The Discovery of Grounded Theory
Strategies for Qualitative Research

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Most writing on sociological method has been concerned with how accurate facts can be obtained and how theory can thereby be more rigorously tested. In this book we address ourselves to the equally important enterprise of how the discovery of theory from data—systematically obtained and analyzed in social research—can be furthered. We believe that the discovery of theory from data—which we call grounded theory—is a major task confronting sociology today, for, as we shall try to show, such a theory fits empirical situations, and is understandable to sociologists and layman alike. Most important, it works—provides us with relevant predictions, explanations, interpretations and applications.

As sociologists engaged in research soon discover, there are as yet few theories of this nature. And so we offer this book, which we conceive as a beginning venture in the development of improved methods for discovering grounded theory. Because this is only a beginning, we shall often state positions, counterpositions and examples, rather than offering clear-cut procedures and definitions, because at many points we believe our slight knowledge makes any formulation premature. A major strategy that we shall emphasize for furthering the discovery of grounded theory is a general method of comparative analysis.

Previous books on methods of social research have focused mainly on how to verify theories. This suggests an overemphasis in current sociology on the verification of theory, and a
resultant de-emphasis on the prior step of discovering what concepts and hypotheses are relevant for the area that one wishes to research. Testing theory is, of course, also a basic task confronting sociology. We would all agree that in social research generating theory goes hand in hand with verifying it; but many sociologists have been diverted from this truism in their zeal to test either existing theories or a theory that they have barely started to generate.

Surely no conflict between verifying and generating theory is logically necessary during the course of any given research. For many sociologists, however, undoubtedly there exists a conflict concerning primary of purpose, reflecting the opposition between a desire to generate theory and a trained need to verify it. Since verification has primacy on the current sociological scene, the desire to generate theory often becomes secondary, if not totally lost, in specific researches.

Our book—especially when we discuss the current emphasis on verification—will indicate many facets and forms that the resolution of this conflict takes among sociologists, but this discussion should not be taken as indicating that we endorse the existence of such a conflict. Rather, our position is that a conflict is created when sociologists do not clearly and consciously choose which will receive relative emphasis in given researches because of too great an adherence to verification as the chief mandate for excellent research.

Grounded Theory

The basic theme in our book is the discovery of theory from data systematically obtained from social research.1 Every chapter deals with our beginning formulation of some of the processes

1. Merton never reached the notion of the discovery of grounded theory in discussing the "theoretic functions of research." The closest he came was with "serendipity"; that is, an unanticipated, anomalous, and strategic finding gives rise to a new hypothesis. This concept does not catch the idea of purposefully discovering theory through social research. It puts the discovery of a single hypothesis on a surprise basis. Merton was preoccupied with how verifications through research feed back into and modify theory. Thus, he was concerned with grounded modifying of theory, not grounded generating of theory. Social Theory and Social Structure (Glencoe, Ill.: Free Press, 1949), Chapter III.

2. In principle any concept can be operationalized in quantitative ways, but the sociologist should develop his concepts to facilitate operation.

3. Of course, the researcher does not approach reality as a tabula rasa. He must have a perspective that will help him see relevant data and abstract significant categories from his scrutiny of the data. We shall discuss this issue more fully in Chapters II and XI.
understand it, while sociologists who work in other areas will recognize an understandable theory linked with the data of a given area.

Theory based on data can usually not be completely refuted by more data or replaced by another theory. Since it is too intimately linked to data, it is destined to last despite its inevitable modification and reformulation. The most striking examples are Weber's theory of bureaucracy and Durkheim's theory of suicide. These theories have endured for decades, stimulating a variety of research and study, constantly exciting students and professors alike to try to modify them by clever ways of testing and reformulation. In contrast, logically deduced theories based on ungrounded assumptions, such as some well-known ones on the "social system" and on "social action" can lead their followers far astray in trying to advance sociology. However, grounded theories—which take hard study of much data—are worth the precious time and focus of all of us in our research, study and teaching.

