Discovery of Substantive Theory:

A Basic Strategy

Underlying Qualitative

Research

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The authors contend that qualitative research should be scrutinized for its usefulness in the discovery of substantive theory. They try to present generic elements of the process of generating substantive theory from qualitative data, and consider how the researcher collects and analyzes qualitative data, maximizes the theory's credibility, puts trust in his theory, and conveys the theory to others. Drs. Glaser and Strauss are affiliated with the University of California Medical Center in San Francisco.

IN SPITE OF the diversity of problems, approaches and conclusions in the writings of sociologists on qualitative research and analysis, all would seem to support one general position: Qualitative research is a preliminary, exploratory effort to quantitative research since only quantitative research yields rigorously verified findings and hypotheses.[1] 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 formulization, hypothesis construction and presentation of evidence. Thus these sociologists over-emphasize rigorous testing of hypotheses, and de-emphasize 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 in the discovery of substantive theory.[2] By the discovery of substantive theory we mean the formulation of concepts and their interrelation into a set of hypotheses for a given substantive area—such as patient care, gang behavior, or education—based on research in the area. To view qualitative research as merely preliminary to quantitative research neglects, hence underestimates, several important facts about substantive theory that is based on qualitative research. First, substantive theory is more often than not the end product of research within a substantive area beyond which few, if any, research sociologists are motivated to move.[3] Second, it is the basis upon which grounded formal theory is generated.[4] Third, 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.[5] Fourth, sociologists (and informed

laymen) manage often to profit quite well in their everyday work life from substantive theory based on qualitative research.[6]

Together these facts raise doubts as to the applicability of the canons of quantitative research as criteria for judging the credibility of substantive theory based on qualitative research. 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, to be used both in discovering substantive theory based on qualitative research and in judging its credibility, is the task of this paper. In so doing, we shall regard *qualitative research*—whether utilizing observation, intensive interviews, or any type of document—*as a strategy concerned, with the discovery of substantive theory*, not with feeding quantitative researches. We shall take up the following pertinent matters: 1) the collection and analysis of data, 2) the maximization of substantive theory's 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 theory, and 5) the relation of discovery of substantive theory to its further rigorous testing.

Joint Collection 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 developing 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.[7]

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.[8] Whatever the answer, it is certain that experienced researchers quickly develop important concepts, basic categories, and significant hypotheses.[9] 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.[10]

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 all—or any—of his notes. Fieldworkers actually run through or reread sections of their notes, in order to verify principal hypotheses. They will also run back "in memory" to verify 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 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, *if* substantive theory is their goal, that they have implicitly coded enough material to write their theory 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 theory—that is, something that will sufficiently modify the *core* concepts and hypotheses of the theory—to make the explicit procedure seem worthwhile. Of course, explicit coding at the study's close can add further elaboration of details to the substantive theory; but the question is always whether or not the additional effort is worthwhile since there is little chance that the core of the theory 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 various segments of the analytic framework get firmed up during chronologically different stages of the fieldwork. Once firmed up, neither more data need be gathered nor analysis rethought for the segment, unless further theoretical work

necessitates those additional operations. Experienced fieldworkers know that their fieldnotes not only reflect this continuous firming up, 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.[11]

The continual intermeshing of data collection 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).[12] 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 substantive theory; again little of core value is learned.[13]

When the researcher is convinced that his analytic framework forms a systematic substantive theory, that it is a reasonably 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 discovery of a substantive theory, while simultaneously developing confidence in the credibility of that theory. This strategy involves the systematic choice and study of several comparison groups.

Fieldwork in sociology arose from the ethnological tradition of studying one society or group at a time. The sustaining rationale consisted of what one anthropologist or sociologist by himself might be able to observe, plus the conviction that social structure ought to be captured "as a whole." Consequently fieldwork monographs have tended through the years to take the form of single case studies. Even today most fieldworkers study one group at a time and few focus upon more than two or three groups simultaneously.[14] Such comparisons as exist for single case studies are either brought into the monograph (or paper) by footnoting compa-

rable materials and discussing them or by publishing several comparable studies together in one volume.

