THE INSTRUMENTALISM-REALISM DEBATE: A CASE FOR A DIACHRONIC PHASE THEORY

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The instrumentalism-realism debate has been for some time one of the more central issues in the philosophy of science.[1] This is fully understandable since the claims about the world made by empirical science are of great interest to philosophers: A serious epistemology cannot avoid examination of the findings of science. Matters are increasingly compounded by the fact that the most advanced and mature sciences are increasingly relying on assumptions that are patently theoretical and not subject to easy empirical evaluation. It is ironic that empirical science initially successful, in part because of support from empiricist-minded philosophers, seems to rely now on claims that would tend to encourage anew the empiricism-metaphysics discussion. Such considerations are indeed the case for the instrumentalism-realism debate.

The instrumentalist thesis[2] states that theories should be

[1] Some contemporary authors view instrumentalism as one of a set of possible anti-realist theses. This set includes other positions like positivism, idealism, operationalism, etc. But for the purpose of investigating the nature of scientific theories, "instrumentalism versus realism" captures better the issues at stake than "realism vs anti-realism."

[2] Contemporary instrumentalists include Stephen Toulmin, *The Philosophy of Science* (New York. Harper and Row, 1960), pp. 134-139, and Bas Van Fraassen, *The Scientific Image* (Oxford: The Clarendon Press, 1980).

In the areas of scientific research proper one might include as an historical example of instrumentalism the controversial Copenhagen interpretation of quantum mechanics, notably the ideas of Bohr and Heisenberg. But note that the Copenhagen model was not acceptable to prominent theorists like Einstein and Planck. More recently, attempts to substantiate the theoretical assumptions of quantum theory have been attempted by Bohr in "A Suggested Interpretation of the Quantum Theory in Terms of Hidden Variables," I and II, *Physical Review* 85 (January 1952): 166-193. Finally, note that instrumentalim has also attracted adherents in the social sciences. The theories of economist Milton Friedman are generally regarded as founded on instrumentalism. See Milton Friedman's "The Methodology of Positive Economics," in *Readings in the Philosophy of the Social Sciences*, ed. May Brodbeck (New York: Macmillan, 1968), pp. 508-528.

properly considered as just tools or instruments for making observable predictions. Thus, the question of whether a predictive theory is true or false or whether its theoretical terms refer is of little moment for the pure instrumentalist. The usefulness of a theory is determined, therefore, by its predictive scope or range of applicability.

The realist thesis, on the other hand, states that science aims at giving a true picture of the world, and that the theoretical terms of successful and well-confirmed theories which purport to refer to existent entites actually refer to existent entities.[3] Furthermore, acceptance of a scientific theory implies that what the theory states is true. Thus, although theoretical terms like electron may not yield to direct observation their existence is not denied. In this regard, the rest mass of an electron taken to be .00055 amu, is assumed by the realist to be the measurement of a real entity; and likewise the measurements for other subatomic particles. The realist's thesis is supported also by the fact that contemporary research scientists believe that the theoretical terms of successful theories do refer and that the measurements ascribed to them should be taken as genuine. The realist, with much support from research scientists, can also appeal to the fact that the basic theoretical units of biochemistry, genetics, and other biological sciences are large molecules which are observable with the help of powerful instruments. The instrumentalist could point out though that the constituents of large molecules are those same theoretical entities whose status is in dispute.

The purpose of this paper is to argue that there is no basis for an inflexible support of either instrumentalism or realism as a viable research methodology. A study of the path of

[3] Good accounts of the realist thesis are those of Karl Popper, *Conjectures and Refutations: The Growth of Scientific Knowledge* (New York: Harper and Row, 1968), pp. 111-119; Paul Feyerabend, "Realism and Instrumentalism: Comments on the Logic of Factual Support," *The Critical Approach to Science and Philosophy*, ed. Mario Bunge (New York: The Free Press, 1964), pp. 280-308; Grover Maxwell, "The Ontological Status of Theoretical Entities," *Minnesota Studies in the Philosophy of Science*, Vol. III, ed. H. Feigl and G. Maxwell (Minneapolis: The University of Minnesota Press, 1962), pp. 3-27; Hilary Putnam, *Meaning and the Moral Sciences* (London: Routledge and Kegan Paul, 1978).

scientific research in recent times would demonstrate that researchers do not tend to rigidly adhere to either of the orthodox philosophical positions of instrumentalism or realism.

A cursory glance at the recent history of science shows that at the early stages in the development of a scientific theory the researcher often formulates his theory with appeal to theoretical terms which are acceptable mainly on instrumentalist grounds. But should the theory prove to be increasingly successful as ongoing research fosters its development, its theoretical terms are increasingly seen to refer. At this point, realism as a viable research methodology is seen to be vindicated. It is important to note too that the research scientist is most comfortable with his theory as it enters its realist phase. Yet the researcher is not exclusively a realist; he is also an instrumentalist. This research methodology is indeed at variance with the orthodox philosophical positions which suggest an incompatibility between realism and instrumentalism. Perhaps, the approach adopted in this paper speaks for a philosophy of science that prefers to pay close attention to the history of science in the short period and to explicate and evaluate both the research methodologies and findings of empirical science in all its areas.

