HELEN E. LONGINO

Values and Objectivity

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 nonarbitrary criteria rather than by our wishes as to how things ought to be. A specification of the precise nature of such involvement is a function of what it is that is said to be objective. In this chapter I will review some common ideas about objectivity and argue that the objectivity of science is secured by the social character of inquiry. This chapter is a first step, therefore, towards socializing cognition.

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, the replacement of a mode of comprehension that simply projects human needs and values into the cosmos by a mode that views nature at a distance and dispassionately "puts nature to the question," in the words of Francis Bacon, is seen as a major accomplishment of the maturing human intellect.[1] The development of this second mode of approaching the natural world is identified, according to 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. 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 of their relations with each other. In the second sense objectivity has to do with modes of inquiry. In this sense to attribute objectivity to science is

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to claim that the view provided by science is one achieved by reliance upon nonarbitrary and nonsubjective 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.

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 sample studied. If a given set of data has been objectively obtained in this sense, one is thereby licensed to believe that it provides a reliable view of the world in the first of the two senses of objectivity distinguished above. . . . While objective, that is, reliable, measurement 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.

In this chapter I will explore more deeply the nature of this second mode of scientific objectivity and its connection with the logic of discourse in the natural sciences. . . . Logical positivists have relied upon formal logic and a priori epistemological requirements as keys to developing the logical analysis of science, while their historically minded wholist critics have insisted upon the primacy of scientific practice as revealed by study of the history of science. According to the former view, science does indeed appear to be, by its very nature, free of subjective preference, whereas according to 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 that is historically unsatisfactory and a historical analysis that 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. My analysis 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 and by historians and sociologists whose task is to make visible those 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 and subjectivity in the competing analyses of the logic of science.

Objectivity, Subjectivity, and Individualism

The positivist 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. Positivists allow for a subjective, nonempirical element in scientific inquiry by distinguishing between a context of discovery and a context of justification.[3] 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. Two things should be noted here. First, these nonempirical elements are understood to be features of an individual's psychology. They are treated as randomizing factors that promote novelty rather than as beliefs or attitudes that are systematically related to the culture, social structure, or socioeconomic interests of the context within which an individual scientist works. Secondly, 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 positivists 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. The subjective elements that taint its origins are purged from scientific inquiry by the methods characteristic of the context of justification: controlled experiments, rigorous deductions, et cetera. 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 positivist model of confirmation simply makes the standard view of scientific practice more systematic and logically rigorous.

As long as one takes the positivist 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. As noted above with respect to evidence and inference, 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, seems to be over the question of whether actual, historical science does or does not realize the epistemological ideal of objectivity. Defenders of the old model have argued that science ("good science") does realize the ideal. Readers of Kuhn and Feyerabend take their arguments to show that science is not

objective, that objectivity has been fetishized by traditionalists. These authors themselves have somewhat more subtle approaches. 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 nonarbitrary criteria in theory acceptance. Such values can be understood as internal to inquiry, especially by those to whom scientific inquiry just is problem solving.[4] Feyerabend, on the other hand, has rejected the relevance to science of canons of rationality or of general criteria of theory acceptance and defends a positive role for subjectivity in science.[5]

. . . How can the contextualist analysis of evidence, with its consequent denial of any logically guaranteed independence from contextual values, be accommodated within a perspective that demands or presupposes the objectivity of scientific inquiry?

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 standard accounts of scientific method tend to conflate the two, resulting in highly individualistic accounts of knowledge. Both philosophical accounts assume that method, the process by which knowledge is produced, is the application of rules to data. The positivist or traditional empiricist account of objectivity attributes objectivity to the practitioner to the extent that she or he has followed the method. Scientific method, on this view, is something that *can* be practiced by a single individual: sense organs and the capacity to reason are all that are required for conducting controlled experiments or practicing rigorous deduction. For Kuhn and for the contextualist account sketched above rationality and deference to observational data are not sufficient to guarantee the objectivity of individuals. For Kuhn this is because these intellectual activities are carried out in the context of a paradigm assented to by the scientific community. But, although Kuhn emphasizes the communitarian nature of the sciences, the theory of meaning he developed to account for the puzzling aspects of scientific change that first drew his attention reduces that community to a solipsistic monad incapable of recognizing and communicating with other monads/communities. Kuhn's account is, thus, as individualist as the empiricist one. The contextualist account makes the exercise of reason and the interpretation of data similarly dependent on a context of assumptions. Why is it not subject to the same problems?

