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**VALIDITY IN QUALITATIVE RESEARCH**

**Why the Worry About Warrant Will Not Wane**

DENIS C. PHILLIPS
Stanford University

*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 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 are going to 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

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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 foster-child 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, or of life in an urban gang, or of 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). 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 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 on 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 is fast becoming a standard volume on the methodology of qualitative inquiry, in an earlier paper in the Educational Researcher 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, like sex, is necessary for certain societal functions to be carried out; but it is also necessary to point out that, again like sex, people have surprising “hangups” about it.

Today truth has a similar status to that occupied by 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 becomes pernicious only when some qualitative researchers claim that there is not “truth,” but still want their account of X to be believed! (It is worth stressing that not all qualitative researchers are guilty of this; and it is not all qualitative researchers who 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 anthropological or ethnographic tradition, but rather it is the newer modes of qualitative work that are the targets here.)

Other euphemisms are common as well; the 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 both by Miles and Huberman, and by Elliot Eisner (to take 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 supportable 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 truth is reidentified as nontruth—what has happened here is that the warrant 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 only come from “competent inquiries” (Dewey, 1966: 8). What should count as the criteria of adequacy and competency is, of course, a sticky question.

(2) It is held by many—including some in the qualitative camp who have been eager to latch on to this—that recent developments in philosophy 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 abandoned in the physical sciences, then qualitative researchers should have no concerns about it at all.) This is a misinterpretation of the contemporary 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 observation, the relation between theory and evidence, the role of auxiliary and ad hoc assumptions—all of these have been elucidated (see the fuller exposition of these matters in Phillips, 1987). It is now recognized 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 verisimilitude 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 believe in intellectual anarchy. (He sees most investigators in a general field, at most times in history, as being in one paradigm, but during revolutionary periods they are spread over two; he 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 Richard Rorty, who wants to do away with Truth (note the capital T), does not want to abandon truth (note the lowercase t) or standards for warrants; 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 changes in those standards which make up intellectual 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. Several of the newer apologists for qualitative methods of research have held this; William Filstead claims that this view is related 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 names for qualitative researchers] make virtually the opposite assumptions [to positivistic, scientific inquirers]. They focus upon the multiple realities that, like the layers of an onion, nest within or compliment one another. Each layer provides a different perspective 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 “nonnaturalistic” 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 psychological 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 Filstead, and Guba and
Lincoln, have something else in mind—possibly they envision multiple but conflicting truths that can, nevertheless, all be true. Perhaps 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 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 Filstead, 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 us further discussion on this extraordinary point.

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, the book by Guba and Lincoln deals with how to produce effective warrants in 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 have to 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 “scare 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: 102]?  

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?

There are a number of methodological problems that, while not entirely confined to the province of qualitative research, are especially serious here. They are somewhat interrelated, so the following listing should not be taken too seriously; 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 one 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 special 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 people being observed must be comprehended. 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 writers in the ethnographic
tradition, offer many positive suggestions. Others take these problems
as indications that the study of human or social affairs can never be
“scientific” (see the discussion in Phillips, 1987), or argue that objectivity
is an unattainable—and perhaps even a misplaced—ideal.
(d) Insofar as qualitative researchers rely on nonformal 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. Elliot
Eisner, on the other hand, seems to gloss over these matters in
elaborating his connoisseurship/criticism approach.
(c) 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 catalog 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 in research that is related to evaluation of programs or settings.
Causes are not always 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 consequents. If those antecedents could not be
severed from one another except in thought or if those consequents 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 consequents 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—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 methodol-
ogists have not seriously grappled with the difficult problems here.
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 SUGGESTED
WARRANTS WORK?

The literature on qualitative research methodology 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 their paper in Educational Researcher
referred to earlier, and in their subsequent book, Miles and Huberman
have raised this concern about qualitative methodology, 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
Elliot Eisner, and Guba and 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); Miles and Huberman are at the other pole—they call themselves
“right wing” qualitative researchers, or “soft-nosed positivists” (Miles
and Huberman, 1984: 23); and Guba and Lincoln are—perhaps—
 somewhere in between.) For want of a better criterion, the discussion
will proceed alphabetically.

