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Scientific Objectivity and the Logics of Science

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This paper develops an account of scientific objectivity for a relativist theory of evidence. It briefly reviews the character and shortcomings of empiricist and wholist treatments of theory acceptance and objectivity and argues that the relativist account of evidence developed by the author in an earlier essay offers a more satisfactory framework within which to approach questions of justification and intertheoretic comparison. The difficulty with relativism is that it seems to eliminate objectivity from scientific method. Reconceiving objectivity as a function of the social character of science, rather than of individually practiced methods, allows us to claim that science is objective even if relativism is true, and provides a more realistic account of scientific objectivity than is possible on either the empiricist or the wholist accounts.

I. Introduction

Objectivity is a characteristic ascribed variously to beliefs, individuals, theories, observations, and methods of inquiry. It is generally thought to involve the willingness to let our beliefs be determined by 'the facts', or by some impartial and non-arbitrary criteria, as opposed to our wishes as to how things ought to be. A specification of such involvement is a function of what it is that is said to be objective.

Some part of the popular reverence for science has its origin in the belief that scientific inquiry, unlike other modes of inquiry, is by its very nature objective. In the modern mythology,[1] the rejection of a mode of comprehension which simply projects human needs and values into the cosmos, and its replacement by a mode which views nature at a distance and dispassionately 'puts nature to the question', in the words of Francis Bacon, are seen as major accomplishments of the maturing human intellect. The development of this second mode of approaching the natural world is identified, on this view, with the development of science and the scientific method. Science is thought to provide us with a view of the world that is objective in two seemingly quite different senses of that term. (1) In one sense, objectivity is bound up with questions about the truth and referential character of scientific theories, that is, with issues of scientific realism. In this sense, to attribute objectivity to science is to claim that the view provided by science is an accurate description of the facts of the natural world as they are; it is a correct view of the objects to be found in the world and their relations with each other; (2) In the second sense, objectivity has to do with modes of inquiry. In this sense, to attribute objectivity to science is to claim that the view provided by science is one achieved by reliance upon non-arbitrary and non-subjective criteria for developing, accepting, and rejecting the hypotheses and theories that make up the view. The reliance upon and use of such criteria, as well as the criteria themselves, are what is called scientific method. Common wisdom has it that if science is objective in the first sense it is because it is objective in the second. I wish in this essay to test the common wisdom by examining how arbitrary and subjective criteria are excluded by and from scientific method.

At least two things can be intended by the ascription of objectivity to scientific method. Often scientists speak of the objectivity of data. By this they seem to mean that the information upon which their theories and hypotheses rest has been obtained in such a way as to justify their reliance upon it. This involves the assumption or assurance that experiments have been properly performed and that quantitative data have not been skewed by any faults in the design of survey instruments or by systematic but uncharacteristic eccentricities in the behavior of the items studied. If a given set of data has been objectively obtained, one is thereby licensed to believe that it provides an objective view of the world in the first of the two senses distinguished above. While this is indeed one crucial aspect of objective scientific method,[2] it is not the only dimension in which questions about the objectivity of methods can arise. In ascribing (or denying) objectivity to a method we can also be concerned about the extent to which it provides means of assessing hypotheses and theories in an unbiased and unprejudiced manner.

My intention is to explore more deeply the nature of this second mode of scientific objectivity and its connection with the logic of discourse in the natural sciences.[3] Philosophy of science in the past twenty to thirty years has been the forum of debate between two major approaches to the analysis of science. Logical empiricists have relied upon formal logic and *a priori* epistemological demands as keys to developing the logical analysis of science,[4] while their wholist critics have insisted upon the primacy of scientific practice as revealed by study of the history of science.[5] On the former view science does indeed appear to be, by its very nature, free of subjective preference, whereas on the latter view subjectivity plays a major role in theory development and theory choice. Witnesses to the debate seem to be faced with a choice between two unacceptable alternatives: a logical analysis which is historically unsatisfactory and a historical analysis which is logically unsatisfactory. This kind of dilemma suggests a debate whose participants talk past one another rather than addressing common issues. Certainly part of the problem consists in attempts to develop a comprehensive account of science on the basis either of normative logical constraints or of empirical historical considerations. The methodological analysis to be articulated and defended in this essay makes no pretense to totality or completion. It suggests, rather, a framework to be filled in and developed, both by epistemologists whose task is to develop criteria and standards of knowledge, truth, and rational belief on the one hand, and by historians and sociologists whose task is to make visible historical and institutional features of the practice of science that affect its content. To make way for this interdisciplinary framework, I begin by briefly reviewing the treatment of objectivity in the competing analyses of the logic of science.

II. Logics of Science

The Empiricist Model

The empiricist model of a finished science or of a finished theory in science is structurally analogous to Descartes's ideal vision of scientific knowledge as proceeding deductively from very general principles through more specific principles to empirical laws to statements describing observations. To be sure, Descartes acknowledged the necessity of calling upon auxiliary hypotheses to assist in the construction of the deductive system, but in both his and the empiricist view the finished product is an axiomatic theory analogous in structure to the geometry of Euclid.[6] It resembles, thus, a triangle or, better, a pyramid with the few most general principles forming its apex and the much more numerous observation descriptions forming its base. The primary difference between Descartes and the empiricists lies in the source of justification. For Descartes, the indubitability of its general principles provided the grounds for accepting a theory, and metaphysical principles certified by reason indicated what kinds of observations would be relevant.[7] Because the statements describing observation were logical consequences of the general principles, the latter's indubitability was transmitted downward to the base of the theory as a matter of logical fact. For empiricists, the direction of justification is reversed: justification proceeds from observation statements through generalizations and on up through the most general principles of the theory. Because the logical relation in this direction is inductive rather than deductive, justification is not certain. Contemporary analysts differ as to the nature of the justification offered, and range from those proposing that the techniques of inductive logic and probability theory provide means of determining the degree of confirmation,[8] to those who reject the idea that hypotheses could be inductively supported in favor of the notion of corroboration through the survival of attempts at falsification.[9] What they do have in common is the belief that the relation between the theoretical principles and the observation statements that support them, or can be used to test them, is a syntactic one.[10]

The empiricist analysis of confirmation guaranteed the objectivity of science by tying the acceptance of hypotheses and theories to a public world over whose description there can be no disagreement. In the empiricist model, the set of true observation statements changes only by becoming larger as more sophisticated instruments of observation are developed. The account of what the bare facts are is fixed and established independently of theories, perspectives, or points of view.[11] One is objective when one allows these facts, rather than one's wishes or prejudices, to determine what one will believe. While there is potentially more disagreement with respect to hypotheses in science, since they are not directly descriptive of observed states of affairs, the purely formal relationship between hypotheses and statements describing their evidence means that there can be no lasting disagreement as to which of these is confirmed and which not, or which is better confirmed.

