# Science as Social Knowledge

*Values and Objectivity in Scientific Inquiry*

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CHAPTER FOUR

# 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 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

[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.



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. In light of the problem of theory ladenness discussed in Chapter Three, this kind of objectivity must be qualified. What can be reliable is the relation of measurements one to another within a particular dimension or kind of scale—for example, the relation between what we label as the pressure and temperature of a gas. Here what is reliable is a certain covariance in the measurements obtained by the use of certain instruments. That pressure and temperature are real properties of real entities or that their measurements provide us an unmediated view of the natural world as it is does not follow from their covariance. Thus, scientists' concern for the objectivity of data does not have implications for the philosophical view known as scientific realism and discussed in Chapter Two. 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. As we saw above, 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

[2] It has become a subject of increased concern lately in light of several alleged incidents of data faking. Compare Broad (1981).

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. The extended case study in chapters Six through Eight shows how it can be applied to the analysis of a particular research program. 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.

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



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. As noted earlier, 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]

The shortcomings of these models as accounts of evidence were discussed in the preceding chapters. How can the contextualist analysis of evidence, with its consequent denial of any logically guaranteed in-

[4] Laudan (1977) does articulate criteria for what counts as progress. These are not necessarily criteria or standards for truth.
[5] Feyerabend (1975).

dependence 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

[6] Grene (1985).

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

[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); Goleman (1987); Cole and Cole (1977); Cole, Cole, and Simons (1981).



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 under-

[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).

score 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. As noted in the previous chapter, 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, by contrast, makes them and hence the authority to criticize

[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.



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 experi-

[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).

ments 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 fifteeth 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

[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).



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, modi-

[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.)

fied, 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]

[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.



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 hypoth-

[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.

eses 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, Values 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. As I suggested in Chapter Two, 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 contin-

[28] Kuhn (1977a).

ued 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, as I will demonstrate in the following chapters, 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

[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. This relationship will be further discussed in Chapter Nine.

[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).



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

[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).



is realized, requires both historical and sociological investigation. The examination of sex differences research in chapters Six through Eight will provide a more concrete and extensive development of these ideas.

## 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 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 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]

[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).

CHAPTER FIVE

# Values and Science

The argument so far has established that contextual values, interests, and value-laden assumptions *can* constrain scientific practice in such as way as to affect the results of inquiry and do so without violating constitutive rules of science. That is, the very character of reasoning in science makes it vulnerable to the influence of context. This is not yet to show that contextual values are always or necessarily implicated in scientific reasoning, or even that they must be implicated in cases of conflicting interpretations of the same experimental or observational data. Background assumptions, it is clear from the previous chapters, may be held for and defended on analytical and metaphysical grounds as well as inadvertently or on normative considerations. Once contextual considerations of any sort are admitted as relevant to scientific argumentation, however, values and interests can no longer be excluded a priori as irrelevant or as signs of bad science. The argument, therefore, does establish the legitimacy of examining research and research projects that are perfectly "good science" for the influence of value-laden considerations. This activity is, of course, immensely important in assessing claims that some scientific research projects can or should displace arguments in terms of values about a subject with purely "scientific" considerations. I shall examine two such areas of research in chapters Six and Seven. The argument as so far presented also legitimates the deliberate choice of assumptions because of the values they embody or support. What can be justified is somewhat more complicated than this simple formulation suggests, and I shall explain this idea more fully in Chapter Nine. In the present chapter I wish to discuss the variety of possible research and value interactions, or constitutive-contextual interactions, to place in context what I will say about the behavioral neuroendocrinological program.

## Varieties of Science Value Interaction

Scholars have already recognized a limited range of ways in which contextual values affect the practice of both pure and applied science. The first is the channeling effect on inquiry of broad values of its social and cultural context. In this country, for instance, while a certain amount of research, especially biological field research, can be pursued at the inclination of the researcher, much work requires major financial support from sources other than the individual, that is, from corporate or governmental sources. The research, pure or applied, that gets funded, and hence, pursued, is that which is seen to further governmental, societal, and corporate goals, whatever those may be.

According to the Mertonian school of history and sociology of science, even before the establishment of this direct and crass connection between social goals and scientific research, social needs and cultural values (for example, the interests of the seventeenth-century bourgeoisie) had an impact on the kinds of research undertaken.[1] From this perspective the questions thought important to investigate are determined as much by the social/cultural context in which science is done as by problems and puzzles internal to scientific inquiry. Merton, however, also thought that the conduct of research, the actual production of knowledge, was governed by internal norms of universalism, disinterestedness, communality, and "organized skepticism"—moral norms that guaranteed the integrity of the products of scientific practice and its insulation from more intimate influence by contextual factors. Contemporary sociologists of science have been exploring the limits of these norms and, as noted earlier, attending to the roles of social interests and values and of the social organization of scientific disciplines in the production of scientific knowledge.[2] Some of this work is in the Mertonian tradition to the extent that it sees problem areas, rather than specific content, determined by these social factors.

