such an account of "scientific method"—it is quite unpositivistic (although it is, one supposes, compatible with Comte’s desire to commandeer scientific method for wider use among those who study human affairs). Furthermore, as philosophers like Dagfinn Follesdal have pointed out, this method is used even in areas such as hermeneutics, which are typically regarded as being far removed from the field of science (Follesdal 1979).

Perhaps it is fitting, by way of conclusion, to recall that among all the defects, the positivists and logical positivists were interested in serious questions, and they gave an interesting (but flawed) series of answers. R. W. Ashby has reminded us that the

logical positivists contributed a great deal toward the understanding of the nature of philosophical questions, and in their approach to philosophy they set an example from which many have still to learn. They brought to philosophy an interest in cooperation. . . . They adopted high standards of rigor. . . . And they tried to formulate methods of inquiry that would lead to commonly accepted results. (Ashby 1964, 508)

QUALITATIVE RESEARCH AND ITS WARRANT

It is generally held that William Topaz McGonagall (d. 1902) was the worst poet ever to have been published in the English language. One commentator has written:

He was so giftedly bad that he backed unwittingly into genius. Combining a minimum feel for the English language with a total lack of self-awareness and nil powers of observation, he became a poet. (Pile 1980, 123)

We can thank our lucky stars that he did not become a naturalistic qualitative researcher—another profession for which he would have been singularly unqualified.

Unlike McGonagall, but like genuine poets, qualitative researchers are supposed to have keen powers of observation, heightened self-awareness and realization of how their own personalities can shape their work, and a sensitive command of the language in which they will report their observations. There is, however, one important respect in which poets and qualitative researchers differ—the works produced by poets may be intended to be enjoyable, insightful, and stimulating, but usually it is not necessary that they be accepted as true. “Half a league, half a league, half a league onward . . . ,” and the rest, is a poetic rendering of the charge of the Light Brigade, but only the innocent (or the Hollywood scriptwriter) would take it to be a factual description of what actually happened on that fateful day in the Crimea. Indeed, in many cases the notion of “truth” does not seem applicable to poetry at all; consider the lines of John Keats: “Thou still unravish’d bride of quietness. Thou fosterchild of Silence and slow Time.” These words are magical—they are evocative and communicate a great
deal; to ask whether they are true or not is to make a serious “category mistake.”

On the other hand, qualitative researchers generally do intend for their findings to be taken as veridical. To say that a description of a classroom, life in an urban gang, or village life in some exotic culture is evocative but is not meant to be true or false is merely another category mistake—it is to identify qualitative research as being poetry or something similar (although, as I argued in chapter 4, even narratives must sometimes be true, especially when they are produced by researchers). Moreover, it is a mistake that is fatal for qualitative research: if a qualitative description or analysis is not true or false (i.e., if in principle these terms are not applicable to it), then the issue of whether that description or analysis is to be believed or acted upon cannot arise—it is not sensible to say that one believes the lines by Keats, just as it is not sensible to say that one believes Mozart’s clarinet concerto and is prepared to base policy or social intervention upon it.

Thus, in order to be believed (or disbelieved), and in order to be (or not to be) the basis for intervention or for policy, it is absolutely necessary to have the property of being true or false; and to have one or other of these properties the statement, finding, theory, or whatever must make some claim about some state of affairs. (This is not to say that we can always, or even often, determine whether the item under consideration is actually true or false.) All of this seems to have been acknowledged by Miles and Huberman, authors of what has become one of the standard volumes on naturalistic qualitative methodology; in an earlier paper they wrote:

The results (of qualitative research, especially the “connoisseurship” approach) are expected to be taken seriously, to be accepted as plausible, even valid, beyond the corps of people using the critical perspective. Otherwise, no one beyond the observer would be illuminated, and no serious claims of connoisseurship could be made that other publics could acknowledge. (Miles and Huberman 1984, 21)