Empiricists allow for a subjective element in scientific inquiry by distinguishing between a context of discovery and a context of justification.[12] The context of discovery for a given hypothesis is constituted by the circumstances surrounding its initial formulation – its origin in dreams, guesses, and other aspects of the mental and emotional life of the individual scientist. In the context of justification these generative factors are disregarded and the hypothesis is considered only in relation to its observable consequences, which determine its acceptability. This distinction enables empiricists to acknowledge the play of subjective factors in the initial development of hypotheses and theories, while guaranteeing that their acceptance remains untainted, determined not by subjective preferences, but by observed reality. When one is urged to be objective or 'scientific', it is this reliance on an established and commonly accepted reality that is being recommended. The logical empiricist model of confirmation simply makes the standard view of scientific practice more systematic and logically rigorous.

The Wholist Account

Recent history of science has undermined the original empiricist edifice. Scholars such as Norwood Hanson, Thomas Kuhn, and Paul Feyerabend (and more recently Gerd Buchdahl and Larry Laudan)[13] have argued that the actual, historical practice of science does not conform to the model sketched above. Kuhn and Feyerabend argued that their historical research shows that at times of major theory change incompatible theories are proposed as explanations for what seem to be the same states of affairs and are seemingly supported by the same evidence. Thus, the adoption of theories cannot be solely a function of seeing which theories are best confirmed by the independently established evidence. The alternative accounts that Kuhn and Feyerabend develop to explain the apparent facts of historical instances of theory choice appeal to what they describe as the fundamental incommensurability of theories – a function of the theory dependence of meaning and observation.[14] To say that meanings are theory-dependent is to say that terms which they seem to share do not have the same meaning in both theories. Thus, if theories T' and T'' are incommensurable and T' says that A's are $\emptyset$ and T'' says that A's are not $\emptyset$, T' and T'' are not contradicting one another because neither 'A' nor '$\emptyset$' retains its meaning from one theory to the other. To say that observation is theory-dependent is to say that there is no independent fund of facts to which one can have recourse in the process of theory choice. Rather, what one observes is a function of the theories to which one adheres. This is sometimes expressed by saying that adherents of competing theories (or paradigms) speak mutually untranslatable languages, hold different world views, and hence inhabit different worlds – worlds whose features are determined by the theories rather than the reverse. The sharp distinction drawn by the empiricists between the subjectivity permissible in the context of discovery and the objectivity required in the context of justification fades.

Recently, Gerald Doppelt has developed a modified version of the Kuhnian analysis.[15] He argues that in order to account for the historical episodes Kuhn analyzes, it is not necessary to appeal to notions of mutually untranslatable languages or incomparable observation. Incompatible paradigms and theories may share terms and data, so that their incompatibility can actually be ascertained. Their incommensurability is a function of their addressing different problems and assigning different importance to the data they do share. Thus, according to Doppelt, 'the root sense in which the concepts of rival paradigms are incommensurable is that they are embedded in incommensurable conceptions of science (involving incommensurable data, problems, and standards)'.[16] Even though incommensurable paradigms and theories may come into some logical contact with one another, incommensurability of data, problems, and standards means that the applicable criteria of evaluation are internal to paradigms.

How can scientific practice possibly be objective on this account? As long as one takes the empiricist analysis as providing a model to which any inquiry must conform in order to be objective and rational, then to the degree that actual science departs from the model it fails to be objective and rational. Both the historians and philosophers who have attacked the old model and those who have defended it have at times taken this position. The only disagreement with respect to objectivity, then, is over the question whether actual, historical, science does or does not realize the epistemological ideal of objectivity. While Kuhn has emphasized the role of such subjective factors as personality, education, and group commitments in theory choice, he also denies that his is a totally subjectivist view. He suggests that values such as relative simplicity and relative problem-solving ability can and do function as non-arbitrary criteria in theory-acceptance. How they can do so has yet to be spelled out satisfactorily.[17] Feyerabend, on the other hand, has rejected the relevance to science of canons of rationality and defended a positive role for subjectivity in science.

Shortcomings of the Two Models

Both the logical empiricist and the wholist view have their attractions. The empiricist account has a certain elegance and simplicity and it also articulates some of our common assumptions about the nature of scientific inquiry. Impressed by its ability to provide coherent explanations of the natural world, we have set science apart from other ways of thinking and understanding, considering it to be characterized primarily by its logical rigor and its grounding in observation and experiment. We explain the apparent success of science by thinking it to be a function of its exemplification of an epistemological ideal. The 'explication' of this ideal has been the work of the empiricists. The wholist account, on the other hand, has seemed better able to account for the facts of the history of science. In addition, because its perspective tends to dethrone established science it is also a source of aid and comfort to radicals and innovators who need to develop new theories and conceptual systems to express their perceptions and understanding.[18] These advantages are outweighed in both instances by shortcomings in the logic of scientific discourse emerging from each account.

The radical theory-dependence of meaning and observation introduced by the wholist perspective has the consequence that theories could never be said to be incompatible with each other, or consistent with one another.[19] It is impossible to take an outsider's view and assess how well a theory measures up to observed reality or how it compares with other theories. Reality is observed from within the standpoint of a theory and is described in terms whose meaning is determined by the principles of the theory. A different theory is expressed in a different language and must similarly be understood from within. The strong incommensurability thesis amounts almost to a private language thesis – but about paradigmatically public assertions rather than about sensation statements – and, because, in its strong form, it has the consequence that the ascertaining of intertheoretical logical relations such as consistency and inconsistency is impossible, it is difficult to understand why one would want to choose between theories, let alone have reason to think one better than another.

The milder interpretation of incommensurability offered by Doppelt still involves the irrelevance of external standards of adequacy that could be used in intertheoretic comparison and appraisal. The problem-centered approach divorces methodological questions from the traditional issues of truth and realism. The value of these issues, however, is not just that they are of moment to the philosophy of science, but that they are the issues of concern to the non-specialist.[20] In assessing the competing claims of hereditarian vs. environmental theory of human capacities and behaviors we are more concerned with which is true or false (or more true or less false) than with which better solves the problems it has posed for itself. The subjectivity of standards implied by the problem-centered approach ought not be accepted unless it can be shown that nothing else will account for scientific practice.