A second type of influence involves the explicit policy decisions about the application of technological developments of scientific knowledge. The debates over the adoption of nuclear energy and, now, over certain aspects of genetic engineering involve both factual and normative disagreements. The perceived conflict and conformity of these technologies with a number of different values has generated explicit conflicts and dissonance between those values and thus between social groups assigning different weights to those values. To a major extent the future of these technologies and of the scientific research associated with them will be determined by resolution of the normative disagreements, that is, by the ascendancy of certain values (for example, public health, popular control of energy sources) over their competitors (for example, centralized governmental or corporate control of energy sources).

[1] Merton (1938).
[2] Mulkay (1977).



A third major type of interaction involves the potential conflict between moral values and specific ways of carrying out research, particularly research with human subjects or research that could endanger the public. As the risks of harming subjects (as in various types of drug research) or violating their rights, such as that to privacy, have become better appreciated, professional associations have developed guidelines for their members. Morally based restrictions on experimentation are not new, as the old prohibition on dissection of human cadavers reminds us, are not always imposed when they should be, as the fate of syphilitic black men in Tuskegee reminds us, and are not always obvious, as the histories of both the Milgram obedience experiments and the NIH guidelines on recombinant DNA research make clear.

Varied as they may be, each of these kinds of interaction between science and values can be analyzed according to an "externality" model. According to this model, while those points of contact between science and the values of the social and cultural context in which it is done may determine the directions of research or of its applications, within the boundaries so determined scientific inquiry itself proceeds according to its own rules. The points of contact with the social and cultural context determine to what areas the rules will be applied. The effect may be a broad one determining what questions will be investigated, for example, astronomy or mechanics, or which practical applications of knowledge will be pursued and which neglected, for example, nuclear technology or conservation technologies. It may be more narrow, determining what paths to the knowledge we want will be followed, which tests and experiments are permissible and which not.

The rules of inquiry, on the other hand, are a function of the constitutive values of science, themselves a function of the goal of science, which in this model is simply assumed to be the development of an accurate understanding of the natural world.[3] While the choice of areas or aspects of the world to be illuminated by application of the rules is a function of social and cultural contextual values, the conclusions, answers, and explanations reached by means of their use and guidance are not. Even those contextual values that do affect science remain external to the real thing, to the doing of science. When they do not, we have a case of bad science. This represents, as I have said, the classical understanding of the relation of knowledge and values, of the value freedom of science.

[3] The existence of multiple and possibly conflicting goals for scientific inquiry must, of course, be disregarded in order to suppose that a mechanical application of such rules would result in a uniquely correct understanding of the natural world.

The contextual account of reasoning and argumentation in science I have offered raises the possibility that some cases are not correctly analyzed according to this "externality" model. It allows us, therefore, not only to extend the list of expected interactions but to see the examples usually cited in a different light.

The extended list of ways in which values apparently contextual with respect to a given research program can shape the knowledge emerging from that program includes at least five distinct types:

1. *Practices*. Contextual values can affect practices that bear on the epistemic integrity of science.
2. *Questions*. Contextual values can determine which questions are asked and which ignored about a given phenomenon.
3. *Data*. Contextual values can affect the description of data, that is, value-laden terms may be employed in the description of experimental or observational data and values may influence the selection of data or of kinds of phenomena to be investigated.
4. *Specific assumptions*. Contextual values can be expressed in or motivate the background assumptions facilitating inferences in specific areas of inquiry.
5. *Global assumptions*. Contextual values can be expressed in or motivate the acceptance of global, frameworklike assumptions that determine the character of research in an entire field.

These are not exclusive categories, for example, 5 and 4 can include 2 and 3. The types of interaction listed *can* but need not occur independently. The detailed discussions of research purporting to establish a biological basis for behavioral and cognitive sex differences contained in the following chapters present examples of the influence of contextual values on data, specific assumptions, and global assumptions. I wish in the remainder of this chapter to describe some examples of the influence of contextual values on practices and data, as well as to illustrate their influence on global assumptions with an example taken from a less controversial area than the biology of behavior.

## Mild Cases

### *Practices (Case 1)*

A moment in the history of the study of human interferon provides the first example. Industrial microbiology has spawned the phenomenon of small firms founded by biochemists, stock in which is owned in part by their founders and in part by large pharmaceutical corporations.



These firms have been developed in order to commercially manufacture and market biological substances produced by the new technologies of recombinant DNA. These corporate arrangements both provide funds for present and future research and ensure that the scientist-entrepreneurs involved in the companies receive a greater share of potential profits than they would as academic scientists only.

In January 1980 interferon was being tested for effectiveness against cancer and as an antiviral agent generally. In that month the microbiological firm Biogen announced in a press conference featuring its director and one of its active researchers that it was the first laboratory to achieve the bacterial production of human interferon.[4] This announcement was followed by a jump in Biogen's stock, a major increase in demand for the substance on the part of cancer victims and their families, and a flurry of corporate-sponsored research as other microbiological and large pharmaceutical firms vied to climb onto the "interferon bandwagon." Unmentioned at the time was a similar experiment in Japan that had been published several months earlier, without the fanfare. Also unmentioned was the fact that Biogen had not succeeded in preventing the death of the bacteria synthesizing the interferon, so that the development of techniques for large-scale production was not yet assured by the experiment. Six months later American studies were published and presented at oncological conferences suggesting that interferon was only marginally more effective and for some cancers even less effective than therapies already in use. Four years later interferon was described as a "miracle cure in search of a disease,"[5] and we now hear sporadically of potential uses for it, none of which seem to bear out the original hope.