Grounded theory can help to forestall the opportunistic use of theories that have dubious fit and working capacity. So often in journals we read a highly empirical study which at its conclusion has a tacked-on explanation taken from a logically deduced theory. The author tries to give his data a more general sociological meaning, as well as to account for or interpret what he found. He uses this strategy because he has not been trained to generate a theory from the data he is reporting so that it will help interpret or explain the data in a general manner. He does this also because he has been trained only to research and verify his facts, not also to research and generate his explanation of them. The explanation is added afterward. For instance, many papers dealing with deviance conclude with an interpretation based on Merton's anomie theory, a classic example of this use of logically deduced theory. An author could, of course, borrow the grounded theory of another sociologist for its general relevance, but—since this kind of theory fits and works—it would readily be seen whether it is clearly applicable and relevant in this new situation. It cannot be tem-

4. And also in trying to advance their personal careers, for one cannot empirically dissociate the need to generate theory from the need to advance careers in sociology.

ously connected, omitting of many other possible explanations, as a tacked-on explanation so often is.

Another opportunistic use of theory that cannot occur with grounded theory is what may be termed "exampleing." A researcher can easily find examples for dreamed-up, speculative, or logically deduced theory after the idea has occurred. But since the idea has not been derived from the example, seldom can the example correct or change it (even if the author is willing), since the example was selectively chosen for its confirming power. Therefore, one receives the image of a proof when there is none, and the theory obtains a richness of detail that it did not earn.

There is also a middle zone between grounded and logico-deductive theorizing, in which the sociologist chooses examples systematically and then allows them to feed back to give theoretical control over his formulations; but often it is hard to figure out when this is happening, even when we are clearly told. Much of C. Wright Mills' work, we believe, is exampled with only little theoretical control, though he claimed that data disciplined his theory. In contrast, grounded theory is derived from data and then illustrated by characteristic examples of data.

In contrasting grounded theory with logico-deductive theory and discussing and assessing their relative merits in ability to fit and work (predict, explain, and be relevant), we have taken the position that the adequacy of a theory for sociology today cannot be divorced from the process by which it is generated. Thus one canon for judging the usefulness of a theory is how it was generated—and we suggest that it is likely to be a better theory to the degree that it has been inductively developed from social research. We also believe that other canons for assessing a theory, such as logical consistency, clarity, parsimony, density, scope, integration, as well as its fit and its ability to work, are also significantly dependent on how the theory was generated. They are not, as some theorists of a logico-deductive persuasion would claim, completely independent of the processes of generation. This notion of independence too often ends up being taken as a license to generate theory from any source—

5. See, for example, Howard S. Becker et al., Boys in White (Chicago: University of Chicago Press, 1961).
happenstance, fantasy, dream life, common sense, or conjecture—and then dress it up as a bit of logical deduction.

Probably we need to emphasize here what we shall discuss later more explicitly. Generating a theory from data means that most hypotheses and concepts not only come from the data, but are systematically worked out in relation to the data during the course of the research. Generating a theory involves a process of research. By contrast, the source of certain ideas, or even "models," can come from sources other than the data. The biographies of scientists are replete with stories of occasional flashes of insight, of seminal ideas, garnered from sources outside the data. But the generation of theory from such insights must then be brought into relation to the data, or there is great danger that theory and empirical world will mismatch. We shall discuss this issue again more fully, particularly in Chapter XI on "Insight, Theory Development, and Reality."

For many colleagues, our position will be at best a hypothesis, to be tested in the years to come; while for many others it is proven fact, and for still others an article of faith. However colleagues may respond, our position is not logical; it is phenomenological. We could not suggest a process of generating theory if we did not believe that people who might use it would arrive at results that potentially may be judged as successful. Furthermore, we believe that grounded theory will be more successful than theories logically deduced from a priori assumptions. Our position, we hasten to add, does not at all imply that the generation of new theory should proceed in isolation from existing grounded theory. (We shall discuss this in Chapter II.)

**Purposes of This Book**

This book is intended to underscore the basic sociological activity that only sociologists can do: generating sociological theory. Description, ethnography, fact-finding, verification (call them what you will) are all done well by professionals in other fields and by layman in various investigatory agencies. But these people cannot generate sociological theory from their...
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The discussion open-minded, to stimulate rather than freeze thinking about the topic. Our suggestions are deliberately interspersed with occasional frank polemic—always, we hope, with purpose—though not at the expense of stopping the flow of suggested procedures or the logic lying behind them. In using examples from research, we have drawn heavily upon our own work—and for a very good reason. We know others' work as published product; we know our own better as work-in-process—and discovering theory as a process is, of course, the central theme of this book.