The empiricist account, on the other hand, is not only problematic as applied to the history of science. Both the raven paradox and Goodman's Grue/Bleen riddle challenge the account's formal adequacy.[21] Even more serious, from the point of view of analyzing science, Hempel's analysis is only applicable to situations in which a hypothesis and statement of evidence share the same descriptive terms, e.g. the empirical generalizations of statistical analysis. For the most part, however, scientific theories and hypotheses are expressed in language which cannot be used to describe the evidence adduced for them (without begging the question). Our theories and hypotheses make reference to items too small, too large, or too remote to be observed in any ordinary sense of that term, and hence cannot be brought into descriptions of the evidence for those theories.[22] The associated theses about meaning and the relationship between observational and theoretical language, which would allow empiricists to brush aside this objection, have been shown unsatisfactory by many analysts.[23] Thus, while it may articulate an epistemological ideal which provides external criteria of appraisal, these are simply inapplicable to most interesting cases.[24]

The Relativist Account

To avoid the problems attendant upon both the empiricist and the wholist analysis, I have proposed a third approach, which I call the 'relativist account'. Like that of the empiricists it has its origins in considerations of a conceptual nature; like that of the wholists, it can account for what seems to be the waywardness of historical scientific practice. Starting from a consideration of what is involved in ascribing the status of evidence to a given state of affairs, I have suggested that the relation between hypothesis and evidence is always mediated by background beliefs or assumptions.[25] Let me briefly summarize the relevant features of such an account.

However we end up characterizing observational data, *they* are what serve as evidence for hypotheses (and theories). Data – descriptions of observations and experimental results – do not on their own, however, indicate that for which they can serve as evidence. Hypotheses, on the other hand, are or consist of statements whose content always exceeds that of the statements describing the observational data. There is, thus, a logical gap between data and hypotheses. In some cases, as noted above, they are related as instance or instances of generalization. Instances, however, are *evidence* for the truth or plausibility of their generalizations only in light of some version of the principle of enumerative induction.[26] As was also noted above, in the interesting cases of scientific reasoning, e.g. that concerning the characterization of and relations among sub-atomic particles, hypotheses contain (as essential components) expressions not occurring in the description of the observations and experimental results serving as evidence for them. In these cases, reliance upon background beliefs or assumptions which assert relations (of causality or correlation) between these different classes of phenomena is much more apparent than in cases of (relatively) simple inductive or probabilistic reasoning. Background beliefs or assumptions, then, are expressed in statements which are required in order to demonstrate the evidential import of a set of data to a hypothesis. As such, they both facilitate and constrain reasoning from one category of phenomena to another. Although I shall use these terms interchangeably, it is appropriate to speak of beliefs when these statements are more or less explicitly adopted as tenets, and of assumptions when their necessity to a bit of evidential reasoning is not explicitly acknowledged.

Relativizing evidential import to background assumptions thus involves abandoning the attempt to specify the relation between evidence and hypotheses by means of syntactic criteria, and seeing this relation as involving substantive assumptions instead. Global, framework-like, assumptions (such as the various atomist, or reductionist, assumptions operative in physics, chemistry, and biology) are the most obvious kinds of background assumption, facilitating inferences from data at the phenomenal or macro-level to hypotheses about events and relations at the micro-level. More specific assumptions, such as the assumption in radiation carcinogenicity studies of a linear, quadratic, or linear-quadratic model in calculations of the dose-response curve, also function in this way. These calculations are based on data concerning response to radiation exposure in experimental animals and on relatively scanty epidemiological data (cancer incidence in uranium miners and survivors of the Hiroshima and Nagasaki bombings). Estimates of the risks to human populations in general of exposure to particular levels of particular types of ionizing radiation are projections from the data using one or another of the models. Which to use is a matter of some controversy. The point here is that some model has to be assumed and only in light of such assumption do the data acquire the status of evidence for any given risk estimate.[27]

This approach to evidence solves a number of problems. Substantive background assumptions can bridge the gap between hypotheses and evidence that the formal ties of the empiricist analysis cannot. Thus, the fact that conflicting hypotheses and theories have seemed to be supported by the same state of affairs can be explained by appealing to the different background beliefs in light of which the state is assigned evidential relevance to one or another hypothesis. The appeal to incommensurability, with its attendant logical difficulties, becomes unnecessary. Background assumptions may not always be explicit, but they can be articulated.

Besides offering a solution to these logical problems, the analysis has two additional strengths. In focusing discussion on the relation between evidence and hypotheses rather than on that between evidence and theories, the analysis can be used to examine the structure of reasoning in current and ongoing research projects that may not yet have developed a system of explanation comprehensive enough to be called a theory. A great deal of contemporary biological research is of this nature and is still structured by background assumptions that mediate inferences between data and hypotheses. Thus, the relativist analysis can be used to compare inference and argument in the formation of theories as well as in the defense of finished theories.[28] Its second strength is in its articulation of demands that non-specialists who look to scientific inquiry for an account of the natural world (ought to) pose to any seriously proposed hypothesis: what is the evidence? and why are these data evidence for this hypothesis?, i.e. why should I believe this? However incommensurability is understood it implies the meaninglessness of this question. The formal requirements of data and a rationale for assigning to the data evidential relevance to the hypothesis proposed at least provides a framework within which externally (and internally) generated questions of belief and justification make sense.

While this approach solves some problems, others loom large. In particular, by relativizing what counts as evidence to background beliefs or assumptions, hypothesis acceptance on the basis of evidence is also thus relativized. This invites the question: how can science so described come close to meeting the ideal of objectivity? The intrusion of subjective preference into evidential relations can, it seems, only be blocked by some absolute, non-relative, means of determining which hypotheses are supported and which not. If background beliefs mediate the relation between hypotheses and their evidence, then, if any states of affairs are evidentially relevant to them, i.e. to the background beliefs, this relevance can itself only be ascertained in light of further background beliefs. In the face of this argument, the promise of external or theory-independent standards of evaluation seems to vanish. In the absence of that clear and distinct perception of the truth of assumptions and fundamental propositions posited by Descartes and other rationalists,[29] the choice of background assumptions is as relative as the determination of evidential relations. Without some absolute and non-arbitrary means of determining acceptable or correct background assumptions, there seems no way to block the influence of subjective preference.