However, it is feasible in more field studies than have attempted it to *build into the research design a comparison of at least several—and often many—social systems.*[15] 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 substantive theories and data from other studies. Comparison groups are then located and chosen in accordance with the purposes of providing new data on categories or combinations of them, suggesting new hypotheses, and verifying initial hypotheses in diverse contexts. It is not too difficult to compare as many as forty groups when one considers that they are compared on the basis of a defined set of categories and hypotheses (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 theory is built around them.

Multiple comparison groups function in several ways to improve the research and consequent substantive theory. First and foremost the comparisons maximize the credibility of the final theory in two fundamental ways:

A) 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 theory is applicable. In increasing the scope and delimiting the generality of his theory, 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 theory is lifted from the reader's shoulders.[16] In short, replication is built into the research.

B) Another way that multiple comparison groups maximize credibility is by helping the researcher to calculate where a given order of events or incidents is most likely to occur or not to occur. This calculus provides an efficient logical guide to groups, for obtaining more data to fill in theoretical gaps and for verifying his hypotheses. This calculus is especially helpful in his efficient search for negative cases that may necessitate reformulation of a hypothesis. Also, the variety lent his study by multiple comparison groups increases the possibility of his being surprised by unanticipated negative cases.

Multiple comparison groups also permit and generate the speedy development of analysis in two principal ways:

A) The constant comparison of many groups rather quickly draws 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 theoretical 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.[17] Category development is much slower on a single terrain, and the result is a less generalized category imbued with less meaning.

B) 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 substantive theory. When a negative case is found in a different group, and since a group is an indicator of a set of structural conditions, while reformulating his hypothesis the analyst compares the set of conditions under which it existed to the set under which it is encountered in order to find the particular structural condition(s) making for the change— which condition(s) can then be taken account of in reformulating the hypothesis. This analytic strategy is far different, more powerful, precise, and informative than comparing positive and negative cases within a single structure.[18] In the latter case, one can only compare the *internal* structure of the negative incident to the positive incidents, since both occur under the same structural conditions. That comparison is likely to sound implausible—even tautological—for one ends up saying that an element of an incident caused itself to be different from all other similar incidents. It is more plausible to point to different sets of *external* structural conditions under which positives and negatives exist and, then, suggest differentiating factors in the cases based on comparison of these sets.

Researchers who work with other types of qualitative data can also utilize this efficient method. Using only interviews, for instance, there is no reason why researchers cannot study comparison groups of interviewees, chosen in accordance with emergent analytic frameworks. And historical documents, or other library materials, lend themselves wonderfully to the comparative method. Their use is perhaps even more efficient, since the researcher is saved much time and trouble in his search for comparison groups which are, after all, found concentrated in the library. As in fieldwork, when his analytic framework is far developed, the researcher who uses library materials can always select additional comparison groups to give himself additional confidence in the credibility of his framework. He will also—like the fieldworker who sometimes stumbles upon comparison groups and then makes proper use of them—occasionally profit from such happy accidents which occur when browsing along library shelves.

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 substantive theory. 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 theory.[19] He has, in fact, discovered a substantive theory about delimited arrays of data, through inductive as well as deductive effort, which he is ready to publish.

If a research team is involved, then of course it is their shared knowledge which constitutes the final substantive theory 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.[20]

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 substantive theory. 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.[21]

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 much substantive theory 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 theory while in the field.

Finally, it is worth special mention that those fieldworkers who do *not* really believe in their own hard-won substantive theory 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. The problem of conveying credibility is dividable into two sub-problems, each of which deserves discussion.

The first sub-problem is that of getting readers to understand the theoretical 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. This presentation is not particularly difficult since there exists an abstract social science terminology which is quite as applicable to qualitative as to quantitative data as well as a common sociological perspective which furthers the communication.

The related second sub-problem is how 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 headings can also help to convey sights and sounds.[22]

The first and second sub-problems of conveying credibility through plausible reasoning are reflected in the type of concepts that the researcher chooses for writing his substantive theory. 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.[23] Formulating concepts of this nature, hence tapping the best of two possible worlds, takes considerable study of one's data.[24]

Several aspects of the presentation enter into how the reader, in turn, judges the credibility of the theory 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 theory 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[25], 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).[26] This discounting by the reader takes several forms: the theory is *corrected* because of onesided research designs[27], *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 a theory 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 a theory "does not fit" a structure, then it is invalid. The invalidation or adjustment of a theory 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 his theory 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 theory. 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 a substantive theory of some generality.[28] (It is also necessary to leave out qualifications in order to write a theory 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 theory and description so that readers can carefully assess the credibility of the theoretical 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. These same rules apply to the reading of qualitative materials from libraries and organizational archives, as well as to the writing of those materials.