(i) 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 multiplicative 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 by 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, according to Eisner, 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 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 under-determination 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).

So Eisner is down to one last criterion, which involves other people having seen the same things; 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 article 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.

(ii) 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 fieldworker 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 is whether or not these subjects agree that the researcher has recorded accurately their beliefs. But this is not central in most of the work done by qualitative researchers of an Eisenian or Gubrian 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 Eisenian 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.

(iii) 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, 1985: 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, Miles and Huberman, 1985: 230).

Of course, a lot depends upon what procedures they have in mind to recommend, and as will be seen shortly it 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 Feyerabedian “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 Eisenian and Gubrian ideas. Mostly their suggested procedures, if followed, would produce consensus among investigators (i.e., multiplicative replication) rather than truth. This direction in their work is clearly revealed in the preambles to their list of tactics:
How do we know whether a conclusion is surreal 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 12 tactics is "looking for negative evidence," and while this not absolutely foolproof, and while it cannot establish that a finding or conclusion is right, it can help in what Popper has called "error elimination." Indeed, this tactic is worthy of elaboration, and deserves a much more central place than they give it—it is buried as number 11 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 a 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 article regard it as such—then the surviving of a serious attack to overthrow provides the strongest basis that probably can ever be attained for belief. Dewey, too, regarded the testing of hypotheses as a vital step in effective inquiry resulting in the warranting of belief, and like Popper he saw that conclusions cannot be proven as true but they can be eliminated as false (he should have said: probably false):

Denial of the consequent grounds, however, denial of the antecedent. When, therefore, operations yield data which contradict a deduced consequence, elimination of one alternative possibility is effected. . . . Elimination of other possibilities progressively reduces the likelihood of fallacious inference [Dewey, 1966: 318-319].

Those readers who do not find Dewey and Popper convincing about the relationship between refutation and growth of knowledge (warranted belief), may find the following statement authoritative—in Proverbs, 12:1, it is written that “Whoso loveth correction loveth knowledge, but he that hateth reproach is brutish.”

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 conclusions, 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. McConagall immortalized the pain and effort that is involved, in the terrible lines he penned in tribute to his physician, Dr. Murison:

He told me 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 [McConagall, 1980: 45].

Qualitative researchers need to have a much clearer understanding of their own limitations than McConagall had.

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Influence on Aims, Conduct, and Outcome

STEPHEN J. THORNTON
University of Delaware

It may seem an oddity that the scientific versus the artistic remains a "perennial issue in qualitative research." After all, there have been several well-known and worthy attempts in recent years to explicate the nature of artistic and scientific approaches (e.g., House, 1979; Eisner, 1981). Yet there remains considerable disagreement, perhaps even confusion, regarding the extent to which the two approaches are similar or different.

My aim in this article is twofold. First, I shall briefly outline some of the conflicting characterizations of the differences among scientific and artistic approaches to qualitative research. Second, I shall provide some illustrations from conceptual and empirical research that demonstrate that these are indeed differences that make a difference. Let me say at the outset, though, that these differences are often more of degree than of kind. While my intention is to provide contrasts, this should not obscure that artistic and scientific approaches to qualitative research have a great deal in common (e.g., an emphasis on meaning in context, and a commitment to "thick description").

There is a wide range of views concerning the differences among scientific and artistic approaches to qualitative research. At one extreme is the view there are no significant differences, or at least no substantive differences. Matthew B. Miles and A. Michael Huberman have argued this view. Discussing one prominent artistic approach, educational connoisseurship and educational criticism, Miles and Huberman write:

A close look at the actual practice of educational criticism and connoisseurship suggests that it is not a question of an "artist's" giving shape to