III. Objectivity

Does scientific method, on the relativist view, provide any constraints which can block the influence of subjective factors and allow us to retain the belief in the objectivity of science? As a first step in answering this question, it is important to distinguish between objectivity as a characteristic of scientific method and objectivity as a characteristic of individual scientific practitioners or of their attitudes and practices. The empiricist account of objectivity has tended to confuse the two, attributing objectivity to the practitioner to the extent she or he followed the method. By 'method' here, I mean whatever logical and systematic procedures are relied upon in accepting a hypothesis (or theory) and incorporating it into the canon of scientific knowledge. Scientific method, on the empiricist view, was something which *could* be practiced by a single individual: rationality and sense organs were all that is required. On the relativist account sketched above, rationality and deference to observational data are not sufficient to guarantee the objectivity of individuals, because their background beliefs and assumptions may ultimately be rooted in subjective preferences. Must we conclude that they are so necessarily?

A shift in perspective is necessary if we are to attribute to scientific method some guarantee of objectivity in the very general sense discussed in the first pages of this paper. The shift I suggest involves regarding scientific method as something practiced not primarily by individuals, but by groups.[30] The social nature of scientific practice has long been recognized.[31] What I wish to emphasize here is the idea that the application of scientific method, that is, of any subset of the collection of means of supporting scientific theory on the basis of experiential data, requires by its nature the participation of two or more individuals. Even brief reflection on the actual conditions of scientific practice suggests that this is so. Scientific knowledge is, after all, the product of many individuals working in (acknowledged or unacknowledged) concert. It is produced not by collecting the products of individuals into one whole, but through a process of critical emendation and modification of those products by the rest of the scientific community. Experiments get repeated with variations by individuals other than their originators; hypotheses and theories are critically examined, restated, and reformulated before becoming an accepted part of the scientific canon. What are known as scientific breakthroughs build, whether this is acknowledged or not, on previous work and rest on a tradition of understandings, even when the effect of the breakthrough will be to undermine those understandings.[32] What is called scientific knowledge, then, is produced by a community (ultimately, the community of all scientific practitioners) and transcends the contributions of any individual or even of any sub-community within the larger community.[33] Once propositions, theses, and hypotheses are developed, what will become scientific knowledge is produced collectively through the clashing and meshing of a variety of points of view.[34] The relevance of these features of the sociology of science to objectivity will be apparent shortly.

Focusing on the logic of scientific discourse rather than on scientific practice, we can note that an important and distinctive feature of scientific inquiry (retained in the relativist analysis) is that it is public. This means several things: (1) Theoretical assertions, hypotheses, and assumptions are all in principle public in the sense of being generally available to and comprehensible to anyone with the appropriate background, education, and interest. (2) The states of affairs to which theoretical explanations are pegged (in evidential and explanatory relationships) are public in the sense that they are intersubjectively ascertainable. (This does not require a commitment to a set of theory-free, eternally acceptable, observation statements, but merely a commitment to the possibility that two or more persons can agree about the descriptions of objects, events, and states of affairs that enter into evidential relationships.) These two aspects of the logical publicity of science make criticism of scientific hypotheses and theories possible in a way that is not possible, for instance, for descriptions of mystical experience, or expressions of feeling or emotion. There is no way to acquire the authority sufficient to criticize the description of a mystical experience or the expression of a particular feeling or emotion save by having the experience or emotion in question, and these are not had in the requisite sense by more than one person. The logical publicity of scientific understanding and subject-matter, by contrast, makes them, and hence the authority to criticize their articulation, accessible to all.[35]

There are a number of ways to criticize a hypothesis. For the sake of convenience, we can divide these into evidential and conceptual criticism, to reflect the distinction between criticism proceeding on the basis of experimental and observational concerns and that proceeding on the basis of theoretical and metatheoretical concerns.[36] Evidential criticism is familiar enough – Martin Gardner ridiculing the theories of Emmanual Velikovsky,[37] Stephen Gould analyzing the statistical data alleged to favor the hypothesis of the genetic basis of I.Q.[38] Such criticism questions the degree to which a given hypothesis is supported by the evidence adduced for it, questions the accuracy, extent, and conditions of performance of the experiments and observations serving as evidence, and questions their reporting.[39]

Conceptual criticism, on the other hand, often stigmatized as 'metaphysical', has received less attention in a tradition of discourse dominated by empiricist ideals. At least three sorts can be distinguished. The first questions the conceptual soundness of a hypothesis – as Einstein criticized and rejected the discontinuities and uncertainties of the quantum theory,[40] as Kant criticized and rejected, among other things, the Newtonian hypotheses of absolute space and time, a criticism which ultimately resulted in the development of field theory.[41] A second sort of criticism questions the consistency of a hypothesis with accepted theory – as traditionalists rejected the heliocentric theory because its consequences seemed inconsistent with the Aristotelian physics of motion still current in the fifteenth and sixteenth centuries,[42] as Millikan rejected Ehrenhaft's hypothesis of subelectrons on the basis not only of Millikan's own measurements but of his commitment to a particulate theory of electricity which implied the existence of an elementary electric charge.[43] A third sort questions the relevance of evidence presented in support of a hypothesis. Relativity theorists could deny the relevance of the Michelson-Morley interferometer experiment to the Lorentz-Fitzgerald contraction hypothesis, by denying the necessity of the ether.[44] Ruth Bleier and others have questioned the relevance of certain observations of animal populations to claims about dominance hierarchies within those populations, by criticizing the assumptions of universal male dominance underlying claims of such relevance.[45] This last form of criticism, though related to evidential considerations, is grouped with the forms of conceptual criticism because it is concerned not with how accurately the data have been measured and reported but with the assumptions in light of which those data are taken to be evidence for a given hypothesis in the first place.

All three of these types of criticism are central to the development of scientific knowledge. Discussions of the nature of human intelligence and whether it can be adequately modeled by computers, of the relation of subjectively experienced psychological phenonema to brain processes, for instance, are essential to theoretical development in cognitive science and neuropsychology, respectively. It is the third type of criticism, however, which amounts to questioning the background beliefs or assumptions in light of which states of affairs become evidence, that is crucial for the problem of objectivity.

What is necessary is a way to block the influence of subjective preference at the level of background beliefs. While the possibility of criticism does not totally eliminate subjective preference, either from an individual's or from a community's practice of science, it does provide a means for checking its influence in the formation of 'scientific knowledge'. Thus, even though background assumptions may not be supported by the same kinds of data upon which they confer evidential relevance to some hypothesis, other kinds of support can be provided, or at least expected.[46] At times the support may be empirical in nature (subject, of course, to the limitations developed above). At times, not. The discussions of intelligence and of the nature of mental phenomena such as subjective consciousness, for instance, involve issues that are metaphysical in nature and that, far from being resolvable by empirical means, must be resolved (explicitly or implicitly) in order to generate questions answerable by such means.