The foregoing argument establishes that truth is necessary for certain societal functions to be carried out; but it is also necessary to point out that people have surprising “hang-ups” about it. Today truth has a similar status to that occupied by the topic of sex in the bygone Victorian era—it is not a topic that refined folk like to discuss, at least in public. (In private, of course, both topics have been the focus of much attention and have been the butt not only of words but of deeds.) In both cases euphemisms have been used, as if the embarrassing topics would vanish if they were not referred to in a direct and forthright manner. Thus, expressions like “X is true” and “X is the truth” are often avoided by qualitative researchers and get replaced by “X is to be believed” or “X can be asserted to”—a harmless enough verbal ploy, because most folk realize that, in general, to believe X is to accept X as being true. The practice only becomes pernicious when some qualitative researchers claim that there is no “truth” but still want their account of X to be believed! (I have confused a number of graduate students who hold this general position by asking them whether or not “it is true that it did not snow at Stanford today” and then by following up and asking if they were claiming this question did not make sense. It is difficult really not to believe in truth; even Rorty, who is often cited in such discussions, does not completely do away with truth, but he insists on the stylistic point that the “t” be printed lower case—he only objects to “Truth.”) It is worth stressing that not all qualitative researchers are guilty of the sin that I am pointing to; and not all qualitative researchers are the butt of criticism in the following discussion. In general, the negative points to be made do not apply to those who work in the well-established anthropological or ethnographic tradition, but rather it is some of the newer modes of qualitative work that are the targets here.

Other euphemisms are common as well; questions about truth are often stated in terms of validity or justification. “Is this conclusion valid?”; “Is it justified?”; “Can this result be trusted?” are questions posed by researchers from different poles of the “newer” qualitative continuum. This word game is somewhat more dangerous, for, if it is not played carefully, it can very easily lead to pernicious results. Before pursuing this discussion, however, some clearing of the terrain needs to take place.

SOME TRUTHS ABOUT TRUTH

1. There is one euphemism that has a great deal to be said in its favor. John Dewey was reluctant to use the term truth, and he decided to replace the term by warranted assertibility. The reasoning is complex (it is to be found scattered through the pages of Logic: The Theory of Inquiry), but it is clear Dewey recognized that when truth claims are made, to be taken seriously they must be supported with appropriate arguments or evidence. It is, indeed, the strength of the warranting argument or evidence that allows a truth to be recognized and labeled as such. This approach, too,
can easily accommodate those cases where what was formerly regarded as
thrust is reidentified as nontruth—what has happened here is that the war-
rant for assertion has been withdrawn; it has been found to be in error. The
great merit of Dewey's language, then, is that it highlights the necessity
to have an adequate warrant—which in his view can come only from "com-
petent inquiries" (Dewey 1966, 8; see also the discussion in Phillips and
Burhules 2000). What should count as the criteria of adequacy and com-
petency is, of course, a sticky question.

2. It is held by many—including some in the qualitative camp who
have been eager to latch onto this, and including also some who have been
infected by postmodernist doubt—that recent developments in philoso-
phy of science have made the notion of truth otiose. (The argument here
is that if philosophers have shown that the notion of truth has to be aban-
donned in the physical sciences, then qualitative researchers should have
no concerns about it at all.) This is a misinterpretation of the contempo-
rary scene. Certainly, there has been a great freeing up with respect to what
counts as evidence for and against the truth of a scientific hypothesis; it
can no longer be held that any single test result can be definitive one way
or the other. The role of theories in influencing observations, the relation
between theory and evidence, the role of auxiliary and ad hoc assump-
tions—all of these have been elucidated in the recent literature (see
Phillips 1987 and also chapter 6 of the present volume). It is now recog-
nized more clearly than ever before that our human judgments about what
is true are fallible and subject to constant revision. And it is recognized
that we cannot even be sure that our constant revisions are bringing us
nearer to the truth; Popper's great attempt to produce a theory of veris-
militude is acknowledged as being a failure, even by his closest admirers.