Objectivity, Criticism, and Social Knowledge

Two shifts of perspective make it possible to see how scientific method or scientific knowledge is objective even in the contextualist account. One shift is to return to the idea of science as practice. The analysis of evidential relations outlined above was achieved by thinking about science as something that is done, that involves some form of activity on the part of someone, the scientist. Because we think the goal of the scientist's practice is knowledge, it is tempting to follow tradition and seek solutions in abstract or universal rules. Refocussing on science as practice makes possible the second shift, which involves regarding scientific method as something practiced not primarily by individuals but by social groups.

The social nature of scientific practice has long been recognized. In her essay "Perception, Interpretation and the Sciences" Marjorie Grene discusses three aspects of the social character of science.[6] One she sees as the existence of the scientific disciplines as "social enterprises," the individual members of which are dependent on one another for the conditions (ideas, instruments, et cetera) under which they practice. Another related aspect is that initiation into scientific inquiry requires education. One does not simply declare oneself a biologist but learns the traditions, questions, mathematical and observational techniques, "the sense of what to do next," from someone who has herself or himself been through a comparable initiation and then practiced. One "enters into a world" and learns how to live in that world from those who already live there. Finally, as the practitioners of the sciences all together constitute a network of communities embedded in a society, the sciences are also among a society's activities and depend for their survival on that society's valuing what they do. Much of the following can be read as an elaboration of these three points, particularly as regards the outcome, or product, of scientific practices, namely scientific knowledge. What I wish particularly to stress is that the objectivity of scientific inquiry is a consequence of this inquiry's being a social, and not an individual, enterprise.

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 shows that this is so. Scientific knowledge is, after all, the product of many individuals working in (acknowledged or unacknowledged) concert. As noted earlier, scientific inquiry is complex in that it consists of different kinds of activities. It consists not just in producing theories but also in (producing) concrete interactions with, as well as models—mechanical, electrical, and mathematical—of, natural processes. These activities are carried out by different individuals, and in this era of "big science" a single complex experiment may be broken into parts, each of which will be charged to a different individual or group of individuals. The integration and transformation of these activities into a coherent understanding of a given phenomenon are a matter of social negotiations.

One might argue that this is at least in principle the activity of a single individual. But, even if we were to imagine such group efforts as individual efforts, scientific knowledge is not produced by collecting the products of such imagined individuals into one whole. It is instead produced through a process of critical emendation and modification of those individual 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.[7]

The social character of scientific knowledge is made especially apparent by the organization of late twentieth-century science, in which the production of knowledge is crucially determined by the gatekeeping of peer review. Peer review determines what research gets funded and what research gets published in the journals, that is, what gets to count as knowledge. Recent concern over the breakdown of peer review and over fraudulent research simply supports the point. The most startling study of peer review suggested that scientific papers in at least one discipline were accepted on the basis of the institutional affiliation of the authors rather than the intrinsic worth of the paper.[8] Commentary on the paper suggested that this decision procedure might be more widespread. Presumably the reviewers using the rule assume that someone would not get a job at X institution if that person were not a top-notch investigator, and so her/his experiments must be well-done and the reasoning correct. Apart from the errors in that assumption, both the reviewer and the critic of peer review treat what is a social process as an individual process. The function of peer review is not just to check that the data seem right and the conclusions well-reasoned but to bring to bear another point of view on the phenomena, whose expression might lead the original author(s) to revise the way they think about and present their observations and conclusions. To put this another way, it is to make sure that, among other things, the authors have interpreted the data in a way that is free of their subjective preferences.

