

THE PURPOSE AND CREDIBILITY OF QUALITATIVE RESEARCH

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MANY sociologists have taken the general position that the purpose of qualitative research is to provide a preliminary, exploratory effort to quantitative research since only quantitative research yields rigorously verified findings and hypotheses. The source of this position is that these sociologists appear to take as a guide to being "systematic" the canons of the quantitative analysis on such issues as sampling, coding, reliability, validity, indicators, frequency distributions, conceptual formulation, hypothesis construction, and presentation of evidence. Thus these sociologists overemphasize rigorous testing of hypotheses; and deemphasize the discovering of what concepts and hypotheses are relevant for the substantive area being researched.

We contend that qualitative research—quite apart from its usefulness as a prelude to quantitative research—should be scrutinized for its usefulness as an end product. To view qualitative research as merely preliminary to quantitative research neglects, hence underestimates, several important facts about qualitative analysis. First it is more often than not the end product of research within a substantive area beyond which few, if any, researchers are motivated to move. Second, qualitative research is often the most "adequate" and "efficient" method for obtaining the type of information required and for contending with the difficulties of an empirical research situation. This applies particularly to the hospital setting with its very active developing situations. Third, sociologists (and informed laymen) manage often to profit quite well in their everyday work life from analyses based on qualitative research.

Together these facts raise doubts as to the applicability of the canons of quantitative research as

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criteria for judging the credibility of qualitative research and analysis. They suggest rather that criteria of judgment be based on generic elements of qualitative methods for collecting, analyzing, and presenting data and for the way in which people read qualitative analyses. The setting out of these generic elements is the task of this paper. In so doing, we shall focus on qualitative analysis of observation, interviews, or any type of document as the end product of qualitative research. We shall take up the following pertinent matters: 1) the collection and analysis of data, 2) the maximizing of the analysis' credibility by using comparative groups in the research design, 3) the researcher's trust in believing what he knows he knows, 4) the researcher's conveying to others in publication what he knows so that others may judge his analysis, and 5) the relation of qualitative analysis to its further rigorous testing.

Joint Collection, Coding, and Analysis of Data

Whether the fieldworker starts out in the confused state of noting everything he sees, because everything may be significant, or whether he starts out with a more defined purpose, observation is quickly accompanied by hypothesizing. When hypothesizing begins, the researcher, even if so disposed, can no longer remain a passive receiver of impressions, but is naturally drawn into actively finding data pertinent to generating and verifying his hypotheses. He looks for that data. He places himself in spaces where his data can be seen "live." He participates in events so that things will pass before his eyes, and so that things will happen to himself which will precipitate further hypothesizing. He may even manipulate events to see what will happen. Although he could manage all these investigatory activities without hypotheses, the hypotheses inevitably arise to guide him.

It is characteristic of fieldwork that multiple hypotheses are pursued simultaneously. Of course, certain events will literally force an important or fascinating hypothesis upon the researcher, so that he spends days or weeks checking out that one hypothesis—especially if its verification is linked with developing social events. Meanwhile other hypotheses are being built into his fieldnotes. Eventually the researcher either actively verifies many of his hypotheses or sufficient verifying events are observed by chance. In either case he no longer packs his notes with evidence pertaining to those particular hypotheses, but goes on to collect data on newer, emerging hypotheses.

The earlier hypotheses may seem unrelated at first, but rather quickly become integrated, to form the basis of a central analytic framework. In fact, fieldworkers have remarked upon the rapid crystallization of that framework, and some have wondered whether later fieldwork does not merely elaborate upon that framework. Whatever the answer, it is certain that experienced researchers quickly develop important concepts, basic categories, and significant hypotheses. Beyond guiding the active search for evidence, these integrated hypotheses immediately provide a central core of theorizing which helps the researcher to develop related hypotheses as well as to prune away those not related. In fact, one hazard of fieldwork is that potentially illuminating perspectives are suppressed in favor of a too rapidly emerging analytic framework.