But nowhere in the mainstream of philosophy (anything can happen,
of course, in the "lunatic fringe") is it held that we are free to believe
whatever we want, that there are no constraints on belief. Even Kuhn, who
has been seen by some as the apostle of rampant relativism, does not be-
lieve in intellectual anarchy. (He sees most investigators in any particular
field of natural science, at most times in history, as being in one paradigm,
but during revolutionary periods they are spread over two. Kuhn certainly
does not see every investigator being in his or her own paradigm. In other
words, he does not do away with truth but sees judgments about what is
true as being made internally to a paradigm.) And, as noted previously,
Richard Rorty, who wants to do away with Truth, does not want to aban-
don truth, nor does he throw out the need for standards or warrants for

belief; toward the end of his influential Philosophy and the Mirror of Nature
he writes of "knowing" as being "a right, by current standards, to believe"
(his acknowledged debt to Dewey is quite evident here), and he goes on
to say that more attention should be given
to the relation between alternative standards of justification, and from
there to the actual change in those standards which make up intel-
lectual history. (Rorty 1979, 389-390)

To say that standards change or evolve is not to say that there are no
standards or that there should not be any!

3. The Kuhnian-inspired notion that there may be rival paradigms,
with their own views of what is true, has led to the development of a more
extreme position—there are multiple realities, so there are multiple sets of
truths, all of which are true at the same time (see the discussion in chap-
eter 6). Several of the newer apologists for qualitative methods of research
have held this; William Filstead, for example, claims that this view is re-
lated to the philosophical position of idealism, and he states:

The qualitative paradigm does not conceive of the world as an external
force, objectively identifiable and independent of man. Rather, there are
multiple realities. (Filstead 1979, 35–36)

A similar statement is to be found in Guba and Lincoln:

Naturalistic inquirers [their name for qualitative researchers] make vir-
tually the opposite assumptions [to positivistic, scientific inquirers]. They
focus upon the multiple realities that, like the layers of an onion, nest
within or complement one another. Each layer provides a different per-
spective of reality, and none can be considered more "true" than any
other. Phenomena do not converge into a single form, a single "truth,"
but diverge into many forms, multiple "truths." (Guba and Lincoln 1982,
57)

On one interpretation, Guba and Lincoln and the others who hold
similar positions are saying something rather trite, and they are mistaken
in thinking that there is a conflict here with what "traditional" or "non-
naturalistic" scientists believe. Of course, a phenomenon can be examined
from different perspectives; a motor accident can be approached in terms
of the physics of the collision, in terms of economics, in terms of the psy-
chological states of the drivers, in medical terms, and so on. Such accounts
may all be true; they are complementary or orthogonal, not conflicting. But it seems as if Filsstead, and Guba and Lincoln, have something else in mind—possibly they envision multiple but conflicting truths that can, nevertheless, all be true. The discussion here can best progress in terms of an analogy: Consider rival religions, which give quite incompatible accounts of the nature of the Deity (one says He or She has property P, and the other holds the opposite). Each religion has its devotees who regard it as true, but it is hard to conceive that all accounts are true at the same time. (Of course, which account is true is not the issue here.) Even a Deity would be hard-pressed to both have, and not have, property P, at the one instant. There is a strong tradition in the philosophy of religion that even a Deity must conform to the laws of logic; it is sobering that according to Filsstead, and Guba and Lincoln, the physical realm outstrips the power of a Deity here (for according to them, it can have opposing properties)! Certainly, they owe their readers further discussion on this extraordinary point. (This issue is pursued further in chapter 6.)