This episode involves transgressions against (at least two) folk traditions in science. While these traditions do not have the same status as methodological rules, they concern practices connected with the constitutive ideal of truth as well as with considerations of justice. The first holds that scientists don't or ought not to profit commercially from their scientific activity. The considerations of justice underlying this maxim are that just as no individual scientist deserves sole credit for her or his discoveries since each stands on the "shoulders of gi-

[4] The basic elements of the Biogen interferon story are available in the news section of *Nature* 283 (24 January 1980), 284 (13 March 1980; 17 April 1980), and 285 (1 May 1980); in the "News and Comment" section of *Science* 207 (1 February 1980; 21 March 1980), and 208 (16 May 1980); and in *Science News* 117 (26 January 1980; 15 March 1980; 7 June 1980). An account is also available in Gurin and Pfund (1980). See also Yoxen (1986).

[5] Hillel Panitch, quoted in Benowitz (1984), p. 231.

ants," so none should profit exclusively from discoveries made possible by the work of others. The epistemological justification is that scientists ought not to have a stake in the outcome of their research because, scientists being human, such a stake might bias their interpretations of results in directions that favor their interests at the expense of the facts. The traditional ban on profit taking is violated by the ownership by scientists of commercial firms formed precisely to profit from the new advances in the biological sciences. It can be argued, of course, that since the firms will only make money if their products work, the epistemological, constitutive concern is inapplicable in this instance, making the issue one of justice solely.

The second tradition is not so easily brushed off. This is a rule about the communication of results—that research should first be presented in professional journals or in papers read at conferences. The justification is again twofold. A standard way of publicizing research results provides a way of justly adjudicating priority of discovery. From the epistemological point of view it is better to submit claims to the scrutiny of those capable of evaluating them before presenting them to the general public. Most members of the public do not for the most part have the time or the familiarity with contemporary science to carry out such scrutiny and are also generally unaware of the context that gives results their significance (or lack of it). When results are communicated by means of a press conference, there is no opportunity to study them for their soundness before they are absorbed into and begin affecting the public mind. The dramatic style of presentation required for newsworthiness undermines attempts at critical understanding or evaluation.[6]

The potential of interferon was, it seems, highly overestimated. Had the announcement of its bacterial production been made by normal or traditional procedures (had it not been a commercial undertaking in the first place), it would have reached the public, if at all, along with disclaimers about its therapeutic value. Biogen's stock would not have experienced its dramatic rise in value, and cancer sufferers and their families would have been spared the disappointment of false hope. The communication of results is an activity engaged in by scientists as scientists. The choice of a mode of communication warranted in the ethic of profit making in preference to a mode warranted in the ethic of truth seeking is an instance of the displacement of constitutive by contextual—in this case, commercial—values. Unlike the expected types of case mentioned above, in this instance values of the context are not so much directing research from outside as entering into and affecting the professional practice of scientists.[7]

[6] This is effectively illustrated by comparing the coverage of the press conference in both daily newspapers and scientific journals (see note 4) with the scientific paper describing the achievement. See Nagota et al. (1980).



*Questions (Case 2)*

An example of the second type of interaction is provided by the history of the development of systemic means of birth control. In a study of the role of values in testing toxic substances, endocrinologist Carol Korenbrot argues that in the course of the development of oral contraceptives the selection of risks to be measured was a function of the extrascientific values of those performing or supervising the testing.[8] She takes as her text Gregory Pincus' *The Control of Fertility*, which is an account of the history and biology of oral contraception by one who was deeply involved as a major researcher and developer of the product "Enovid."[9] While it is true that Pincus was supported by a drug company and so may have been influenced by commercial considerations, the one social/cultural theme to which he continually harks is that of the dangers of unchecked population growth and the necessity for its control.[10] Korenbrot suggests that his explicit commitment to the need for an effective method of limiting population growth strongly influenced how he tested Enovid for effects other than its inhibition of ovulation.[11] In spite of the availability of data showing a relationship between estrogens and reproductive tract cancers and between estrogens and blood coagulability, the chapter in Pincus' work entitled "Some Biological Properties of Ovulation Inhibitors in Human Subjects" emphasizes their prophylactic and therapeutic properties and minimizes their hazards.[12] The tests reported on and tables presented are concerned, to a great extent, with conditions that improve or might improve or be prevented with use of oral contraceptives, conditions such as dysmennorhea, endometrial dysplasia, endometritis, and even breast cancer. The data included on conditions that may deteriorate—cervical erosion and thromboembolism—are presented with extensive qualifications and explanations tending to exonerate Enovid as a causal factor.

[7] For additional examples and reflection on the impact of commercial interests in science see Dickson (1984), especially pp. 56–106.

[8] Korenbrot (1979), pp. 11–42.

[9] Pincus (1965).

[10] Ibid., p. viii.

[11] Korenbrot (1979), pp. 17–19.

[12] Pincus (1965), chap. 12, especially pp. 252–259, 263, 281.