The analytic framework generally appears on paper in two forms. Analytic comments get written directly into the fieldnotes and are written into occasional memos addressed specifically to matters of analysis. If a research team is involved, the researchers write collective as well as individual memos. Characteristically, researchers withdraw periodically from active field pursuits to reflect upon their observations and write analytic memos. Most field situations force such periods upon the researcher because of the natural lulls in social life. But more important, such respites from active fieldwork are taken by some fieldworkers to avoid collecting huge masses of data without adequate systematic reflection on their research directions and purposes, as guided by their emergent analytic framework.

These reflective periods are immensely important for two additional reasons—other than that the researcher needs occasional relief from observational duty. One reason is, of course, that systematic analysis can better proceed when the researcher thinks uninterruptedly about his observations, interviews, and personal field experiences. If a research team is involved, the members can work together better than when scattered about the observational field.

Second, it is necessary to reflect upon what amounts to a process of implicit coding that has been underway since the outset of the research. This reflection by the researcher consists of thinking systematically about the data, in accordance with his basic analytic categories. He need not, however, explicitly code his notes. He need merely note

categories in the margins of his field notes. Fieldworkers actually run through or reread sections of their notes, in order to verify principal hypotheses. In either case, they do something akin to what ordinarily is termed coding, but do not necessarily raise coding to prominent independent status. Indeed, even when collecting data, researchers will often have an "ah ha!" experience when they recognize that some observed event belongs in a given category. Moreover, strategic memorable events generate new categories and hypotheses, or cast doubt on the efficacy of certain categories and provide negative evidence against previous hypotheses. Those memorable events are either analyzed immediately after they occur, or keep recurring in memory with nagging persistence until systematically analyzed during memo writing periods.

In short, in qualitative work, just as there is no clear-cut line between data collection and analysis (except during periods of systematic reflection), there is no sharp division between implicit coding and either data collection or data analysis. There tends to be a continual blurring and intertwining of all three operations from the beginning of the investigation until its near end.

This implicit coding in margins goes on even when researchers do not intend to exploit it purposively, but plan to code explicitly all collected material at the close of fieldwork and then to accomplish the major analysis. However, they may soon realize that they have coded enough material to write their analysis already. Therefore, the explicit coding operation can become perceived as a stultifying tedium of little worth, for two reasons.

First, the researchers may find that they are not learning anything new enough about their analysis—that is, something that will sufficiently modify the core concepts and hypotheses of their analysis to make the explicit procedure seem worthwhile. Their analysis is *saturated*. Of course, explicit coding at the study's close can add further elaboration of details; but the question is always whether or not the additional effort is worthwhile since there is little chance that the core of the analysis will change, and details below the level of generality of the theory seldom add to its wider import and applicability.

Second, little more is likely to be learned by explicit coding after data collection because when various segments of the analytic framework get saturated during chronologically different stages of the fieldwork, neither more data need be gathered nor analysis rethought for the segment. Experienced fieldworkers know that their fieldnotes not only reflect this continuous saturation, but cannot always be read intelligently by outsiders precisely because at later stages of the research a shorthand reporting occurs which is based upon matters long since firmly known.

The continual intermeshing of data collection, coding, and analysis has direct bearing upon how the research is brought to a close. The researcher can always try to mine his data further, and he can always collect more data to check hypotheses or to force new ones. And when writing is done within or

near the field, the temptation is especially strong to dash back into the field. This last search for data understandably tends to be either of a specifically confirmatory nature (the researcher moving now with considerable sureness and speed) or of an elaborative nature (the researcher wishing to round out his work by exploring some area that was previously untouched or even unconsidered). This last search can be a strong temptation if personal relations formed in the field are satisfying or if exciting new events are developing there. However, collection and analysis of additional data can be a waste of time because the work merely further elaborates details of the analysis; again little of core value is learned.

When the researcher is convinced that his analytic framework forms a systematic, accurate statement of the matters studied, and that it is couched in a form possible for others to use if they were to go into the same field—then he can publish his results with confidence. He believes in his own knowledgeability and finds no reason to change that belief. He believes not because of an arbitrary judgment but because he has taken very special pains to verify what he thinks he may know every step of the way, from the beginning of his investigation until its publishable conclusion.

