

1 Subjectivity and Objectivity: An Objective Inquiry

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A person does not have to read very widely in the contemporary methodological or theoretical literature pertaining to research in the social sciences and related applied areas, such as education, in order to discover that objectivity is dead. When the term happens to be used, it is likely to be set in scare-marks—"objectivity"—to bring out the point that a dodolike entity is being discussed. Or "there is no such thing," authors confidently state, unmindful of the fact that if they are right, then the reader does not have to break into a sweat—because if there is no such thing as objectivity, then the view that there is no such thing is itself not objective. But, then, if this view is the subjective judgment of a particular author, readers are entitled to prefer their own subjective viewpoint—which, of course, might be that objectivity is *not* dead!

A couple of illustrations should suffice to set the stage. The first is from Gunnar Myrdal (1969):

The ethos of social science is the search for "objective" truth. The faith of the student is his conviction that truth is wholesome and that illusions are damaging, especially opportunistic ones. He seeks "realism," a term which in one of its meanings denotes an "objective" view of reality. . . . How can a biased view be avoided? (p. 3)

After an interesting discussion of the deep-seated sources of bias and opportunism in belief, Myrdal suggests that some techniques exist to help achieve at least a degree of objectivity.

A second example comes from Elliot Eisner; unlike Myrdal, he does not try to soften the blow but boldly sets out to face a future in which the demise of objectivity is not mourned:

What I have even more quarrel with is the view that a scientifically acceptable research method is "objective" or value-free, that it harbors no particular point of view. All methods and all forms of representation are partial. (1986, p. 15)

Or, from a different work, "What is meant by objective? Does objective mean that one has discovered reality in its raw, unadulterated form? If so, this conception of objectivity is naive" (1979, p. 214).

It is not intended that the present chapter will develop into a paradoxical discussion of the self-referential puzzles generated by such remarks. But it is the intent, at the outset of the inquiry, to point out the oddity of trying to write an essay for a learned symposium—a paradigm case of an exercise in the marshaling of objective considerations—if, indeed, there is no escape from subjectivity. It would be too quixotic; and it would be better to take the bull by the horns and proceed by using rhetoric (much as is being done now), or special pleading, or appeals to the readers' baser motives.

Believing the task *not* to be quixotic, the present author is inspired to inquire why objectivity has sunk into such disrepute and to investigate whether it deserves the fate that has befallen it. Because the issues concerning objectivity and subjectivity transcend disciplinary and methodological boundaries, the discussion will have to be far-ranging, but it will keep returning to the specific issues raised by qualitative research.

The issues, then, are these: Why is it doubted that qualitative research—or, indeed, any research—can be objective, and are these doubts reasonable? What notion of objectivity is involved here? Is Eisner correct in suggesting that the traditional notion of objectivity is naive? If all views are subjective, are they all on a par, or are some more subjective than others? (And does the notion of degrees of subjectivity make sense?)

One further point remains to be made in this prelude. It is clear that in normal parlance the term *objective* is commendatory, while *subjective* carries negative connotations. After all, it is not a good thing for a judge, a physicist, an anthropologist, or a professor to be subjective. It is even worse to be biased—this latter term being sometimes used to mark the contrast with objectivity.¹ (Such negative evaluations are likely to change, of course, if it turns out the objectivity is dead, and that there is no option but to be subjective.) In what follows, the discussion will attempt to avoid using the terms in a judgmental way—at least until it has been established, objectively, that either term can justifiably be so used.

THE INTELLECTUAL ROOTS OF THE ATTACK ON OBJECTIVITY

The fields of philosophy of science and epistemology have undergone something of a revolution in recent decades. The traditional foundationalist or justificationist approach to epistemology has largely been abandoned in favor of a nonfoundationalist approach; in philosophy of science, the work of Popper, closely followed by that of Kuhn, Hanson, Feyerabend, and Lakatos, has been the center of much debate. Acting under these influences, some individuals have moved in the direction of relativism (although this is not what had been intended by most of the individuals just mentioned). But the very same forces—supplemented by one or two others—have also given rise to the strong attack on objectivity. It will be as well to discuss the major influences in turn.