The concern over the breakdown of peer review, while directed at a genuine problem, is also exaggerated partly because of an individualist conception of knowledge construction. Peer review prior to publication is not the only filter to which results are subjected. The critical treatment *after* publication is crucial to the refining of new ideas and techniques. While institutional bias may also operate in the postpublication reception of an idea, other factors, such as the attempt to repeat an experiment or to reconcile incompatible claims, can eventually compensate for such misplaced deference. Publication in a journal does not make an idea or result a brick in the edifice of knowledge. Its absorption is a much more complex process, involving such things as subsequent citation, use and modification by others, et cetera. Experimental data and hypotheses are transformed through the conflict and integration of a variety of points of view into what is ultimately accepted as scientific knowledge.[9]

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 subcommunity within the larger community.[10] 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. The relevance of these features of the sociology of science to objectivity will be apparent shortly.

The social character of hypothesis acceptance underscores the publicity of science. This publicity has both social and logical dimensions. We are accustomed to thinking of science as a public possession or property in that it is produced for the most part by public resources—either through direct funding of research or through financial support of the education of scientists. The social processes described underscore another aspect of its publicity; it is itself a public resource—a common fund of assertions presumably established to a point beyond question. It thereby constitutes a body of putative truths that can be appealed to in defense or criticism of other claims.

From a logical point of view the publicity of science includes several crucial elements. First, theoretical assertions, hypotheses, and background 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. Second, 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. Both features are consequences of the facts (1) that we have a common language which we use to describe our experience and within which we reason and (2) that the objects of experience which we describe and about which we reason are purported to exist independently of our seeing and thinking about them.[11]

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. First, a common language for the description of experience means that we can understand each other, which means in turn that we can accept or reject hypotheses, formulate and respond to objections to them. Second, the presupposition of objects existing independently of our perception of them imposes an acceptance of constraints on what can be said or reasonably believed about them. Such acceptance implies the relevance of reports and judgments other than our own to what we say or believe. There is no way, by contrast, 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. By contrast, the logical publicity of scientific understanding and subject matter makes them and hence the authority to criticize their articulation accessible to all.[12] It should be said that these constitute necessary but not sufficient conditions for the possibility of criticism, a point I shall return to later. It is the possibility of intersubjective criticism, at any rate, that permits objectivity in spite of the context dependence of evidential reasoning. Before developing this idea further let me outline some of the kinds of criticism to be found in scientific discourse.

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.[13] Evidential criticism is familiar enough: John Maddox, editor of *Nature*, criticizing Jacques Benveniste's experiments with highly diluted antibody solutions suggesting that immune responses could be triggered in the absence of even one molecule of the appropriate antibody;[14] Richard Lewontin analyzing the statistical data alleged to favor Jensen's hypothesis of the genetic basis of I.Q.,[15] Stephen Gould criticizing the experiments of David Barash purporting to demonstrate punitive responses by male mountain bluebirds to putative adultery on the part of their female mates.[16] 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 analysis and reporting.[17]

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;[18] as Kant criticized and rejected, among other things, the Newtonian hypotheses of absolute space and time, a criticism that contributed to the development of field theory.[19] 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;[20] 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 that implied the existence of an elementary electric charge.[21] 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;[22] Thelma Rowell 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;[23] critics of hypotheses about the hazards of exposure to ionizing radiation direct their attention to the dose-response model with which results at high exposures are projected to conditions of low exposures.[24] Thus most of the debate centers not on the data but on the assumptions in light of which the data are interpreted. 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 has been measured and reported but with the assumptions in light of which that data is taken to be evidence for a given hypothesis in the first place. Here it is not the material presented as evidence itself that is challenged but its relevance to a hypothesis.

All three of these types of criticism are central to the development of scientific knowledge and are included among the traditions of scientific discourse into which the novice is initiated. 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. Objectivity in the sense under discussion requires 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.[25] And in the course of responding to criticism or providing such support one may modify the background assumption in question. Or if the original proponent does not, someone else may do so as a way of entering into the discourse. Criticism is thereby transformative. In response to criticism, empirical support may be forthcoming (subject, of course, to the limitations developed above). At other times the support may be conceptual rather than empirical. Discussions of the nature of human judgment and cognition and whether they can be adequately modelled by computer programs, and of the relation of subjectively experienced psychological phenomena to brain processes, for instance, are essential to theoretical development in cognitive science and neuropsychology respectively. But these discussions involve issues that are metaphysical or conceptual 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. The contextual analysis of evidential relations shows the limits of purely empirical considerations in scientific inquiry. Where precisely these limits fall will differ in different fields and in different research programs.