The Issue of Further Rigor

The presentation of substantive theory, developed through analysis of qualitative data, is often done at a sufficient level of plausibility to satisfy most readers. The theory can be applied and adjusted to many situations with sufficient exactitude to guide thinking, understanding and research. Given certain structural conditions under which sociologists 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 the substantive theory 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 both the importance of discovering substantive theory based on qualitative research and the fact that this substantive theory is more often than not the end product of research within the substantive area beyond which few, if any, research sociologists are motivated to move.

Substantive theory discovered through qualitative analysis is often the end product of research for a variety of reasons. First, those researchers who do try to move beyond substantive theory 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.[29] 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,

quantitative data is often used not for rigorous demonstration of theory but as another way to discover more theory, and qualitative data results often in a *de facto* conclusive analysis 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 substantive theory. 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 sociologists who carefully analyse their qualitative data. This ability to profit from substantive theory based on qualitative research forestalls the need for future highly rigorous research among most sociologists and laymen.[30] Since the theory works well enough, it is typically only modified, if even that, not by further demonstrative research on a specific hypothesis but by additional related theory. The researcher's primary effort in working with this related theory is to discover new theory, not to correct or test older theory. Once new theory is discovered and developed, any modification of older theory that then occurs will receive post-hoc recognition.

And third—much the most important reason—a great deal of sociological work, unlike physical science research, never gets to the stage of rigorous demonstration because the social structures which sociologists study are undergoing 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 sociological 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.

Concluding Remarks

Most writing on sociological method has probably been concerned with how theory can be more rigorously tested. In this paper we have addressed ourselves to the equally important enterprise of how the discovery of substantive theory can be furthered. The formulation of fruitful substantive theory for a substantive area through careful research—as against constructing formal theory for a conceptual area (such as deviance, status congruency, reference groups or hierarchy)—is a major task in sociology. Substantive theory faithful to the empirical situation cannot be formulated, we believe, by merely applying a formal theory to the substantive area. First a substantive theory must be formulated, in order to see which of diverse formal theories are applicable to help further the substantive formulation.[31] And in its turn then substantive theory helps in formulating and reformulating formal theory. Thus substantive theory becomes a strategic link in the formulation and development of formal theory based on data. We have called the latter "grounded" formal theory to contrast it with formal theory based on logical speculation.[32]

Some forty years ago, Thomas and Znaniecki hazarded that one type of qualitative data (autobiographies) might be the most useful kind of data for sociological theory.[33] Anthropologists, however, then and now generally believe that fieldwork data—encompassing observations and interviews as well as case studies and autobiographical accounts—are most useful. And in the recent literature of sociology, there has been some argument on the comparative virtues of various types of qualitative data: for instance, interview *versus* fieldwork data and historical *versus* contemporary data.[34] Regardless of the type of qualitative data preferred, all seem admirably suited for discovery of substantive theory pertaining to the areas and problems with which sociologists are concerned.

Footnotes

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1. See, e.g., A. H. Barton and P. F. Lazarsfeld, "Some Functions of Qualitative Analysis in Social Research," reprinted in Lipset and Smelser, *Sociology: The Progress of a Decade.* Englewood Cliffs: Prentice-Hall, 1961, 95-122; B. Berelson, *Content Analysis.* Glencoe: Free Press, 1952, 114-134, esp. 133; S. A. Stouffer, *Social Research to Test Ideas.* New York: Free Press, 1962, 1-10, esp. 10; R. K. Merton, *Social Theory and Social Structure.* Glencoe: Free Press, 1957, 15-16 and chaps. II and III. One author, somewhat ambivalent on this position, seems to agree that discovery in qualitative research is preliminary to rigorous testing, but prefers to establish testing methods applicable to qualitative data using the canons of quantative work (H. S. Becker, "Problems of Inference and Proof in Participant Observation," *Amer. Soc. Rev.*, Dec., 1958, 653). For a critique of this general position see H. Blumer, "Sociological Analysis and the 'Variable,' " *Amer. Soc. Rev.* 21, 1956, 683-690. Perhaps the latest perspective is the "multiple methodology" approach which sees qualitative analysis as preliminary for some kinds of information (e.g., an enumeration) and the end product for other kinds (e.g., a norm) (M. Zelditch, Jr., "Some Methodological Problems of Field Studies," *Amer. J. Soc.*, March, 1962, 567; and A. Vidich and J. Bensman, "The Validity of Field Data," *Hum. Org.*, 13, 1954, 20-27; and R. N. Adams and J. J. Preiss, *Human Organization Research.* Homewood: Dorsey Press, 1960, 223-4.