Korenbrot claims that Pincus' extrascientific commitments biased him in favor of oral contraceptives. This bias in turn led him to actively seek positive rather than negative effects—additional inducements to use oral contraceptives rather than possible reasons to be wary of them. This approach makes sense when one remembers that those concerned with population growth are primarily concerned with population growth in Third World countries where there are often strong cultural and economic inhibitions against limiting births or against "artificial" birth control.[13] Surely one's case for use is strengthened if users find relief from painful or life-threatening conditions, especially if relief from pregnancy or reducing the number of one's children are not immediately perceived as benefits. If one regards the issue as one of potential opposition to what one perceives to be in the long run beneficial, one will want to make the strongest case possible. Pincus' concerns in this regard and his treatment of the biological effects of internally administered contraceptives make it likely that this attitude is in part responsible for the inadequate testing of oral contraceptives before they were commercially distributed. This instance is, by the way, one of the cases that made apparent the need for more rigorous control of food and drug testing by an independent agency.[14]

### *Reflection*

Each of these cases requires a bit more discussion. The issues raised by the commercialization of industrial microbiology tend to be perceived as primarily moral ones, involving fairness and freedom of inquiry. I have suggested that the practices that raise moral questions also raise epistemological ones. The problem identified above is only one of many ways in which scientific communication is affected by its new commercial context. Trade secrecy, for instance, generates problems similar to those generated by the requirements of public image and identity. The need to establish priority, rights, and, through the patent laws, ownership, is already stifling interchange among biological researchers just as alleged requirements of national defense have imposed secrecy on weapons-related aspects of research in physics, chemistry, and computer science.[15] Such privatization of knowledge cannot help but influence the development of knowledge if only by insulating

[13] Djerassi (1981); Hardin (1968).

[14] As Ruth Doell points out in conversation, it also raises the question of what constitutes adequate testing of a substance that will have borderline effects at certain concentrations of use. The decisive implication of oral contraceptives in thromboembolism required a study involving sixty thousand women.

[15] This is discussed in Longino (1986).



mainstream investigation from discoveries in classified and "privately held" inquiry. I argued in the previous chapter that a central feature of scientific objectivity consists in the availability of the research process and its results to criticism. While the press conference format may protect claims temporarily from disputation or refutation, the witholding of results for patenting purposes prevents that knowledge from being used to enrich, refute, or otherwise alter hypotheses in mainstream research. The dual circumvention of traditional norms and constraints governing the communication of scientific information that is imposed by commercial requirements will surely produce a body of scientific knowledge that is different from what might have been produced under a different set of circumstances.

This concern may be dismissed by observing that the supervenience of constitutive values by values of the commercial context effects only temporary interruptions in the development of scientific knowledge, interruptions whose effects will be corrected over time. Over time, however, not only will public confidence in the institutions of science be eroded but the ability of the scientific community to make the distinctions between the true and the false, the sound and the unsound, the plausible and the implausible, will be undercut. One solution might be the adoption of professional protocols enjoining scientists from making a commercial profit from the results of their work. The overriding of epistemologically sound conventions by nonconstitutive values is simply a function of role conflict: individual scientists taking on roles governed by nonscientific values, for example, the commercial values governing the behavior of entrepreneurs. The particular difficulty in the interferon case is that both roles, the scientific and the entrepreneurial, are focussed upon the same activity—the production of a substance with possible medical, and hence commercial, value. The lure of discovery and the lure of profit dangle together.

Disentangling them in this instance, however, will not address the full dimensions of the problem because commercial values are not the only values putting these kinds of pressures on the profession. We live in a society increasingly dependent on science-based technologies, a society reliant on scientific research for new modes of production and of communication, new materials for consumption, new sources of energy, and regulative guidelines for the use of all of these. As the demands for new resources and ways to develop them become more critical, there will be greater and greater pressure on science for immediate answers regardless of the lack of consensus among scientists.[16] This

[16] This point was made in conversation by Paul Schulman.

impatience will tend to undercut more and more the time-consuming procedures, such as publication in professional journals, necessary to achieve genuine consensus and relative certainty regarding the possibilities and consequences of particular technologies.

The determining factor in the oral contraceptive case discussed by Korenbrot is not the fusion of activities structured by quite different goals that generate incompatible rules but ignorance. Endocrinology was still, in the early 1960s, not well enough developed to provide much guidance regarding the potential somatic effects—harmful or beneficial—of estrogen compounds.[17] It is easy to see how an almost messianic (if also paternalistic and ethnocentric) belief in the necessity of population control would incline one towards testing for their beneficial rather than their harmful effects. Where we do not know enough about a material or phenomenon either to predict its activity or to choose appropriate methods for predicting its activity, there arises the opportunity for the determination of scientific procedures by social and moral concerns that have little to do with the factual adequacy of those procedures. The demand for information about a phenomenon, which originates in the particular context in which research is done, means that choices must be made about what sorts of effects to test for and what sorts of methods will be used in those tests. When ignorance about the phenomenon frees those choices from the constraints imposed by constitutive norms, they are left vulnerable to other contextual pressures such as beliefs in the social utility of population growth and skepticism regarding its value or interest in competing concerns such as health. Constitutive norms and values are not so much displaced, as they were in the interferon case, as replaced, when lack of sufficient initial data makes them inapplicable, by nonconstitutive, contextual considerations.

