Yes, but...
Some Skeptical Remarks on Realism and Anti-Realism

by Howard STEIN *

Summary

This paper argues that the much discussed issue between "scientific realism" and "instrumentalism" has not been clearly drawn. Particular attention is paid to the claim that only realism can "explain" the success of scientific theories and — more especially — the progressively increasing success of such theories in a coherent line of inquiry. This claim is used to attempt to reach a clearer conception of the *content* of the realist thesis that underlies it; but, it is here contended, that attempt fails, and the claim itself hangs in the air. A series of increasingly sophisticated versions of the "instrumentalist" thesis is considered, and both these and the contentions of realism are placed in relation both to particular examples of scientific development and positions historically maintained by philosophers and by scientists. The author's conclusion is that, when the positions are assessed against the background of the actual history of science, (a) each of the contrary doctrines, interpreted with excessive simplicity, is inadequate as a theory of the dialectic of scientific development; (b) each, so interpreted, has contributed in important instances to actual damage to investigations by great scientists (Huygens, Kelvin, Poincaré); whereas (c) in both the theoretical statements and the actual practice of (in the author's opinion) the most sophisticated philosophers/scientists, important aspects of realism and instrumentalism are present together in such a way that the alleged contradiction between them vanishes.

Résumé

On oppose souvent le réalisme scientifique et l'instrumentalisme. Mais l'alternative n'est pas clairement formulée. On insiste particulièrement sur la thèse que seul le réalisme peut «expliquer» le succès des théories scientifiques et plus spécialement le succès grandissant avec le temps de ces théories dans une ligne cohérente de recherches. Cet argument est utilisé dans le but de *clarifier* la position réaliste, mais — ainsi que le montre l'auteur — ce but n'est pas atteint et la thèse elle-même reste suspendue en l'air. Une série de versions de plus en plus sophistiquées de la thèse «instrumentaliste» sont examinées et ces versions — ainsi que les prétentions du réalisme — sont confrontées d'une part à des moments particuliers du développement scientifique, d'autre part à des positions qui ont été défendues par des philosophes et par des scientifiques. L'auteur conclut que, lorsque les positions sont évaluées sur la base de l'histoire réelle de la science a) aucune des deux doctrines contraires, interprétée avec une simplicité excessive, ne rend adéquatement compte de la dialectique du développement scientifique; b) chacune d'elle, interprétée ainsi, a eu des conséquences néfastes sur les recherches de grands savants (Huygens, Kelvin, Poincaré); alors que c) aussi bien dans les énoncés théoriques que dans la pratique des philosophes/savants considérés par l'auteur comme les plus subtils, des aspects importants de réalisme et de l'instrumentalisme sont simultanément présents de telle manière que la soi-disant contradiction entre eux s'évanouit.

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Zusammenfassung

In diesem Papier wird dafür argumentiert, dass der vieldiskutierte Streitfall zwischen «wissenschaftlichem Realismus» und «Instrumentalismus» nicht klar beschrieben worden ist. Besondere Aufmerksamkeit wird der Behauptung geschenkt, dass nur Realismus den Erfolg wissenschaftlicher Theorien und insbesondere den ständig wachsenden Erfolg solcher Theorien in einer kohärenten Untersuchungsfolge «erklären» kann. Diese Behauptung wird verwendet, um eine klarere Konzeption vom *Inhalt* der ihr zugrundeliegenden realistischen These zu erreichen; aber es wird hier behauptet, dass der Versuch fehlschlägt und die Behauptung selbst in der Luft hängt. Eine wachsende Reihe hochentwickelter Versionen der «instrumentalistischen» These wird betrachtet und sowohl diese Versionen als auch die Behauptungen des Realismus werden zu speziellen Beispielen wissenschaftlicher Entwicklung, sowie zu historisch von Philosophen und von Wissenschaftern vertretenen Positionen in Beziehung gesetzt. Die Schlussfolgerung des Autors ist, dass — wenn die Positionen vor dem Hintergrund der aktuellen Wissenschaftsgeschichte beurteilt werden — (a) jede der beiden gegensätzlichen Doktrinen bei einer übermässig vereinfachten Interpretation als Theorie der dialektischen Wissenschaftsentwicklung inadäquat ist; (b) jede — so interpretiert — massgeblich zu tatsächlichen Schäden an den Untersuchungen grosser Wissenschafter (Huygens, Kelvin, Poincaré) beigetragen hat; wohingegen (c) sowohl in den theoretischen Aussagen als auch in der aktuellen Praxis der (gemäss Meinung des Autors) tiefsinnigsten Philosophen/Wissenschafter Aspekte des Realismus und des Intrumentalismus gemeinsam vorhanden sind, und zwar so, dass der angebliche Gegensatz zwischen ihnen verschwindet.

By the word "skeptical" I do not mean to suggest, primarily, disbelief; my ideal skeptic is Socrates, not Pyrrho. Among the claims put forward in recent years in the name of "scientific realism" there are many things I agree with; but there is also an admixture of what seems to me unclear in conception, or unconvincingly argued. This is not so very different from what I thought, in my student days, about the doctrines of logical empiricism — which have since been pretty harshly dealt with. In the latter proceeding, I believe that some rather valuable philosophical lessons have been (at least partly) lost or obscured; and I fear that unless a sufficient ferment of Socratic skepticism is cultured within the realist brew, it will go stale and its vogue too will soon pass. Indeed, it may be late in the day to express such a fear; the pendulum has by now swung perceptibly in the other direction; and yet I hope that it may be possible to regain hold of what was of positive worth in positivism, and to retain possession of what is of real value in realism.