In the first section—Comparative Analysis—we shall present a strategy whereby sociologists can facilitate the discovery of grounded theory, both substantive and formal. This strategy involves the systematic choice and study of several comparison groups. In Chapter II we discuss the purpose of our use of comparative analysis. In Chapter III we discuss theoretical sampling—the process of collecting data for comparative analysis designed to generate substantive and formal theory. In Chapter IV we take up the transition from substantive to formal theory. And in Chapter V we offer our method for the comparative analysis of qualitative data. In Chapter VI we clarify and assess a number of previous comparative studies in terms of several important questions.

In the second part of the book—The Flexible Use of Data—we consider in detail the generation of theory from qualitative (especially documentary) and quantitative data (in Chapters VII and VIII, respectively).

In the third part of the book—Implications of Grounded Theory—we consider the credibility of grounded theory (Chapter IX) and its practical implications (Chapter X). Lastly, in Chapter XI we discuss insight, theory development and reality. We close with an epilogue summarizing our position on the relations of theory to research.

Before moving on to these chapters, we shall discuss the contemporary emphasis on verification, the influential style of logico-deductive theorizing, which encourages the drive toward verification, and the distinction usually drawn between qualitative and quantitative data—a distinction useless for the generation of theory.

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**Verification and “Grand” Theory**

Verification of theory is the keynote of current sociology. Some three decades ago, it was felt that we had plenty of theories but few confirmations of them—a position made very feasible by the greatly increased sophistication of quantitative methods. As this shift in emphasis took hold, the discovery of new theories became slighted and, at some universities, virtually neglected. Those who still wished to generate theory had to brook the negative, sometimes punitive, attitudes of their colleagues or professors.

Part of the trend toward emphasizing verification was the assumption by many sociologists that our “great men” forefathers (Weber, Durkheim, Simmel, Marx, Veblen, Cooley, Mead, Park, etc.) had generated a sufficient number of outstanding theories on enough areas of social life to last for a long while. Although we, their sociological offspring, could never equal their genius, we did know how to modify and re-formulate their theories with our new-found abilities in verification—and so that was the next job of sociology. As a result, many of our teachers converted departments of sociology into mere repositories of “great-man” theories and taught these theories with a charismatic finality that students could seldom resist. Currently, students are trained to master great-man theories and to test them in small ways, but hardly to question the theory as a whole in terms of its position or manner of generation. As a result many potentially creative students have limited themselves to puzzling out small problems bequeathed to them in big theories. A few men (like Parsons and Merton) have seen through this charismatic view of the great men sufficiently to generate “grand” theories on their own. But even these few have lacked methods for generating theory from data, or at any rate have not written about their methods. They have played “theoretical capitalist” to the mass of “proletariat” testers.

10. The following are the words of a young theoretical capitalist modestly asking the proletariat testers to correct his conjectured theory: “Whereas empirical tests would undoubtedly prove a good proportion of the inferred predictions to be incorrect, these negative findings would provide a basis for refining the theory, whereas as no such refinements are possible if a theory fails to yield operational hypotheses that can be negated by empirical evidence.” Thus to encourage the testers he carefully writes his theory so it can be readily operationalized and proven wrong in several ways—a temptation for those who like to prove the theorist wrong. These proletariat testers do not realize that allowing themselves to be tempted simply puts the refined theory and the theorist on firmer ground, while they are soon forgotten. See Peter Blau, *Exchange and Power in Social Life* (New York: John Wiley and Sons, 1964), p. 9. We can only say that it is our position that theorists be responsible for the grounding of their theories from the start.


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Verification or Generation?

The following account is an example of the kind of historical circumstance that put the generation of grounded theory into second place, and made verification the dominant orientation in virtually all sociological work.

During 1938 the Social Science Research Council struck upon the idea of subjecting to critical appraisal a series of significant contributions to social science. In sociology, Herbert Blumer was assigned the task of appraising Thomas and Znaniecki's great monograph, _The Polish Peasant in Poland and America_. A year later Blumer's critique was published by the Council. The volume included comments on Blumer's analysis by Thomas and by Znaniecki, as well as a reprinting of the proceedings of a conference that discussed the analysis (the conference included such participants as Murdock, Wirth, Bain, Wiley and Wailer).

Blumer noted that Thomas and Znaniecki had been much concerned with methodological issues and had taken a stand against several types of knowledge then much advocated. These latter included "common sense generalization," "planless empiricism," mere statements of uniformities of social behavior in response to social influences," "statements of causal influences which hold true 'on the average,' or 'in a majority of cases,'" and a type of misleading oversimplification in which "effort is made to resolve what must be taken as a primary relation into simpler elements." In contrast, the monograph was directed at furthering general sociological theory and giving a very detailed interpretation of Polish peasant society in Europe and America.