To support the above thesis I shall proceed in the following way: first there will be a quick survey of the positions taken historically on the debate; this will be followed by a closer discussion of current views of the issue. Finally, case studies of theories in the history of science will be made as substantiating evidence for what may be called an instrumentalist-realist phase theory of scientific progress.

Recall the erstwhile popularity of instrumentalism as promoted by the logical positivists and the operationalists. This methodology was defended by the argument that what counted most in the formulation of a scientific theory were the theory's observation terms and sentences. This was defended on the ground that only observation statements were subject to empirical test. One perceives in this approach a strong reaction to classical metaphysics and its set of referential yet nonempirical terms. An exception was made for theoretical terms on the grounds that they were linked to observation terms by means of correspondence rules. But note the interesting, but eventually refuted, attempts made by some instrumentalists to dispense with the appeal to theoretical terms in the formulation of theories.[4] Theoretical terms are indeed recognized as being necessary for the formulation of fruitful theories.

In the recent literature, the debate is still unsettled though realism seems to have gained much support. Consider for example, Putnam's thesis of " 'convergence' in scientific knowledge" based on Richard Boyd's scientific realism.[5] According to Boyd, (1) terms in a mature science typically *refer,* and (2) the laws of a theory belonging to a mature science are typically approximately *true.*[6] Putnam is sympathetic to this thesis on the grounds that it offers a good description of actual scientific practice. Putnam argues that if some new theory T_2 were to be a proper candidate to replace some existing but problematic theory T_1 then the proper concern should be about whether T_2 can imply the "approximate truth" of laws of T_1. Putnam writes:

> Now, if all I, know is that T_1 leads to (mainly) true predictions in some observational vocabulary (a notion I have criticized elsewhere), then all I know about T_2 is that it should imply most of the 'observation sentences' implied

4 See Frank Ramsey, *The Foundations of Mathematics and Other Logical Essays* (New York: Harcourt Brace and World, 1931), pp. 212-236, and William Craig, "On Axiomatization Within a System," *Jornal of Symbolic Logic,* 18 (1953): 30-32, for interesting attempts to rid theories of any need to appeal to theoretical terms for purposes of formulation. Hempel's well-known counter-thesis disputed this possibility. See Carl Hempel, "The Theoretician's Dilemma: A Study in the Logic of Theory Construction," *Minnesota Studies in the Philosophy of Science,* Vol. II, ed. Feigl, Scriven, and Maxwell (Minneapolis: The University of Minnesota Press, 1958), pp. 37-98.

5 Hilary Putnam, *Meaning and the Moral Sciences* (London: Routledge and Kegan Paul, 1978), p. 20. Note that Richard Boyd's ideas on scientific realism are discussed in his forthcoming book *Realism and Scientific Epistemology* (Cambridge: Cambridge University Press, 1982).

6 It could be argued that Boyd's thesis applies more to "mature theories" than to "mature sciences." One assumes that mature sciences contain both mature and immature theories. And that the theoretical terms of their immature theories need not refer.

by T_1. But it does *not* follow that it must imply the truth of the *laws* of T_1 in some limit.[7]

Accordingly, it is better to determine whether referents can be assigned to the terms of T_1 from the vantage point of T_2.

Putnam demonstrates his idea of " 'convergence' in scientific knowledge" by the following:

> Yet it is a fact that we can assign a referent to "gravitational field" in Newtonian theory *from the standpoint* of relativity theory (though not to "ether" or "phlogiston"; a referent to Mendel's "gene" from the standpoint of present –day molecular biology; and a referent to Dalton's "atom" from the standpoint of quantum mechanics. These retrospective reference assignments depend on a principle that has been called the "principle of benefit of the doubt" or the "principle of charity", but not an *unreasonable* "charity".[8]

Yet questions can be raised as to whether some stretching of the imagination is required before any of the connections Putman proposes between the terms of the old and new theories may be made. But one need not go so far as to adopt the positions on this issue taken by Kuhn and Feyerabend. According to Putnam both Kuhn and Feyerabend would argue that the same term cannot have the same referent in different paradigms. Thus, for example, "if we use present theory to answer the question 'was Bohr referring when he used the term "electron"?', the answer has to be 'no', according to Kuhn and Feyerabend."[9] But how could the appropriate distinctions be made between the significance of terms like "Bohr's electron" and "phlogiston", if one were to accept the extreme position taken by Kuhn and Feyerabend? From the vantage point of modern chemistry, it is clear that "phlogiston" does

[7] Putnam, *op. cit* p. 21.
[8] *Ibid.*, p. 22.

not refer. Yet, on the other hand, the connection that Putnam proposes between, say, "Bohr's electron" and what we now call "electron" is effected only when the appropriate adjustments are first made. In this sense Putnam's thesis is somewhat extreme. Thus "retrospective reference assignments" are determined not merely by some "principle of charity", but by re-orderings of some significance. In this sense, one may argue that Mendel's "gene" and the "gene" of contemporary biology do not refer to the same entities. However, one is still quite free to claim that some terms of old theories have no reference whatever, as in the case of phlogiston.

This point may be elaborated on if we consider, for example, Putnam's reference to Mendel's "gene" and Dalton's "atom". Note too that a similar kind of argument could be made in the case of the idea of "gravitational field" in Newtonian theory from the standpoint of relativity theory.