Maximizing Credibility Through Comparison Groups

In this section we shall present a strategy whereby fieldworkers can facilitate the qualitative analysis, while simultaneously developing confidence in its credibility. This strategy involves the systematic choice and study of several comparison groups.

It is feasible in field studies to build into the research design a comparison of at least several—and often many—social systems. This is particularly so in the hospital where comparison groups can be wards, wings, services, floors, and hospitals in a region. The strategy of choosing multiple comparison groups is guided by the logic of the researcher's emerging analytic framework. Significant categories and hypotheses are first identified in the emerging analysis, during preliminary fieldwork in one or a few groups, and while scrutinizing theories and data from other studies. Comparison groups are then located and chosen in accordance with the purpose of providing new data on categories or combinations of them, suggesting new hypotheses, and verifying initial hypotheses in diverse contexts. This calculus provides an efficient logical guide to groups where a given order of events or incidents is most likely to occur or not to occur.

It is not too difficult to compare as many as 40 groups when one considers that they are compared on the basis of a defined set of categories and hypotheses such as the "social loss" of patients, not compared on the basis of the "whole" group, and that groups within groups are compared (e.g., different and similar wards within different types of hospitals). These groups can be studied one at a time or a number can be studied simultaneously. They can also be studied in quick succession in

order to check out major hypotheses before too much analysis is built around them.

By precisely detailing the many similarities and differences of the various comparison groups, the analyst knows better, than if he only studied one or a few social systems, under what sets of structural conditions his hypotheses are minimized and maximized, hence to what kinds of social structures his analysis is applicable. In increasing the scope and delimiting the generality of his analysis, he saves his colleagues work. Ordinarily, readers of fieldwork must figure out the limitations of a published study by making comparisons with their own experience and knowledge of similar groups. By comparison, they figure that the reported material jibes just so far and no further—for given structural reasons. By using multiple comparison groups, much of this burden of delimiting relevant boundaries for the analysis is lifted from the reader's shoulders. In short, replication is built into the research.

Multiple comparison groups help generate the speedy development of analysis by drawing the observer's attention to many similarities and differences among groups that are important for his theory. From these similarities and differences are generated the categories to be used, their full range of types or continuum, their dimensions, the conditions under which they exist more or less, and their major consequences. In this way, the full generality and meaning of each category is established. Category development is much slower on a single terrain, and the result is a less generalized category imbued with less meaning. In addition, the differences and similarities among groups speedily generate generalized relations among the categories, which of course become the hypotheses soon integrated into the analysis.

Trust in One's Own Credible Knowledge

The analytic framework which emerges from the researcher's collection and scrutiny of qualitative data is equivalent to what he knows systematically about his own data. Let us discuss why the fieldworker trusts what he knows.

If there is only one fieldworker involved, it is he himself who knows what he knows about what he has studied and lived through. They are his perceptions, his personal experiences, and his own hard-won analyses. The fieldworker knows that he knows, not only because he's been there in the field and because of his careful verification of hypotheses, but because "in his bones" he feels the worth of his final analysis. He has been living with partial analyses for many months, testing them each step of the way, until he has built his final analysis. What is more, if he has participated in the social life of his subjects then he has been living by his analyses, testing them out not only by observation and interview but also in daily livable fact. Hence by the close of his investigation, his conviction about his theory would be hard to shake—as most fieldworkers would attest. This conviction does not mean that his analysis is the only plausible one

that might be based on his data, but only that the researcher himself has high confidence in its credibility. What he has confidence in is not a scattered series of analyses, but a systematic ordering of them into an integrated analysis of delimited arrays of data which he is ready to publish.

If a research team is involved, then of course it is their shared knowledge which constitutes the final analysis offered to colleagues. Each fieldworker not only knows his own fieldnotes intimately, but has shared his colleagues' observations and experiences by virtue of numerous discussions, "talking out," and memo-writing sessions. The inevitable debates among team members contribute also to the development of a shared analytic framework.