Whatever these various writers mean, they cannot coherently hold that any view that anyone cares to assert must be accepted as being true. They do not want to eradicate the need to put forward warrants for belief (indeed, this and other books by Guba and Lincoln deal with how to produce effective warrants in social program evaluation settings). They seem to realize that not everyone who postulates an alternative reality is right—it is possible for such a person to be paranoid, deluded, or simply in error. So, then, there must be criteria for judging the warrants that are advanced on behalf of claims to have detected new realities. Guba and Lincoln raise this issue in the following terms, using quotation marks around the word "truth" to warn their readers that they are unhappy with it and intend to replace it by "credibility": nevertheless, the concern with warrant is still apparent:

How can one establish confidence in the “truth” of the findings of a particular inquiry for the subjects with which—and the context within which—the inquiry was carried out? (Guba and Lincoln 1982, 103)

This, then, is the moral of the discussion so far: The worry about what will count as a satisfactory qualitative warrant for beliefs or truth claims will not wane. On all but the most exotic (and incomprehensible) views of the nature of truth and knowledge, there arises the issue of why the account of some phenomenon that is given by a qualitative researcher (or, for that matter, any researcher) should be believed.

IS QUALITATIVE WORK MORE SUSPECT THAN QUANTITATIVE OR EXPERIMENTAL?

The points made so far apply to all research. All truth claims, in all areas, need to have warrants; and all truth claims, in all areas except perhaps logic and mathematics, are never absolutely established—they may be strongly supported by warrants, but they never reach the stage where they are immune from revision in the light of the results of further inquiry. So why, then, should qualitative research be singled out for special attention?

A number of methodological problems, while not entirely confined to the province of qualitative research, are especially serious here. They are somewhat interrelated, so the following listing is not exhaustive or precise; the categories could easily be collapsed or expanded:

a. As N. R. Hanson and many others have shown, observation is theory-laden. It is somewhat easier to correct for (or control) the biasing effects of prior knowledge and beliefs when one is observing inanimate nature than it is when observing human or social phenomena. For we ourselves are human, and our beliefs about humankind are strongly held and are bound up with our feelings and our valuations.

b. It is unlikely that an observer will enter into social relationships with any inanimate or subhuman entities that are under study; this is quite likely to occur in the human or social domains. The point, of course, is that in social relationships the behaviors, beliefs, and perceptions of the parties concerned are likely to be affected; people do and say things partly with the likely reactions of the other actors in mind; and emotional bonds start to form. It is hard to know what to make of observations that are made under these conditions, unless the observer has been especially sensitive and has taken careful precautions.

c. An observer does not have to make especial efforts to understand or empathize with inanimate objects, but there are good grounds to believe that if observation of human and social phenomena is to be sensible, then it is often unavoidable that the reasons held by the people being observed must be comprehended (see the discussion in chapter 2). But attainment of this understanding of the reasons and beliefs held by other people often results in some fellow feeling with them—it is difficult to be distant and unconcerned; in short, it is difficult to be objective.

These problems are widely understood by qualitative methodologists; and Miles and Huberman, and, of course, many writers in the ethnographic
tradition, offer positive suggestions. Others take these problems as indications that the study of human or social affairs can never be "scientific" or argue that objectivity is an unattainable—and perhaps even a misplaced—ideal.

d. Insofar as qualitative researchers rely on informal or "intuitive" modes of data processing, they have to face squarely the fact that "whatever its other strengths, the mind is apt to make errors of judgment and inference" (Sadler 1982, 199). For example, human observers are quite prone to be unduly influenced or "anchored" by their first impressions of a situation, they are overinfluenced by positive instances supporting a hypothesis or bias but undervalue negative instances, they incorrectly estimate "base rate" frequencies of behaviors they are studying, they do not allow properly for missing data even when they know it is missing, and so forth. Again, some—but by no means all—qualitative methodologists are sensitive to the problems here, and they take care to minimize the threats to the validity of their studies (while others call such precautions the "illicit remnant of positivism") (see Bryman 1984, esp. 85).