One of the critical points on which I feel myself at odds with the contenders for realism concerns the characterization of the central contrast under discussion — that between a "realist" and an "instrumentalist" view of theories. It seems to be widely assumed that this contrast is tolerably clear. I, on the other hand, do not see the issue between realism and instrumentalism as well-joined at all. This opinion may strike you as having about it a smack of Carnap and of Ernest Nagel; and that, in turn, may suggest that I myself, after all, am an instrumentalist (since I believe that realists do so classify both Carnap and Nagel). Well, I have already told you that logical empiricism has strong merits in my eyes; but part of my plea is: let us not be hasty in applying labels.

I shall invoke another honored empiricist name (and nostalgic reminiscence): Friends of mine, when I was a student, had taken a course of Hempel's at Queens College in New York; and they reported that Hempel had discussed a pair of related fallacies, which he called "the fallacy of nothing but", and "the fallacy of something more". My friends did not elaborate; but this terse fragment seemed intellectually nourishing, and I still snack upon it.

How, then, is one to understand the position that theories are "mere instruments" — that they are "nothing but" instruments? First of all, instruments for what? One not infrequently receives, from physics students who have been indoctrinated with a crude version of positivism, the formulation — intended to debunk philosophical questioning — that a theory is "nothing but" an instrument for calculating the outcomes of experiments. The tacit implication here is that the experiments in question form a rather special domain: both restricted — so that theories are quarantined from any commerce with our views of "what the world really is"; and technical — so that (to put it in its most banal form) to *understand* a theory is "nothing but" to be able to calculate the solutions to examination exercises. But the tacit implication is false. I once heard Eugene Wigner make the point most elegantly, in a lecture on philosophical problems of quantum mechanics and quantum field-theory: He remarked that it is often claimed that all one ever does in quantum theory is calculate the results of scattering processes. But, said Wigner, while this is *almost* true, it is not *quite* true; one also uses quantum theory, for example, to calculate the density of aluminum.

The trite form of debunking instrumentalism makes a false estimate of the scope of a theory *as an instrument*. There are, of course, broader and narrower theories; but in principle, the scope envisaged by theoretical natural science as a whole has been, and is still, all the phenomena of nature. Such a scope was envisaged, for instance, explicitly (and with eloquence) by Newton; and in our own time an enormous advance has been achieved towards its realization, since the quantum theory has made possible an understanding, in fundamental terms, of the quantitative and qualitative properties of ordinary matter: the density of aluminum, the magnetizability of iron, the colors of natural bodies.

Now, the expansion of scope beyond that of "experiments" as narrowly conceived — to all of what in quasi-Kantian language may be called "the world of experience" — raises a certain difficulty for the formulation of an instrumentalist view; for it is no longer appropriate to speak of simply "calculating the outcomes". In point of fact, the problem arises already in the narrower domain, as critical discussion of the "observational/theoretical" dichotomy has made abundantly clear. But this difficulty can certainly be dealt with

— since it is *in fact dealt* with in scientific practice (although perhaps not with the formal exactness that philosophers of science tend to prefer). I should like, for the present purpose, simply to assume that a reasonable account of the relation of a theory to phenomena is available, and to use the phrase "the theory affords a *representation* of the phenomena" for that relation. Then the instrumentalist's claim can be read as: A theory is "nothing but" an instrument for representing phenomena.

From the foregoing schematic formulation of a somewhat liberalized instrumentalism, I turn to the realist and ask, what is it that you demand of a theory beyond this? — what "more" should a theory afford than a representation — in a suitable sense correct, and in a suitable sense adequate — of phenomena? The standard response to this question seems to me, I am afraid, no less trite and shallow than the trite and shallow version of instrumentalism: it is that the terms (predicates, function-symbols) of the theory should "genuinely refer", its quantifiers have associated domains, and its sentences consequently be true or false (its *asserted* sentence preferably true). The trouble with this can be seen from two sides. First, there is the classical philosophical problem of the epistemology of transcendent metaphysics — the problem that Kant (we may say) posed to the ghost of Leibniz, and that led Kant himself to the view that all theoretical knowledge is *of* the phenomenal world: the world of experience. The nub of the matter can be put very simply: it is, How can you *know* that things are as you say they are? If the claimed "reference"of the theory is something beyond its correctness and adequacy in representing phenomena — if, that is, for a given theory, which (we may suppose) does represent phenomena correctly and adequately, there are still two possibilities: (a) that it is (moreover) *true*, and (b) that it is (nonetheless) *false* — then how in the world could we ever tell what the actual case is?

I shall return to this question; but first I want to describe what I have referred to as the other side of the trouble with the appeal to reference. It is simply that one can "Tarski-ize" *any* theory that is constructed in the standard way — that is, with the help of a metalanguage of suitable strength, and also containing the extra-logical resources of the language of the theory T under discussion, one can supplement T with a theory of reference for it, T′; and can do so in such a way that whenever Θ is a theorem of T, the statement that Θ is true is a theorem of T′. Thus, whereas the Kantian critical objection seemed to put reference and truth utterly beyond knowledge, the Tarskian theory of reference and truth in a rather serious sense *trivializes* the desideratum put forward by the realists. (There is, of course, no paradox in this contrast: Tarski's theory was never intended to explicate the Kantian "transcendent" or "noumenal" realm.)

