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The Goodness of Qualitative Research

ALAN PESHKIN

Research that is not theory driven, hypothesis testing, or generalization producing may be dismissed as deficient or worse. This narrow conception does an injustice to the variety of contributions that qualitative research can make. In this article I draw upon studies conducted by means of qualitative research methods in order to demonstrate the breadth of desirable outcomes.

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This article has its origin in the complaints of doctoral students and younger colleagues whose talk boils down to this sad point: Someone in authority had dismissed, undermined, or distorted their research plan because it failed to honor one or all elements of the blessed trinity. Their proposed research, said the authority, was not theory driven, hypothesis testing, or generalization producing. Moved by this too-persistent intellectual insularity to a touch of testiness, I vowed to write a justification; indeed, I thought I'd construct an arsenal of arguments for vulnerable qualitative researchers to draw upon in their joustings with trinitarian detractors.

Thus, at a very early stage in the conception of this article, "Justifying Qualitative Research" was both title and mood of what I planned to write about. The epigraphs for the article with this title would have been "Sometimes the more measurable drives out the more important," words of René Dubos quoted by Bernie Siegel (1986, p. 20), and "Why do we downgrade . . . integrative and qualitative ability, while we exalt analytical and quantitative achievement? Is one better, harder, more important than the other?" (Gould, 1989, p. 100). Not only would I have underscored writing that reveled in the virtues of qualitative research (Komarovsky, 1967, pp. 346-351); I would have sought support in Marshall and Rossman's (1989) chapter "Defending the Value and Logic of Qualitative Research."

I held fast to this title and mood until a student, not one of the embattled type, brought me a single line from Nel Noddings' book *Caring*: "The search for justification often carries us farther and farther from the heart of morality" (1984, p. 105). I read this line to say that in the case of the article I ought to prepare, the "heart of morality" lies in respecting—not defending—the integrity of the qualitative paradigm and that this respect does not derive from taking as one's starting point the issues and premises as defined by nonqualitative proponents. It derives, rather, from taking qualitative inquiry as the starting point and asking, "What is its generative promise?"

In one way or another, the researchers I cite hereafter perceived a need. In response to what they perceived as unknown, known thinly, known uncertainly, or known

wrongly, they could have conducted many different types of studies. To qualitative researchers, what is to be learned does not invariably necessitate a particular study design involving theory, hypotheses, or generalization, though it may. It necessitates a judgment that leads them to decide what research designs they should frame to produce one or more of many imagined and as yet unimagined outcomes, some of which I address in this paper.

The proof of research conducted by whatever means resides in the pudding of its outcomes (see Howe & Eisenhart, 1990, p. 6). In search of puddings, I dug into the large and growing literature of qualitative studies, ranging from well-known works deemed classic or near-classic to papers prepared for AERA presentations by relative newcomers to the profession. These studies were conducted by interview, observation, and participant observation, alone or in some combination. I intended to explore the varied types of outcomes that qualitative research has engendered. Deciding whether the outcomes of any particular study should be deemed a worthy example of its type is beyond the scope of this article.

For my purpose, moreover, it is not material to differentiate between the outcomes of a study that its investigators promised before they conducted the research and those that emerged as the pleasant surprises enabled by the open, opportunistic nature of qualitative inquiry. Whatever the case, I have placed the outcomes within the four categories of description, interpretation, verification, and evaluation. In terms of the literature I cite below, I devote relatively more attention to description and interpretation.

The more I attempted to locate a particular outcome within one of these four categories and then within one of several subcategories, the more the locations blurred and blended, so that thinking of the outcomes as discretely classifiable entities became increasingly untenable. For example, in Table 1 below, I note that the objects of description include relationships, systems, and processes. Since these objects could as well be included under the category of evaluation, a degree of arbitrariness characterizes my classification scheme. What I mean at least to do with my categories and subcategories is to provide some structure for the feast of possibilities that may result from qualitative research. I offer the image of the gustatorily promising "feast" (in place of the above-mentioned "arsenal") to suggest an array of

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useful, if not delectable, outcomes, including, of course, those of the trinitarian's. The variety of this array reflects the breadth of our need to know as we try to understand the inevitably complex phenomena of educational research.