2. The richness of qualitative research for the discovery of substantive theory is well known; for detailed explanations of why this is so see Becker, *op. cit.*, 652-3 and 657-8, and H. S. Becker and B. Geer, "The Analysis of Qualitative Field Data," in *Human Organization Research, op. cit.*, 262-3.

3. This fact is discussed above in the section, "The Issue of Further Rigor."

4. This fact is discussed in concluding remarks.

5. Zelditch, *op. cit.*, 575.

6. This fact is brought out in many ways throughout this paper.

7. Becker and Geer, *op. cit.*, 270-1.

8. B. Geer, "First Days in Field Work," in P. E. Hammond, *Chronicles of Social Research.* New York: Basic Books, Inc., 1964.

9. *Cf.*, B. Paul, "Interview Techniques and Field Relationships," in A. L. Kroeber (ed.), *Anthropology Today.* Chicago: University of Chicago Press, 1953, 430-51.

10. Some inexperienced or compulsive researchers, afraid that they are missing important events, fail to create sufficient breathing space for careful analytic memo writing. The same failing is also characteristic of researchers who work with other kinds of qualitative data.

11. Again, what is true of fieldwork seems equally true of research using other kinds of qualitative data, such as historical materials, in contrast, say, with researchers who use quantified content-analysis methods upon caches of data.

12. *Cf.*, A. Strauss, L. Schatzman, R. Bucher, D. Ehrlich, and M. Sabshin, *Psychiatric Ideologies and Institutions.* New York: Free Press, 1964, chap. 2. See also H. Becker, B. Geer, E. Hughes and A. Strauss, *Boys in White.* Chicago: University of Chicago Press, 1962, re interviews after field observation.

13. Though highly unlikely, there is, of course, the small chance that additional data can "explode" an otherwise finished analytic framework and cause the researcher to spend months or years before he is satisfied enough to publish. This hazard is not confined to work with qualitative data, but is especially characteristic of qualitative work.

14. Two recent field studies which have compared attributes of a specific type of organization, utilizing many such organizations in many nations, are: W. A. Glaser, "American and Foreign Hospitals: Some Sociological Comparisons," in E. Freidson (ed.), *The Hospital in Modern Society.* New York: Free Press, 1963, 37-73; and N. Kaplan, "The Western European Scientific Establishment in Transition," in *Amer. Behav. Sci.*, vi, Dec., 1962, 17-20.

15. The logic of our strategy complies with Nagel's directive for "controlled investigation" in any science: "However, every branch of inquiry aiming at reliable general laws concerning empirical subject matter must employ a procedure that, if it is not strictly controlled experimentation, has the essential logical functions of experiment in inquiry." E. Nagel, *The Structure of Science.* New York: Harcourt, Brace & World, Inc., 1961, 453. We ourselves used this method in a study reported in a forthcoming volume, *Awareness of Dying.*

16. See, i.e., J. Q. Wilson's strictures on D. C. Thompson's, *The Negro Leadership Class,* in *Amer. Soc. Rev.*, 28, Dec., 1963, 1051-52.

17. Consider the full discussion of this point in R. Bendix, "Concepts and Generalizations in Comparative Sociological Studies," *Amer. Soc. Rev.*, 28, Aug., 1963, 532-9.

18. The initial advocates in sociology of a search for negative cases have either focused on them within one structure or omitted explicit focus upon structural conditions. They have, rather, focused upon the search for negative incidents within categories of analysis under which they have amassed a series of positive incidents. Becker and Geer, *op. cit.*, 287-8; A. Lindesmith, *Opiate Addiction*. Bloomingdale: Principia, 1947, chap. 1.