Nonfoundationalist Epistemology

Traditional epistemologies, whether of rationalist or empiricist persuasion, were foundationalist or justificationist in the sense that they regarded knowledge as being built upon (or justified in terms of) some solid and unchallengeable foundation. It was the presence of this solid foundation that served as the justification for the knowledge claims that were made. Where the traditional schools of epistemology fell out with each other was over the issue of what, precisely, constituted this foundation. Empiricists (such as Locke, Berkeley, and Hume) saw the foundation as being human experience—sense impressions or some such item. Rationalists (like Descartes) claimed it was human reason; the starting place for the construction of knowledge was to be those beliefs that appeared indubitable after scrutiny in the light of reason.

In the twentieth century there has been a steady erosion of foundationalism of both varieties. It is now recognized that there is no absolutely secure starting point for knowledge; nothing is known with such certainty that all possibility of future revision is removed. All knowledge is tentative. Karl Popper (1968) is probably the best-known advocate of this newer perspective, but he is not, by far, a solitary figure. In his words:

The question about the sources of our knowledge . . . has always been asked in the spirit of: "What are the best sources of our knowledge—the most reliable ones, those which will not lead us into error, and those to which we can and must turn, in case of doubt, as the last court of appeal?"

I propose to assume, instead, that no such ideal sources exist—no more than ideal rulers—and that *all* “sources” are liable to lead us into error at times. And I propose to replace, therefore, the question of the sources of our knowledge by the entirely different question: “*How can we hope to detect and eliminate error?*” (p. 25; emphasis in original)

It is important to note that abandonment of the notion that knowledge is built on an unshakable foundation does not mean that the traditional notion of truth has been abandoned. Popper constantly reminds his readers that truth is an essential regulative ideal. He offers this nice image:

The status of truth in the objective sense, as correspondence to the facts, and its role as a regulative principle, may be compared to that of a mountain peak which is permanently, or almost permanently, wrapped in clouds. The climber may not merely have difficulties in getting there—he may not know when he gets there, because he may be unable to distinguish, in the clouds, between the main summit and some subsidiary peak. Yet this does not affect the objective existence of the summit. . . . The very idea of error, or of doubt . . . implies the idea of an objective truth which we may fail to reach. (p. 226)

It makes little sense to search for a summit if you do not believe that a summit exists; and it makes little sense to try to understand some situation if you believe that *any* story about that situation is as good as any other. In this latter case, to inquire is to waste one’s energy—one might as well have just invented any old story. But if some stories are regarded as being better than others, then this belief, upon unpacking, will be found to presuppose the notion of truth as a regulative ideal.

The crucial point for the present discussion is that it does not follow from any of the recent developments in epistemology outlined above that the notion of objectivity has been undermined. This would only follow were objectivity equated with certainty. This is to say that the following argument is a *non sequitur*, at least until some further premise is added to link the antecedent to the consequent: If no knowledge is certain, then there is no possibility for any viewpoint to be objective. It might be objected here that Popper himself referred to the real existence of his cloud-covered mountain top and that he said it might never be possible to know one had reached it—showing that attainment of “objective truth” might not be possible. But it is crucial to note that here he was not discussing objectivity, he was discussing truth. When we abandon foundationalism, we abandon the assurance that we can know when we have reached the truth; but, as Popper’s

story also illustrates, we do not have to abandon the *notion* of truth, and we do not have to abandon the view that some types of inquiries are better than others.

Leaving the notion of truth, and returning to the issue of the objectivity of inquiries, there is good reason to hold that certainty and objectivity should not be linked. For if they were, all human knowledge would thereby become subjective (for no knowledge is certain), and this would have the effect of washing out a vital distinction. Consider two observers of a classroom in which a science teacher has been conducting a lesson on a difficult topic. One observer claims to have noticed that the students did not understand the material, but the only evidence she gives is that “I did not understand the material myself”; the other observer also claims that the students did not learn, but offers by way of evidence the test scores of the students, a videotape of the classroom showing the puzzled demeanor of the students, and interview protocols where a random sample of the students seemed rather confused about the topic. The new epistemology would have us recognize that neither of these two views is absolutely certain, but it is not the consequence of the new epistemology that we would have to judge both views as being equally subjective. For it is evident that one of the observers was greatly influenced by her own personal reactions to the lesson, and this unduly affected how she perceived the classroom; whereas the other observer had taken pains to marshal relevant evidence (even if that evidence was not absolutely incorrigible). In a straightforward and nontroublesome sense, the second observer’s opinion would be regarded by all normal language users as being more objective (even if the opinion later turned out to be wrong).