The "real life" character of fieldwork knowledge deserves special underscoring, especially as many critics think of this and other qualitatively oriented methods as merely preliminary to real (scientific) knowing. A firsthand immersion in a sphere of life and action—a social world—different from one's own yields important dividends for the fieldworker. The fieldworker who has observed closely in this social world has had, in a profound sense, to live there. He has not only been sufficiently immersed in the world to know it, but has retained enough detachment to think theoretically about what he has seen and lived through. His informed detachment has allowed him to benefit not only as a sociologist but as a human being who must "make out" in that world. This is true despite the fact that the people there generally do not expect perfect adherence to their ways from the outsider. His detachment has served also to protect him against going more than a little native while yet doing more than a little passing as a native, when the people whom he is studying either have temporarily forgotten his outsider status or have never recognized it. Meanwhile his display of understanding and sympathy for their mode of life permits sufficient trust in him so that he is not cut off from seeing important events, hearing important conversations, and perhaps seeing important documents. If that trust does not develop, his analysis suffers.

The evolving systematic analysis permits the fieldworker quite literally to write prescriptions so that other outsiders might get along in the observed sphere of life and action. That is one benefit of his analysis. If he has avoided trouble within the particular social world by following these prescriptions, then presumably they accurately represent the world's prominent features; they are workable guides to action and therefore they can, on this account too, be accorded our confidence in their credibility.

In effect this is how shrewd or thoughtful visitors to any social world feel about their knowledge of these worlds. Not infrequently people successfully stake their money, reputations, and even lives as well as the fate of others upon their interpretations. What the fieldworker does is to make this normal strategy of reflective persons into a successful research strategy. In doing so, of course, a trained, competent researcher is much more systematic in formulating his ideas than is the ordinary visitor;

and if a superior researcher, his knowledge is likely to be generalized and systematically integrated into a theory. In addition, he is much more systematic at verifying his ideas than is the ordinary visitor. Such bias as he brings to the field is more likely to be checked upon, while his hypotheses are more likely to arise within the field of observation than to be imported from the outside. In the latter regard, he also differs from researchers who bring such a working baggage of formal theory into the field that they end not by discovering a new analysis but manage principally to write footnotes to the imported theory. They are not likely either, to do very well in the pragmatic test of living by their analysis while in the field.

Finally, it is worth special mention that those fieldworkers who do not really believe in their own hard-won analyses are tempted toward a compulsive scientism. Because they do not trust themselves—their own ability to know or reason—they rely additionally upon questionnaires or other "objective" methods of collecting and analyzing quantified data. Used for this purpose these methods do not necessarily lead to greater credibility, but they do permit the insecure researcher to feel greater security in his "results" without genuine consideration of what queries do or do not need this additional "hard" data. It is also true that the insecure fieldworker may know that he is running away from himself, because of a failure of confidence in his ability to render his knowledge credible, but he cannot stop running!

Conveying and Judging Credibility

When the researcher decides to write for publication, then he faces the problem of conveying to colleagues the credibility of his discovered theory so that they can make some sensible judgment about it. Conveying credibility presents two problems.

First is to get readers to understand the analytic framework. This is generally done by giving an extensive abstract presentation of the framework and its associated theoretical statements, generally at the beginning and/or end of the publication but usually also in segments throughout the publication.

Second is to describe the social world studied so vividly that the reader can almost literally see and hear its people—but see and hear in relation to the theoretical framework. To do this, the researcher ordinarily utilizes several of a considerable armamentarium of standard devices. He can quote directly from interviews or conversations which he has overheard. He can include dramatic segments of his on-the-spot fieldnotes. He can quote telling phrases dropped by informants. He can summarize events or persons by constructing readable case studies. He can try his hand at describing events and acts; and often at least he will give backdrop descriptions of places and spaces. He will even offer accounts of personal experience to show how events impinged upon himself. Sometimes he will unroll a narrative. Chapter or section headings can also help to convey sights and sounds.

These problems of conveying credibility through plausible reasoning are reflected in the type of concepts that the researcher chooses for writing his analysis. With regard to the first problem, his concepts are analytic—sufficiently generalized to designate the properties of concrete entities (not the concrete entities themselves). With regard to the second problem, his concepts also are sensitizing—yield a “meaningful” picture—abetted by apt illustrations which enable one to grasp the reference in terms of one’s own experience. Formulating concepts of this nature, hence tapping the best of two possible worlds, takes considerable study of one’s data.