## Global Assumptions (Case 5)

The third type of science and values interaction that I wish to discuss in this chapter is the interplay between contextual values and those global assumptions that determine the character of research in an entire field. It is harder to establish that any given case instantiates this type because the development and adoption of global frameworks is a very complex phenomenon with many formative influences. Nevertheless it is, I think, possible to tell the outlines of a story and in the telling

[17] There was data for mice, but not for humans, on the connection between estrogens and reproductive tract cancers.



address some of the concerns about the relation of physical and chemical sciences to contextual values.

What is troubling about claims that even these sciences might be value-laden is their instrumental success. Were they subject to the vagaries of shifts in contextual social and political values (think of the early Soviet condemnation of relativity theory) we should not have the pragmatically fruitful theoretical structures we have. This observation is reminiscent of a protoargument that Hilary Putnam gives in *Meaning and the Moral Sciences*, albeit for the referential character and truth of theories, that is, for scientific realism, not for the independence of inquiry from contextual values. "But if these objects [gravitational fields, the metric structure of space-time] don't really exist at all, then it is a *miracle* that a theory which speaks of gravitational action at a distance successfully predicts phenomena; it is a *miracle* that a theory which speaks of curved space-time successfully predicts phenomena."[18] The nuclear plant produces electricity, our rockets get to the moon and send back pictures of Saturn and of Jupiter, the hybrid corn survives the drought, the bacterial plasmid produces interferon. When we rely on the theoretical claims of science to guide our technological interventions in the natural world, they work. What else would explain their working, if not that they refer to and accurately describe real things? This somewhat transcendental argument considered in conjunction with Francis Bacon's remark nearly four centuries ago that "the roads to human power and to human knowledge lie close together and are nearly the same"[19] provides a clue to the interplay of constitutive and contextual values in these successful sciences.

That a theory "works," that it can be used to predict correctly the empirical consequences either of naturally occurring events or of human intervention and manipulation of events in nature is often taken to be a reason for accepting it. But accepting a theory for practical or instrumental purposes and asserting it to be true are quite different acts. "Working" is not an epistemological notion. The scientific realists' arguments that would give it epistemological significance were seen in Chapter Two to fail to establish that the predictive success of a theory requires us to conclude that such a theory refers to and veridically represents real objects. As Bas van Fraassen has put it in his formulation of constructive empiricism, the success of science can be explained by supposing only that science aims for empirical adequacy.[20]

[18] Putnam (1978), p. 19.

[19] Bacon (1960), p. 122.

[20] For van Fraassen's arguments that truth is not required to explain the instrumental success of theories, see Bas van Fraassen (1980), especially chapters 2 and 3.

Acceptance of a theory on this view involves belief not in its truth but in its empirical adequacy. We can use a theory to guide our interactions with the natural world, even be committed to so using the theory, without being committed to belief in its literal truth. "Working" is a practical notion, then, one connected to the practical goals of gaining greater control of our lives and our environment. This goal represents a value belonging to the context in which science is done. It also provides a constitutive goal of (some) science that sanctions the mechanistic analysis of phenomena in such a way as to facilitate our interventions. Such analysis has since functioned as a desideratum guiding reasoning about the physical world. I will develop these ideas through a consideration of scholarly research on the development of early modern science and the mechanistic philosophy of nature that nurtured it.[21]

While we take it for granted, sixteenth- and seventeenth-century philosophers and scientists created and argued for a conception of matter whose real properties were common to all matter, were quantitatively characterized, and were capable, at least in principle, of precise measurement. The mechanistic philosophy had its origin in an analogy between natural phenomena and machines, but during this formative period it developed into a general theory of the motion of material objects. The theory of instruments, tools, and machines was only one of many applications. Historian E. J. Dijksterhuis describes this as the emancipation of mechanics as a science from its origin in the study of machines.[22] Mechanism itself evolved from a philosophy of nature that saw the world as a very large machine to one that saw the world as composed of essentially lifeless, inert matter describable in mathematical terms. All change, according to this view, was to be explained by reference to external forces or impact, and the relevant properties of matter were shape, size, and motion.

It is important to remember that the mechanical conception of nature did not arise from a newly unprejudiced examination of the data. Many historians of science have commented on the congruence of new fundamental needs of European societies in the fifteenth and sixteenth centuries and the mechanistic model of nature that emerged from this period, swamping organicist and hermeticist alternatives. Even those historians of science who resist "externalist" explanations of the scientific revolution describe the competition between empirically indistinguishable mechanistic and antimechanistic explanations of the same

[21] In addition to the historians whose work is mentioned in the text, I have benefited from reading Burtt (1927).