I believe that we are dealing here with something that has gone seriously wrong in the philosophical tradition of recent decades. Some thirty-eight years ago Carnap offered his distinction between questions of existence "internal" to a linguistic or theoretical framework, and such questions construed (as one might say) "absolutely" ("external questions of existence"); and discussed that distinction in a way that made really quite clear the situation I have just been speaking of. But at about the same time there appeared Quine's attack upon Carnap's approach to the problem of "meaning"; and the trend of philosophical opinion went increasingly with Quine and against Carnap, in particular in preferring "reference" to "meaning" as a clearer and more reliable basis for philosophical considerations, and also in rejecting Carnap's whole notion of a clearly defined "linguistic framework" (a rejection perhaps reinforced by the suggestion that such a notion has a taint of "relativism" and an affinity with the views later advanced by Kuhn). I cannot do more here than barely allude to this history, and suggest that it is one of the developments in which analyses of genuine value by the logical empiricists have gotten lost. (Hilary Putnam evidently noticed some years ago that there is merit in those arguments of Carnap.)

So far, then, I maintain, no issue has been joined: reference to reference has failed to stake out any claim to which a sophisticated instrumentalist should feel a need to object. The point can be restated this way: The semantics of reference and truth (for a given theory) is itself a *theory*. One who considers theories in general to be "mere" instruments need have no hesitation, and no qualms of intellectual conscience, in accepting this theory *as an "instrument"* (thus the innocuousness of realism in its "internal" aspect). If, on the other hand, the realist objects that this is not what he meant by his realistic semantics; that he meant the semantics not as a mere instrument, but as genuinely giving the real or true reference of the theoretical vocabulary; then the attempt to explicate a clear issue has fallen into a clear circle: we are back in the problem of the noumena, or the fallacy of "something more".

But I have left hanging the question how we could possibly tell whether a theory, beyond its presumed satisfactory relation to phenomena, is "moreover" true or not (*more* having been demanded — but a not clearly characterized "more" — how to tell whether the desired extra has been delivered). On this question a certain proposal has been made recurrently, and has been pursued with energy in recent years by Richard Boyd: namely, the proposal that consideration, not simply of individual theories in their separate relation to phenomena, but of the connections of theories with the *ongoing process of scientific inquiry,* can — and indeed does — afford evidence that our theories have not merely instrumental but also "real" validity.

My own opinion is that this appeal to what may be described as the historical dialectic of inquiry — and with "historical" explicitly understood to imply that the circumstances appealed to are *empirical* in nature — does indeed direct philosophic attention to real and important issues. With much that Boyd has said in detail on this topic I strongly agree. But as a mode of argument for "realism" over "instrumentalism", the attempt seems to me to fail completely. What the move does accomplish so far as *this* ideological issue is concerned — and accomplish without any need to argue in detail — is to point out that the instrumentalist himself, if he wishes to do justice to the role actually played by theories in science, had better extend still further his conception of what theories are instruments *for,* to include their role as resources for inquiry; especially as sources of clues in what Peirce called "abduction": the search for good hypotheses.

Can an instrumentalist do this? Boyd appears to believe that he cannot do so successfully — that there are methodological principles of inquiry, fruitful in practice, that are in some sense available to the realist but not to the instrumentalist. But this claim is caught in a dilemma: Either the methodological principles in question, and the fact of their success thus far in the history of inquiry, are susceptible of clear formulation in terms of the relations of theories to phenomena (and this, I think, is what Boyd intends) — in this case, the instrumentalist *ought* to adopt those principles, on the grounds of the evidence for their instrumental success; or they are not susceptible of such formulation — but then, where is the argument? In short, the instrumentalist's own principles entail no prohibitions: what the realist can do, the instrumentalist can do also.

To be sure, I have distorted Boyd's central argument in an important way. His own claim is that what the realist can do and the instrumentalist not is *explain* the success of the methodological principles that he takes to be characteristic of realism. This raises some large and complex questions, to which on this occasion I shall give the very shortest shrift. (a) Argument to a better, or the best, explanation is a doubtful business, which I should prefer to view as abductive, or heuristic, or tentative at best; and in the present case, I do not see what investigations are to follow the abduction. (b) I do not believe the explanation does in fact explain. After some labor at trying to compress the reasons for my doubts into a small space, there occurred to me a one-step proof: namely, that until we have succeeded in attaching a clear sense — not the bare noumenal one — to the realist thesis, that thesis cannot serve as a basis for the explanation of anything.

Well, I am afraid that may sound unfair — that it may seem question-begging. But the situation is this: We are seeking *clarification* of the realist claim. Boyd suggests a case for realism — namely, that evidence for it is to be found in the role theories play in inquiry. We are considering his arguments in a dialectical spirit, hoping from the suggested *case for* realism to arrive at a clarification of the *content* of the thesis being argued for. An example in which an analogous strategy succeeds is that of Newton's argument against the "absolute rest" of either the earth or the sun: a careful consideration of the evidence and arguments does in fact lead to genuine clarification of the very concept of rest and motion involved. In the present instance, however, the strategy fails, because the alleged evidence in favor of the realist thesis — that it provides a valuable explanation — *itself* remains unintelligible until the realist thesis is clarified. Indeed, the attributes our theories must possess for them to serve as resources for inquiry are all attributes that concern the logical structure of the theories and their relation to phenomena. Hypostatizing entities cannot be of any help in formulating these attributes *or in explaining their usefulness*, unless those entities are put in relation to phenomena. But then the entities are postulated *within* a natural-scientific theory, and we are back in the old dialectical circle.