This example suggests the following hypothesis: “Objective” seems to be a label that we apply to inquiries that meet certain procedural standards, but objectivity does not *guarantee* that the results of inquiries have any certainty. (It implies that the inquiries so labeled are free of gross defects, and this should be of some comfort—just as a consumer prefers to buy an item that has met rigorous inspection standards, although this does not absolutely insure that it will not break down.) The other side of the coin is that a biased, bigoted person who jumps to some subjective conclusion about, say, a political candidate who happens to be of different ethnicity may not always be wrong. His or her biased judgment may turn out to be true. Thus the narrow-minded black Democrat who had no time for Richard Nixon, and who claimed he would be a dishonest president, nevertheless turned out to be right. (Just as a consumer who purchases a shoddy piece of merchandise occasionally “lucks out” and never has any trouble with it.) Or, to use a

less loaded example but one that is historically accurate, in its heyday Newtonian physics was supported by a wealth of objective evidence, that is, evidence that was free from personal contamination and that was, in large part, accepted by an international community whose members had subjected it to critical scrutiny and cross-check. Nevertheless, in our day evidence has accumulated that makes it hard to believe that the Newtonian framework is anything but a reasonably good approximation of the truth (but not as good, for example, as the Einsteinian framework, which itself is probably not absolutely true). Thus those scientists of earlier times who rejected Newton for their own personal (subjective) reasons turned out to have been right in doing so (although, of course, whatever positive views they did hold may well have been defective also).

To put the point pithily, neither subjectivity nor objectivity has an exclusive stranglehold on truth. But why, then, should objectivity be preferred if it is not guaranteed to lead to the truth? The answer is implied in the discussion above: At any one time, the viewpoint that is the most objective is the one that currently is the most warranted or rational—to deny this is to deny that there is any significant difference between the warrants for the views of the two classroom observers in the earlier example. If we give this up, if we hold that a biased or personally loaded viewpoint is as good as a viewpoint supported by carefully gathered evidence, we are undermining the very point of human inquiry. If a shoddy inquiry is to be trusted as much as a careful one, then it is pointless to inquire carefully. The philosopher Ernest Nagel (1979) put it well:

Those attacks on the notion that scientific inquiry can be objective are tantamount to an endorsement of the view that the grounds on which conclusions in the sciences are accepted are at bottom no better than are the grounds on which superstitious beliefs are adopted. Those attacks may therefore . . . justify almost any doctrine, no matter how unwarranted it may be. (p. 85)

In light of these remarks, it would seem that Elliot Eisner (1979) was both right and wrong when he stated that “to hold that our conceptions of reality are true or objective to the extent that they are isomorphic with reality is to embrace a hopeless correspondence theory of truth” (p. 214). He was right to criticize the identification of objectivity as “isomorphic with reality”; however, he was wrong to treat “objective” and “true” as synonyms, and he was wrong to suggest that nonfoundationalism leads to the rejection of the correspondence theory

of truth. It is worth commenting here, to forestall a philosophical misunderstanding, that the correspondence theory of truth is firmly entrenched in contemporary philosophy, and it is supported by weighty—but not by absolutely conclusive—considerations. Eisner runs together two issues that philosophers keep separate, for good reasons: The first is the issue of what account best clarifies the *meaning* of the term “truth,” and it is here that the correspondence theory is alive and well, as Popper’s story of the cloudy mountain illustrates. The second is the issue of what *test* or *criterion* we can rely upon in order to judge if a theory actually is isomorphic with reality. On this second matter, nonfoundationalists would answer that there is no such test or criterion, as once again Popper’s allegory illustrates. Eisner has reasoned back, invalidly, from the negative response to the second, to a negative judgment about the first.²

Hansonism

It is now widely accepted that observation is always theory-laden. Due largely to the work of N. R. Hanson (although Wittgenstein and Popper could claim priority), researchers are aware that when they make observations they cannot argue that these are objective in the sense of being “pure,” free from the influence of background theories or hypotheses or personal hopes and desires.³ Qualitative researcher John Ratcliffe (1983) was reflecting this view when he wrote that “most research methodologists are now aware that *all* data are theory-, method-, and measurement-dependent” (p. 148; emphasis in original). And he continued on to turn this point into a thinly veiled attack on objectivity: “That is, ‘facts’ are determined by the theories and methods that generate their collection; indeed, theories and methods *create* the facts” (p. 148; emphasis in original). If the observer’s prior theoretical commitments do, indeed, determine what he or she sees as being the facts of a situation, then subjectivity would seem to reign supreme.