Several aspects of the presentation enter into how the reader, in turn, judges the credibility of the analysis that the writer is trying to convey. First of all, if a reader becomes sufficiently caught up in the description so that he feels vicariously that he also had been in the field, then he is more likely to be kindly disposed toward the researcher’s analysis than if the description seemed flat or unconvincing.

Second, a judgment of credibility will also rest upon assessments concerning how the researcher came to his conclusions. The reader will note, for instance, what range of events the researcher saw, whom he interviewed, who talked to him, what kinds of experiences he had, and how he might have appeared to various people whom he studied. That is, the reader will assess the types of data utilized from what is explicitly stated as well as from what can be read between the lines. It is absolutely incumbent upon the reader to make such judgments, partly because the entire publication may be a complete fabrication, but more usually because any analysis may require some qualification.

Such qualification we may term “the discounting process.” Readers surely discount aspects of many, if not most, analyses which are published (whether resting upon qualitative or quantitative data). This discounting by the reader takes several forms: the theory is corrected because of onesided research designs, adjusted to fit the diverse conditions of different social structures, invalidated for other structures through the reader’s experience or knowledge, and deemed inapplicable to yet other kinds of structures. It is important to note that when the analysis is deemed inapplicable to a social world or social structure, then it cannot be invalidated by their conditions. It is not correct to say that because it “does not fit” a structure, then it is invalid. The invalidation or adjustment of an analysis is only legitimate for those social worlds or structures to which it is applicable.

This ongoing discounting process of qualification by the reader allows the researcher to write in general form, because the researcher knows that the reader will make the necessary corrections, adjustments, invalidations, and inapplications when thinking about or using the analysis. These are qualifications that he could not begin to cover for even a small percentage of one type of reader and, more important, they are qualifications which the researcher must learn to gloss over or to ignore in order to write an analysis of some generality.

(It is also necessary to leave out qualifications in order to write analysis that is readable, because the rhetoric of qualification is as onerous to read as to write.)

The researcher and his readers thus share a joint responsibility. The researcher ought to provide sufficiently clear statements of description so that readers can carefully assess the credibility of the analytic framework offered in his publication. A cardinal rule for the researcher is that whenever he himself feels most dubious about an important interpretation—or foresees that readers may well be dubious—then he should specify quite explicitly upon what kinds of data his interpretation rests. The parallel rule for readers is that they should demand explicitness about important interpretations, but if the researcher has not supplied the information then they should assess his interpretations from whatever indirect evidence may be available.

The Issue of Further Rigor

The presentation of qualitative analysis is often done at a sufficient level of plausibility to satisfy most readers. It can be applied and adjusted to many situations with sufficient exactitude to guide thinking, understanding, and research. Given certain structural conditions under which researchers work (such as designing specific action programs, or working in a rather well developed substantive area), then more rigorous testing may be required to raise the level of plausibility of some hypotheses.

Under these conditions, it should be a matter of empirical determination as to how the further testing can best be accomplished—whether through more rigorous or extensive fieldwork itself, or through experiments or survey methods. The two essential points in this decision on method are, first, that the testing be more rigorous than previously (not which of all methods is the most rigorous); and, second, that the more rigorous approach be compatible with the research situation in order to yield the most reliable findings. What should not enter into the determination of further testing are the researcher’s ideological commitments (with associated career contingencies) to only one method; for instance, that a survey is a more rigorous mode of achieving a high degree of plausibility than field observation, and therefore it is the best and only mode to use in all cases. In the actual research situation, a survey may not be feasible nor worth the time or money, nor yield the type of information needed, and indeed it may even distort the information yielded. An approach to an increased, required level of plausibility should be based, therefore, on the use of the method or methods best suited to the socially structured necessities of the sociologist’s research situation.