To explain a little further what I mean about the futitility of "hypostatizing", let me discuss two cases — one directly relevant, the other obliquely so.

(1) Recurrently since the Greek atomists, whether things "really have" the very qualities we perceive in them has seemed a problem (and there has been, correspondingly, recurrent malaise in the face of the presumed teaching of physics and/or philosophy that "the grass isn't really green" — as if physics told us that grass is somehow black, or ghastly gray, or perhaps invisible). But suppose that, to rescue the comfortable image of a lovely world, it were argued as follows: From the doctrine that our perceptions are the "mere" result of exchanges of energy and momentum with the environment, which give rise to the transmission of nerve-impulses to appropriate regions of the brain, we can never "understand" why our sensations have *the very qualities they do*; and we are impotent before the question whether any two of us experience the same qualities — since we can form no intelligible connection between, say, the particular look of grass (I should say, *to a particular person* — or perhaps even to a particular person at a particular time) and the physiological process that either "constitutes" or "causes" or "occasions" the perception. But if we will only postulate that the grass itself *really possesses* the quality we perceive, the difficulty disappears: we are able to "explain" the quality we perceive by associating it with the very same (or perhaps a "quite similar") quality in the grass; and we then have evidence that we all perceive the same (or similar) qualities — since our perceived qualities are all the same

as, or all similar to, the "real" color of the grass. It may even be urged that without such an explanation, the qualitative character of our perceptions stands as a miracle.

This mode of argument has from time to time made a powerful impression, and I think there is something psychologically very tempting in it. But I submit that it is in fact a snare and a delusion. (Note, by the way, its totally unacceptable conclusion that so-called "color-blind" people see *false* colors.) — There is perhaps more than one thing wrong with the argument; but what I see as its fatal defect is this: that the postulate in question doesn't explain anything. For we know — whatever the truth may be about the grass — that we actually *see* when certain nerve-processes occur, and that these processes are stimulated by exchanges of energy and momentum with the environment. Therefore, even if we do assume — whatever it may mean — that the grass "really has" a color like the quality we perceive, the facts of our perception do not follow from the postulated property of the grass: between the grass itself and our perception of it there still intervene the very physical/physiological processes whose connection with the quality is alleged to be unintelligible.

I call this case directly relevant because it is one in which a notion is introduced as "explanatory", which under examination is seen to be in effect *disconnected from its explanandum*. (The positivists liked to speak of pseudo-questions; here we have a pseudo-answer.)

(2) The second example comes from the history of science: it is the attempt by a very great physicist, Christiaan Huygens, to "explain" the phenomenon of weight, in accordance with the metaphysical principles shared by most "progressive" natural philosophers of the seventeenth century. (Huygens himself refers to "the philosophy of the present time" — or, as he also calls it, "the true philosophy" — according to which all natural effects are to be explained as results of bodies in motion impinging upon one another, and thereby changing one another's motions; and he expresses the opinion that if we cannot provide such explanations, all hope is lost of understanding anything in physics.)

The theory Huygens proposes, as in his opinion the only possible one to account on such principles for the weight of bodies towards the earth, is this: There exists a very subtle — i.e., finely divided — matter surrounding the earth, and moving very rapidly in circulation about it. Ordinary terrestrial bodies do not participate in this circulatory motion; therefore the tendency of the swirling matter to recede from the center has the effect upon such terrestrial bodies of tending to sweep them towards the center — that is, towards the earth. But the tendency towards the earth is spherically symmetric, whereas an ordinary vortical swirl is about an axis rather than a center; and moreover, if the swirl were all in one direction, it could not fail eventually to be communicated to ordinary bodies. For both these reasons, Huygens says, it is necessary to posit that there is not one single swirling motion, but rather a complex of such in which swirling occurs in constantly changing directions, so that the average state is spherically symmetric, and so that ordinary bodies do not have time to be swept one way before the direction changes and they are swept another way. The picture is of a kind of rotational Brownian motion; and Huygens mentions the analogy of heated droplets that are observe to execute rotatory oscillations about constantly changing axes. After he had read Newton's argument in the *Principia* for the conclusion that weight towards the earth (and towards the sun and all the other planets) varies in magnitude inversely as the square of the distance from the center — an argument he found fully convincing — Huygens added to his hypothesis that the velocities of rotation of his vortices would have to vary with distance in such a way as to yield the Newtonian inverse-square variation of weight.

Now, it is really quite extraordinary that a man of Huygens's great intellectual clarity and power should have been in any way satisfied with this "explanation". Let us set aside any qualms about the dynamical effects that a motion of the kind proposed would actually have upon ordinary bodies; conceding all that Huygens asserts about this, let us just ask what in the world could cause the matter of Huygens's vortices to behave in such an extraordinary way? — One thinks of Byron's satirical lines on Coleridge:

> Explaining metaphysics to the Nation:
> I wish he would explain his explanation!

Here, then, we have a case where — at least if we make some (possibly unjustified) concessions — a proposed explanatory contraption might in fact have the consequences required of it; but where a judgment not seduced by the great desire to *feel* that it understands (an example of what I believe is sometimes called "false consciousness") will regard the explanation as quite arbitrary: as, so to speak, a quasi-pseudo-answer.