Pure categories, in some Platonic sense, may exist. I could not find them among the shadows and reflections of the collective research experience I draw upon. Take description, for example, the first of the four categories I will use. How absurd to think that any description is a complete rendering of its object; description inevitably is selective. When we select for the purpose of describing, we select on some basis. However implicitly, we use the results of our own and others' judgments based on interpreting, verifying, and evaluating activities. Pure, straight description is a chimera; accounts that attempt such a standard are sterile and boring. The point is that placing an outcome of research within the category of "description," or within any other category, is far from a definitive decision.

Before discussing the four categories, I present Table 1, which contains a complete picture of the categories and their respective subcategories. I will not elaborate on the nature of the subcategories. I also will not characterize the many types of outcomes I mention below in terms that distinguish them in their qualitative guise. Much as I value such clarification, it is the work of another article.

Table 1

Types of Outcomes From Qualitative Research: Categories of Analysis and Subcategories of Outcomes

Categories of analysis	Subcategories of outcomes
Description ^a	Processes Relationships Setting and situations Systems People
Interpretation	Explaining and creating generalizations Developing new concepts Elaborating existing concepts Providing insights that: change behavior refine knowledge identify problems Clarifying and understanding complexity Developing theory
Verification	Assumptions Theories Generalizations
Evaluation	Policies Practices Innovations

^aLinguistic parallelism did not seem to work in the construction of this table. For example, the category of description refers to outcomes that describe processes, relationships, and so forth. Similarly, verification refers to outcomes that verify assumptions, theories, and so forth, and evaluation to outcomes that evaluate policies, practices, and so forth. The exception is interpretation, each of whose subcategories include the relevant verb. Thus, the outcomes of interpretation lead to a range of possibilities: the *creation* of generalizations, the *development* of new concepts, and so forth.

Description

When applied to a study, "description" may be a label of condemnation, its dismissive qualifier "mere" taken for granted. When it is present, it accentuates the injury: "The worst of human narrowness pours forth in the negative assessment of monographic work as merely descriptive" (Gould, 1989, p. 100). To the contrary, I want to emphasize the importance of what traditionally is condemned as the lowliest expression of research. When description is so viewed, we fail to appreciate the foundational character of good description for all research, as well as for its often-slighted academic companion—prescription. Mary Haywood Metz advises: "We need to know what the schools do, why they do it, and with what consequences before we prescribe what they should do differently" (1978, p. ix). Clearly, the soundness of the nondescriptive and the prescriptive aspects of research rests essentially on what has been provided by the accuracy, sensitivity, and comprehensiveness of its descriptive foundation.

Within the category of description, I include the subcategories (italicized here and elsewhere in the article) of *processes, relationships, settings and situations, systems, and people*. These subcategories refer to objects of description in a suggestive listing that does not begin to exhaust the possibilities. In reference to *process*, the first of the subcategories, Arthur Vidich and Joseph Bensman (1968) introduced their study of a small town in upstate New York saying: "Our central concern was the processes by which the small town (and indirectly all segments of American society) are continuously and increasingly drawn to the central machinery, processes, and dynamics of mass society" (p. xi). Their pseudonymous Springfield was subject to the forces of mass society, the village's way of life determined largely by the impact of institutions beyond its control.

Similarly, the foreword to Elliot Liebow's (1967) *Tally's Corner* contains this characterization (written by Hylan Lewis) of his study:

One of the important contributions of this study is that it goes far toward factoring out the Negro streetcorner man from other urban Negro types. . . . It goes far toward helping us understand the process by which "losers" like these streetcorner men are generated. (p. xii)

When we understand the processes by which a life or small town or classroom takes on its particular character, we understand something of value.