It is here that the distinction between low-level and high-level observation becomes relevant. The distinction is similar to the one that research psychologists have in mind when they speak of “high-inference” and “low-inference” variables. While observation is never theory-free, it does not follow that many (or most) observations are such that people from a wide variety of quite different theoretical frames will be in total disagreement about the facts of the case. There are many situations where all frameworks are likely to lead to the same results—they overlap, as it were. This is particularly so in cases of low-level observations, such as “there is a patch of red,” or “the object on the left is heavier.” Even people who do not share the same language can agree

on such matters, for the only problem they face is the relatively trivial one of translation. (Thus my Korean students might not understand when I speak of “a patch of red,” but with the help of a bilingual dictionary they can quickly come to comprehend—and to agree with me!) To put it in a nutshell, relatively speaking, low-level observation is high in objectivity, in the sense that the reports of my observations transcend the merely personal or subjective. My observations are open for cross-check, testing, and criticism by other inquirers, and there is nothing in Hanson to suggest that people with beliefs that differ from my own are *bound* to disagree with me about such observations. Contrary to what some radical Hansonists claim, there is no evidence that people with markedly different theoretical frames—for example, Freudians and behaviorists—actually see different things at the basic or low-inference level being discussed here. They might notice—or fail to notice—different things, but when these are brought to their attention they agree about what they have seen. Of course, they might still disagree about the significance of what they have observed, but this is not a point under contention in the present context.

Even Hanson’s (1965) famous claim that the astronomers Tycho Brahe and Johannes Kepler would see different things while watching the dawn is a claim that can be recast to support the point being made here.⁴ Both scientists would agree that the sun was moving higher in the sky relative to the horizon—a point Hanson acknowledges; but of course Brahe would interpret this as the sun’s moving, while Kepler would regard it as a case of the earth’s rotating away from the sun. Their disagreement is spectacular, and Hansonists get good mileage from it, but what gets obscured is the agreement of the two men at the “low-inference” level. Ernest Nagel (1979) has made a similar point, using a different example:

It is simply not true that every theory has its own observation terms, none of which is also an observation term belonging to any other theory. For example, at least some of the terms employed in recording the observations that may be made to test Newton’s corpuscular theory of light (such terms as “prism,” “color,” and “shadow”), underwent no recognizable changes in meaning when they came to be used to describe observations made in testing Fresnel’s wave theory of light. But if this is so, the observation statements used to test a theory are not necessarily biased antecedently in favor of or against a theory; and in consequence, a decision between two competing theories need not express only our “subjective wishes,” but may be made in the light of the available evidence. (p. 93)

If, however, the results of observation are couched in abstract theoretical terms—in “high-inference” terms—then there might well be disagreement or misunderstanding. Consider the following example: Most people, whether Freudians or behaviorists, Republicans or Democrats, Americans or Australians or Koreans, deists or atheists, astrologers or astronomers, would agree upon a visit to a classroom that they saw a teacher working with a particular number of pupils. They also probably would agree with the low-inference observation that at a certain stage in the lesson the teacher asked one pupil a series of questions. They might not all agree, however, with the high-inference observation that at this point the teacher was forcing the pupil to do some high-order cognitive task involving Piagetian abstract reasoning. For all the observers to agree with *this* observation and, more to the point, to be able to discuss, to criticize, and to evaluate warrants, they would all have to share the same theoretical framework as well as speak the same language (and this is what Hanson seemed to have in mind when he wrote of “theory-laden perception”). And it is worth noting, in passing, that even if they all did have the same framework, it is not certain that they would necessarily agree—for some might judge that the Piagetian categorization of the pupil’s task was erroneous. Similarity of framework is, at best, a guarantee of communication, but not of much else.