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 incorporates as elements both the social character of the production of knowledge and the public accessibility of the material with which this knowledge is constructed.

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 were to be understood as the simple sum of finished products of individual activity, then not only would there be no way to block or mitigate the influence of subjective preference but scientific knowledge itself would be a potpourri of merrily inconsistent theories. Only if the products of inquiry are understood to be formed by the kind of critical discussion that is possible among a plurality of individuals about a commonly accessible phenomenon, can we see how they count as knowledge rather than opinion.

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 that constitute hypothesis testing through comparison with experiential data—in principle, if not always in reality, an activity of individuals. What I have argued here is that scientific method involves as an equally central 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 social rather than an individual activity.[26]

The respect in which science is objective, on this view, is one that it shares with other modes of inquiry, disciplines such as literary or art criticism and philosophy.[27] The feature that 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 that makes scientific inquiry empirical. In the positivist 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. But, as I've argued, most evidential relations in the sciences cannot be given this syntactic interpretation. In the contextual analysis of evidential relations, however, that a method is empirical in the above sense does not mean that it is also objective. A method that involved the appeal to observational or experimental 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 some bit of evidence *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 that themselves may not be subject to empirical confirmation or disconfirmation, and that may be infused with metaphysical or normative considerations, it would be a mistake to identify the objectivity of scientific methods with their empirical features alone. 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 and acceptability 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.

Scientific knowledge is, therefore, social knowledge. It is produced by processes that are intrinsically social, and once a theory, hypothesis, or set of data has been accepted by a community, it becomes a public resource. It is available to use in support of other theories and hypotheses and as a basis of action. Scientific knowledge is social both in the ways it is created and in the uses it serves.

Objectivity by Degrees

I have argued both that criticism from alternative points of view is required for objectivity and that the subjection of hypotheses and evidential reasoning to critical scrutiny is what limits the intrusion of individual subjective preference into scientific knowledge. Are these not two opposing forms of social interaction, one dialogic and the other monologic? Why does critical scrutiny not simply suppress those alternative points of view required to prevent premature allegiance to one perspective? How does this account of objectivity not collapse upon itself? The answer involves seeing dialogic and monologic as poles of a continuum. The maintenance of dialogue is itself a social process and can be more or less fully realized. Objectivity, therefore, turns out to be a matter of degree. A method of inquiry is objective to the degree that it permits *transformative* criticism. Its objectivity consists not just in the inclusion of intersubjective criticism but in the degree to which both its procedures and its results are responsive to the kinds of criticism described. I've argued that method must, therefore, be understood as a collection of social, rather than individual, processes, so the issue is the extent to which a scientific community maintains critical dialogue. Scientific communities will be objective to the degree that they satisfy four criteria necessary for achieving the transformative dimension of critical discourse: (1) there must be recognized avenues for the criticism of evidence, of methods, and of assumptions and reasoning; (2) there must exist shared standards that critics can invoke; (3) the community as a whole must be responsive to such criticism; (4) intellectual authority must be shared equally among qualified practitioners. Each of these criteria requires at least a brief gloss.

Recognized Avenues for Criticism

The avenues for the presentation of criticism include such standard and public forums as journals, conferences, and so forth. Peer review is often pointed to as the standard avenue for such criticism, and indeed it is effective in preventing highly idiosyncratic values from shaping knowledge. At the same time its confidentiality and privacy make it the vehicle for the entrenchment of established views. This criterion also means that critical activities should receive equal or nearly equal weight to "original research" in career advancement. Effective criticism that advances understanding should be as valuable as original research that opens up new domains for understanding; pedestrian, routine criticism should be valued comparably to pedestrian and routine "original research."

Shared Standards

In order for criticism to be relevant to a position it must appeal to something accepted by those who hold the position criticized. Similarly, alternative theories must be perceived to have some bearing on the concerns of a scientific community in order to obtain a hearing. This cannot occur at the whim of individuals but must be a function of public standards or criteria to which members of the scientific community are or feel themselves bound. These standards can include both substantive principles and epistemic, as well as social, values. Different subcommunities will subscribe to different but overlapping subsets of the standards associated with a given community. Among values the standards can include such elements as empirical adequacy, truth, generation of specifiable interactions with the natural or experienced world, the expansion of existing knowledge frameworks, consistency with accepted theories in other domains, comprehensiveness, reliability as a guide to action, relevance to or satisfaction of particular social needs. Only the first of these constitutes a necessary condition that any research program must meet or aspire to meet, and even this requirement may be temporarily waived and is subject to interpretation.