In *Growing Up American* (1978), I explored the *relationship* between a village high school and its host community. The study uncovered the fact that such schools play an important noneducative role—their communal function—by contributing to the survival of their communities. I refer to *uncovering* the school's communal function in order to indicate that I did not initiate my study with an intention to document this function. Consequently, I cannot refer to a gap in the literature as my basis for emphasizing it. Diana Kelly-Byrne, however, does so when she says about her study of one teacher and one young child that:

To date, little work has been done in our attempts to understand the details of successful relationships with children, although studies like this suggest that it behooves

us to pay more attention to the phenomenon as an important variable in research (1989, p. 232)

(For more discussion on relationships and qualitative research, see Barton & Lazarsfeld, n.d., pp. 341–345).

As objects of description, the subcategories of *settings and situations* seem to collapse in that all situations occur in a setting of particular characteristics, and there is no setting of any interest to social researchers unless there is something we can call a situation. Thus, in Sally Morgan's (1989) biography of Jack McPhee, the *setting* is 20th century Australia, and the *situation* is that which is often created in a restrictive, racist society when a man of mixed aboriginal and White parentage tries to create a life for himself. Morgan's telling of McPhee's life slowly and painfully unravels the implications of the interaction between a particular setting and situation at a given point in time.

The *setting* for S. C. Dube's (1967) community case study is Shamirpet, a village in central India where the *situation*—a juxtaposing of Hindus and Muslims, of low- and high-caste persons—allows him to portray the social dynamics generally prevailing in hundreds of villages throughout the subcontinent. Moreover, Dube identifies a rationale for his study that applies to many qualitative projects: "In India, where ways of life and thought are changing very fast, we require a large number of such community studies. . . . We owe it to posterity that we leave careful records of contemporary life and cultures" (1967, p. 12). In Dube's words we hear a variation of the baseline-case rationale (see also Dillon, O'Brien, & Ruhl, 1989; Wylie, 1974) for organizing descriptive accounts of that which will or could be the subject of later scholarship.

Howard Becker and his colleagues (Becker, Geer, Hughes, & Strauss, 1961) identified as the basis for the descriptive aspect of their study the institution or *system* that was designed to train medical students. On the basis of extensive interviews with students, the authors wrote that they "felt that . . . we would uncover the basic dimensions of the school as a social organization and of the students' progress through it as a social psychological phenomenon" (p. 20).

A final subcategory of description is *people*. Because I assume a general familiarity with biography, I'll cite no specific examples. It is worth noting that a 1990 prize-winning dissertation, one of two recognized by AERA's combined Division D and Qualitative Research SIG, was a biography of an educator.

I think we would benefit from "stories" of the several types of persons who participate in the educational enterprise. I visualize biographies, life histories (see Langness & Frank, 1981), or case studies (see Coles, 1977) addressing, for example, students "at risk"; students who by all rights shouldn't have succeeded, but did; students with gifts; and students with handicaps. And addressing also teachers—varied by gender, age, subject taught, instructional style, location of school—who succeed and fail, with success and failure variably defined. More examples would be redundant. The assumption behind the story of any particular life is that there's something worth learning. "One hopes," says Geoffrey Wolfe:

that one's case will touch others. But how to connect? Not by calculation, I think, not by the assumption that . . . I have discovered a "universal condition of consciousness." One may merely know that no one is alone and hope that

a singular story, as every true story is singular, will in the magic way of some things apply, connect, resonate, touch a magic chord. (1985, p. 72)

Wolfe's poetic "magic way of some things" may reassure qualitative researchers of the broader applicability of their limited-case findings, while verifying for detractors the "softness" of qualitative "science."

Interpretation

I move next to interpretive outcomes. This category contains the nonexhaustive subcategories of good research outcomes that *explain or create generalizations, develop new concepts, elaborate existing concepts, provide insights, clarify complexity, and develop theory*.