As long as background beliefs can be articulated and subjected to criticism from the scientific community, they can be defended, modified, or abandoned, in response to such criticism. As long as this kind of response is possible, the incorporation of hypotheses into the canon of scientific knowledge can be independent of any individual's subjective preferences. Their incorporation is, instead, a function in part of the assessment of evidential support. And while the evidential relevance to hypotheses of observations and experiments is a function of background assumptions, the adoption of these assumptions is not arbitrary, but is (or rather, can be) subject to the kinds of controls just discussed. This solution is compatible with both the sociological point of view and the logical. Sociologically and historically, the molding of what counts as scientific knowledge is an activity requiring many participants. Even if one individual's work is regarded as absolutely authoritative over some period – as, for instance, Aristotle's and later Newton's were – it is eventually challenged, questioned, and made to take the role of contributor rather than sole author – as Aristotle's and Newton's have been. From a logical point of view, if scientific knowledge is understood as the simple sum of finished products of individual activity, then there is no way to block or mitigate the influence of subjective preference. Only if the finished products are understood to be formed by the kind of critical discussion that is possible among a plurality of individuals, can we see how the production of scientific knowledge can be objective.

Objectivity, then, is a characteristic of a community's practice of science, rather than of an individual's, and the practice of science is understood in a much broader sense than most discussions of the logic of scientific method suggest. These discussions see what is central to scientific method as being the complex of activities which constitute hypothesis testing through comparison with experiential data – in principle, if not always in reality, an activity of individuals. What is suggested here is that scientific method involves as an additional integral aspect the subjection of hypotheses and the background assumptions in light of which they seem to be supported by data to varieties of conceptual criticism, which is a collective rather than individual activity.[47]

The respect in which science is objective, on this view, is one which it shares with other modes of inquiry, disciplines such as literary or art criticism and philosophy.[48] The feature which has often been appealed to as the source of the objectivity of science, that its hypotheses and theories are accepted or rejected on the basis of observational, experimental, data, is a feature which makes science empirical. In the empiricist account, for instance, it was the syntactically and deductively secured relation of hypotheses to a stable set of observational data that guaranteed the objectivity of scientific inquiry. That a method is empirical does not mean, however, that it is also objective. A method that involved the appeal to such data, but included no controls on the kinds of background assumptions in light of which their relevance to hypotheses might be determined, or that permitted a weekly change of assumptions so that a hypothesis accepted in one week on the basis of *E* would be rejected the next on the same basis, would hardly qualify as objective. Because the relation between hypotheses and evidence is mediated by background assumptions which themselves may not be subject to empirical confirmation or disconfirmation, and which may be affected by metaphysical or evaluative bias, it would be a mistake to identify the objectivity of scientific methods with their empirical features. The process that can expose such assumptions is what makes possible, even if it cannot guarantee, independence from subjective bias, and hence objectivity. Thus, while rejecting the idea that observational data alone provide external standards of comparison and evaluation of theories, this account does not reject external standards altogether. The formal requirement of demonstrable evidential relevance constitutes a standard of rationality independent of and external to any particular research program or scientific theory. The satisfaction of this standard by any program or theory, secured, as has been argued, by intersubjective criticism, is what constitutes its objectivity.

Objectivity, nevertheless, does turn out to be a matter of degree. The objectivity of a method of inquiry consists in the degree to which both its procedures and its results are available to the kinds of criticism described. Scientific communities will be objective to the degree that their methods of accepting and rejecting hypotheses and theories include this form of critical scrutiny of background assumptions. The objectivity of individuals consists in their participation in the collective give-and-take of critical discussion. Thus understood, objectivity is dependent upon the depth and scope of the critical discussion which occurs in any given scientific community. Any situation in a given scientific community which restricts the number of assumptions that can be criticized, or which protects any category of assumption from criticism, will diminish the objectivity of that community's practice. When, for instance, background assumptions are shared by all members of a community they acquire an invisibility which renders them unavailable for criticism. In addition, the substantive principles determining standards of rationality within a research program or tradition are impermeable to criticism by means of those standards. The greater the number of different points of view included in a given community, the more likely it is that its scientific practice will be objective. The smaller the number, the less likely this will be.[49] Ascertaining the practices and institutional arrangements that facilitate or undermine objectivity in any particular era or current field, and thus the degree to which the ideal of objectivity is realized, requires both historical and sociological investigation.

In a final remark, I wish to draw attention to a problem raised by the connection between objectivity and truth (or objectivity in the first of the senses distinguished above). Objectivity of method is desirable because it is thought to facilitate, if not guarantee, the development of true (or objective) theories. On the relativist account of evidential relations, however, we cannot know that a theory is true, in the classical, correspondence sense of 'true'. We can neither measure our theories and hypotheses directly against observed reality, nor can we hold the background beliefs and assumptions in light of which they seem evidentially supported to be beyond revision and rejection. And yet, somehow, scientific inquiry does succeed in providing accounts of the world that work, i.e. they enable us to do a lot more by way of explanation and prediction than we could do without such accounts.[50]

That the claim that scientific method is objective seems incompatible with the claim that the view of the world provided by science is objective is not, I think, a weakness of the analysis developed here. On the contrary, it suggests that we need a new account of the concept of 'truth' in science, if we are to keep it at all. Several recent discussions,[51] though otherwise quite different, of the relation of the content of current physical theory to the physical world seem to lead to the same conclusion. A new account of 'true' as a predicate of theories and hypotheses in science is beyond the scope of this paper. That substantive as well as methodological considerations cast doubt on the classical conception makes the provision of such an account highly desirable.

IV. The Scientific Enterprise

In addition to differing with respect to the proper analysis of scientific discourse, the theorists whose work has been discussed in these pages also seem to differ in their general conception of the enterprise of science. There are, I think, two conceptions discernible in a dichotomy that cuts across the division between empiricists and wholists. According to one conception, the proper concern of science is the construction of comprehensive accounts of the natural world. This involves the piecemeal working out of puzzles, the gradual extension of a theory to more and more facts. Scientific inquiry, on this view, is the search for roughly accurate or adequate descriptions of the natural world that allow for the prediction and control of certain of its aspects. Both Hempel and Kuhn seem to perceive the enterprise of science in this way. They differ, however, in their views of which analysis will bring out most perspicuously the logical features of the scientific enterprise: for Hempel the relation between observation and theory is context-free, while for Kuhn it can only be articulated in the context of a paradigm. But, with or without a paradigm, it is the gradual accretion of systematically related hypotheses, experiments, and observations that constitutes scientific growth or progress. Even though it is Kuhn's notion of revolutionary science that has captured the imagination of many, if not most, of his readers, Kuhn himself insists that the real work of science begins once a community has adopted a paradigm.