e. There is an especially difficult problem that can arise in some—but not all—qualitative research. It does not arise if the aim of the research is to catalogue the beliefs that are held by the people who are being studied, and it also does not arise if the purpose is entirely descriptive. But it does arise when qualitative research aims to uncover causes—and this is not uncommon, especially in research that hopes to result in advice on how to improve performance (e.g., how to improve effectiveness of teaching or how to combat juvenile delinquency) or in research that is related to evaluation of programs or settings. Causal links are rarely accessible to unaided observation; in most settings there are many interacting factors at work, and to tease out those that are causally responsible for effects is no easy task. Usually, a degree of control will have to be exercised—some factors will have to be held constant, while others are varied. The classic statement of this is in the work of John Stuart Mill:

In every instance which comes under our observation, there are many antecedents and many consequences. If those antecedents could not be severed from one another except in thought or if those consequences never were found apart, it would be impossible for us to distinguish (a posteriori, at least) the real laws, or to assign to any cause its effect, or to any effect its cause. To do so, we must be able to meet with some of the antecedents apart from the rest and observe what follows from them,
or some of the consequences and observe by what they are preceded. We must, in short, follow the Baconian rule of varying the circumstances. (Mill 1950, 210)

The qualitative researcher who seeks causes thus has to become an experimenter (even if the experiments are not true, randomized ones)—a matter that those in the anthropological tradition have long recognized. In short, naked observation is generally a poor device for warranting causal claims or for warranting advice on intervention or on future policy (for such advice itself is dependent upon having causal knowledge of situations). Many of the newer qualitative methodologists have not seriously grappled with the difficult problems here.

By way of summary of this section of the discussion, it seems appropriate to cite the assessment given by Martin Hammersley of the work of the influential Chicago qualitative sociologist Herbert Blumer. Hammersley writes that nowhere is

Blumer clear about the nature of the process of testing that he claims is involved in naturalistic (i.e., his form of qualitative) research. He seems to place faith in the idea that by "going directly to the social world" and examining it we will discover its nature. I think he sees any fixed procedure as a barrier to such discovery because it impairs the flexibility of the researcher. The latter must be free to adapt to, to be moulded by, the world. In my view, though, while exploration, flexibility, and creativity are necessary, the idea that if one adopts a flexible attitude towards the world in one's interactions with it, one will come to discover its nature amounts to a naive form of realism. It underestimates the potential for bias and error. (Hammersley 1989, 189)

Hammersley could have added that observation, or even "flexible" interaction with the world, is not sufficient to sort out the causal chains that operate in the social world. The point, simply, is this: the social world can be given many descriptions, and the issue arises as to why we should accept the description that a particular researcher happens to favor. The fact that the researcher happens to favor it is not a sufficient warrant.

In the light of all these complexities, the issue again arises as to how well the warrants that are suggested in the literature fare—will the warrants favored by qualitative researchers (when, indeed, they explicitly favor a warrant) stand up to scrutiny?
WILL THE SUGGESTED WARRANTS WORK?

Qualitative methodology has won a foothold in many branches of the "pure" and "applied" social sciences; it has, of course, long been a feature of some branches, such as anthropology. But the foothold was not always won easily in the other branches. In sociology, for example, several journals ran symposia in the late 1970s (see Bryman 1984, 76); and there was an attendant amount of name-calling and labeling:

In some cases writers have chosen not to use the quantitative/qualitative distinction and have instead used terms which have been used as synonyms. The terms "positivist" and "empiricist" often denote the same fundamental approach as "quantitative," while "naturalistic" field research, "ethnographic," "interpretivist," and "constructivist" are sometimes used instead of "qualitative." (Bryman 1984, 77)

A strikingly similar debate raged in the field of educational research during the 1980s and early '90s, chiefly in North America; some of the contributors here (including Miles and Huberman, and Guba and Lincoln) have been influential more broadly across the applied social sciences. To make the following discussion manageable, it is this more recent debate that shall be the focus of attention—the points are applicable across the social sciences. (Some of the issues to be discussed here were also touched upon in chapter 8, in the context of Popperian research methodology.)