Writing at a time when scholars valued qualitative research for its service to quantitative research, sociologist Mirra Komarovsky (1967) concluded, on the basis of her study of 58 marriages, that:

One of the functions of case studies is to suggest *explanatory clues for empirical generalizations* derived by quantitative techniques. We encountered in recent researches correlations that have mystified their discoverers because no rationale could be found for them [italics added]. (p. 349)

Allen Barton and Paul Lazarsfeld (n.d.) provide another illustration. (It is one of several they characterize as a "significant advancement of our understanding of social phenomena," p. 324.) It involves the famous Western Electric experiments with incentives to factory workers; the resulting increased productivity was inexplicable to the researchers until they followed up their experiments with observations. These observations, labeled "exploratory research" by Barton and Lazarsfeld, "turned up a number of surprising qualitative observations . . . in conflict with what the management and the researchers had expected" (pp. 324–325).

When qualitative researchers derive generalizations from their qualitative work, they do not do so convinced that reputable work necessitates generalizations (see Kennedy, 1979, for a contrary view), though they should be well aware of the vexatious nature of making generalizations. On this issue, see, for example, the writing of Robert Donmoyer (1990) and Janet Schofield (1990).

Shirley Brice Heath (1983) says that "three *general points* stand out" from her "ethnographies of communication in Roadville and Trackton" (p. 344). Hylan Lewis thinks Liebow's (1967) study of "Tally's Corner . . . helps us to understand all men who face the fact that they are 'losers' . . . but who are not so lost that they abdicate completely the quest for dignity" (p. xiii). Some qualitative researchers generalize timidly, possibly having cut their methodological teeth on the positivist's biscuit. For example, Deborah Dillon drew general points from her fine study of a single high school teacher and his classroom. Then she stepped back to add that "although this study . . . appears to be limited in generalizability . . ." (1989, p. 256). Dillon has much company (see Merriam, 1988, pp. 173–177) in her reservations about generalizing by means of qualitative research.

Finally, writing generally about generalizations, Gary Wehlage cites Lee J. Cronbach's much-quoted 1975 paper, in which Cronbach seemed to retreat from the acclaimed

psychometric path of his decades-long previous work: "The goal of our work . . . is not to amass generalizations atop which a theoretical tower can some day be erected" (Wehlage, 1981, p. 212). Wehlage paraphrases Cronbach as believing that "the most fundamental goal . . . is to describe as accurately as possible the status of contemporary fact" (p. 212). Still, Wehlage does not use Cronbach as authority to abandon generalizations as an outcome of qualitative research. He joins a school of thought that locates *n* of 1 generalizations in the relationship between text and reader: "The consumer of the research, not the author, does the generalizing It is up to the consumer to decide what aspects of the case apply in new contexts" (1981, p. 216; see also Kennedy, 1979; Stake & Trumbull, 1982; Walker, 1980; Wilson, 1979).

As a result of her interviews with three prominent change agents, Paula Harter (1989) derived three concepts—universal, developmental, and shared—that typify what she calls the "visions" of different types of change agents. Harter's contribution was to develop new concepts that could be used to characterize and to analyze the practices of change agents. This conceptual yield generates frameworks for directing subsequent research.

Interpretation not only engenders new concepts but also elaborates existing ones. For her dissertation research, Barbara Thomas (1988) studied the concept of compliance as it applies to persons with chronic lung diseases. Her literature review had verified that what was known about the compliance of such persons was insufficient to get them to do what physicians deemed necessary for their optimal well-being. Typically, people so afflicted are on a strict program of life-supporting activities. By means of four case studies, Thomas explored the concept of compliance in order to identify aspects of it that were ignored or neglected in previous research.

Andrea Fishman's (1988) book on *Amish Literacy: What and How It Means* highlights insights related to *changing behavior*—one of the three types of insights noted here—as readers see in the words of Glenda Bissex who wrote the book's foreword:

This research was undertaken not to justify particular educational practices or theories but to extend a teacher's understanding. As she reflects on where meaning is found or how it is made, she [Fishman] remarks that she "never knew those were things to worry about." (p. xi)

Bissex concludes her commentary with the observation that "once we see differently, we act differently" (p. xi).