The list shares some elements with the list Thomas Kuhn presents in his essay "Objectivity, Value Judgment, and Theory Choice,"[28] and like the items in his list they can be weighted differently in different scientific communities and they must be more precisely formulated to be applicable. For example, the requirement that theories have some capability to generate specifiable interactions with the natural or experienced world will be applied differently as the sorts of interactions desired in a community differ. The particular weighting and interpretation assigned these standards will vary in different social and historical contexts as a function of cognitive and social needs. Furthermore, they are not necessarily consistent. . . . The goals of truth or accurate representation and expansion of existing knowledge frameworks exist in some tension with each other.

Standards do not provide a deterministic theory of theory choice. Nevertheless, it is the existence of standards that makes the individual members of a scientific community responsible to something besides themselves. It is the open-ended and nonconsistent nature of these standards that allows for pluralism in the sciences and for the continued presence, however subdued, of minority voices. Implicit or explicit appeals to such standards as I've listed underwrite many of the critical arguments named above.

Community Response

This criterion requires that the beliefs of the scientific community as a whole and over time change in response to the critical discussion taking place within it. This responsiveness is measured by such public phenomena as the content of textbooks, the distribution of grants and awards, the flexibility of dominant world views. Satisfaction of this criterion does not require that individuals whose data and assumptions are criticized recant. Indeed, understanding is enhanced if they can defend their work against criticism.[29] What is required is that community members pay attention to the critical discussion taking place and that the assumptions that govern their group activities remain logically sensitive to it.

Equality of Intellectual Authority

This Habermasian criterion is intended to disqualify a community in which a set of assumptions dominates by virtue of the political power of its adherents.[30] An obvious example is the dominance of Lamarckism in the Soviet Union in the 1930s. While there were some good reasons to try experiments under the aegis of a Lamarckian viewpoint, the suppression of alternative points of view was a matter of politics rather than of logic or critical discussion. The bureaucratization of United States science in the twentieth century tends similarly to privilege certain points of view.[31] The exclusion, whether overt or more subtle, of women and members of certain racial minorities from scientific education and the scientific professions has also constituted a violation of this criterion. While assumptions about race and about sex are not imposed on scientists in the United States in the way assumptions about inheritability of acquired traits were in the Soviet Union, . . . assumptions about sex structure a number of research programs in biology and behavioral sciences. Other scholars have documented the role of racial assumptions in the sciences.[32] The long-standing devaluation of women's voices and those of members of racial minorities means that such assumptions have been protected from critical scrutiny.

The above are criteria for assessing the objectivity of communities. The objectivity of individuals in this scheme consists in their participation in the collective give-and-take of critical discussion and not in some special relation (of detachment, hardheadedness) they may bear to their observations. Thus understood, objectivity is dependent upon the depth and scope of the transformative interrogation that occurs in any given scientific community. This communitywide process ensures (or can ensure) that the hypotheses ultimately accepted as supported by some set of data do not reflect a single individual's idiosyncratic assumptions about the natural world. To say that a theory or hypothesis was accepted on the basis of objective methods does not entitle us to say it is true but rather that it reflects the critically achieved consensus of the scientific community. In the absence of some form of privileged access to transempirical (unobservable) phenomena it's not clear we should hope for anything better.

The weight given to criticism in the formation of knowledge represents a social consensus regarding the appropriate balance between accurate representation and knowledge extension. Several conditions can limit the extent of criticism and hence diminish a scientific community's objectivity without resulting in a completely or intentionally closed society (for example, such as characterized Soviet science under Stalin or some areas of Nazi science).