Blumer's principal criticism of _The Polish Peasant_ was directed at what he believed was an important methodological flaw in it—one that needed to be discussed as an issue basic to sociological research rather than as pertinent merely to this particular monograph. The authors claimed that their analyses rested largely on numerous "human documents": letters, agency records, life histories, court records. Blumer noted first that not all—perhaps not even the major—theoretical conceptions used by Thomas and Znaniecki were grounded on those documents. Indeed, "the major outlines are foreshadowed in the previous writings of Thomas," and even "their particular interpretations of Polish peasant life were not formed solely from the materials they present; we have to assume that the familiarity with Polish peasant life which enabled their interpretations was made in a wide variety of ways."

But this was only a minor criticism. Blumer's major concern was this: "the important question is whether the materials adequately test the generalizations (regardless of their source) which are being applied to the materials . . . ." But "the answer is very inconclusive." Some interpretations seemed to him to be borne out by the materials; some did not. Worse yet, usually one could not say that "the interpretation is either true or not, even though it is distinctly plausible." (pp. 74-75). Blumer agreed that these plausible interpretations made the materials more significant and made "theoretical interpretation more understandable." Yet the very puzzling issue of plausible interpretation versus genuine verification remained.

Therefore Blumer concluded, first, that the materials were not a decisive test of theoretical interpretations, although they did more than simply illustrate them; second, that a test of "theory would have to come in other ways, such as in its internal consistency, in the character of its assumptions, in its relation to other theories, in its consistency with what seems to be 'human,' or in other kinds of data than those provided by human documents"; and, third, that the authors' use of human documents would seemingly imply that their essential function "would be to . . . yield to a sensitive and inquiring mind
hunches, insights, questions suitable for reflection, new perspectives, and new understandings” (pp. 75-76). In short, the data were useful for theorizing but not adequate for verification. Blumer’s critique was written during the period when Stouffer, Chapin, Lazarsfeld, Guittman and other advocates of better (quantitative) measures for checking theory began to exert great influence in sociology. The emphasis in Blumer’s critique on verification, then, fit the mood of the day. Yet the enormous influence of The Polish Peasant for two decades was less the result of its demonstrable findings than of its stimulating theory. With hindsight, we can wonder what might have happened if Blumer had focused less on the problem of verification and more on generation. He did, of course, come close to emphasizing the latter, since he raised the issue of how to theorize from data rather than from the armchair. But, as we see it, whatever his intent, Blumer threw the weight of his analysis toward an examination of verification, rather than toward the question of how to generate grounded theory. He left that latter problem largely untouched, apparently assuming that the most one could say was that good theory is produced by a fortunate combination—an inquiring mind, rich experience, and stimulating data.15

Znaniecki’s rejoinder to Blumer’s critique on the verification issue is also instructive. He agreed that his monograph’s materials did not always provide a good test of the theoretical formulations, but he attributed this to “the inadequacy of that general conceptual framework with which we approached our data.”

15. A year later, Blumer published an admirable article, addressing himself to the gap between ungrounded theories and the countless empirical studies unguided by any theories. Operationalism was then coming into dominance, and he attacked it effectively as not offering a solution to closing the gap. Closing it, he believed, would depend on “developing a rich and intimate familiarity with the kind of conduct being studied and in employing whatever relevant imagination observers may fortunately possess. The improvement in judgment, in observation, and in concept will be in the future, as . . . in the past, a slow maturing process.” His emphasis on the meaning of the theory-data gap and on the requisite need for good qualitative data, we agree with thoroughly. Blumer’s solution to getting better theory, and in close relation to data, was—again—blunted because he was poised in too sharp a posture against verification (operationalism in this instance), and too ready to give up on the problem of how to generate better theory except by the general formula of sticking close to the data being studied. See his “The Problem of the Concept in Social Psychology,” American Journal of Sociology (1940), 707-19; the quotes are from pp. 718-19.

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By “framework,” Znaniecki referred to the “excessive simplicity of the ‘attitude-value’ conceptual combinations”—the principal theoretical conception that organized the monograph. Znaniecki would substitute a more sophisticated conception involving “system” and “pattern” (which he believed had been implicit anyhow in the monograph) which would have demanded fuller qualitative data of various kinds. He was still thinking of the generation of theory largely in terms of a pre-existent conceptualization; he was still not emphasizing methods for generating grounded theory.