As Bissex introduces Fishman's book, so Erich Lindeman, Harvard Medical School professor of psychiatry, introduces Herbert Gans's (1962) *Urban Villagers*. This is a study of the "forced relocation of families with different types of ethnic and cultural origin." The results, wrote Lindeman, were important beyond Gans's description of the process of relocation:

The book has great significance for the medical profession Its insights concerning their [low-income persons'] attitudes toward health and illness, perceptions of the doctor and medical care . . . will be valuable for the future conduct of programs of medical service. (p. vi)

Another insight, drawing again from Gans's (1962) *Urban Villagers*, can be seen as related to *changing behavior*, *refining knowledge*, or both. Gans explored the impact of a city's urban renewal procedures. By doing so, he learned that a social-class factor operated that was the basis for misperception, misconstrual, and misery. The middle class background of both the town planners and the involved "caregivers" interfered with their grasp of the lives of the lower class people who were the object of the planning and caregiving. Gans's analysis refined our knowledge—and, potentially, the behavior of urban renewal planners and implementors—of the interaction of these otherwise ordinarily noninteracting groups of people.

In the several studies so far identified in the subset of outcomes I call *changing behavior*, none were explicit about making this particular contribution. Louis Smith (Smith & Geoffrey, 1968) and Deborah Dillon (1989) were. Smith expressed the hope that his description of the nature of a classroom would help preservice and practicing teachers (p. vi). Dillon shared the same hope based on her 3-month study of a low-track classroom in rural Georgia. She would like her findings "to enable teachers in similar settings with similar students" to see in her work the "means of examining their own teaching behaviors and student reactions in order to improve instruction" (p. 256).

Problem finding is a type of insight that may result from interpretation. To know what is problematic about a teacher, student, classroom, or school is to have learned something of value. For example, when I studied Riverview High School (1991), I learned about a type of student—poor and minority—who predictably dominated the school's statistics of failure. I also learned about their teachers: They worked hard, cared about students, were not racist, and participated in many different alternative educational programs, all of which removed students from the academic mainstream of the school. I identified the problems of good educators' failing to do good, and of both conventional and alternative academic programs' failing to make a difference.

This class of insights—*problem finding*—is among the richest of all types of outcomes. To be sure, writers do not always identify their contribution as problem finding, but in recognition of the always incomplete nature of social research, research outcomes that lead usefully to subsequent research are invaluable (see Barton & Lazarsfeld, n.d.; Lincoln & Guba, 1988). Barton and Lazarsfeld (n.d.) elaborate this general point in their conceptual article, "Some Functions of Qualitative Analysis in Social Research." Other writers conclude their empirical studies by reference to the point.

For example, in her study of a desegregated middle school that tried to promote a positive relationship between Black and White students, Janet Schofield states:

Since my goals are to heighten awareness of distinctions that need to be made in order to think accurately and effectively about intergroup relations and to draw attention to social processes that are likely to operate in racially mixed schools, this epilogue should be of interest to theorists and researchers as well as educators. (1989, p. 213)

And Smith (Smith & Geoffrey, 1968) anticipates subsequent investigations following his own. He writes that "our many models contain concepts in the form of hypotheses which

need testing" (p. vi). Finally, Sara Lawrence Lightfoot (1983) joins Schofield and Smith. She acknowledges finding "compelling organizational themes worthy of further disciplined study" (p. 25).

Clarifying and understanding complexity, another outcome of interpretation, is important because most of what we study is truly complex, relating to people, events, and situations characterized by more variables than anyone can manage to identify, see in relationship, or operationalize.

Accordingly, Harry Wolcott (1973) does a case study of one elementary school principal, portraying him in the full range of administrative activities over the course of 1 entire school year. I explore (1991) the variability of social relations among Black and non-Black students in a large high school where the meaning of color and ethnicity have been redefined. And Lightfoot (1983) underscores the necessity of

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understanding complexity when she notes that goodness, as in good or effective high schools, the focus of her AERA award-winning book *The Good High School*, cannot be grasped except in context: "Goodness," she says, "is a quality that refers to the complex whole" and it is "situationally determined" (p. 23).