This cardinal rule for determining which method to use for increasing the plausibility of a quantitative analysis is broken in another way by researchers who are ideologically committed to quantitative methods. They assume out of context that all research requires a rigorously achieved high level of

plausibility and that quantitative research, more rigorous than most qualitative methods, is therefore the best method to use in all research situations. Thus, whatever qualitative research may be done is seen merely as a preliminary provision of categories to use in the ensuing quantitative research. As noted at the beginning of our paper, this position neglects the fact that this qualitative analysis is more often than not the end product of research within the substantive area beyond which few, if any, researchers are motivated to move.

Qualitative analysis is often the end product of research for a variety of reasons. First, those researchers who do try to move beyond it by testing it with quantitative data are often told by colleagues and editorial boards that they are simply proving what everyone knows sufficiently well already. They are told their work is trivial and a waste of resources. To "save" their work, they are forced to turn their quantitative work of testing the "already known" hypothesis into an effort at discovering, in their data, new substantive fact and theory. Thus qualitative data then results often in a *de facto* conclusive, rather than a preliminary, analysis.

Second, it is an old story in social science that contemporary interest switches from certain phenomena once that interest is saturated with qualitative analysis. This switch usually occurs long

before satisfactory quantitative research pertaining to the phenomena has taken place. Meanwhile, informed laymen and social scientists manage to profit quite well by the merely plausible work of discovery published by researchers who carefully analyze their qualitative data. This ability to profit from qualitative research forestalls the need for future highly rigorous research among most sociologists and laymen. Since the analysis works well enough, it is typically only modified, if even that, not by further demonstrative research on a specific hypothesis but by additional related analysis. The researcher's primary effort in working with this related analysis is to discover more, not to correct or test, and any modification of previous analysis receives only post-hoc recognition.

And third—much the most important reason—a great deal of social science, unlike physical science, never gets to the stage of rigorous demonstration because the social structures which are studied undergo continuous change. Older structures frequently take on new dimensions before highly rigorous research can be accomplished. The changing of social structures means that a prime task is the exploration—and even literal discovery—of emerging structures. Undue emphasis on being "scientific" is simply not reasonable in light of our need for discovery and exploration amidst very considerable structural changes.

1966 Mary M. Roberts Writing Awards

Six Mary M. Roberts Writing Awards will be made again in 1966 to registered professional nurses who enter this year's national writing competition. Nurses wishing to compete must submit one sample of unpublished writing. An official application form, in duplicate, must accompany each entry.

The Awards will be made to the six winners in a national writing competition held for registered professional nurses. The winners will receive fully paid room, board, and tuition at Bread Loaf Writers' Conference to be held August 17-31, 1966 at Middlebury College, Middlebury, Vermont. Transportation to and from the conference is included in the Awards.

The purpose of the Awards is to give nurses with writing ability the opportunity of hearing the writing process described by conference staff members, all outstanding writers, at informal sessions rather than in a formal, classroom atmosphere. Because winning nurses attend the conference on a contributor level, they have the opportunity to discuss samples of their writing with a staff member. Winners may bring these samples at the time they attend Bread Loaf.

John Ciardi, director of the Bread Loaf Writers' Conference and Poetry Editor of the *Saturday Review*, is to be the final judge of all material submitted for the contest. The winners will be notified in April 1966.

The deadline for submitting material to be judged for the contest is March 1, 1966. Only material postmarked March 1, 1966 or earlier can be considered. To be eligible, an applicant

shall: a) be a registered nurse; b) be a member in good standing of the American Nurses' Association; c) be free to attend the Bread Loaf Writers' Conference at Middlebury, Vermont for the period August 17-31, 1966. Former winners of Mary M. Roberts Journalism or Writing Awards, and members of the staff or board of directors of the American Journal of Nursing Company are not eligible.

The contestant must submit one sample of unpublished writing which can be on any subject and in any literary form, except that plays or television scripts are not acceptable. The sample must be typed clearly on one side of 8½ x 11 sheets; name and full address should appear on the first page of the sample submitted for the contest. The sample will be returned only if accompanied by a self-addressed envelope and full postage, or clear directions for return by express collect. Some time may pass before the sample can be returned, so it will be to the contestant's advantage to keep a copy. The American Journal of Nursing Company assumes no responsibility for manuscripts lost in the mails or otherwise, but all possible care will be taken to prevent such loss.

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