19. This theme is extensively developed throughout H. Zetterberg, *On Theory and Verification In Sociology*. N. J.: Bedminster Press, 1963. It is important that one distinguish between the researcher's conviction in the credibility of his theoretical analysis and his conviction that he understands much about the perspectives and meanings of his subjects. Researchers will readily agree that their own theoretical formulations represent credible interpretations of their data—which could be interpreted differently by others—but it would be hard to shake their conviction that they have correctly understood much about the perspectives and meanings of the people whom they have studied.

20. For a fieldwork account of how tightly closed doors were finally opened after trust was established, see, R. Wax, "Twelve Years Later: An Analysis of Field Experience," *Amer. J. Soc.*, 63, 1957, pp. 133-42.

21. The most vigorous of quantitative researchers may write a methodological article from "heart" with no data collection or coding because he simply knows what he knows. He has lived it and he was successful. People will believe him because they know he has been through it. In writing this article, he is merely doing fieldwork on himself.

22. The researcher's task of conveying credibility is actually much like that of the realistic novelist. The latter generally leaves his analytic framework—his interpretation—much more implicit than does the researcher. Often the novelist's tactics for getting the reader to imagine social reality are more subtle, because he may not only be a more skilled writer but may feel that he can use more license in his presentation. Sometimes too his descriptive task is simpler because his analytic framework is much simpler. Nonetheless, the great novelists have conveyed societal views which readers have long felt to be both complex and real (i.e., credible). We say this not to pit researchers against novelists, but to point out where their respective tasks may be similar and where different.

23. On sensitizing concepts see H. Blumer, "What is Wrong with Social Theory," *Amer. Soc. Rev.*, 19, Feb., 1964, 3-10, quote on page 9.

24. Consider the effort in this direction by H. L. Zetterberg, *Social Theory and Social Practice*. New Jersey: Bedminster Press, 1962, chap. 3. The concepts should also be clearly specified so that they can be readily measured by existing techniques, *if* researchers desire to test quantitatively hypotheses based on them.

25. Note for instance how gullible or unsuspecting readers can believe wholly in purposely fake accounts, such as the papers reprinted in R. Baker (ed.), *Psychology in the Wry*. Princeton: Nostrand, 1963.

26. *Cf.*, B. Berger's review of J. Coleman's quantitative study, *The Adolescent Society*, in *Soc. Prob.*, 10, 1963, 394-400; also J. Q. Wilson, *op. cit.* And whether analysis is quantitative or qualitative, later generations of scholars will discount it by placing it within a larger context of public rhetoric, *cf.*, "Appendix: A Note on Imagery in Urban Sociology," in A. Strauss, *Images of the American City*. Glencoe: Free Press, 1961, 255-58.

27. For instance, when we read that someone has done fieldwork with workers in a factory, we suspect that his interpretive account (as it pertains even to the workers) needs some correction because the administrators have not been similarly studied. What correction is needed may not, of course, be so evident: for instance, some sociologists have studied state mental hospitals from a perspective really borrowed from psychiatry and thus interpreted its structure and functioning from a quasi-psychiatric viewpoint. The needed correction was read in by at least one set of readers, who themselves later studied a mental hospital and came to a rather different conclusion about such institutions (R. Bucher and L. Schatzman, "The Logic of the State Mental Hospital," *Soc. Prob.*, 9, 1962, 337-349). This latter instance suggests that readers are not always merely readers, but can also be or become researchers upon topics about which they have read.

28. Consider the discussion of social laws by E. Nagel, *op. cit.*, 459-66.

29. For a few (or many) diverse comments of concern on the trivial results of "precise" quantitative research, see: on their laboring of the obvious, R. K. Merton, "Problem Finding in Sociology," in R. K. Merton, L. Broom and L. S. Cottrell, Jr. (eds.), *Sociology Today*. New York: Basic Books, 1959, IV-I; on their uselessness for theory construction, H. L. Zetterberg, *On Theory and . . .*, preface, 36, 52, and 67; and on their worth in verifying what is already known, A. Etzioni, "Book Review," *Amer. J. Soc.*, LXVII, Jan., 1962, 466.

30. "While we cannot count on very many research workers being stimulated to conduct crucial tests of middle-range theories, they are likely to be especially stimulated by the concepts that enter into such theories." H. Hyman, "Reflections on the Relations Between Theory and Research," *The Centennial Review*, VII, No. 4, Fall, 1963, 449.