What is common to the two cases is a relaxation of intellectual standards under what I may call ideological pressure. Let me cite, by way of contrast, an example of a problem that does genuinely call for explanation by a naturalistic and evolutionary epistemology such as Boyd advocates; a problem that is almost surely not ripe for attack, but on which one may hope for great enlightenment some day. This is the question how it is that our own natural endowment — which has evolved for its "instrumental" value in coping with far more immediate aspects of the world — has also proved to be an "instrument" capable, under favorable circumstances, of (e.g.) discovering quantum mechanics. One reason I think it important to be chary of pseudo-explanations is precisely that they tend to mask real problems, and to dull the taste for what real solutions are like.

I have said that I think there are genuinely interesting and important issues concerning the role of theories in the ongoing process of inquiry. I do not believe that these are issues that divide naturally along the line "realist"/"instrumentalist". Years ago, I wrote a paper — which I have not published, but have read to a number of audiences — discussing the work of Poincaré on electrodynamics, and its relation to the special theory of relativity. In that paper, I pointed out what I take to have been striking failings in Poincaré's view of the proper role of theories in inquiry. Now, Poincaré was certainly an instrumentalist; and in my discussion, I took him to task for unwillingness to regard certain things as "real". At the end of the paper, however, I remarked that it should be evident to the audience that, despite my use of such "metaphysical" notions as "real", the whole analysis could be recast in methodological terms — in effect, that the true issue was not realism *vs.* instrumentalism. The situation in question is a doubly ironic one. To explain this, let me single out just one of the points of physical theory involved. The contested reality was that of the ether; and Poincaré, because he regarded the ether as a fiction rather than a reality, was unwilling to take very seriously (although he was willing to play with) the idea that charged particles exchange momentum with the ether. This, however, is a very odd position for an instrumentalist to take (the first irony); for there is no warrant at all in the instrumentalist view for *grading* the entities of a theory in *degrees* of reality or fictitiousness — regarding particles as *more real* than the ether. This is a double standard that recurs in the empiricist tradition, but it is not itself a genuine part of empiricism or of instrumentalism — it is, on the contrary, a bit of unregenerate realism, doing the work of the Devil among the empiricists and instrumentalists.

The second irony I had in mind is the circumstance that Hilary Putnam, writing as an exponent of realism, has used the notion of the ether, in contrast with that of atoms, as an example of a scientific term that failed to "refer" — there is no such thing as the ether, whereas there are atoms (note that this endorses the discrimination committed by Poincaré); and that Larry Laudan has exploited this as one example among others in an argument *against* realism — namely, against the view that there is any strong connection between "instrumental success" (here, of the wave theory of light and of Maxwell's electrodynamics, both of which posited an ether) and "real reference". For my part, I throw up my hands at this: Why should we say that the old term "ether" failed to "refer"? — and that the old term "atom" did "refer"? Why, that is, except for the superficial reason that the word "atom" is still used in text-books, the word "ether" not? — It would be possible to do a lengthy dialectical number on this; but in brief: our own physics teaches us that there is *nothing* that has all the properties posited by nineteenth-century physicists for the ether *or* for atoms; but that, on the other hand, in *both* instances, rather important parts of the nineteenth-century theories are correct. For instance, so far as Poincaré's conviction that momentum conservation must hold among the "particles" taken by themselves is concerned, it is established now beyond a doubt that ordinary bodies do exchange momentum with "the ether" — i.e., with the electromagnetic field; and even that this field has to be regarded as the seat of a distribution of *mass,* and as participating in gravitational interactions. The two cases — that of the ether and that of atoms — are, in my view, so similar, that the radical distinction made between them by the referential realists confirms in me the antecedent suspicion that this concern for reference — and associated with it, another Quinean motif, the concern for what is called the "ontology" of theories — is a distraction from what really matters.

I agree wholeheartedly with Boyd that *we have learned* — that is to say, scientists have indeed learned, in their practice; and in our philosophical reflections upon science, we *should* by now have learned explicitly — that successful scientific theories are to be taken very seriously as clues to the deeper understanding of phenomena: i.e., clues in the search for better and more fundamental theories. But this process of development, from the less to the more fundamental (*Tieferlegung der Fundamente,* as, according to Hermann Weyl, Hilbert liked to call it) has very little to do with the "referential" relations of theories or with the relationships of their "ontologies" in the sense of Quine. To borrow from the ancient philosophical tradition, what I believe the history of science has shown is that on a certain very deep question Aristotle was entirely wrong, and Plato — at least on one reading, the one I prefer — remarkably right: namely, our science comes closest to comprehending "the real", not in its account of "substances" and their kinds, but in its account of the "Forms" which phenomena "imitate" (for "Forms" read "theoretical structures", for "imitate", "are represented by"). In mathematics itself — concerned with Forms without regard to phenomena — the deepest *general* discovery of the whole modern epoch, initiated by Gauss and notably advanced through the work of Galois, Riemann, Dedekind, Lie, Klein, Poincaré, Hilbert, and Emmy Noether, to an enormous flowering in our own time, is that in the Hilbertian "Tieferlegung" deeper understanding of the Forms or structures of mathematics characteristically consists in their being seen in unexpected relations to one another, or to newly discovered structures, which play a unifying or generalizing or expanding role in our understanding of the more familiar ones (the Forms "participate in one another"). And in the development of physics in the same period — but above all in our own century — an analogous, in some ways even more astonishing discovery has emerged; more astonishing because it involves our understanding of *ordinary, familiar things*, and because the key to this understanding turns out to lie in mathematical structures, and *improved* understanding in *structural "deepening"*, of just such kind as characteristically occur in pure mathematics itself. And, I should add, more astonishing not least because of the circumstance I am here concerned to emphasize, that in this structural deepening what tends to persist — to remain, as it were, quasi-invariant through the transformation of theories — is on the whole (and especially in what we think of as the "deepest" — or most "revolutionary" — transformations) not the features most conspicuous in referential semantics: the substances or "entities" and their own "basic" properties and relations, but the more abstract mathematical forms.