The literature on qualitative methodology in the educational research domain contains a variety of suggested warrants and a host of ways of conceptualizing warrants—ways that are generally notable for their avoidance of the embarrassing term truth. Some writers admit that there is a problem here—that is, they acknowledge that the warrants that have been suggested are not adequate for the task in hand. In the work of Miles and Huberman referred to earlier, this concern about qualitative methodology has been raised, and they have written that "As we have said often, qualitative analyses can be evocative, illuminating, masterful, and downright wrong" (Miles and Huberman 1985, 230). In the discussion that follows, their own suggestions concerning warrants will be examined, as will the views of the Stanford researcher Elliot Eisner, and Egon Guba and Yvonna Lincoln. (These three sets of authors are considered because between them, they seem to cover the whole spectrum of the newer qualitative methodologies. At one pole, Eisner is a self-declared eclectivist, relativist, and instrumentalist [Eisner 1983, 14], and a connoisseur to boot; Miles and Huberman are at the other pole—they call themselves "right-wing" qualitative researchers, or "soft-nosed positivists" [Miles and Huberman 1985, 23]; and Guba and Lincoln are—perhaps—somewhere in between.)

For want of a better criterion, the discussion will proceed alphabetically.

1. Elliot Eisner. Eisner sees the issue of the truth of qualitatively generated findings in terms of "validity" and "trustworthiness." He asks, "How can we know if educational criticism (his version of qualitative investigation) can be trusted?" (Eisner 1979, 213). He goes on to provide three criteria—structural corroboration, referential adequacy, and multipli-cative replication.

The first of these, structural corroboration, is easily dealt with. Eisner himself admits—after advocating its use—that it is not a reliable yardstick. For structural corroboration is the process in which various parts of the account or description or explanation give each other mutual support; it is a process of "gathering data or information and using it to establish links that eventually create a whole that is supported by the bits of evidence that constitute it" (Eisner 1979, 215). Possession of this type of corroboration, of course, shows that the account is coherent, but coherence is not correlated with truth. As Eisner notes, a swindler's story is coherent and convincing!

Turning to the second criterion: A work (for example, a description of a classroom) has referential adequacy, when it enables us to see features that it refers to but that we may not ourselves have noticed:

When the critic's work is referentially adequate we will be able to find in the object, event, or situation what the cues point to. (Eisner 1979, 216)

The problem here, of course, is that seeing what the critic or qualitative researcher is talking about does not mean that the account is true. Thus, I can read Hitler's description of (among other things) the post-World War I Germany in Mein Kampf, and had I been alive at the time I might—with a little effort—have been able to see the world through his eyes, but this does not mean that his account would have been veridical.

Or, to take a less loaded example, it is possible to look at an autistic child after having studied the Freudian theory about this condition; one can see what the Freudians are talking about. (One can do the same with the behaviorist theory.) The fact that this can be done does not establish the truth of the theory.

An argument drawn from contemporary philosophy of science can be used to bolster this conclusion. For any data set, no matter how large, an
infinite number of theoretical explanations can be given—a phenomenon that has come to be called “the underdetermination of the theory of nature.” So the fact that we may all see the same things does not speak to the truth of any one theoretical account. But it must be stressed again that there are problems for Eisner’s criterion at less lofty levels than the realms of theory—the actual description of the situation that is observed may be challengeable. Just because I can see what the Freudian is referring to, does not mean that I thereby endorse that his or her description is the correct one. (After all, I can also see what the rival behaviorist is referring to.) Another way to put this is that observations do not come with the appropriate descriptive or explanatory language pre-attached by nature; multiple descriptions seem always to be possible.

So Eisner is down to one last criterion, which involves other people having seen the same thing; he calls this “multiplicative replication,” and he himself does not place much weight on it. For consensual validation (which is what the criterion amounts to) is a two-edged sword; all sorts of cults and fads have been “corroborated” in this way, but one would be hard-pressed to say this was a sign of their truth. (On occasion Eisner bravely bites the bullet and suggests that there is no such thing as truth, it is only a matter of what a community believes. This, of course, has the consequence that it is true that the earth is both spherical and flat, because there are communities who believe either thing. On the positive side, it must be acknowledged that this nicely solves the problem with which this chapter began—there is no problem about the truth of qualitative research findings, because each one of them is true, providing that a community can be found that will subscribe to it!)