The moral of the example is this: Just because, on some accounts, the more abstract description is “less objective” in the sense that it is less “pure” and more “contaminated” by theory, it does not follow that there is no hope for observers to enter into mutual and fruitful discussion, criticism, and evaluation. At a lower level of abstraction there might well be full overlap of categories and terminology (and thus the possibility of a higher degree of objectivity), and this more objective, low-inference observation would serve both as a constraint on the nature of the abstract accounts that could be put forward and as a springboard for critical evaluation.

Israel Scheffler (1967) seems to have had something like this in mind when he stated that the fact that none of the statements we assert

can be *guaranteed* to be an absolutely reliable link to reality does not mean that we are free to assert any statements at will, provided only that they cohere. That the statement “There’s a horse” cannot be rendered theoretically certain does not permit me to call anything a horse. (p. 119; emphasis in original)

Scheffler points out that language offers constraints on what is to count as a horse (just as, in the earlier examples, it provided constraints on what is to count as a patch of red and what is to count as a pupil answering a question), and “such constraints generate credibility claims which enter my reckoning critically as I survey my system of beliefs” (p. 119). In short, then, Hanson has pointed to a problem that ought to be in the forefront of the minds of observers; but in pointing out the theory-laden nature of high-inference observations he has not offered grounds for abandoning the notion of objectivity.

There is a further consideration that strengthens this optimistic conclusion. In the earlier discussion the point was made that the term *objective* is used more or less as a seal of approval, marking the fact that an inquiry or conclusion meets certain quality standards. There are poor inquiries, infected with personal biases, and there are more worthy inquiries, where the warrants that are offered are pertinent and have been subjected to critical scrutiny. The same situation exists with respect to observations. There are certain well-documented factors that influence observers and that can make their work less credible. (In social science terminology, they can be spoken of as “threats to the validity” of observational or qualitative work.) For example, it is known that observers are prone to misjudge frequencies of occurrence of events they are watching, unless they use some quantitative scoring; and they are prone to be overinfluenced by positive instances and underinfluenced by negative instances. (For a discussion of the significance of these factors, see Phillips, 1987b.) Thus the conclusions reached by a shoddy observer who has not controlled these factors would be properly judged by the research community as being less objective than the conclusions reached by a more careful person. Once again, objectivity is seen to be a vital notion, and its abandonment would be fatal for the integrity of the research endeavor.

The Myth of “The More the Merrier”

In an influential essay, Michael Scriven (1972) points out that sometimes objectivity is thought about in terms of the number of inquirers or observers—data that only one person has been able to collect are regarded as subjective and dubious, but there is usually a more favorable judgment when a number of people have been involved.⁵ Scriven argues, however, that quality and number of investigators do not always go together. Thus he distinguishes between qualitative objectivity, where the data are of high quality (no matter how many observers or inquirers were involved), and quantitative objectiv-

ity, where more than one person has replicated the findings (which does not guarantee veracity). Scriven writes of the two types of objectivity:

Now it would certainly be delightful if these two senses coincided, so that all reports of personal experience, for example, were less reliable than all reports of events witnessed by a large number of people. But as one thinks of the reliability of reports about felt pain or perceived size, on the one hand, and reports about the achievements of stage magicians and mentalists on the other, one would not find this coincidence impressive. (pp. 95–96)

Scriven’s points are crucial; he has shown that it is untenable to give an account of objectivity solely in terms of group consensus—qualitative objectivity is not reducible to quantitative. Thus the audience consensus that a magician has made a woman levitate freely in the air and the group consensus that the world is flat are objective views in the quantitative sense only, that is, those things are what the groups concerned are agreed upon. But the consensus is *only* that; and the agreement does not mean that the views concerned are correct, or warranted, or that they have been reached in a way that has avoided sources of bias and distortion. And yet the number of observers remains a crucial factor in many influential accounts of objectivity. Fred Kerlinger (1973), for example, in his widely used textbook on behavioral research, refers to an “objective procedure” as “one in which agreement among observers is at a maximum” (p. 491). Kerlinger neglected to point out that what is crucial is *how* the agreement was brought about!