According to the other conception, which informs the work of both Popper and Feyerabend, the work of science is the discovery of the truth about the natural world. Both of them are skeptical about the possibility of any methodical way of making such judgments, but both regard the consensus that is required in Hempel's and Kuhn's models as a stifling dogmatism that can only hinder scientific progress. Again, these thinkers differ on the details of the logical analysis they provide – Popper, remaining true to some of the empiricist tenets, emphasizes falsifiability and falsification and thus the reduction in number of contending hypotheses, while Feyerabend, if he urges anything, urges the proliferation of hypotheses and theories.[52]

The dichotomy in these thinkers' approaches reflects a tension within science itself between its knowledge-extending mission and its critical mission.[53] Because of its subject, this paper has of necessity focused on the critical mission. If we are to have an adequate understanding of the scientific enterprise, however, both tasks must be understood in relation to one another. While such a comprehensive account lies far beyond the scope of this paper, the interdisciplinary methodological framework developed here implies that, ultimately, science performs its knowledge-extending task only if it also performs its critical task.

V. Conclusion

On the empiricist analysis of scientific method it is hard to understand how theories purporting to describe a non-observable underlying reality, or containing descriptive terms whose meaning is independent of that of so-called observational terms, can be supported. On the anti-empiricist wholist account it is just as difficult to understand how the theories that are developed have a bearing on intersubjective reality. Each of these approaches is also unable to account for certain facts about the actual practice of science. The absolute and unambiguous nature of evidential relations on the empiricist view cannot accommodate the facts of scientific change. The incommensurability of theories on the wholist view cannot do justice to the lively and productive debate that can occur among scientists committed to different theories.

Each of these modes of analysis emphasizes one aspect of scientific method at the expense of another, and each produces a logic of scientific method which fails adequately to articulate the structure of scientific discourse. The logic which does reflect this structure will have to abandon some of the simplicity of the empiricist account, but what it loses in elegance it will regain in application.

The relativist view produces a framework within which it is possible to capture the complexity of science, to do justice to the historical facts and to the current practice of science, and to avoid paradox. In addition, it articulates a standard of comparison independent of, and external to, any particular theory or research project. In making intertheoretic comparison possible, it offers the basis upon which to develop criteria of evaluation. Finally, the account of objectivity that it makes possible seems more true to the fact that scientific inquiry is not always as free from subjective preference as we would wish it to be. At the same time our intuition that scientific inquiry at its best *is* objective remains intact.[54]

NOTES

1 This mythology originates with the founders of modern science (cf. Isaac Newton's 'Rules of Reasoning in Philosophy', in H. S. Thayer [Ed.], *Newton's Philosophy of Nature*, Hafner, New York 1953, pp. 3–5) and has come to be the standard view.
2 It has become a subject of increased concern lately in light of several alleged incidents of data faking. Cf. W. Broad, 'Fraud and the Structure of Science', *Science*, Vol. 212 (1981), pp. 137–41.
3 Because the social sciences are thought to pose special problems regarding objectivity, my discussion is limited to the objectivity of the natural sciences.
4 I identify as empiricists all those methodologists who hold that observation is the sole arbiter between competing hypotheses or theories. This group includes such otherwise disparate theorists as Carnap, Hempel, and Popper.
5 By the term 'wholist' I refer to the work of scholars such as Thomas Kuhn, Paul Feyerabend, and N. R. Hanson, whose analyses require us to consider the complex system of a theory and its explananda and evidence as a whole, none of whose parts can be understood independently of one another.
6 It is instructive to read Descartes's remarks in the *Meteorology* in connection with those in the *Discourse on Method* (*Discourse on Method, Optics, Geometry, Meteorology*, Bobbs Merrill, Indianapolis 1960, pp. 49–62, 263–8). For the empiricist account of theories, see R. Carnap, *Foundations of Logic and Mathematics*, University of Chicago Press, Chicago 1939, pp. 29–69.
7 Cf. the discussion of the piece of wax at the end of the Second Meditation in Descartes, *Meditations*, Bobbs Merrill, Indianapolis 1960, pp. 87–91.
8 R. Carnap, *The Logical Foundations of Probability*, University of Chicago Press, Chicago 1952.
9 K. Popper, *The Logic of Scientific Discovery*, University of Toronto Press, Toronto 1959.
10 The classic exposition of this view is C. Hempel's 'Studies in the Logic of Confirmation', in *Aspects of Scientific Explanation*, The Free Press, New York 1965, pp. 3–51.
11 Classic expositions of this view are to be found in R. Carnap, 'Testability and Meaning', *Philosophy of Science*, Vol. III (1936), pp. 419–71, Vol. IV (1937), pp. 1–40, and C. Hempel, 'The Theoretician's Dilemma', in H. Feigl, M. Scriven, and G. Maxwell (Eds.), *Minnesota Studies in the Philosophy of Science*, Vol. II, University of Minnesota Press, Minneapolis 1958, pp. 37–98.
12 C. Hempel, *Philosophy of Natural Science*, Prentice-Hall, Englewood Cliffs 1966, pp. 3–18, and K. Popper, *Conjectures and Refutations*, Basic Books, New York 1962, pp. 42–59.
13 For the original wholist views, see N. R. Hanson, *Patterns of Discovery*, Cambridge University Press, Cambridge 1958; P. Feyerabend, 'Explanation, Reduction, and Empiricism', H. Feigl and G. Maxwell (Eds.), *Minnesota Studies in the Philosophy of Science*, Vol. III, University of Minnesota Press, Minneapolis 1962, 'Against Method', M. Radner and S. Winokur (Eds.), *Minnesota Studies in the Philosophy of Science*, Vol. IV, University of Minnesota Press, Minneapolis 1970, *Against Method*, Verso, London 1975; and T. Kuhn, *The Structure of Scientific Revolutions*, University of Chicago Press, Chicago 1970, 2nd ed. While Buchdahl and Laudan agree in denying the adequacy of the empiricist approach to the history of science, their concerns do not intersect closely enough with those of this essay to be treated here. But see Notes 17 and 36 below.
14 In 'Theory Change as Structure Change: Comments on the Sneed Formalism', *Historical and Philosophical Dimensions of Logic, Methodology, and Philosophy of Science*, R. Butts and J. Hintikka (Eds.), D. Reidel, Dordrecht 1977, pp. 289–309, Kuhn has seemed to retreat from the strong version of the incommensurability thesis discussed here, urging that 'incommensurable' does not mean 'incomparable', but signals the absence of a common language in which to translate two different theories for purposes of comparison. Rough translations are now held to be possible. His remarks concerning the determination of common applications and of similarity relations suggest, however, that this retreat is more apparent than real. While Kuhn and Feyerabend do share views about meaning and

logical relations in science, their over-all views about the character of scientific research differ markedly. These differences are discussed below, pp. 100–1.