Qualitative vs. Quantitative Data

Historically linked with the change in relative emphasis from generation to verification of theory was the clash between advocates of quantitative and qualitative data. The generators of theory in the late 1930’s, by and large, had used qualitative data in a nonsystematic and nonrigorous way (when they used data at all), in conjunction with their own logic and common sense. In addition, monographs based on qualitative data consisted of lengthy, detailed descriptions which resulted in very small amounts of theory, if any.16 The effort in these monographs was to “get the story straight.” In short, the work based on qualitative data was either not theoretical enough or the theories were too “impressionistic.”

Meanwhile, beginning in the late 1930’s, and especially after World War II, quantitative researchers made great strides both in producing accurate evidence and in translating theoretical concepts into research operations. The result was an ability to begin the challenge of testing theory rigorously.

Thus, advances in quantitative methods initiated the zeal to test unconfirmed theories with the “facts.” Qualitative research, because of its poor showing in producing the scientifically reproducible fact, and its sensitivity in picking up everyday facts about social structures and social systems, was relegated, by men like Stouffer and Lazarsfeld, to preliminary, exploratory, groundbreaking work for getting surveys started. Qualitative research was to provide quantitative research with a few sub-

16. For example, see the various studies of the Chicago school on the gang, the ghetto, the taxi-dance hall, the hoboes, etc.
stantive categories and hypotheses. Then, of course, quantitative research would take over, explore further, discover facts and test current theory.

The strength of this position, which soon swept over American sociology, was based on the emerging systematic canons and rules of evidence of quantitative analysis: on such issues as sampling, coding, reliability validity, indicators, frequency distributions, conceptual formulation, hypothesis construction, and parsimonious presentation of evidence. The methods of qualitative researchers on these issues had not been developed to the point where they offered any assurance of their ability to assemble accurate evidence and to test hypotheses. Indeed, in sociology the only qualitative methods receiving much development were for the quantification of qualitative data. The assumption behind, and because of, these developments was that sociology was embarked on a straight-line course of progress toward becoming a science, by virtue of quantitative verifications of hypotheses.

A smaller number of sociologists did take other positions, in their research and teaching, but they began—and still continue today—to use the verification rhetoric in talking of qualitative data (testing, proving, tentativeness, demonstrating, and so forth). One position was “since we are so accustomed to qualitative data, let’s verify with such data, as they do with quantitative data.” These advocates tried to systematize the ways they collected, assembled and presented qualitative materials. Sometimes they used quantifying techniques, but their systemization was far broader. Virtually every maneuver was accomplished according to precise patterns—for example, how interviews or observation were recorded, coding procedures accomplished, modeled analyses done, and concepts clarified. The path to systematization was guided (as this book has been) by the pressure that quantitative verifications had put on all sociologists to clarify and codify all research operations, no matter what the type of data or the content of the research report.17

17. For clarifications and codifications of qualitative methods see, for example, the articles in Richard N. Adams and Jack J. Preiss (Eds.), *Human Organization Research* (Homewood, Ill.: Dorsey Press, 1960). The call to codify and clarify all methods, including qualitative research was earlier given in 1949 by Robert K. Merton, op. cit., p. 390.

Another position taken by advocates of qualitative data has been that these data were their media and therefore were still the best and richest for theorizing about social structures and social systems. Also, qualitative method still was the only way to obtain data on many areas of social life not amenable to the techniques for collecting quantitative data. The fascinating fact about people who have taken this stand is that they have continued to generate theories from qualitative data, realizing its importance, and yet they have not explicitly referred to their work as generating theory (or have not described how they generated theory or how it was relevant) because they have been too concerned with formulating their ideas within the rhetoric of verification! In reading their writings, one constantly finds that they make qualifications using the verification terminology, such as “the hypothesis is tentative,” “we had only a few cases,” “we need more denite proofs in future research,” and “we checked this out many times.” We cannot evaluate how well their theories were generated, because we are seldom told of what use the theories are in prediction, application and explanation, or what procedures led to suggested hypotheses.

The position of the logico-deductive theorists also became subordinated to the rhetoric of verification. Since they did not use data for generating theory anyway, they supported qualitative verifications as the best way to reformulate and modify their theories. This meant, of course, that they supported the trend in sociology that pointed toward the perfection of their own theories by other men. They could not lose. As we have remarked earlier, they never mentioned the lost emphasis on generating theory, since perhaps they wanted their work to be tested and only slightly modified rather than replaced.