Theory development is a likely consequence of qualitative research, though not yet a common one. Smith (Smith & Geoffrey, 1968) puts his commitment to this end in the subtitle of his case study of an urban classroom: *An Analysis Toward a General Theory of Teaching*. Most commonly, though not exclusively, qualitative researchers turn to the grounded-theory notion of Barney Glaser and Anselm Strauss (1967). By doing so, they are indicating that they will not begin their research with theory; they intend, rather, to generate theory in the course of data collection and analysis. For example, J. J. Blase (n.d.) clarifies that he followed Glaser and Strauss when he developed the data for his paper "The Politics of Favoritism." Blase, Smith, and others who pursue theory as an outcome of their studies are not following a particular strain of qualitative research. The disposition of qualitative researchers to learn as their data collection progresses may incline them toward inquiry that is not driven by theory.

Verification

Verification is my general term for what is perhaps more often referred to as testing the validity of claims. I hesitate to use this latter term because testing, in its customary association with quantitative research, requires particular procedures. These are not the procedures of qualitative research. Nonetheless, qualitative researchers indicate that they engage in verification and seem not to hesitate, as I

believe they should not, to report their findings. Thus Schofield (1989) reports on what she calls the "assumptions" of intergroup relations that her results call into question. She labels three assumptions "erroneous," warning that "those involved with desegregated schools or interested in theory" could well go astray if they "overlook[ed] complexities," as she came to realize them from her case study (p. 214). *Verifying*—or establishing the utility of—*assumptions* is one type of outcome in this category.

Another involves *verifying theories*. Accounting for the academic failure of American minority students has attracted the work of many scholars. Foremost among them is anthropologist John Ogbu (1978). Kitty Lou Smith and Loyd Andrew (1988) observed and interviewed 46 African-American students in Fairfax County, Virginia. Their purpose "was to determine if the values, attitudes, and beliefs characteristic of a caste minority, as defined by Ogbu, are exhibited in an affluent suburban school district" (p. 8). Their results "corroborated in Fairfax" what Ogbu had found in Stockton, California.

Barton and Lazarsfeld (n.d.) devote the last section of their paper on the "functions of qualitative analysis" to "qualitative support of theory." While favoring the application of qualitative research to what they deem the exploratory phases of research, they still find application to theory in the nonquantitative work of Erich Fromm (1941), who studied the self-alienation of normal people, and of C. Wright Mills (1951), who studied the insecure social status of white collar workers.

Qualitative researchers are far more likely to search for generalizations than to verify them, but in this last illustration of a verification outcome we see Gans's (1962) interest in *verifying a generalization*. At the outset of his book on urban renewal he clarifies this intent:

Contemporary city planning and professions . . . use middle class values to help low-income populations solve their problems. . . . As a sociologist and city planner, I wanted to test the validity of this approach. (p. ix)

Evaluation

Evaluation, the last of my four categories, covers a vast territory of possibilities which encompasses much of what I have discussed in the above categories and subcategories, but which for now I will confine to *policies, practices, and innovations*: Have they been implemented? With what impact? What has the process been like? How do they work? For whom do they work? Are there exceptions? There is a large and growing literature on qualitative evaluation, some of the best of it associated with the work and writing of Michael Quinn Patton (1990), Robert Stake (1978), and Egon Guba and Yvonna Lincoln (1989).

Becker et al. (1961) were interested in the impact of existing institutional practices. In their *Boys in White*, they not only explored what a medical school did, its "collective forms of social action," but meant to learn what the results of the school's organizational *practices* were. Their assessment focused on the school's routine, standard curriculum. Other studies focus on *innovative practices*, the result of a change effort that elicits predictable interest in knowing the extent to which an innovation was actually introduced and, thereafter, what impact, if any, it made.

Though "qualitative evaluators" is not the designation

that John Hostettler and Gertrude Huntington (1971) would give themselves, they indicated their intent to *evaluate the policy* decision made by the Amish to establish Amish-controlled schools. In their ethnographic account, they wondered, on the one hand, if these schools measured up to the standards of the public schools Amish children had attended, and, on the other, if they measured up to the hoped-for Old Order Amish standards (p. 105).

