming to get closer and closer to the truth, he has stressed often enough that the idea of any convergence in the development of science is at odds with the existence of scientific revolutions.

But what is left of realism if even the watered-down "epistemological ingredient" is excluded? What remains is a genuinely *conjectural realism* — a position which forms the core of Popper's view of scientific theories. According to conjectural realism, our theories are attempts truly to describe the structure of the universe (and not merely to "save the phenomena"). Theories are true-or-false attempted descriptions of reality, both observable and "hidden". Our present best guide not only to the phenomena but also to the structure of the reality hidden behind the phenomena is the guide supplied by our presently best theories. But a different theory, one which gives a quite new account of "hidden reality", may become "presently best" tomorrow (indeed, if history is any guide it will). So, while we can perhaps be confident that, if our presently accepted theories are eventually replaced, then the new superseding theories will somehow or other explain the empirical success of the old, we cannot have any guarantee that our present highest level picture of reality will be preserved, even approximately, in the new theory. If our present theories (in mechanics, in optics, in heat theory and elsewhere) are true, then our earlier theories, whilst accounting for many observable phenomena, were nonetheless false — plain false, we may as well admit, not approximately true. We may perhaps have presently hit on the truth, but we have surely not, at the highest levels, approached it. The best we can say is that our present theory in a given field yields our best guess about the truth in that field — but may well be wrong nonetheless.

This is what conjectural realism amounts to. It is hardly likely to be accused of being over-ambitious. Indeed, many will find it so unassuming as to be near-empty, arguing that if that is all that realism is then it is scarcely worth distinguishing from instrumentalism. The two positions, when sympathetically developed and amended in the light of various criticisms, do indeed seem much closer together than a cursory glance at their core doctrines might suggest. But still they are different.

First, of course, conjectural realism does allow high-level theories to be true-or-false in the usual correspondence sense. Nor is it merely a "semantic" view. The epistemic assumption it carries may be less weighty than some would like but it is not non-existent: the assumption, to reiterate, is that our presently best theories (according to our decidable methodological criteria) are our present best guesses about the truth (which is of course not decidable). The only anti-realist argument which threatens to make even this assumption untenable, namely, the argument from underdetermination, is nullified once any reasonably sophisticated account of empirical support is adopted.

But the main argument for conjectural realism is, as Popper discerned, negative. Its virtues only become visible when it is compared with its rivals. I see no point in trying to deny that realism would be much more strongly placed were the development of science continuous. But continuity cannot be restored without a radical, and damaging, positivistic reinterpretation of scientific theories. If we accept the discontinuities then any version of realism stronger than this weak conjectural kind, whilst not actually inconsistent with the history of science, does nonetheless seem to fly in the face of history. But why, having been denied the whole loaf, settle for no bread? Especially since, whatever instrumentalism's attractions for some philosophers because of the paucity of its assumptions, the position does in the end appear to be, psychologically speaking, well-nigh untenable. The belief that there is more to the universe than we can directly observe and that our best guide to what more there is is that supplied by science, seems to be one which the hard-headed philosopher inside his study might manage to banish by dint of hard argument, but which he finds irresistibly returning to him once he leaves his study. Morever, even inside the study it is difficult to develop the view with complete consistency: certainly even the best anti-realists like Duhem and Poincaré have succumbed to what might be called "creeping realism". For example, Duhem's introduction of a "natural classification" (towards which science may be leading us) is widely and correctly regarded as a major concession to realism. Similarly, Poincaré allowed that science, whilst not capable of revealing truth, could nonetheless reveal to us real relationships between things.[36] More tellingly, the logic of several of Duhem's and Poincaré's arguments ought to have led them to regard even observational laws like those of Kepler or of Gay-Lussac as neither true nor false but rather codifications of (low-level) empirical data. These laws, for example, are mathematically precise and therefore not fully determined by the inevitably imprecise observational data; and the laws are not testable, at any rate at the level of crude fact (meter readings, lengths of mercury columns, angles of inclination of telescopes), except in conjunction with other assumptions. Yet neither Duhem nor Poincaré could really bring himself to adopt the view that even Kepler's laws are not true-or-false of the world. More generally, scientists influenced by instrumentalism invariably reserve their positivistic scruples for the highest-level (and therefore least familiar) theories whilst happily adopting a fully realistic attitude to somewhat lower-level but still highly theoretical assumptions.

The chief virtue of conjectural realism, as I see it, is simply that it adds no more to Duhem-Poincaré instrumentalism than is consistent with the facts of scientific development, whilst at the same time adding enough to allow us to follow our realist inclinations. The price of adopting it is a large dose of fallibilism, but this is surely medicine which we must swallow in any case.[37]

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[36]In fact, although Poincaré was certainly an instrumentalist in that he denied scientific theories are true or false in the sense of the correspondence theory of truth, many of his remarks support some kind of "structural realist" view.

[37]An early version of the first part of this paper formed the basis of a lecture at a conference on the philosophy of Sir Karl Popper organised by Analisis and La Sociedad de Ex-Alumnos y Amigos de la "London School of Economics" in Caracas, Venezuela, in September, 1980. It was my pleasure to deliver talks based on an earlier version of the second part to various groups in Warsaw and Cracow in October, 1981. I received and greatly benefited from critical comments from Colin Howson, Alan Musgrave, Peter Urbach, John Watkins and especially Elie Zahar.