First of all, if scientific inquiry is to have any effect on a society's ability to take advantage of natural processes for the improvement of the quality of its life, criticism of assumptions cannot go on indefinitely. From a logical point of view, of course, criticism of background assumptions, as of any general claim, can go on ad infinitum. The philosophical discussion of inductive reasoning is an example of such unending (though not useless) debate. The utility of scientific knowledge depends on the possibility of finding frameworks of inquiry that remain stable enough to permit systematic interactions with the natural world. When critical discussion becomes repetitive and fixed at a metalevel, or when criticism of one set of assumptions ceases to have or does not eventually develop a connection to an empirical research program, it loses its relevance to the construction of empirical knowledge. It is the intrinsic incapacity of so-called "creation science" to develop a fruitful research program based on its alleged alternative to evolutionary theory that is responsible for the lack of attention given to it by the contemporary United States scientific community. The appeal by its advocates to pluralistic philosophies of science seems misguided, if not disingenuous.

Secondly, these critical activities, however crucial to knowledge building, are de-emphasized in a context that rewards novelty and originality, whether of hypotheses or of experimental design. The commoditization of scientific knowledge—a result of the interaction of the requirements of career advancement and of the commercial value of data—diminishes the attention paid to the criticism of the acquisition, sorting, and assembling of data. It is a commonplace that in contemporary science papers reporting negative results do not get published.

In the third place, some assumptions are not perceived as such by any members of the community. When, for instance, background assumptions are shared by all members of a community, they acquire an invisibility that renders them unavailable for criticism. They do not become visible until individuals who do not share the community's assumptions can provide alternative explanations of the phenomena without those assumptions, as, for example, Einstein could provide an alternative explanation of the Michelson-Morley interferometer experiment. Until such alternatives are available, community assumptions are transparent to their adherents. In addition, the substantive principles determining standards of rationality within a research program or tradition are for the most part immune to criticism by means of those standards.

From all this it follows again that 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, that is, that it will result in descriptions and explanations of natural processes that are more reliable in the sense of less characterized by idiosyncratic subjective preferences of community members than would otherwise be the case. The smaller the number, the less likely this will be.[33] Because points of view cannot simply be allowed expression but must have an impact on what is ultimately thought to be the case, such diversity is a necessary but not a sufficient condition for objectivity. Finally, these conditions reinforce the point that objectivity is a matter of degree. While the conditions for objectivity are at best imperfectly realized, they are the basis of an ideal by reference to which particular scientific communities can be evaluated. Ascertaining in greater detail 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. . . .

▪ | Conclusion

On the positivist analysis of scientific method it is hard to understand how theories purporting to describe a nonobservable underlying reality, or containing descriptive terms whose meaning is independent of that of so-called observational terms, can be supported. On the antiempiricist 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 presented in the positivist view cannot accommodate the facts of scientific change. The incommensurability of theories in 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 an individualist logic of scientific method that fails adequately to reflect the social nature of scientific discourse. Furthermore, the emphasis on theories distorts scientific growth and practice. Scientists rarely engage in the construction or evaluation of comprehensive theories. Their constructive, theoretical activity tends to consist much more in the development of individual or interrelated hypotheses (as laws, generalizations, or explanations) from the complex integration of observation and experiment with background assumptions. Success in expanding the scope of an explanatory idea via such complex integration plays as important a role in its acceptance as the survival of falsifying tests. Accounts of validation in the sciences must take account both of the role of background assumptions in evidential reasoning and of the roles of (sometimes) conflicting goals of inquiry with respect to which hypotheses and theories are assessed. The logic that reflects the structure of this activity will have to abandon some of the simplicity of the positivist account, but what it loses in elegance it will surely regain in application.

The analysis conducted in this chapter means that values can enter into theory-constructive reasoning in two major ways—through an individual's values or through community values. The fact that a bit of science can be analyzed as crucially dependent on contextual values or on value-laden background assumptions does not necessarily mean that someone is attempting to impose his/her wishes on the natural world without regard to what it might really be like. More customarily such analysis should be taken as showing the way in which such contextual features have facilitated the use of given data or observations as evidence for some hypothesis by an individual or by a community. Because community values and assumptions determine whether a given bit of reasoning will pass or survive criticism and thus be acceptable, individual values as such will only rarely be at issue in these analyses. When an individual researcher's values enable her or him to make inferences at variance with those of the scientific community, this is less evidence of strongly eccentric individualism than of allegiance to some other social (political or religious) community.[34]