Something more is needed to account for the qualitative sense of “objectivity”—some account has to be given of what makes a viewpoint objective in the sense of having a respectable warrant and being free from bias. Alternatively, one could follow Elliot Eisner’s lead; in effect he denies that there is any such thing as qualitative objectivity, and thus there is *only* group consensus or quantitative objectivity. The problem here—apart from the issue of whether he is right about the null status of qualitative objectivity—is that quantitative objectivity is not worth very much. Indeed, it is not worthy of the label “objectivity” at all; a more appropriate term is simply “consensus.” And the problem, of course, is that consensus about an incorrect or untrustworthy or substandard position is hardly worth writing home about. Eisner’s (1979) view has the same defect as Kerlinger’s: “What so-called objectivity means is that we believe in what we believe and that others share our beliefs as well. This process is called consensual validation” (p. 214).

It is important to realize, along with Scriven, that “consensual” and “validation” are uncomfortable bedfellows. Scriven makes it clear that “validity” is a term that belongs with “qualitative objectivity,” not with “quantitative” or “consensus.” Nevertheless, Eisner’s and Kerlinger’s concern with the role of the community of believers is not entirely misplaced, as will soon be seen.

The missing ingredient, the element that is required to produce objectivity in the qualitative sense, is nothing mysterious—but it has nothing to do with consensus. Gunnar Myrdal, Karl Popper, and Israel Scheffler have put their fingers on it: It is acceptance of the *critical tradition*. A view that is objective is one that has been opened up to scrutiny, to vigorous examination, to challenge. It is a view that has been teased out, analyzed, criticized, debated—in general, it is a view that has been forced to face the demands of reason and of evidence. When this has happened, we have some assurance (though never absolute assurance) that the view does not reflect the whim or bias of some individual or group; it is a view that has respectable warrant. Myrdal (1969) states:

The method of detecting biases is simple although somewhat laborious. When the unstated value premises of research are kept hidden and for the most part vague, the results presented contain logical flaws. When inferences are confronted with premises, there is found to be a *non sequitur* concealed, leaving the reasoning open to invasion by uncontrolled influences. . . . This element of inconclusiveness can be established by critical analysis. (pp. 53-54)

Popper (1976) expresses a similar point in a manner that makes even clearer that a community of inquirers can only hope to be qualitatively objective when conditions allow them to subscribe to—and actually apply in practice—the critical spirit:

What may be described as scientific objectivity is based solely upon a critical tradition which, despite resistance, often makes it possible to criticize a dominant dogma. To put it another way, the objectivity of science is not a matter of the individual scientists but rather the social result of their mutual criticism, of the friendly-hostile division of labour among scientists, of their co-operation and also of their competition. For this reason, it depends, in part, upon a number of social and political circumstances which make criticism possible. (p. 95)

Thus Eisner and Kerlinger need to do two things to strengthen their accounts. In the first place, they have to stress that the community of inquirers must be a critical community, where dissent and reasoned

disputation (and sustained efforts to overthrow even the most favored of viewpoints) are welcomed as being central to the process of inquiry. Second, they must abandon their references to agreement or consensus. A critical community might never reach agreement over, say, two viable alternative views, but if both of these views have been subjected to critical scrutiny, then both would have to be regarded as objective. (Once again, the term *objective* does not mean true.) And even if agreement is reached, it can still happen that the objective view reached within such a community will turn out to be wrong—this is the cross that all of us living in the new nonfoundationalist age have to learn to bear!

Kuhnism

Thomas S. Kuhn popularized the notion that inquirers always work within the context of a paradigm—a framework that determines the concepts that are used and that also contains exemplars, or model inquiries, which direct attention toward some problems as being key and away from other problems or issues regarded (from that perspective) as somewhat trivial. Many scholars have interpreted Kuhn as supporting a relativistic position whereby it does not make sense to ask which one of various competing paradigms is the correct one; since such judgments can only be made from within a paradigm, inquirers are not able to step outside to examine their paradigms etically. In a sense, then, all inquirers are trapped within their own paradigms; they will judge certain things as being true (for them) that other inquirers in other paradigms will judge as being false (for them). To those who have taken such relativism seriously, there has seemed to be little place in the Kuhnian universe for objectivity.