[22] Dijksterhuis (1961), p. 498.



phenomena. Richard Westfall, for instance, relates the different uses of the same experiment by both Boyle and van Helmont. The experiment involved placing a small tree in a pot containing a carefully measured amount of earth, watering the tree regularly, and after measurable growth by the tree, removing it from the pot and weighing the earth. What the experimenter discovered is that there is very little, if any, diminution in the quantity of earth. (Presumably the times of measuring are in the same relation to the prior times of watering.) Van Helmont believed that nothing (whether organic or inorganic) comes to be in nature save "by a getting of the water with childe."[23] For him all matter is water that is constrained into its living form by a "vital" principle. So the fact that the earth's weight is constant is for van Helmont evidence that the increased weight of the tree represents the added water that was converted to wood by the vital principle.[24] Robert Boyle, on the other hand, saw the experiment as confirmation of a quite different hypothesis. For Boyle all bodies are formed of a uniform matter consisting of particles that differ only in shape and motion. Transformation in bodies occurs by the addition and subtraction of matter and not by the action of vital principles. So he took the experiment as showing that water, earth, and wood are all ultimately made of a uniform stuff in such a way that they are all changeable into one another.[25]

Mechanism of course was not always applied to such ambiguous situations and when applied to problems involving not transformation but translocation produced useful results. Mechanism as a philosophy of nature, moreover, was compatible with a variety of theories of mechanics, for example, the otherwise quite different Newtonian and Cartesian theories. As a philosophy of nature mechanism facilitated certain interpretations of observed phenomena (for instance, the tree experiment), and it made certain aspects of phenomena (the mathematically representable so-called primary qualities) more central than others. Once one has decided that these are the properties that matter, then one can go about ascertaining and measuring them and making inferences about their functional dependencies on one another.

Not surprisingly, given the origin of mechanistic thinking, the phenomena most susceptible to such treatment turned out also to be those of concern to craftworkers involved in the projects of the developing economy. Dijksterhuis lists the classes of craftworker whose practices

[23] Westfall (1977), p. 28.

[24] Ibid., pp. 28–29.

[25] Ibid., p. 77.

and needs in the fifteenth and sixteenth centuries provided fertile ground for the development of mechanics. They included artist-engineers and architects who designed and built canals, locks, military fortifications, and weapons, and who developed new tools with which to carry out these endeavors, as well as instrument makers catering to the needs of navigation, clock making, cartography, military technology, and others.

> A good deal of the knowledge and skill displayed by these men was still purely empirical, but the constant handling of matter, which is always refractory, could not fail to stimulate the desire for a causal explanation and to induce efforts to devise a more rational working-method. It thus becomes understandable that the first branch of science in which the revival was to take place was mechanics (at first still in the sense of the science concerned with tools and implements). In this case empirical knowledge did not have to be sought deliberately, but arose naturally from the pursuit of technical trades; the waiting was only for theoretical reflection, which however, was helped by the fact that there is no single department of physics which calls more urgently for mathematical treatment and lends itself more naturally to it than mechanics.[26]

Though he does not reject social explanations in principle, Dijksterhuis rejected a number of specific explanations of the rise of modern science that link it directly to socioeconomic developments of the period. And certainly thinkers like Galileo and Newton were not directly catering to the needs of the emerging capitalist class. But as Carolyn Merchant has demonstrated, the degradation of the environment—for instance, the destruction of the great European forests for marine and other construction—required that new forms of production be developed and new resources made available.[27] These requirements could in turn only be satisfied if a new conceptualization of the relationship between humans and nature were developed to legitimate these new forms of interaction with (or action upon) the natural world. What the social and economic developments associated with the rise of the emerging bourgeoisie did was to provide an environment in which could thrive a science that rationalized and extended the powers of craftspersons and artisans, whose products were necessary to that class. The idea of the world as a machine need not have been derived from or inspired by or otherwise caused by economic factors in order

[26] Dijksterhuis (1961), p. 243.
[27] Merchant (1980).



that we should see these as playing a major role in the eventual triumph of the mechanistic way of thought. The idea of the world as a machine or a mechanically organized collection of machines decomposable into quantitatively describable and manipulable parts makes real the ideal of control articulated by Bacon. Knowledge and power may not be two aspects of the same phenomenon, as he claimed, but mathematical mechanical knowledge and manipulational power are one another's intellectual and manual correlates.

And so it seems perfectly reasonable to suppose that the reason (or a major reason) for the eventual triumph of mechanism is that, unlike the hermetic tradition with which it contended, the overall philosophy of nature that guided research was compatible with the needs of the socioeconomic climate in which it developed. This would explain why mechanical rather than hermetic theories reached a stage of development and elaboration that enabled their adherents to persuasively claim demonstration of their views. In addition to being able to solve real problems, as a philosophy of nature it legitimated certain modes of exploitation of resources—for instance, mining—it put control of phenomena, at least in principle, in human hands rather than in control of independent vital spirits, and it made understanding accessible to all rather than only to those who had special sympathetic abilities.[28]

Mechanism did this by characterizing matter in a particular way: from being the formless and so incomplete substratum of Aristotelianism it came to be all there really is in nature, and its properties—quantitatively determinate shape, weight, and motion—were the only real properties. The inertness of matter, which is a part of this picture, requires that explanations of changes in its state not only appeal solely to external forces or impact but that they be unidirectional, that is, that causal factors be independent of and temporally prior to their effects so that effects would not themselves influence the causal factors. It is this unidirectionality together with the mathematical expression of functional dependencies that makes possible prediction and sometimes control of certain features of the physical world.