The contextualist view produces a framework within which it is possible to respect 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 is possible to articulate a standard of comparison independent of and external to any particular theory or research project. In making intertheoretic comparison possible it offers the basis (an expanded basis) upon which to develop criteria of evaluation. Finally, the social account of objectivity and scientific knowledge to which the contextualist account of evidence leads seems more true to the fact that scientific inquiry is not always as free from subjective preference as we would wish it to be. And even though the resulting picture of objectivity differs from what we are used to, our intuition that scientific inquiry at its best is objective is kept intact by appealing to the spirit of criticism that is its traditional hallmark.[35]

Notes

1. This mythology originates with the founders of modern science—compare Isaac Newton's "Rules of Reasoning in Philosophy" in Newton (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. Compare Broad (1981).

3. Hempel (1966), pp. 3–18, and Popper (1962), pp. 42–59.

4. Laudan (1977) does articulate criteria for what counts as progress. These are not necessarily criteria or standards for truth.

5. Feyerabend (1975).

6. Grene (1985).

7. James Watson's account of the discovery of the molecular structure of DNA, read in conjunction with the story of Rosalind Franklin's contributions to that discovery in Sayre (1975), provides a vivid example of this interdependence. See Watson (1968). Participant accounts of recent developments in one or another science usually offer good illustrations of this point. Weinberg (1977) and Feinberg (1978) account for the mid-1970s 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.

8. See Peters and Ceci (1982, 1985) and the associated commentary. For additional discussion of peer review see Glazer (1988); Coleman (1987); Cole and Cole (1977); Cole, Cole, and Simons (1981).

9. In what I take to be a similar vein, Bruno Latour (1987) claims that in science a statement made by an individual becomes a fact only as a consequence of what others do with the statement. Latour, however, emphasizes the agonistic as opposed to the cooperative dimension of social relations in the sciences.

10. 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 Addelson (1983).

11. One might say that the language game of science presupposes the independent existence of objects of experience. Contemporary arguments about scientific realism can be understood as arguments about (1) the nature of this presupposition and (2) what categories of objects it covers.

12. 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. Locke (1968) discusses the different ways in which privacy is properly and improperly attributed to subjective states (pp. 5–12).

13. 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, by Laudan in a discussion of the problems that give rise to the development of theory, and by 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 Gerd Buchdahl (1970); Larry Laudan (1977); and Kenneth Schaffner (1974).

14. Maddox, Randi, and Stewart (1988) and Benveniste's reply in Benveniste (1988). The chapter "Laboratories" in Latour (1987) can be read as providing a series of examples of evidential criticism (pp. 63–100).

15. Lewontin (1970, 1974).

16. Gould (1980).

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

18. Bernstein (1973), pp. 137–177.

19. Williams (1966), pp. 32–63. A somewhat different account is presented by Hesse (1965), pp. 170–180.

20. Kuhn (1957), pp. 100–133, 185–192.

21. Holton (1978).

22. Jaffe (1960), pp. 95–103.

23. Rowell (1974).

24. See Longino (1987).

25. 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.)

26. 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.

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

28. Kuhn (1977) [pp. 102–18, above].

29. Beatty (1985) makes a similar point.

30. Invocation of this criterion confirms the kinship of this account of objectivity with the account of truth that Jürgen Habermas has developed as part of his theory of communicative competence. . . .

31. See Levins and Lewontin (1985), pp. 197–252, for further discussion of this point.

32. See Gould (1981); Lewontin, Rose, and Kamin (1984); Richardson (1984).

33. 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 (1978).

34. This should not be taken to mean that social inequality and marginalization are necessary for objectivity but rather that differences in perspective are. A scientific community existing in a (utopian at this point) society characterized by thoroughgoing inclusivity and equality might indeed encourage the persistence of divergent points of view to ensure against blindness to its own assumptions.

35. Note added in proof. Three books read since completing the manuscript also draw attention in varying degrees to the social character of cognitive processes in science: Peter Galison, *How Experiments End* (Chicago, IL: University of Chicago Press, 1987); David Hull, *Science as a Process* (Chicago, IL: University of Chicago Press, 1988); and Sharon Traweek, *Beamtimes and Lifetimes: The World of High Energy Physicists* (Cambridge, MA: Harvard University Press, 1988).

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