In case, however, there are some readers who do not find this satisfactory, the discussion will turn to the work of Guba and Lincoln.

2. Guba and Lincoln. These writers argue that the question of “truth value” can be reduced to the question of “credibility” (Guba and Lincoln 1982, 104–105). They suggest various techniques, such as reducing involvement with the human subjects the field-worker is interacting with; they also build upon Eisner’s notion of structural corroboration. However, after a detailed discussion of techniques that are useful here, they make a significant remark:

the techniques discussed above do not themselves establish credibility—at best they simply increase the probability that data and interpretations will be found credible.

What then is their answer?

The determination of credibility can be accomplished only by taking data and interpretations to the sources from which they were drawn and asking directly whether they believe—find plausible—the results. This process of going to the sources—often called “member checks”—is the backbone of satisfying the truth value criterion. (Guba and Lincoln 1982, 110)

It is worth noting that this same procedure is standardly used by ethnographers working within the anthropological tradition.

In one sense this is no advance, indeed it is a retrograde suggestion; but in another sense it is sound. The heart of the matter here is the precise nature of the findings or account the “credibility” of which is being probed. If the account that the qualitative researcher is dealing with is an account of the beliefs held by an individual or by a group of subjects—and this is the central focus in ethnographic work—then the appropriate criterion may well be whether or not these subjects agree that the researcher has indeed accurately recorded their beliefs. But this is not central in most of the work done by qualitative researchers of an Eisnerian or Gubarian stamp. When the account produced by the qualitative researcher is an account of a classroom, or of the effects of some educational or social program, or the like, then it is clear that the endorsement of the participants in the classroom or program in question has little or nothing to do with the truth of the account. A qualitative researcher’s account of an interaction between a therapist and an autistic child might be true or false quite independently of the assent or dissent of the two participants; similarly, an account of a classroom might be true even though the teacher (or the pupils) disagree with it.

This is such a major point that it is worth stating in another way. If the purpose of a piece of qualitative work is emic, that is, if the intent is to give an account of how the participants in a situation see it, then checking the account with the participants (or with a selected “informant”) is a vital step. On the other hand, if the intent is etic, that is, if the purpose is not to describe a situation from a participant’s viewpoint but from, say, an Eisnerian connoisseur’s outside perspective, then getting the imprimatur of the participants is beside the point—their judgments about “credibility” are irrelevant.

Guba and Lincoln are paying the price, here, of misidentifying truth with credibility. Credibility is a scandalously weak and inappropriate
surrogate for truth or veracity—under appropriate circumstances any nonsense at all can be judged as “credible.” It is time, then, to turn to the next set of authors to see if they fare any better.

3. Miles and Huberman. These writers start in a promising way by noting that qualitative analyses can be illuminating, masterful, and evocative, but also wrong (Miles and Huberman 1984, 27 and 230). They also use the expression “truth space.” But then they start to drift off target by identifying the attainment of truth with the possession of certain data-processing methods:

The problem is that there is an insufficient corpus of reliable, valid, or even minimally agreed upon working analysis procedures for qualitative data. (Miles and Huberman 1984, 22; see also 1985, 230)

Of course, a lot depends upon what procedures they have in mind to recommend, and as will be seen shortly they undoubtedly have some important ideas. But in general it must be recognized that there are no procedures that will regularly (or always) yield either sound data or true conclusions. If there were such procedures, then steady progress in human understanding would be guaranteed—indeed, it would probably become a matter of following routines, and eventually knowledge generation could be taken over by computers. The words of philosopher of science Paul Feyerabend are worth quoting in this context:

The idea of a method that contains firm, unchanging, and absolutely binding principles for conducting the business of science gets into considerable difficulty when confronted with the results of historical research. We find, then, that there is not a single rule, however plausible, and however firmly grounded in epistemology, that is not violated at some time or another. It becomes evident that such violations are not isolated events. . . . On the contrary, we see they are necessary for progress. . . . More specifically, the following can be shown: considering any rule, however “fundamental,” there are always circumstances when it is advisable not only to ignore the rule, but to adopt its opposite. (Feyerabend 1970, 21–22)

Feyerabend states that, in fact, there is one rule: “Anything goes.” The point of this is not to strengthen the skepticism (or Feyerabendian “anarchism”) that is already rampant in the modern academic world. The point is merely to issue a caution to those who read Miles and Huberman as saying that the formulation of true belief is simply a matter of finding, and following, certain analytic procedures. They themselves recognize this danger, and they warn against “overpreoccupation with method rather than substance and the development of a crippling, mechanical orthodoxy” (Miles and Huberman 1984, 28).

Miles and Huberman suggest a dozen “verification tactics,” many of which have already been alluded to—they draw liberally on Eisnerian and Gubier ideas. Mostly, their suggested procedures, if followed, would produce consensus among investigators (i.e., multiplicative replication) rather than research findings that are true. This direction in their work is clearly revealed in the preambule to their list of tactics:

How do we know whether a conclusion is unreal or real? By “real” we mean another competent researcher, working independently at the same site, would not come up with wholly contradictory findings. (Miles and Huberman 1984, 27)

It must be stressed that no objection is being made here to having researchers, as far as possible, independently check each other’s work. On the contrary, this is a counsel of wisdom. But the point is that this is a relatively weak guarantee of “reality” or “truth” (as the history of anthropology and of physics bear witness). It may be the best that we can hope for, but it should be recognized for what it is, warts and all.

In fact, Miles and Huberman succeed in doing a little better. One of their twelve tactics is “looking for negative evidence,” and while this is not absolutely foolproof and cannot establish that a finding or conclusion is right, it can help in what Popper has called “error elimination” (see the discussion of falsification in chapter 8). Indeed, this tactic is worthy of elaboration and deserves a much more central place than they have given it—it is buried as number eleven in their list and does not seem to play a role at all in the methodology recommended by Eisner or Guba and Lincoln (Miles and Huberman 1984, 28). Popper, in various places, makes the telling point that any fool can find confirmations for an hypothesis, but what is crucial is whether or not refuting evidence can be found. (Of course, it has to be actively sought.) Again, this does not guarantee truth, but if believability is important—and all the qualitative methodologists considered in this chapter regard it as such—then surviving a serious attempt at refutation provides the strongest basis that probably can be attained for belief. Dewey, too, regarded the testing of hypotheses as a vital
Qualitative researchers need to have a much clearer understanding of their own limitations than McGonagall had.


UNSATISFYING CONCLUSION

The worry about the warrant for conclusions drawn from a qualitative inquiry will not wane, largely because the worry about the warrant for conclusions drawn from any inquiry will not wane. But we should not be fobbed off by purported resolutions to this worry that really do not address the relevant issues. Believability, credibility, consensus, coherence—all these things are no doubt important, and a piece of research would be the better for possessing them; but these things do not guarantee the truth of the research conclusion, indeed, they might not even be indicators of truth. Nevertheless, truth is a regulative ideal; it is much better to strive for it, even though it is akin to the impossible dream of the Man from La Mancha, than it is to strive for something less worthy.

Qualitative research is hard work, and, as indicated, it is work that is not always destined to meet with success. But it may not be as hard as writing poetry. McGonagall immortalized the pain and effort that is involved in the terrible lines he penned in tribute to his physician, Dr. Murison:

He told me at once what was ailing me;
He said I had been writing too much poetry,
And from writing poetry I would have to refrain,
Because I was suffering from inflammation on the brain.
(McGonagall 1980, 45)
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