Recall first that Mendel never used the term "gene", but rather "factor" for the basic hereditary unit. The two terms are not quite synonymous since the contemporary term "gene" derives its full meaning only if the term "chromosome" assumes a knowledge of the structural components of the cell or its nucleus. But Mendel's "factor" was formulated before the research facts about cell division were established and the chromosome theory of inheritance developed. Despite the fact that Mendel's theory of inheritance was established on the purely macroscopic data which revealed the modes whereby phenotypical traits of organisms were manifested through generations, the explanations offered by the theory did not contradict subsequent empirical findings at the microscopic level. In other words, from the vantage point of contemporary molecular biology, Mendel's factor hypothesis was quite insightful.

In this regard, subsequent research has confirmed Mendel's first law (The Law of Segregation), but Mendel's second law (The Law of Independent Segregation) has been modified in the light of research data in chromosome theory. But note again, that Mendel knew nothing about chromosomes. It seems, therefore, that Mendel's "factor" does not have the same referent as "gene" in contemporary biology.

The same may be said for Dalton's "atom." For Dalton, "atom" encompassed not only "atom" in the contemporary sense, but also "molecule." Further proof of the incompatibility of Dalton's "atom" with that of contemporary theory is had from the fact that Dalton represented a molecule of water as HO while contemporary theory represents water as H_2O.

Recall that the justification that Putnam offers for his theory of reference is based on what is referred to as "principle of benefit of the doubt." Thus, it is reasonable to assume that "the 'gene' discussed in molecular biology is the gene (or rather 'factor') Mendel intended to talk about." (Putnam, p. 22) This "principle of benefit of the doubt" is supported by the assumption that theory convergence is acceptable only on the grounds that different theories employing the same terms refer to the same entities. To guarantee these results, it is necessary that the principle of benefit of the doubt be applied appropriately. As Putnam put it, "benefit of the doubt can be *unreasonable;* we don't carry it so far as to say that *phlogiston* referred." (*ibid.*, p. 25).

Should this strategy not be implemented then talk of reference and truth with regard to scientific theories would be unwarranted. But Putnam's thesis cannot be sustained here since reference has been maintained in scientific research in spite of the fact that Lavoisier's quantitative chemistry had the phlogiston theory as its inmediate predecessor. The same may be said for Darwinian theory vis à vis Lamarckian theory. As was pointed out above there are discontinuities between the "gene" of molecular biology and Mendel's "factor", the "atom" of quantum mechanics and that of Dalton's. It is just not acceptable, therefore, to characterize the above examples as instances of different theories having the *same* entities as objects of reference. It may be argued, however, that the "gene" of molecular biology and Mendel's "factor" are related in the sense that Mendel's program of research into the dynamics of biological inheritance is *conceptually* akin to research in molecular biology. This point is reinforced by the conceptual incompatibility of research programs in Lamarckian theory and contemporary genetics.

The convergence of knowledge of which Putnam speaks, applies properly, therefore, to theories within conceptually compatible research programs. Thus later theories within a particular program may have earlier theories as limiting cases, yet there may be discontinuities in reference between the same terms used in different theories. In other words, contemporary molecular biology has Mendel's theory of inheritance as a limiting case, but Mendel's "factor" is referentially discontinuous with the "gene" of modern biology. One cannot correctly speak in this instance of a "different theory of the *same* entities".

But this argument (as mentioned briefly above) does not, on the other hand, lend total support to Kuhn's or Feyerabend's theses that the same term cannot have the same referent in different theories. Putnam's strategy in response to this claim is that "scientific terms are not synonymous with descriptions," since the same term may have different descriptions and that a description may be modified without discarding the term. To accept Kuhn's and Feyerabend's theses is to argue for a methodology of research which would be at loss to explain why researchers show, for example, a decided preference for the research program of modern quantitative chemistry over that of the period before Lavoisier; or why Lamarckian theory has been discarded but Mendelian theory modified and maintained.

I want to argue though that terms are synonymous with descriptions and that some descriptions depict imaginary entities, others are partially correct, yet others quite accurate. The term "phlogiston" does not refer, hence describes a fictitious entity. On the other hand "electron" in the sense of Bohr's theory is not the same term as "electron" in the sense of modern quantum theory. Properly speaking, the appropriate term of Bohr's "electron" is not simply "electron," but "Bohr's electron," (e_b^-) a term different from "quantum theory electron," (e_q^-). And both terms have different descriptions —though the distinction is minimal.

But are there cases where different terms yielding different descriptions refer to the same entity? There are ready examples. Consider in turn the classical (Arrhenius), protonic, and Lewis definition regards an acid as "any species which can The proton theory defines an acid as a "proton donor;" the Lewis definition regards an acid as "any species which can accept a pair of electrons." The terms are quite distinct: one speaks of "Lewis acid," "protonic acid," etc.

One saves the idea of theory convergence while acknowledging that terms are synonymous with descriptions. From the vantage point of tested and proven descriptions, inaccurate and fictive descriptions may be shown to be in error. Thus there are discontinuities between terms in a well-tried and maturing theory, but not of a qualitative or conceptual nature. In this regard, more mature theories have earlier theories as "limiting cases." But those cases where discontinuities between terms are of a conceptual nature (scientific revolutions and paradigm change inmediately come to mind) must also be considered in a general theory of scientific explanation.