31. Ignoring the task of discovering substantive theory that is relevant to a given substantive area is the result, in most cases, of believing that formal theories can be directly applied to an area, and that these formal theories supply all the necessary concepts and hypotheses. The consequence is often a forcing of data and neglecting of the relevant concepts and hypotheses that may emerge. Allowing substantive concepts and hypotheses to emerge first on their own, enables the analyst to ascertain which of diverse formal theories may be inclusive of his substantive theories, thus enabling him to be more faithful and less forcing of his data (or more objective and less theoretically biased). This means that one cannot merely apply Parsonian categories right off, but must wait to see whether they are linked to the emergent substantive theory concerning the issue in focus.

32. An outstanding example of "grounded" social theory is H. S. Becker, *The Outsiders*. New York: Free Press, 1962.

33. W. I. Thomas and F. Znaniecki, *The Polish Peasant in Europe and America*. New York: Knopf, 1918.

34. See H. S. Becker and B. Geer, "Participant Observation and Interviewing: A Comparison," *Hum. Org.*, XVI, 1957, 28-34; M. Trow, "Comment," *Hum. Org.*, XVI, 1957, 33-35; and Becker and Geer, "Rejoinder," *Hum. Org.*, XVII, 1958, 39-40.
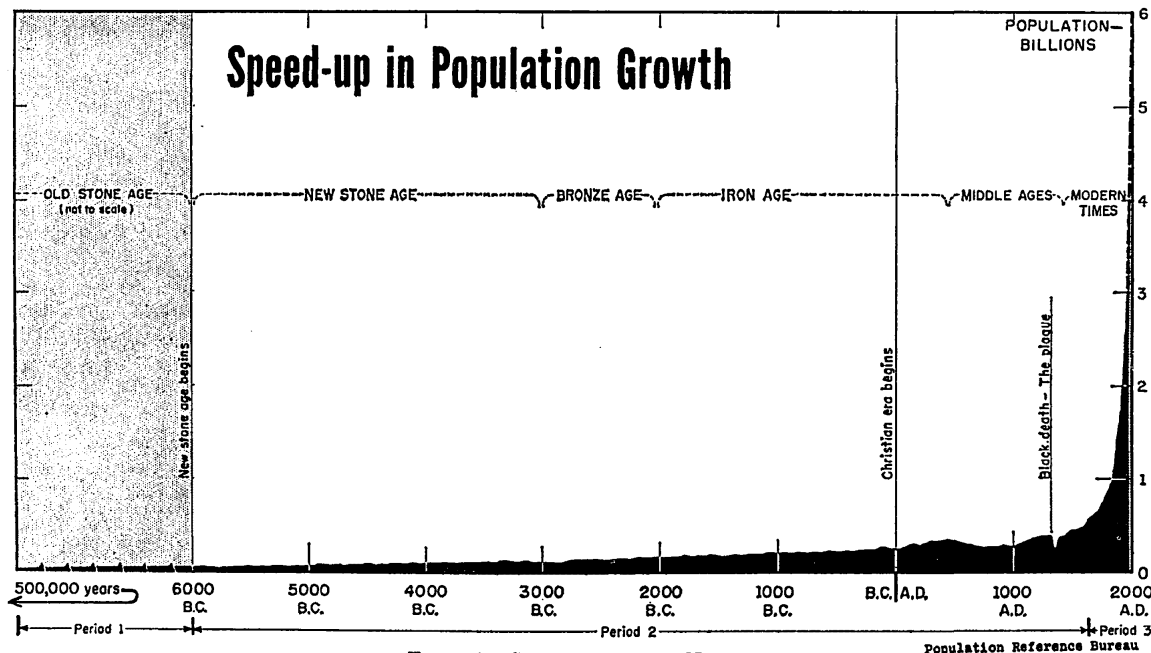
New Facts . . .

FIGURE 1.—GROWTH OF HUMAN NUMBERS

It has taken all the hundreds of thousands of years of man's existence on earth for his numbers to reach 3 billion. But in only 40 more years population will grow to 6 billion, if current growth rates remain unchanged. If the Old Stone Age were in scale, its base line would extend 35 feet to the left! (From "*How Many People Have Ever Lived on Earth?*" by ANNABELLE DESMOND.)