What I have just said is pretty general. Let me give two examples of what I mean: the first a very simple one, which I hope will serve for clarification; the second, one of contemporary relevance.

(1) Newton shocked the natural philosophers of his time by his claim to have discovered, and proved, that sunlight is a mixture of physically distinct kinds of light. His formulation of this result was extremely careful, and remains entirely cogent today. The basic "entity" in his analysis is what he calls a "ray" — by which he means not (as we ordinarily do) a line of propagation of light, but rather an "elementary part of light", conceived as spatially localized and as propagated along a definite trajectory: sunlight, he says, contains rays of indefinitely many distinct kinds, which may be separated from one another, and which prove to have distinct and immutable physical properties; among these distinguishing properties are distinct indices of refraction (for a given medium), powers of stimulating sensations of distinct colors, and a certain quantity having the dimensions of a length (which we know as the wave length).

Now light, as we have come to understand it, whether we "represent" it by means of the classical wave theory of Fresnel, the electromagnetic theory of Maxwell, or (now most fundamentally) by the quantum theory of the electromagnetic field, does not have "parts" that are propagated along definite trajectories. One cannot say, of the light that reaches a certain point P at a certain time t, that at an earlier time t' this same bit of light was at a point Q; the notion of *identity of localized parts through time* is simply inapplicable at all to light.

Therefore, one either has to say that Newton's term 'ray' fails to "refer", or one has to strain to find some way to associate that term with an "entity" of one of the later and deeper theories — at best a pedantic and unrewarding move. On the other hand, it is an essential task of physical understanding, and a most instructive one, to exhibit the formal relations of the Newtonian theory of rays to the later theories. One finds, when one does this, that there are several different structural aspects of the later theory, each of which has a significant bearing upon the success of the notion of a ray in serving to represent the optical phenomena dealt with by Newton. Thus the principle of "cutting at the joints" — a motif in recent realism that I heartily applaud — seems to indicate *not* using referential semantics as the basis of comparison of theories (at least in the case cited; but I offer it as typical).

(2) A number of attempts have been made to give a so-called "realistic interpretation" of quantum mechanics. These have characteristically taken the form of specifications of "realistic semantics" for the theory. The results seem to me, in each case, not only unpersuasive, but — what is much worse — fundamentally uninteresting. (The author of one of those theories once expressed to me the apprehension that his attempt might be subject to the criticism said to have been pronounced by Pauli on another proposal: Pauli's remark was, "That theory isn't even wrong!") My reason for regarding these essays in quantum realism as uninteresting is this: All the "realist" interpretations take it for granted that certain specific constituents of the mathematical structure of the theory — the projection operators — are to be interpreted as standing for *attributes* of certain basic "entities" (particles); because that is what they correspond to in the classical theory. But if one examines carefully how phenomena are "represented" by the quantum theory — and this requires attention not only to what physicists say, but to how the physics actually works — then precisely this interpretation in terms of "entities" and "attributes" can be seen to be highly dubious. I do not claim to have a definitive formulation of the meta-physics of quantum mechanics; but I believe rather strongly that the difficulties it presents arise from the fact that *the mode in which this theory "represents" phenomena* is a radically novel one. In other words, I think the live problems concern the relation of the Forms — indeed, if you like, of the Instrument — to phenomena, rather than the relation of (putative) attributes to (putative) entities, and that the ideological motives of "realism" have here served as a kind of scholastic distraction, turning attention away from what is "real" in the subject.

I have so far discussed at far greater length what I see as ideological and delusive in the claims of recent realism than I have the corresponding faults of anti-realism; the latter I have only touched on in speaking of the shallower versions of instrumentalism. Yet this last — a shallow instrumentalism — seems to me no less inimical to sound philosophic understanding. Since I have mentioned the case of quantum mechanics in the former connection, it is only fair to cite it again here. I have just referred to the "live problems" of philosophic understanding posed by quantum mechanics. There are those who have argued that no such problems exist in fact — on the grounds that, in concrete cases, physicists agree on how the theory is to be applied to phenomena, and that is all we are entitled to expect of a theory.