Putnam's rationale for extending the principle of benefit of the doubt only to those terms which have been proven to have reference is to safe-guard the thesis of theory convergence. To assure that later theories have earlier theories as "limiting cases" one cannot afford to extend the idea of reference to, say, phlogiston. It is indeed a fact that the history of science has been marked by convergence in knowledge and the discarding of theories which did not adequately refer. But it is important to investigate the epistemological grounds on which discarded theories were once accepted.

A plausible explanation for this is that the criteria for the acceptance or rejection of theories have been made more stringent over the years, thereby making it more difficult for contemporary theorists to accept the kinds of explanations offered in pre-modern theories. This has been aided by improved instrumentation and more properly controlled experimental conditions. It is fair to say though that researchers, have, in principle, been concerned to formulate true theories but wherever test conditions and the amount of explanatory evidence have been less than adequate, "second best" criteria of acceptability have been adopted. And "second best" criteria satisfy instrumentalism more easily than realism. It is technically much less demanding to claim that a theory is acceptable because it happens to predict than to show that its claims are true.

The point is that the strict realist who pays little attention to the history of the growth of knowledge (characterized by quantitative changes in methodology or research, etc.) would be at loss to explain how some subsequently discredited theories were accepted for lengthy periods of time. Likewise, the pure instrumentalist could not explain why some theories, given their long tenure (suggesting some kind of predictive and explanatory usefulness), were eventually discarded.

A similar case may be made against arguments which raise questions about the viability of empirical realism and opt instead for an instrumentalist approach to theories. Witness, for example, J. Margolis's critique of the Putnam-Boyd thesis.[9] Margolis argues that Putnam's "principle of charity" the basis for his support of empirical realism

> has become so attenuated in Putnam's account that it is just as easy to construe the argument for realism as a transcendental one (as applied to idealism or instrumentalism, of course) as it is to construe it as empirical.[10]

One recalls that Putnam appeals to the "principle of charity" to determine those theoretical terms which have approximate sameness of reference in orden to explain theory convergence. Note too that theory convergence was possible only if later theories were connected to specific earlier theories:

[9] *Ibid.*, p. 23.

[10] Joseph Margolis, "Realism's Superiority over Instrumentalism and Idealism: A Defective Argument," *Southern Journal of Philosophy*, Vol. 17 (1979), pp. 473-478.

earlier theoretical terms are best described as incomplete profiles of later and more complete theoretical terms.

Margolis's critique of Putnam's defense of realism derives from Quine's thesis of "ontic commitment." This thesis suggests that the truth status of entities proper to particular theories is decidable only in terms of those theories. On this basis, according to Margolis, a theory may be false though according to Putnam's principle of charity claims may be made for the entities postulated by its theoretical terms. Margolis writes:

> On Quine's view, therefore, it becomes extremely difficult to say, in any phase of a changing science, that what we have *taken* to exist in our theory actually exists. . . One may then construe instrumentalism as a cautious refinement à la Quine *within* the framework of such a realist piety.[11]

True, a good case can be made for the role of instrumentalism in the formulation of theories, but the diachronic path of science would be inexplicable were researchers not able to justify their commitments to the theoretical entities of their theories on grounds of realism. Margolis does not sufficiently recognize that the history of science shows that a crucial distinction must be made between theoretical terms that do not refer like phlogiston, for example, and those that do refer, as say, the theoretical structure proposed for the benzene molecule. Recall that the first satisfactory structure for C_6H_6 was first proposed by Kekulé though other plausible structures like those suggested by James Dewar and others were serious candidates.[12] Contemporary organic chemistry knows that the theoretical structure for the benzene molecule refers while that for phlogiston does not. Thus contrary to Margolis's claim, it is possible to tell the difference between realist and nonrealist interpretations of theoretical terms. In fact, progress in science depends on knowing how to make this distinction.

[11] *Ibid.*, p. 478.
[12] *Ibid.*, p. 475.

And, the basis for the develpment of a theory from its immature stage to maturity, is determined by the theory's capacity to demonstrate that its key theoretical terms refer.

A similar case could be made against B. Van Fraassen's "constructive empiricism" an anti-realist position founded on the idea of "empirical adequacy."[13] Van Fraassen's anti-realism may be summarized as follows:

> This divides the anti-realists into two sorts. The first sort holds that science is or aims to be true, properly (but not literally) construed. The second holds that the language of science should be literally construed, but its theories need not be true to be good. The anti-realism I shall advocate belongs to the second sort.[14]

For Van Fraassen, to make claims for a scientific theory is not to assert it but to "display" it, to claim "empirical adequacy" rather than truth for it. The basis for Van Fraassen's anti-realism is that "the assertion of empirical adequacy is a

[13] Kekulé's structure for C_6H_6 (Benzene) was rivaled by suggestions of open ended chains and cyclic structures like that of Dewar's. But although Kekulé's structure was eventually considered the most satisfactory, it nevertheless was not without problems.

From the standpoint of our discussion, one might say that the period of the formulation of the molecular structure of benzene represents the instrumentalist stage of an immature theory of aromatic compounds in a still immature organic chemistry.