Thus, sometimes when the possibility of achieving objectivity is being questioned, the focus of attention is the framework within which inquiry is being pursued. For example, Freudians use a particular theoretical frame—they are guided by distinctive concepts and hypotheses—and, of course, for a dedicated worker in this psychoanalytic tradition, the possibility of using some quite different framework does not arise as a practicable alternative. The same situation exists, it has been argued, even if the inquirer does not subscribe to some well-known paradigm; for even here, the inquirer must be working with *some* concepts and hypotheses that serve as bedrock for the endeavor. Thus, to repeat Ratcliffe (1983):

Most research methodologists are now aware that *all* data are theory-, method-, and measurement-dependent. That is, “facts” are determined by

the theories and methods that generate their collection; indeed, theories and methods *create* the facts. And theories, in turn, are grounded in and derived from the basic philosophical assumptions their formulators hold regarding the nature of and functional relationship between the individual, society, and science. (p. 148; emphasis in original)

Gunnar Myrdal, Elliot Eisner, and the “anarchist” philosopher of science Paul Feyerabend (1978) are among those making similar points.

It is a somewhat controversial point whether choice of a framework or paradigm can be made objectively; but it is clear that the tide of philosophical debate has been running steadily against Kuhn (and relativism) and hence in favor of the view that it *is* possible to judge as better or worse the considerations that are advanced in support of any particular paradigm. (For a summary of the relevant arguments, see Newton-Smith, 1981; Siegel, 1987.) More to the point, the following is also very clear: *Within* any particular framework inquirers can go about their work with more or less facility. Not all Freudians are equally adept; some are bunglers, some are misogynists or suffer from homophobia, and some may even be anti-Republican or anti-Democrat in orientation, and their work as Freudians might be indelibly stamped by these predilections. So sometimes when objectivity is being discussed, the focus of interest is whether it is possible to escape from bias while working or making judgments inside one’s framework. Myrdal (1969) seems to have had this focus when he wrote:

Biases are thus not confined to the practical and political conclusions drawn from research. They are much more deeply seated than that. They are the unfortunate results of concealed valuations that insinuate themselves into research at all stages, from its planning to its final presentation. As a result of their concealment, they are not properly sorted out and thus can be kept undefined and vague. (p. 52)

The point, of course, is that the two foci—choice between paradigms, and choices and work within a particular paradigm—must not be confused. An argument that establishes that at one of these levels objectivity is impossible to achieve (accepting, for the moment, that such an argument could be mounted) does not address the issue of whether the other type of objectivity lies out of reach. There are, however, grounds for believing that this confusion does exist. Eisner, for example, argues strongly that it is naive to believe in framework objectivity, but his published advice on the methodology of qualitative research does not stress the dangers of bias in judgment within frame-

works, and he does not discuss in any detail the steps that can be taken to avoid it. As was seen earlier, with one broad stroke he does away with objectivity in all its senses, replacing it with consensual validation. (For a further discussion of these issues, see Phillips, 1987b.)

Can objectivity of judgment within a framework or paradigm be achieved? It seems clear that the answer is in the affirmative. Consider a group of qualitative researchers who are working on similar problems, using the same intellectual framework to shape their approaches. What property must their judgments have in order to be regarded as objective? As was shown earlier, it will not suffice for these inquirers merely to *agree* in their judgments. Instead, they would have to show that their own personal biases and valuations had been exposed to critical examination, and the role that these predilections played in their investigations would need to have been rigorously examined. Furthermore, as already mentioned, qualitative research (no less than quantitative research) is subject to a variety of threats to its validity—qualitative researchers are liable to misjudge the frequency rate of certain behaviors that are of interest, they are likely to be unduly influenced by positive instances and not so sensitive to the significance of negative instances, they are likely to be unduly influenced or “anchored” by experiences undergone early in the research, and so on (Sadler, 1982). To achieve objectivity within a paradigm, then, the researcher has to ensure that his or her work is free from these problems, and again the presence of a critical tradition is the best safeguard. When work is sent to blind peer review, when researchers are forced to answer their critics, when researchers are supposed to be acquainted with the methodological and substantive literature (and when others can point out when they are not), and when researchers try honestly to refute their own dearly held beliefs, then bias and the other obvious shortcomings are likely to be eliminated, and the judgment (or judgments) reached by the community of scholars should be objective in the relevant sense.