15 G. Doppelt, 'Kuhn's Epistemological Relativism: An Interpretation and Defense', *Inquiry*, Vol. 21, No. 1 (Spring 1978), pp. 33–86.

16 Ibid., p. 49.

17 L. Laudan, *Progress and its Problems*, University of California Press, Berkeley 1977, does articulate criteria for what counts as progress. These are not necessarily criteria or standards for truth.

18 Only Feyerabend would accept this use of the wholist perspective. The innovators who find comfort in Kuhn's work are appealing to his concept of revolutionary science, rather than that of normal science, which he takes to be a description of a science in its mature state.

19 This is argued by P. Achinstein in *Concepts of Science*, Johns Hopkins University Press, Baltimore 1968, pp. 91–98, and by D. Shapere in 'The Structure of Scientific Revolutions', *Philosophical Review*, Vol. 73 (1964), pp. 383–4, and in 'Meaning and Scientific Change', in R. Colodny (Ed.), *Mind and Cosmos*, University of Pittsburgh Press, Pittsburgh 1966, pp. 41–85.

20 The case for non-specialism or 'fundamentalism' is argued by N. Maxwell in 'Science, Reason, Knowledge and Wisdom: A Critique of Specialism', *Inquiry*, Vol. 23, No. 1 (March 1980), pp. 19–81.

21 For the raven paradox and similar issues see Hempel, 'Studies in the Logic of Confirmation', op. cit., pp. 14–20, 47–56, and R. Grandy, 'Some Comments on Confirmation and Selective Confirmation', *Philosophical Studies*, Vol. 18 (1967), pp. 19–24; for the Grue/Bleen riddle, Nelson Goodman, *Fact, Fiction and Forecast*, Bobbs Merrill, Indianapolis 1965, pp. 59–83.

22 These objections are developed at greater length in H. Longino, 'Evidence and Hypothesis', *Philosophy of Science*, Vol. 46 (1979), pp. 35–56.

23 See Achinstein, op. cit., pp. 72–81, G. Maxwell, 'The Ontological Status of Theoretical Entities', in H. Feigl, and G. Maxwell (Eds.), *Minnesota Studies in the Philosophy of Science*, Vol. III, op. cit., and H. Putnam, 'What Theories are Not', reprinted in Putnam, *Mathematics, Matter and Method, Philosophical Papers*, Vol. I, Cambridge University Press, Cambridge 1975, pp. 215–27.

24 Any analysis which seeks to secure the objectivity of science by means of such syntactical devices is, for this reason, bound for failure. The prospect of such failure, however, frees us from the fiction that the relation to be analyzed is one between sentences. What we appeal to (and expect others to appeal to) as evidence is not sentences but objects, events, and states of affairs. We can only appeal to them under some description, to be sure, but what is described and not the description plays the supporting role.

25 Longino, op. cit., pp. 37–41.

26 The empiricist account of confirmation can, then, be seen as a limiting case describing the relation of hypothesis *h* and evidence *e* when *e* is assigned evidential relevance to *h* by the principle of simple induction. The abstractness of the principle of induction, it should be noted, does not make it a formal or syntactic principle. As has been noted since Hume, it involves a substantive claim about the persistence of regularities in the natural world. And a Popperian falsificationist would urge that we cease to rely upon it.

27 For discussion of the atomist and reductionist assumptions in the synthetic theory of evolution, see G. Webster and B. Goodwin, 'The Origin of Species: A Structuralist Approach', *The Journal of Social and Biological Structures*, Vol. 5 (1982), pp. 15–47, and S. J. Gould, 'Darwinism and the Expansion of Evolutionary Theory', *Science*, Vol. 216 (23 April 1982), pp. 380–7. Regarding the modeling assumptions relied upon in radiation carcinogenicity studies, see Committee on the Biological Effects of Ionizing Radiation, *The Effects on Populations of Exposure to Low Levels of Ionizing Radiation: 1980*, aka *BEIR III (1980)*, National Academy Press, Washington, D.C. 1980, pp. 11–36, 227–64, and J. Gofman, *Radiation and Human Health*, Sierra Club Books 1981, pp. 368–415.

28 C. Glymour's *Theory and Evidence*, Princeton University Press, Princeton 1980, advances a view not dissimilar to the one developed here. In spite of the greater detail of Glymour's

discussion and the consequent ability of his account to provide dimensions of intertheoretic comparison, his account does not escape the problems of relativizing evidential relevance discussed below (Cf. Longino, 'The Idea of a Value-Free Science', to be delivered at the March 1983 meeting of the American Philosophical Association – Pacific Division, Berkeley, California.) A corrective requirement that background theory be well established would make his analysis applicable more in hindsight to finished theories than to evidential reasoning in the context of theory formation. This limitation may also vitiate the analysis as a reconstruction of historical episodes. See the review by A. Morton, *Philosophy of Science*, Vol. 48, No. 3 (September 1981), pp. 498–500.

29 I do not mean to suggest that we are confronted here with a human shortcoming – a lack of clear and distinct perception – that we could remedy if we tried hard enough. The situation described is instead a fundamental aspect of the language available to us to express a certain kind of theoretical understanding of the world.

30 The account of objectivity which follows is in outline very close to Popper's in *The Open Society and Its Enemies* (Routledge & Kegan Paul, London 1945, pp. 205–8), where he emphasizes the roles of criticism and publicity and the sociality of science. His account remains programmatic, however, and when supplemented by the analysis of corroboration and falsification, and thus, of evidential relations, in *The Logic of Scientific Discovery*, op. cit., and *Objective Knowledge*, Oxford University Press, London 1972, is quite different from the account offered here (cf. pp. 96–100 below). The emphasis on the social nature of science is, of course, not unique to us, and is central to the evolutionary epistemology which develops Popper's views in a different direction.