Some of the problems associated with the Kekulé model were that the Kekulé structure suggested addition rather than substitution reactions, and that the benzene molecule was shown to be more stable than the Kekulé structure. Furthermore, the Kekulé structure predicts two separate 1, 2– dibromobenzenes, while there has been evidence for only one 1, 2– dibromobenzene. Yet the Kekulé structure was not rejected by the researchers in a still-developing theory.

The problems posed by the Kekulé structure were eventually resolved by the resonance theory of molecular bonding of quantum mechanics. According to Kekulé the benzene molecule should be represented as two separate molecules in equilibrium: resonance theory, on the other hand, argues that the two Kekulé structures are not separate molecules but rather separate profiles of the same molecule.

The key point made here is that the Kekulé structure for benzene and its subsequent refinements cannot be viewed in the same light as theoretical terms like "phlogiston," "ether," and the like.

[14] Bas C. Van Fraassen, *The Scientific Image* (New York: Oxford University Press, 1980).

great deal weaker than the assertion of truth, and the restraint to acceptance delivers us from metaphysics."[15]

Yet once again, the criticism could be made that this interpretation of scientific research does not acknowledge its dynamic nature: the initial stages of eventually successful scientific theories are generally instrumentalist; their mature stages increasingly realist. Van Fraassen's thesis of empirical adequacy seems to correspond rather to the instrumentalist stage of developing theories. But what must be recognized is that what motivates researchers in science to develop further theories which have proven to be empirically adequate is that they are concerned to discover genuine facts about the phenomena under investigation. To aim merely for the criterion of empirical adequacy in the formulation of theories is to lower the hard-won standards of scientific research and to divest it of that level of rigor which gives it its special status in the cognitive enterprise.

Van Fraassen's thesis of empirical adequacy cannot really accomodate those scientific theories which contain theoretical terms that are observable with the aid of appropriate instruments. For if, according to Van Fraassen, truth claims could be made about hypotheses that refer to the observable, then the same kind of claims could be made about those scientific theories that make reference to observables. Clearly, those theoretical sciences like microbiology, whose theoretical terms refer to observable molecules, are better served by an epistemology that asks more of theories than mere empirical adequacy.

It seems rather that the idea of empirical adequacy as a sufficient criterion for scientific theories is more applicable to the immature research theories of sciences like quantum physics which explore the fundamental structure of the material world. The same may be said for those sciences whose researchers are not in complete control of their research data. Examples of such sciences are paleoanthropology and astronomy. Given the fact that sufficient but not more than

[15] *Ibid.*, p. 10.

adequate data is not always forthcoming for the formulation of theories in the above mentioned areas, researchers in these areas often have no alternative but to make claims for their theories on grounds of empirical adequacy. However, the limitations of the model of empirical adequacy are to be recognized from the fact that such research areas are characterized more than other areas by a number of competing theories.

But there are areas in scientific research in which, on Van Fraassen's own terms, one can make claims about truth. Van Fraassen writes:

> When the hypothesis is solely about what is observable, the two procedures amount to the same thing. For in that case, empirical adequacy coincides with truth. But clearly this procedure leads us to conclusions, about what the observable phenomena are like, which go beyond the evidencc available. Any such evidence relates to what has already happened, for example, whereas the claim of empirical adequacy relates to the future as well.[16]

But many scientific theories make claims about observable phenomena. Witness some of the theories in molecular biology in which explanations depend on observables such as bacteria, microbes and so on. Also, claims entailing truth are quite justified in theoretical work in that growing area of molecular biology known as recombinant DNA. Recall that in the earlier stages in the development of the theory of recombinant DNA claims about truth would have been somewhat excessive, though evaluation in terms of empirical adequacy would have been acceptable.

Van Fraassen would argue though that since scientific theories make claims that go beyond the merely observable, evaluation of theories in terms of truth (no matter how mature or successful the theory) instead of empirical adequacy incurs the arguments raised against scientific realism. But it must be recognized too that ordinary language statements about

[16] *Ibid.*, p. 69.

observable phenomena are no less at risk than scientific statements made about observable phenomena like bacteria, viruses, etc. Van Fraassen anticipates this counterargument and attempts to rebut it by claiming that theories which do not recognize an essential difference between purely observational language terms like tables and trees and theoretical terms like forces fields and absolute space are untenable.[17]

Van Fraassen writes that "such entities as sense-data, when they are not already understood in the framework of observable phenomena ordinarily recognized are theoretical entities. . . of an armchair psychology that cannot even rightfully claim to be scientific."[18] But a vast literature in experimental psychology shows that observation terms are implicitly theoretical terms since they are meaningful only within the context of their language frameworks. On this basis, therefore, observation terms are not far removed from theoretical terms as to the mechanics of their formulation.

But this is not the emphasis of my argument. I argue that effective instrumentation and the relative observability of the referents of the theoretical terms of mature scientific theories in research areas like biochemistry and molecular biology lead us to make the same kind of truth claims about these terms as we make about the observation terms of ordinary language. In those areas of scientific research where "empirical adequacy coincides with truth" it is certainly preferable to speak of truth than of empirical adequacy. The problem with the idea of "empirical adequacy" is that it is a useful criterion for some highly theoretical areas of scientific research, but researchers in science are not or ought not to be satisfied with just empirical adequacy as a sufficient standard for scientific research.