The Conflation of the Contexts of Discovery and Justification

The philosopher of science Hans Reichenbach (1953) drew what is now a well-known distinction between the context of discovery in science and the context of justification. In recent years some have argued that the distinction between these is blurry at best, and a few seminal writers seem to have ignored the distinction altogether—though with arguably disastrous results.⁶ Nevertheless, for heuristic purposes Reichenbach’s distinction turns out to be a very fruitful one.

The relevant point in the present context is this: processes involved in, and even central to, the *making* of discoveries during the pursuit of a research program may not be involved—and might be counterproductive if allowed to intrude—when the discoveries are *checked* and *tested* and *critically evaluated*. Both Israel Scheffler (1967) and Karl Popper (1976) see this distinction as crucial for understanding objectivity in research. Thus Popper, having in mind the context of discovery, writes that

We cannot rob the scientist of his partisanship without also robbing him of his humanity, and we cannot suppress or destroy his value judgments without destroying him as a human being *and as a scientist*. Our motives and even our purely scientific ideals . . . are deeply anchored in extra-scientific and, in part, in religious valuations. Thus the “objective” or the “value-free” scientist is hardly the ideal scientist. (p. 97; emphasis in original)

Objectivity in research is not, for Popper, a property of the individual researcher: “It is a mistake to assume that the objectivity of a science depends upon the objectivity of the scientist” (p. 96). Objectivity, in this view, is a property of the context of justification; as we have seen in the earlier discussion it is in a sense a social matter, for it depends upon communal acceptance of the critical spirit.

CONCLUSION

Before bringing this discussion to a close, a penultimate point must be made. It may have been noted that, throughout, nothing has been made of the distinction between quantitative and qualitative inquiry. For many authors, of course, the distinction is crucial, and qualitative inquiry can only be objective insofar as it approximates to quantitative inquiry. Fred Kerlinger (1973) seems to be representative of this stance:

Objective methods of observation are those in which anyone following the prescribed rules will assign the same numerals to objects and sets of objects as anyone else. An objective procedure is one in which agreement among observers is at a maximum. In variance terms, observer variance is at a minimum. This means that judgmental variance, the variance due to differences in judges’ assignment of numerals to objects, is zero. (p. 491)

He acknowledges that all methods of observation are inferential but sees procedures that assign numbers as “more objective.”

From the point of view of the new nonfoundationalist epistemology, there is little difference between qualitative and quantitative inquiry. Bad work of either kind is equally to be deplored; and good work of either kind is still—at best—only tentative. But the good work in both cases will be objective, in the sense that it has been opened up to criticism, and the reasons and evidence offered in both cases will have withstood serious scrutiny. The works will have faced potential refutation, and insofar as they have survived, they will be regarded as worthy of further investigation.

Another way of putting this is that in all types of inquiry, insofar as the goal is to reach credible conclusions, there is an underlying epistemological similarity. Even in hermeneutics—a mode of qualitative inquiry that at first sight seems far from the “objective” science of physics—there is appeal to evidence, there is testing and criticism of hypotheses (Follesdal, 1979).

It turns out, then, that what is crucial for the objectivity of any inquiry—whether it is qualitative or quantitative—is the critical spirit in which it has been carried out. And, of course, this suggests that there can be *degrees*; for the pursuit of criticism and refutation obviously can be carried out more or less seriously. “Objectivity” is the label—the “stamp of approval”—that is used for inquiries that are at one end of the continuum; they are inquiries that are prized because of the great care and responsiveness to criticism with which they have been carried out. Inquiries at the other end of the continuum are stamped as “subjective” in that they have not been sufficiently opened to the light of reason and criticism. Most human inquiries are probably located somewhere near the middle, but the aim should be to move in the direction that will earn a full stamp of approval!

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NOTES

1. Myrdal seems to use “bias” in this way throughout his book.
2. A similar confusion bedeviled critics of William James’s work; see Phillips (1984).

3. Hanson's work and its general impact—and the ways in which it has been misinterpreted—is discussed in Phillips (1987a).
4. See particularly the concession Hanson (1965) makes at the bottom of p. 23.
5. Ernest House (1980) discusses Scriven's essay admiringly and in some depth.
6. See, for example, the mischief this causes in some of Piaget's work, as discussed by Phillips (1982).

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