It is certainly not a contemptible argument — not one to be dismissed out of hand. But it is necessary to use some circumspection in evaluating it. The relevant question is closely related to the criticism I have earlier made of the physics student's charm against philosophy: How the theory is to be applied to *which* phenomena? (There is a wonderful story — whether true or apocryphal I do not know — about a discussion between a distinguished mathematician and a very distinguished physicist on the problem of axiomatizing quantum field theory. The physicist said, so the tale has it: "Why do we need axioms? The job of physics is to calculate the results of experiments. If you describe an experiment for me, I can perfectly well calculate its outcome." To which the mathematician replied — in a Polish accent that helps the story —: "Dot's very nice, Dick; but vot vill ve do *after* you?" [a situation in which, lamentably, we now find ourselves].) The point is that quantum mechanics — or its more fundamental, but, to date, from a mathematical point of view less clearly formulated modification, relativistic quantum field theory — is one of the two basic and universal theories of contemporary physics (general relativity is the other); its presumptive scope, therefore, is all the phenomena of nature. But we genuinely do not understand, not merely how *in detail* to apply this theory to all natural phenomena, but *in principle* how such application may be possible; for instance, no one really knows how quantum mechanics could be applied to the universe as a whole; and yet such application seems to be necessary if there is to be a unification of quantum theory with general relativity. And this difficulty, which may well strike you as after all something quite technical that ought to concern physicists rather than philosophers, is intimately connected with the famous problem of what is called the "reduction of the wave packet", which has been at the center of philosophical discussion of the theory from its beginning — and which is precisely the problem that the instrumentalist move I have deprecated is intended to exorcise.

Note again that — as in the case I have mentioned of Poincaré — the criticism I have just made need not be put in "realist" terms. For myself, I do tend to think in such terms — to ask what the theory implies about the world's real constitution. But the problems I have criticized the instrumentalist argument for evading are, if I am right in regarding them as real, then real *instrumentalist* problems. Indeed, I said at the outset that the issue between realism and instrumentalism seems to me not to be clearly posed; and what I really believe is that between a cogent and enlightened "realism" and a sophisticated "instrumentalism" there is no significant difference — no difference that *makes* a difference. I shall try, in my remaining remarks, to clarify this claim and make it plausible.

Let us briefly consider the idealism of Berkeley: the doctrine that what is real is just minds and their perceptions, and that *all* our beliefs about the physical world are just "instruments" for organizing and anticipating experience. Can this doctrine be refuted? Need this doctrine be refuted?

It is an interesting point that we are unable to formulate our information about the world in a way that restricts reference to that which is — or even that which is possibly — perceived (the failure, more or less recognized eventually by Carnap, of phenomenalism as a "basis" for a scientific language). But although this is a fact of great importance for the methodology of science *and* of philosophy, it does not refute Berkeley. It can even be construed as implying a peculiarity of our conceptual faculty, rather than of "the world" (cf. Kant).

Can we argue that only *some* further contraption behind the appearances could — or could with any likelihood — produce regularities in phenomena of the kind represented in our beliefs? Why is such an argument any more plausible than that to a "mechanical" cause of gravitation, in the sense of Huygens; or to a material ether supporting the electromagnetic field? I have alluded to both these scientific issues earlier; although it may seem to be far removed from the question I have raised about Berkeley, I should like to discuss the electromagnetic case in just a little more detail now.

When Maxwell proposed his great theory, he called it a *dynamical* theory of the electromagnetic field; and explained that he used that term because the theory postulated the existence of matter in motion in all the regions within which electrical and magnetic interactions occur — in the regions, that is, *between* and *around* the electrically charged bodies and electric currents. In his first attempts to work out a theory of this kind, he made, tentatively, detailed assumptions about this matter, that is about what was called the "ether"; but he regarded these hypotheses as mere aids to the development of the theory, not as plausible proposals about the true constitution of the ether. When the theory was fully worked out, Maxwell uncoupled it from the earlier detailed ether model, and proposed instead — with some success — to subsume his theory under the principles of the *generalized* dynamics of Lagrange, which made it possible to state dynamical laws for a system whose internal constitution was not fully specified. (I say with some success: Maxwell carried out this program only partially; his work was later extended, first by G. F. FitzGerald, and then, to its full completion, by H. A. Lorentz.)

Now, Maxwell's own view of this situation was unquestionably realistic. Here is what he says in the last paragraph of his *Treatise on Electricity and Magnetism,* after reviewing a number of theories, alternative to his own, which postulated the transmission of energy in some form with a finite velocity through space from particle to particle:

> But in all of these theories the question naturally occurs: — If something is transmitted from one particle to another at a distance, what is its condition after it has left the one particle and before it has reached the other? . . . In fact, whenever energy is transmitted from one body to another in time, there must be a medium or substance in which the energy exists after it leaves one body and before it reaches the other, for energy, as Torricelli remarked, 'is a quintessence of so subtile a nature that it cannot be contained in any vessel except the inmost substance of material things'. Hence all these theories lead to the conception of a medium in which the propagation takes place, and if we admit this medium as an hypothesis, I think it ought to occupy a prominent place in our investigations, and that we ought to endeavour to construct a mental representation of all the details of its action, and this has been my constant aim in this treatise.

A bit confusing perhaps, since I have said that Maxwell eliminated the detailed model of the ether from his developed account of the theory. But the "details of its action" that he means, in this statement, are those detailed concepts by which he represents the state of the electromagnetic field itself — conceived, indeed, as representing the state of the ether, but without a detailed account of *how* the field quantities are determined by an underlying mechanical medium — and those detailed laws that govern the behavior of the field, and that express the kinetic and potential energy of the presumed medium as functions of the field quantities. This purgation of the theory from *other* details about the medium, this rendering of it in a more "abstract" or general form, was done by Maxwell because of another principle he cherished: that of distinguishing, as far as one can, between what is known with some security, or held at least with some probability, and what is bare and even implausible conjecture. (This principle he explicitly associates with Newton.)