II

It is clear from the discussion above that neither realism nor instrumentalism (or any variant of anti-realism) fully

[17] *Ibid.*, p. 72.
[18] *Ibid.*

captures the processes involved in the formulation and development of scientific theories. The problem, it seems, derives from the undue emphasis that philosophers of science place on developments in theoretical physics, an important though admittedly restricted area of scientific research. A diachronic study of the develpment of scientific theories leads to the following conclusions: research is carried on with the intent of ultimately discovering the true nature of phenomena. In the process of the formulation of successful theories researchers would admit to instrumentalist and realist phases of these theories. Thus, although researchers may be satisfied with evaluations of empirical adequacy for the instrumentalist stages of immature theories, this is not sufficient for the aims of genuine research.

The following examples of the actual processes involved in scientific research are stated in support of this thesis. Take as a first case the theories proposed by researchers in theoretical chemistry to explain the basis for bonding between molecules at the subatomic level. The theories to be discussed are the valence bond theory, the crystal field theory, and the molecular orbital theory. In retrospect, the valence bond theory and the crystal field theory may be regarded as instrumentalist theories while the molecular orbital theory as exemplifying the early stages of a realist theory.

When theorists in the area of theoretical chemistry discovered that atoms combined to form molecules, the immediate question was what were the forces that led to this fusing together, strong in some cases, weaker in others? The valence bond theory seeks to explain chemical bonding in terms of electron sharing as opposed to ionic bonding proposed by the alternative electrostatic theory.

Before discussion of the valence bond theory, it is useful to bear in mind the following rules for the processes involved in covalent bonding. (1) Valence bond theory assumes that bonding between two atoms A and B takes place only when the energy of the electron on atom A equals the energy of the electron on atom B. (2) The atomic orbitals on the bonding atoms must overlap. (3) Two electrons alone are necessary to pair up for bonding to take place. The Pauli exclusion principle is the basis for this assumption. (4) A maximum of eight electrons is allowable in the valence sheel of most molecules. (5) For elements with available d orbitals, more than eight valence electrons are allowable. (6) Electron-electron repulsions will be minimized. (7) The molecule will seek the lowest overall energy.

The basic thesis behind the valence bond theory is as follows: consider two isolated hydrongen atoms described by the wave functions φ_a and φ_b. The wave function for the two atoms is $\varphi = \varphi_{(A)1}\ \varphi_{(B)2}$ where A and B designate the number 1 and 2 represent the electrons. With consideration given to exchange of energy when the two electrons are brought together, and subsequent *ad hoc* modifications to our wave function like ionic contributions, shielding, etc., the theoretical energy curve increasingly approximates the experimental values.

The initial wave function could finially be expressed as follows: $\varphi = \varphi_{cov} -+ \lambda'\varphi_{h+h} -+ \lambda'\varphi_{h-h}$ + were the first term represents the covalent content of the bonding, and the second and third represent the ionic and shielding contributions. What should be obvious here is that the formulation of the valence bond theory from the quantum mechanical point of view shows that the theorist does not really focus initially on the realism of theoretical posits like wave functions and so on, but on how best one can give an account for the experimental data. The theoriest is obviously an instrumental data. The theorist is obviously an instrumentalist at this stage and his hopes are for empirical adequacy. Commitment to the real existence of the theoretical posits of the valence bond theory comes only with the success of the theory in competition with alternative theories.

In support of this latter point, one must consider the application of valence bond theory to the explication of the structure of coordination compounds. In fact, a satisfactory model structure of these compounds may be conceived by the application of the valence bond theory to the above mentioned coordination compounds. For example, according to valence bond theory the bonding of $Cr(CO)_6$ is explained as d^2sp^3

hybridization and its model structure conceived as octahedral.[19] $Fe(CO)_5$ is regarded as dsp^2 hybridized with the geometric structure of the trigonal bypyramid, and so on.

But the valence bond theory is not the only theory which attempts to explain chemical bonding. As mentioned above, the crystal field theory has been a serious rival in recent times. The valence bond theory is not a complete theory of chemical bonding since it does not explain the color of complexes and must appeal to the *ad hoc* assumption of Pauling's electroneutrality principle in order to explain the build-up of negative charge on the metal ion of the coordination compounds. Furthermore, the valence bond theory is unable to explain the extent of the energy gap observed between high spin and low spin complexes. But so long as the valence bond theory satisfactorily answered the more relevant questions that theorists were asking about coordination compounds, they were willing to overlook shortcomings of the theory until a more comprehensive theory emerged. Clearly the approach here is one of instrumentalism tempered by a guarded realism.[20]

The crystal field theory,[21] in contrast to the valence bond model assumes ionic bonding between the metal and ligands in metal complexes. In the case of *d* orbitals, this would mean a pictorial model of a central metal atom of positive

[19] *Ibid.*

[20] The valence bond model for coordination compounds is as follows: Recall Hund's rule which states that "electrons enter each orbital of a given type singly and with identical spins before any pairing of electrons of opposite spins occurs within those orbitals." Thus the $4s$ electrons of the metal atoms may be promoted to the singly occupied $3d$ orbitals so that it may be occupied by some ligand. In the case of $Gr(CO)_6$ the singly occupied $3d$ and $4s$ orbitals of Cr are paired off to leave room for the six incoming CO ligands, from which one derives the d^2sp^3 hybridization.