This very general picture, of course, is not mechanics itself. As noted above, the science of mechanics was a field of contention between expositors of quite different theories. The general picture, however, did

[28] A number of recent scholars have also argued that it served important political uses. The radical distinction it introduces between the scientist and nature mirrors or is mirrored in the construction of human others: women and a rebellious working class. See Merchant (1980); Keller (1985), chaps. 2 and 3; Thomas (1980); Jacob and Jacob (1980). Legitimation of control is also at issue in the debates highlighted by these scholars. Political control is understood as analogous to technical control.

provide an overall characterization of the natural world, and contending theories can be understood as contesting for the best way to articulate the overall characterization for particular phenomena observed. "Working," that is, achieving pragmatic success, directly vindicates not the philosophy but a particular theory of mechanics. The philosophy of nature, however, is vindicated indirectly as the source or generic model of the successful theory.

## The Object of Inquiry

These reflections on the development of early modern science are intended to provide an illustration of one type of contextual value influence on scientific inquiry, that in which values are expressed in or motivate the adoption of frameworklike assumptions that determine the character of research in an entire field. What I've tried to show is that a very general conception of the sort of phenomenon they were investigating permeated the questions early modern scientists asked of nature, and that those questions were themselves linked to the needs and interests of the socio-economic-cultural context in which they conducted their inquiry. This is not an original idea, but the contextualist account of scientific inquiry can provide an analysis of science and values interaction that denies neither the genuineness and integrity of the science nor its shaping by contextual interests. I've suggested the directions such an analysis might take, primarily (1) highlighting the role of mechanistic assumptions about appropriate elements of description and explanation in mediating reasoning between observations and hypotheses, and (2) an explanation of the persistence and eventual triumph of mechanistic philosophy in terms of its provision of a world picture that satisfied emerging needs and interests of its European social and economic context. That such a story *can* be told of early modern science frees us from supposing that the pragmatic successes of physics and chemistry are proof that the theories involved are *true* in the sense of being accurate representations of an underlying or fundamental reality, or that these models of what science is are achieved through value-free inquiry. I would like now to draw out some general reflections suggested by this story.

Any science (or any methodical inquiry) must characterize its subject matter at the outset in ways that make certain kinds of explanation appropriate and others inappropriate. This characterization occurs in the very framing of questions. Teleological questions, for instance, unlike mechanistic questions, are not appropriately addressed to the behavior of matter conceived as inert. The first difference is the one emphasized in early debates about teleology, namely the difference between causal and purposive accounts of change of state. In addition, the answers to teleological questions, unlike those to mechanistic questions, do not satisfy practical goals, and so teleological explanations have no place in sciences whose goal is the practical one of control of natural processes. A complete articulation of the goals of any scientific inquiry includes a description of the *kind* of explanation or understanding (for example, mechanical) that such inquiry aims to provide. This description, or preliminary characterization of the inquiry's subject matter, can be called the specification, or constitution, of the object of inquiry.[29]

The object of inquiry is never just nature or some discrete part of the natural world but nature under some description, for example, nature as a teleological system or nature as a mechanical system or nature as a complexly interactive system. Certain descriptions make certain kinds of questions meaningful and appropriate that would not be so in the context of another overall characterization. Because the characterization of the object of inquiry depends not on what nature tells us but on what we wish to know about it, that description will link the inquiry to the needs and interests it satisfies. Mechanistic philosophy, which characterized the natural world in a particular way, made certain kinds of questions appropriate and others inappropriate. It also, as we have seen, functioned as the source of assumptions mediating between data and explanatory hypotheses. Both proponents of and audience for a particular inquiry bring such assumptions to bear on the subject of investigation. As long as data are brought to bear on the choice of those hypotheses, they will be evidentially supported albeit via the mediation of background assumptions.

In addition to being the source of such assumptions the conception of the object of inquiry is a stabilizing factor that limits the range of hypotheses that are even candidates for consideration. In Chapter Two I distinguished between two missions of the scientific enterprise: knowledge extension and true representation. In the course of extending a given explanatory framework to more and more phenomena, the conception of the object of inquiry prescribes the character of hypotheses and determines the character of reasoning. As a greater number and variety of data are brought under a field's explanatory umbrella,

[29] This phrase is, of course, Michel Foucault's. Our accounts of the construction of the object differ, however, reflecting our divergent disciplinary orientations. His complex theory of the emergence of objects of inquiry is outlined in Foucault (1982). Donna Haraway has also used this concept in her studies of twentieth-century primatology. See my discussion of Haraway's work in Chapter Nine.

the assumptive character and contestatory history of its formative ideas fades from view. What matters is success in meeting practical objectives and that success moots the metaphysical and conceptual questions previously at issue. Only when philosophical analysis reminds us of the underdetermination of theories by their evidence (if even then) is the possibility of an alternative conceptualization of the objects of knowledge leading to equally supportable alternative hypotheses and theories even entertained. And for scientists these abstract philosophical possibilities are much less compelling than substantive contextual or constitutive reasons to seek alternatives.