31 Cf. J. Ravetz, *Scientific Knowledge and its Social Problems*, Oxford University Press, London 1971, and J. Ziman, *Public Knowledge*, Cambridge University Press, Cambridge 1968, and *The Force of Knowledge*, Cambridge University Press, Cambridge 1976.

32 James Watson's account of the discovery of the molecular structure of DNA (Watson, *The Double Helix*, Atheneum, New York 1968) read in conjunction with the story of Rosalind Franklin's contributions to that discovery (Nora Sayre, *Rosalind Franklin and DNA*, W. W. Norton, New York 1975) provide a vivid example of this interdependence.

33 The precise extension of 'scientific community' is here left unspecified. If it includes those interested in and affected by scientific inquiry, then it is much broader than the class of those professionally engaged in scientific research. For a discussion of these issues and some consequences of our current restricted understanding of the scientific community see K. P. Addelson, 'The Man of Professional Wisdom', in S. Harding and M. Hintikka (Eds.), *Discovering Reality: Feminist Perspectives on Epistemology, Metaphysics, Methodology, and Philosophy of Science*, D. Reidel, Dordrecht, forthcoming 1983.

34 Two recent books offer good illustrations of this point. S. Weinberg in *The First Three Minutes*, Basic Books, New York 1977, and G. Feinberg in *What Is the World Made of?* Anchor Press, New York 1978, account for the current (at the time of writing) states of cosmology and microphysics, respectively. Each presents what can be called the current canon in its field, making clear the dependence of its production upon the activity and interaction of many individual researchers.

35 To avoid possible confusion about the point being made here, I wish to emphasize that I am contrasting the descriptive statements of science with expressions of emotion. *Descriptions* of emotion and other subjective states may be as objective as other kinds of description, if the conditions for objectivity can be satisfied. Objectivity as it is being discussed here involves the absence (or control) of subjective *preference*, and is not necessarily divorced from our beliefs about our subjective states. Don Locke, in *Myself and Others*, Oxford University Press, London 1968, pp. 5–12, discusses the different ways in which privacy is properly and improperly attributed to subjective states.

36 The distinction between the different kinds of concerns relevant to the development and evaluation of theories is discussed for different purposes and with significant differences in detail by Buchdahl (in a discussion of criteria choice), Laudan (in a discussion of the problems which give rise to the development of theory), and Schaffner (in a discussion of categories for comparative theory evaluation). A more complete categorization of concerns and types of criticism than that offered here requires a more thorough study of

past and present scientific practice. See G. Buchdahl, 'History of Science and Criteria of Choice', in R. Steuwer (Ed.), *Minnesota Studies in the Philosophy of Science*, Vol. V, University of Minnesota Press, Minneapolis 1970, pp. 204–30, Laudan, op. cit., and Schaffner, op. cit.

37 M. Gardner, *Fads and Fallacies in the Name of Science*, Dover, New York 1957, pp. 3–14.

38 S. Gould, 'Jensen's Last Stand', *New York Review of Books*, Vol. XXVII, No. 7 (1980), pp. 38–44, and *The Mismeasure of Man*, Norton, New York 1981, pp. 146–233.

39 The latter two kinds of questions are concerned with the objectivity of data, a notion mentioned above.

40 J. Bernstein, *Einstein*, William Collins & Son, Bungay 1973, pp. 137–77.

41 L. Pearce Williams, *The Origins of Field Theory*, Random House, New York 1966, pp. 32–63. A somewhat different account is presented by M. Hesse in *Forces and Fields*, Littlefield Adams, Totowa 1965, pp. 170–80.

42 T. Kuhn, *The Copernican Revolution*, Random House, New York 1957, pp. 100–33, 185–92.

43 G. Holton, 'Subelectrons and Presuppositions and the Millikan-Ehrenhaft Dispute', in Holton, *The Scientific Imagination*, Cambridge University Press, Cambridge 1978, pp. 25–83.

44 B. Jaffe, *Michelson and the Speed of Light*, Doubleday & Co., Garden City 1960, pp. 95–103.

45 R. Bleier, 'Bias in Biological and Human Sciences: Some Comments', *Signs*, Vol. 4, No. 1 (Autumn 1978), pp. 159–62.

46 Conceptual criticism of this sort is a far cry from the criticism envisaged by Popper. For him metaphysical issues must be decided empirically, if at all. (And if they cannot be so tested, they lack significance.) For criticism of the background assumptions used as examples above (p. 93), see the materials cited in Note 27, which make use of a variety of the argumentative strategies noted here.

47 This is really a distinction between the number of points of view (minds) required. Many individuals (sharing assumptions and points of view) may be involved in testing a hypothesis (and commonly are in contemporary experiments). And though this is much rarer, one individual may be able to criticize her or his own evidential reasoning and background assumptions from other points of view.

48 This is not to deny the importance of distinguishing between different modes of understanding – for instance, between scientific and philosophical and literary theories – but simply to deny that objectivity can serve as any kind of demarcation criterion.

49 This insistence on the variety of points of view required for objectivity is developed on a somewhat different basis for the social sciences by Sandra Harding in 'Four Contributions Values Can Make to the Objectivity of the Social Sciences', in P. Asquith and I. Hacking (Eds.), *Proceedings of the 1978 Biennial Meeting of the Philosophy of Science Association*, Philosophy of Science Association, East Lansing 1978, pp. 199–209.

50 This is the kind of consideration appealed to by Putnam in defense of realism. Cf. H. Putnam, *Meaning and the Moral Sciences*, Routledge & Kegan Paul, London 1978, pp. 19 f. The argument that scientific theories must be true or very nearly true and hence referential because they are so successful in application is very different from an argument that a given theory is true because supported unequivocally by the evidence. The character of this argument is discussed by Putnam on pp. 97–117. See also J. Leplin, 'Truth and Scientific Progress', *Studies in History and Philosophy of Science*, Vol. 12, No. 4 (December 1981), pp. 269–91.

51 N. Cartwright, 'The Truth Doesn't Explain Much', *American Philosophical Quarterly*, Vol. 17 (1980), pp. 159–63 and Geoffrey Joseph, 'The Many Sciences and the One World', *The Journal of Philosophy*, Vol. LXXVII (1980), pp. 773–91.

52 While the distinction being drawn here may resemble that between the sociology and the philosophy of science, these are not at all the same thing. Both tasks produce problems requiring the logical and epistemological analysis of philosophers, and both involve the group behavior of scientists which is the subject of sociological study.

53 Cartwright, op. cit., discusses a similar tension between the demand for explanation and that for truth.

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