[21] Consider the following statement which highlights the point made: "Until about 25 years ago, the valence bond theory was about the only one applied to coordination compounds by chemists. In the following decade a revolution occured and today few inorganic chemists use simple valence bond theory in discussions of these complexes. It is not because valence bond theory is wrong—it isn't. It has simply proved to be more convenient to explain the properties of these molecules in different terms. The valence bond theory of complex compounds is still alive. As we shall see (p. 640), Pauling is currently interpreting some unusual properties of coordination compounds in valence bond terms." James E. Huheey, *Inorganic Chemistry* (New York: Harper and Row, 1978), pp. 348-349.

charge and surrounding ligands on the x, y and z axes bearing negative charges. The shapes and orientations of the five d orbitals determine the extent of the electrostatic repulsion between the electrons in the orbitals and the surrounding negative charges. This explains the energy gap between the split d orbitals in an octahedral field. Now the extent of the energy gap determines whether the complex is high spin or low spin, which in turn depends on the metal and the surrounding ligands. Furthermore, one could predict the energy gap between the d orbitals of a complex by merely observing its color, hence determine whether the complex in high spin or low spin. The same principles at work here could be used to explain square planar and tetrahedral structures.

In sum, the matching between the value of the energy gap between split d orbitals in transition metals ($10\,Dq$) obtained from spectroscopically obtained data, the pairing energy and the spin state of the complex, offer strong conforming evidence for the greater operational strength of the crystal field model over the valence bond model. But one should note too the different assumptions on which the crystal field and the valence bond theories are constructed. One important theoretical distinction between the two models is that while valence bond theory assumes that the unoccupied orbitals of low spin states are involved only in the formation of hybrid bonds, in the crystal field theory these orbitals are minimally operational since they are repelled by the ligands.

Thus we have two theories on the formation of atomic structure, the one formulated on the assumption of hybridization, the other on the idea of point charges, both offering cognitively satisfying explanations for a set of observed phenomena, but not of equal strength in the explanation of other phenomena.

Does it mean that the theoretical assumptions of the valence bond theory are false or that the entities or processes described by the theory are nonexistent? This is a distinct possibility. But it is also possible that the valence bond theory represents a special case of the more inclusive crystal field model; or that both theories represent special cases of a more general model.

This theoretical puzzle could be resolved only by further research. A new theory synthesizing relevant portions of the valence bond theory and the crystal field theory would state that atomic bonding entailed both the idea of point charges, i.e., ionic bonding, and the idea of covalent linkages.

In fact, this latter alternative, referred to as the molecular orbital theory, has been the resolution for now of the problem. The reason for this approach is that the crystal field theory has demonstrated palpable weaknesses in predictive power. Consider, for example, the fact that the ligand that displays the greatest field strength in the electro-chemical series is the unionic ligand CO. Furthermore, the examination of the radial wave functions for metal and ligand atoms suggest the very strong likelihood of metal-ligand overlap. Other evidence comes from the nephalauxetic[22] effect and electron spin resonance.[23]

To demonstrate the way in which the molecular orbital theory offers a more complete explanation for the idea that atom bonding entails both ionic and covalent linkages, consider the following model: Some base B has one orbital with one pair of electrons to be donated, and some acid A^+ has two *sp* orbitals and a single odd electron. The approach of the ligand lone pair will split the orbitals into a higher energy orbital A_1 and A_2. According to the crystal field theory, the electron on A^+ will be positioned on the A_2 orbital being minimally repelled by the ligand.

But the molecular orbital theory assumes that at the approach of B, A_1 (the orbital on A facing B) will mix covalently with B. Consequently, a bonding and antibonding orbital will be formed: $\varphi_b = A_1 + B$, $\varphi_a = A_1 - B$. A_2 remains a nonbonding orbital because it does not face B. The resulting energy gap between A_1 and A_2 is evaluated as 10 Dq. Recall that the crystal field theory assumes no covalent bonding between A and B, the issue on which it spells out its major difference

22 Hans Bethe, *Ann. Physik* 5, 3 (1929): 135.

23 The nephalauxetic effect measures the electron-electron repulsion in metal complexes which is less for the complexes than for the free ion.

with the valence bond theory. However, the molecular orbital theory is able to accomodate both the covalent bonding and crystal field models by varying the electronegativity of B. The assumption of equal orbital energies on A and B yields the covalent model, while the crystal field model is arrived at by increasing the electronegativity of B. Increased electronegativity on B leads to bonding that is completely ionic.

Thus the evident explanatory incompleteness of both the valence bond theory and the crystal field model have been apparently resolved by the more comprehensive molecular orbital theory. To be sure, the molecular orbital theory presents its own problems, though largely of a computational nature. Measurement of appropriate overlap integrals, because of a lack of accurate wave functions, is a case in point.