The idea of the object of knowledge can help to show how contextual values are transformed into constitutive values. The picture I'm suggesting is the following: in any historical period one can find a variety of research traditions, ways of conceptualizing either the natural world generally or particular corners of it more specifically. These conceptualizations, that is, characterizations of the fundamental properties and relations of the objects studied, are what I am calling the constitution of the object of study. This constitution is a function of the kind of knowledge sought about these objects and hence a matter of *decision, choice, and values as much as of discovery*. It is important to acknowledge that these choices are not often, if ever, perceived as such and thus could as well be described as the unconscious projection of human needs onto nature. And it is community adherence to the values and assumptions dictated by one (contextually fixed) set of goals that provides some measure of protection against the influence of individuals' idiosyncratic values.[30] Paradoxically, such community adherence also protects the daily practice of science from contextual incursions, especially from the direct encoding of socio-economic needs into scientific hypotheses. Thus contextual values are transformed into constitutive values.

The kind of knowledge sought and represented in a specification of the object of inquiry functions as a goal determining constitutive values. It stabilizes inquiry by providing assumptions that highlight certain kinds of observations and experiments and in light of which those data are taken to be evidence for given hypotheses. It also provides constraints on permissible hypotheses, as celestial mechanics is guided by

[30] These play a crucial role in the criticism of theories and hypotheses discussed in Chapter Four. As Evelyn F. Keller suggests, the scientific community's identification with a particular set of values is a selective factor which ensures that, for the most part, only those who also hold those values will attempt to join it. The identification of scientists with values is never total or complete, nor are the values entirely consistent. This makes it possible for factions to form and mavericks to join.



the goal of explaining the mechanisms of celestial motion. But the decision to seek a particular kind of knowledge, for example, the decision to seek proximate causes rather than functions and purposes or vice versa, reflects contextual rather than constitutive or epistemological values. The sciences seek not simply truths but particular sorts of truths.

It is quite possible for incompatible research traditions to coexist. Kuhn, perhaps because of his exclusive focus on the physical sciences, thought that the presence of several paradigms indicated the immaturity of a field. The continued coexistence of research traditions guided by incompatible ideas of the object of inquiry could be as well explained by the complexity of the phenomenon studied. The more complex, the less likely that all its features will be adequately accounted for in any one set of models generated by a given set of interests or needs. This is an idea to which I shall return in the concluding chapters.

I've discussed the way the characterization of the object of inquiry is a pivotal meeting point of contextual and constitutive values with respect to some field of inquiry. Let me complicate things a bit. First, fields can be demarcated in different ways, and their correlated objects of inquiry may differ depending on how the lines are drawn. Are the objects the same for the general field of psychobiology and any of its subfields, for example, the study of aging in sensory nervous systems or the study of biological bases of behavioral sex differences? One needs to examine the structure of argumentation in these fields in order to answer this question. Secondly, much of the work that gives us some understanding of complex processes and events is interdisciplinary. How do interdisciplinary inquiries settle on a common object of inquiry? Finally, not every undertaking identified as scientific is carried out under the aegis of an object of inquiry. Toxicity testing, for instance, seeks to predict the health effects of exposure to various substances. Here we are not seeking to understand a phenomenon but simply to know its effects in a restricted set of circumstances. This requires the application of several disciplines, at least physiology (of the organisms exposed) and chemistry (of the substance in question). The application of these disciplines to such a problem does not seem to require the conceptualization of an object of inquiry. Toxicity testing looks on the surface like a simple correlation problem, solvable by techniques of simple induction. As the discussion of Enovid testing suggested, there was no guiding set of assumptions that could guide research. The absence of an object of inquiry left decisions about what effects to test for open to contextual influences. These examples, therefore, point from another direction to the role that the assumptions involved in the characterization of a domain or object of inquiry play in reducing the vulnerability of an area of inquiry to individuals' subjective preferences. Individual values are held in check not by a methodology but by social values.

CHAPTER SIX

# Research on Sex Differences

THE biological study of sex differences in behavior, temperament, and cognition is particularly amenable to treatment in the analytic terms developed in the earlier chapters of this book. The attempts to show a biological basis for such alleged differences have both provoked angry and hostile reactions from feminist scholars and activists and inspired popularizers of science to wax ever more lyrical about sexual difference and complementarity. I shall consider the cultural dimensions of this work more closely in later chapters, focussing here and in Chapter Seven on the structure of the scientific arguments. To this end I shall review recent work in physical anthropology, physiological psychology, and neuroendocrinology. These areas of research house work on human evolution, the dependence of human gendered behavior on gonadal hormones, and the sexual differentiation of the brain.

All the studies have as part of their purpose the elucidation of human nature and behavior. The light they intend to throw is of quite different kinds. The evolutionary studies are concerned with the description of human descent: what happened—the temporal sequence of changes constituting the evolution of humans from an ancestral species; and how it happened—the mechanisms of evolution. Behavioral neuro-endocrinology and physiological psychology are concerned not to tell a story but to articulate general laws of the hormonal control of (or influence upon) anatomical development, physiology, behavior, and cognition. In the former case, researchers, using principles of the general synthetic theory of evolution, seek a historical reconstruction that can help clarify what is human and what is natural about human nature. In the latter case no history is sought but rather universals about the natural, in the form of causal generalizations, are developed on the basis of contemporary observations made primarily in experimental settings.

Both types of inquiry take place within established programs or directions that address particular kinds of questions and abide by particular conventions concerning how to go about answering these questions. By carefully examining the data permitted as evidence and the hypotheses entertained or supported on the basis of that data, it will be possible to see the role of background assumptions in creating the