Furthermore, Hostettler and Huntington (1971) examined the impact of a policy decision on Amish schools in several states. Despite their focus on an unusual American subculture, they saw *implications for policy* that apply to the teaching of African-American, Indian, and Hispanic children. Specifically, they extended their findings to conventional teacher certification, asserting that when it is construed too narrowly, it constrains the legitimation of persons who may be most qualified to work with the youth of a particular group (p. 109).

I concluded my study of the school-community relationship in rural Mansfield (Peshkin, 1978) with an eye to the policy making of the legislatures of Illinois and other states. Traditionally, our lawmakers find financial savings and academic virtue in consolidated or larger school districts. In light of what I had learned about the communal function of schools, I wanted to influence decision makers so that their policies would not ignore the contribution of schools to the maintenance and viability of villages and towns. I wanted them to consider the point of the political cartoon whose caption read "Close the school, kill the town."

Conclusion

No research paradigm has a monopoly on quality. None can deliver promising outcomes with certainty. None have the grounds for saying "this is it" about their designs, procedures, and anticipated outcomes. At the outset of the discussion section of their paper, Donald Willower and Jonathan Smith (1986) say, "It is well to begin this discussion with the usual caveats" (p. 12), their caveats based on the fact that their fieldwork was confined to two sites. This is unnecessarily apologetic. In the nondefensive mood that befits our times, I endorse the declaration of worthy research outcomes unencumbered by the left hand's removing what the right hand has brought forth. Many types of good results are the fruits of qualitative research. Its generative potential is immense: This is the point of this article. Surely, more felicitous categories and subcategories can and will be constructed. It is the exemplification—not categorization—of types of outcomes that I mean to provide. In doing so, I draw attention to the too-limited conventional focus on a theory-driven, hypothesis-testing, generalization-producing perspective, though not to denigrate but to place it within a broader framework that sees goodness—and therefore warrant for the inquiries of educational researchers—in a multitude of types of outcomes.

Anthropologist Edward Bruner (1990), in strong reaction to views he heard expressed at a recent meeting of the American Anthropological Association, urged his colleagues to avoid the conflict of an either-or polarization: science or humanism, as he heard the issue joined for his colleagues, qualitative or quantitative research, as the issue is joined for many of us. Such controversy, Bruner argued, is "divisive . . . misleading and destructive," because while some

anthropologists "seek generalizations, laws, and causal relationships," others no less rigorously seek "a humanistic understanding of other cultures in all their richness, complexity and ambiguity" (p. 28).

For its avoidance of this controversy, I like Donald Warwick's (1973) view that "every method of data collection is only an approximation to knowledge. Each provides a different and usually valid glimpse of reality, and all are limited when used alone" (p. 190). I see the "different and usually valid glimpse[s] of reality" in the intentions and outcomes of the many studies I have cited. "Reality," a slippery notion at best, does not become clarified by any one person's construction or approach to inquiry.

I also like Claire Selltiz's observation that "social research is a continuing search for truth in which tentative answers lead to a refinement of the questions to which they apply" (Selltiz, Jahoda, Deutsch, & Cook, 1959, p. 23). The qualifiers he used, "continuing search" and "tentative answers," point rightly to the proper approach to bring to our quest for understanding (not for "truth"), so that our conception of useful outcomes is appropriately broad. The approach is further captured in Selltiz's reference to the "refinement" of the questions that direct our research, because refinement conveys the progressive, processual nature of research. We should look to doing ever more wisely what we do: asking better and better questions, appreciating that wisdom has many antecedents and forms, and that the quest for it is endless.

As regards the progressive, processual nature of research, I value Vidich and Bensman's (1968) conclusion that "at best, [the researcher] . . . can feel that he has advanced his problem along an infinite path . . . there is no final accumulation and no final solution" (p. 396). The travels we take down the "infinite path" can only be facilitated by a type of research that gets to the bottom of things, that dwells on complexity, and that brings us very close to the phenomena we seek to illuminate.

In light of the general words of Warwick, Selltiz, Vidich, and Bensman, and the particular words of the many writers cited here, I conclude that there is no prototype qualitative researchers must follow, no mold we must fit in, to ensure that we are bound for the right track.

Note

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