I have already intimated, in my earlier remarks about Poincaré, and about Putnam and Laudan, what the sequel was: The details that Maxwell knew he didn't know proved to be impossible to construct at all in a satisfactory way on the basis of a mechanical ether in the traditional sense; but those more abstract principles of dynamics under which Maxwell subsumed the laws of the field have been taken over, first into the relativistic dynamics that essentially established the impossibility of a traditional one, and then into quantum electrodynamics. Of the principles of Lagrangian dynamics — that Form which has survived these radical transformations of physics — Whitehead was moved to declare that "the beauty and almost divine simplicity of these equations is such that these formulae are worthy to rank with those mysterious symbols which in ancient times were held directly to indicate the Supreme Reason at the base of all things".

Now, I have defended the "reality" of the ether — in a certain sense — against the claims both of Putnam as a realist and Laudan as an anti-realist; and I have quoted the undeniably realist sentiment of Maxwell on the subject. Yet, as I have said, the physicist who completed Maxwell's subsumption of the laws of the field under the Lagrangian principles was H. A. Lorentz. Lorentz did this in his celebrated memoir of 1892 on electrodynamic phenomena in moving bodies — where the motivation he cites for carrying out Maxwell's program can fairly be described as an "instrumentalist" one. He refers there to the theoretical work of Heinrich Hertz on the same subject; notes that Hertz hardly concerns himself with a mechanical account of processes in the field (it should be remembered that Hertz said, in this connection, that "Maxwell's theory is Maxwell's system of equations"); comments that this method has its advantages, but that on the other hand "one is always tempted to revert to mechanical explications"; and then makes the crucial remark that his own purpose, which was to develop the theory for bodies that move through the ether without any entrainment of the latter, "seemed to me difficult to achieve ... without having as guide some theoretical idea. The views of Maxwell [namely, on the Lagrangian dynamics of the field] may serve as foundation for the theory sought".

How does this "instrumentalist" view of the usefulness of theoretical ideas as guides for research — guides, it may be noted, to be used opportunistically: Lorentz in no way implies that mechanical representations are a *sine qua non,* and on other occasions he denies this explicitly — how does this view square with the "realist" commitment of Maxwell? Here is a methodological statement of the latter, taken from his article on "Attraction" in the ninth edition of the *Encyclopaedia Britannica:*

[W]e must bear in mind that the scientific or science-producing value of the efforts made to answer these old standing questions is not to be measured by the prospect they afford us of ultimately obtaining a solution, but by their effect in stimulating men to a thorough investigation of nature. To propose a scientific question presupposes scientific knowledge, and the questions which exercise men's minds in the present state of science may very likely be such that a little more knowledge would shew us that no answer is possible. The scientific value of the question, How do bodies act on one another at a distance? is to be found in the stimulus it has given to investigations into the properties of the intervening medium.

This dialectical tension, as it may be called, between a realist and an instrumentalist attitude, existing together without contradiction, seems to me characteristic of the deepest scientists. Besides Maxwell, whom I have quoted on both sides, it is very clearly manifested in Newton, for example — and in Einstein. I have already mentioned Poincaré as an example of a scientist — a great scientist, and a philosopher — whose work in physics suffered from an imbalance on the instrumentalist side. Two examples of great physicists whose work suffered to a degree from the opposite imbalance are Huygens — who could not accept Newton's theory of gravitation because he saw no possibility of a satisfactory "mechanical explanation" of universal attraction, and Kelvin — Maxwell's good friend, closely associated with Maxwell's own development of his electromagnetic theory, who to the end of his life — in 1907, more than forty years after the first publication of Maxwell's theory, maintained a vacillating and somewhat negative attitude towards that theory because no satisfactory mechanical model of the electromagnetic ether had been found.

Back then, once more, to Berkeley. Can one argue that if the natural world consisted in nothing but the perceptions of minds, it would be a "miracle" that these perceptions follow rules formulated in terms of the "entities" conceived by science — or by ordinary common sense? Why should this seem any more miraculous than the universal gravitational attraction of bodies (which was, indeed, rejected by Huygens, and by Leibniz as well, on just such grounds)? Or how is it more miraculous than the fact that such an apparently "abstract" thing as the electromagnetic field just *does* satisfy the equations of Maxwell and the dynamics of Lagrange, without anything more substantial that "underlies" it? Indeed, if one examines the explanation offered by science today for the existence and properties of aluminum (to revert to an earlier example), it is hard not to feel that this explanation, at least as much as Berkeley's, grounds ordinary things upon a miracle. But the simple fact is that whatever our science adopts, perhaps provisionally, as its "ultimate" principles, just because they have no further ground, remain "inexplicable"; and the farther they are from the familiar, the more they will seem "miraculous".

What we are left with is that other, provisionally, "ultimate" or unexplained fact, that we *do* find ourselves compelled to formulate our beliefs in non-phenomenalistic terms; and in this process, atoms, electrons, fields, and the like are in a case quite analogous to that of chairs, tables, and the like in Berkeley. The *justifiable* claim to "reality" possessed be those "theoretical entities" is of the same kind as the justifiable claim — not after all denied by Berkeley — of these ordinary objects. To hold this is to reject the faulty-empiricist double standard; and "realism" in this sense I endorse unreservedly. Berkeley, at this point, in my view, has been, not refuted, but seen to be irrelevant to any real issue in the understanding of science. And my general conclusion is: Realism — Yes, but ... instrumentalism — Yes also; No only to anti-realism.