What is clear from the case study above is that the initial stages an scientific research entail the formulation of theories that are best viewed as instruments which the prediction then gives the theorist justification for commitment to the theoretical assumptions for commitment to the theoretical assumptions of his hypothesis. In the case of the valence bond and crystal field theories, it is understood that their respective theorists were committed to the assumptions of their models. But it was the predictive failures of both theories that led to a questioning of their theoretical assumptions. The point is that the main impetus for the formulation of a new theory to replace both valence bond and crystal field theories was a desire on the part of the theorists working in the area of molecular bonding to offer more than just instrumentalist support for the theories in question. It is thus evident that the researcher is committed to realism, but until a theory has matured in the sense of being so successful that the formulation of rival theories is unnecessary, the theorist must be content with some variant of instrumentalism supported by criteria of empirical adequacy.

A survey of some other theories in the history of science demonstrate how theories develop from the instrumentalist stage to the realist stage by way of an intermediary instrumentalist-realist stage. The germ theory of disease has safely attained the realist stage of development; DNA theory has practically attained it, while the Pauli-Fermi neutrino hypothesis is at the instrumentalist-realist stage of development. These theories will be discussed in turn.

The germ theory of disease first formulated by biochemist Louis Pasteur, but not immediately accepted by the scientific community, is now fully accepted. This was made possible by the invention of microscopes which allowed researchers to directly observe disease-causing bacteria or viruses. This is a case of a theoretical construct ("germ") passing from the theoretical stage to the realist stage.[24] As a result, there are no serious scientific competitors to the germ theory of disease.

The Watson-Crick model (1953) of the DNA molecule is another good example of one theory demonstrating a complete dominance over rival theories. After proposing a different structure for the nucleic acid from the one afforded by L. Pauling and R. B. Corey, Watson and Crick[25] proposed a model which led to a genuine revolution in the history of biological science. The direct empirical evidence from X-ray diffraction techniques and the persistent confirmation in later research offer testimony of the dominance of the Watson-Crick theory of the structure of the DNA molecule over earlier rivals. Empirical evidence offered by X-ray diffraction techniques and from on-going research lends support to the view that the DNA theory has practically entered the realist stage.

Consider again, as an example of passage from the merely instrumental stage to the instrumentalist-realist stage of theory development, Pauli's (1931) and Fermi's (1934) appeal to the construct "neutrino" to preserve the exceedingly important conservation laws that seemed to be violated during beta decay. The laws of the conservation of mass and energy assume that in any physical or chemical process the total mass and

[24] The experimental evidence for metal ligand overlap by means of ESR is quite firm in the form of spectral data.

[25] The ultimate triumph of the germ theory of disease as it entered the realist stage on the observation of germs by means of microscopes would find strong support in Grover Maxwell's paper. "The Ontological Status of Theoretical Entities." *Minnesota Studies in the Philosophy of Science*, Vol. III, ed. H. Feigl and G. Marxwell (Minneapolis: The University of Minnesota Press, 1962) pp. 3-27.

energy of the system at t_1 equals mass and energy at t_2. Thus in the spontaneous disintegration of some atom A with the emission of beta particles, the conservation laws predict that the kinetic energy of every beta particle would be 1.87×10^{-13} joules. But it was found that on the average the emitted energy was much less than anticipated. Researchers in this area were faced with the choices of either rejecting the conservation laws or assuming that some undetected particles were emitted along with the beta particles during beta decay. Each of these undetected particles was assumed to a) travel at a speed approaching that of light, b) possess the missing energy, and c) have very negligible mass—in fact the mass of the nucleus of A before disintegration equals the sum of the product masses after disintegration. The name given to this hypothetical particle, as mentioned above, was "neutrino." But researchers were not content to remain at this instrumentalist stage. Subsequent tests to detect the existence of the neutrino proved successful thereby ensuring the passage of the neutrino hypothesis from the instrumentalist-realist stage.

III

The discussion in this paper offers an alternative to rival theories which attempt to evaluate scientific theories in terms of the truth claims of their theoretical terms and the theories themselves. Putnam's thesis as a contemporary version of realism was discussed along with the anti-realism of Margolis and Van Fraassen. The opposing theses were found wanting. A diachronic analysis of theories showed that successful yet immature theories are best evaluated from an instrumentalist standpoint. Mature theories can empirically identify their theoretical terms – either directly or indirectly. At this point, the theorist has attained his aims: claims entailing talk of truth or realism are in order. Case studies taken from the recent history of science support this point.

[26] See J. Watson and F. Crick, "Molecular Structure of Nucleic Acids," *Nature*, April 25, 1953, p. 737.

RESUMEN

En sus valoraciones de las teorías científicas, los filósofos de las ciencias exactas han optado por el apoyo que les ofrece, o bien, el instrumentalismo, o bien, el realismo. Sin embargo, un estudio del progreso actual de los conocimientos científicos señala que varias teorías se desarrollan a partir de una fase inicial instrumentalista y emprenden el camino hacia una fase realista.

En este trabajo se sostiene la tesis siguiente: el estudio diacrónico de las teorías científicas demuestra que no hay ninguna razón para mantener una rígida adherencia al realismo o al instrumentalismo en la valoración de las teorías científicas. También se discuten los trabajos recientes sobre el tema de Putnam y Van Fraassen.

[L. K.]