Our position in this book is as follows: there is no fundamental clash between the purposes and capacities of qualitative and quantitative methods or data. What clash there is concerns the primacy of emphasis on verification or generation of theory—to which heated discussions on qualitative versus quantitative data have been linked historically.18 We believe that each form

18. In the 1930’s, men like E. W. Burgess attempted to mediate between the antagonists, using both types of data in their research. But inevitably they leaned toward the Stouffer-Lazarsfeld position that qualitative data was exploratory in function, thus neutralizing its generative possibilities.
of data is useful for both verification and generation of theory, whatever the primacy of emphasis. Primacy depends only on the circumstances of research, on the interests and training of the researcher, and on the kinds of material he needs for his theory.

In many instances, both forms of data are necessary—not quantitative used to test qualitative, but both used as supplements, as mutual verification and, most important for us, as different forms of data on the same subject, which, when compared, will each generate theory (see Chapter III).

To further this view, we seek in this book to further the systematization of the collection, coding and analysis of qualitative data for the generation of theory. We wish particularly to get library and field research off the defensive in social research, and thereby encourage it. Although the emphasis on qualitative data is strong in our book, most chapters also can be used by those who wish to generate theory with quantitative data, since the process of generating theory is independent of the kind of data used. (See particularly Chapters II and VIII, on theoretical sampling and quantitative data.)

We focus on qualitative data for a number of other reasons: because the crucial elements of sociological theory are often found best with a qualitative method, that is, from data on structural conditions, consequences, deviances, norms, processes, patterns, and systems; because qualitative research is, more often than not, the end product of research within a substantive area beyond which few research sociologists are motivated to move; and because qualitative research is often the most "adequate" and "efficient" way to obtain the type of information required and to contend with the difficulties of an empirical situation. We wish also through this book to provide sociologists with a set of categories for writing their theories within a rhetoric of generation, to balance out that of verification.

19. See James Coleman's discussion of the relative merits of qualitative and quantitative research in analyzing the "working parts of a system," "Research Chronicle: The Adolescent Society," in Philip E. Hammond, op. cit., pp. 190-193, 206. Coleman agrees with us, but he is not aware that the benefits that he suggests for a "comparative quantitative analysis" can also be obtained with a "comparative qualitative analysis," as we shall show in this book.
II

Generating Theory

The term comparative analysis—often used in sociology and anthropology—has grown to encompass several different meanings and thereby to carry several different burdens. Many sociologists and anthropologists, recognizing the great power of comparative analysis, have employed it for achieving their various purposes. To avoid confusion, we must, therefore, be clear at the outset as to our own use for comparative analysis—the generation of theory. We shall first contrast our use of this method with certain other uses. Then we shall define and describe what kind of theory can be generated through comparative analysis.

Comparative analysis is a general method, just as are the experimental and statistical methods. (All use the logic of comparison.) Furthermore, comparative analysis can, like those other methods, be used for social units of any size. Some sociologists and anthropologists customarily use the term comparative analysis to refer only to comparisons between large-scale social units, particularly organizations, nation, institutions, and large regions of the world. But such a reference restricts a general method to use with one specific class of social units to which it has frequently been applied. Our discussion of comparative analysis as a strategic method for generating theory assigns the method its fullest generality for use on social units of any size, large or small, ranging from men or their roles to

1. In Chapter VI, we discuss in detail a number of studies in which "comparative method" was used, examining them for their specific purposes and distinguishing them from our own suggested purpose.
nations or world regions. Our own recent experience has demonstrated the usefulness of this method for small organizational units, such as wards in hospitals or classes in a school.

Before distinguishing our purpose in using comparative analysis from other purposes, we should mention one unfortunate use of comparisons: to debunk, disprove, or discount the work of colleagues. From his own readings, a sociologist can almost always find, if he wants to, some piece of data that disproves the fact on which his colleague has based a theoretical notion. Many sociologists do! If each debunker thought about the potential value of comparative analysis, instead of satisfying his urge to “put down” a colleague, he would realize that he has merely posed another comparative datum for generating another theoretical property or category. That is all he has done. Nothing is disproved or debunked, despite what those who are overly concerned with evidence constantly believe. Kinds of colleagues, who present a sociologist with one or more negative case but are afraid of impairing his motivation, usually will suggest that some qualification in his theoretical assertion may be advisable. Their comparative analysis aids him in rounding out his own comparative analysis and further generating his theory.

We also intend to hold a dialogue with those who “put down” the comparative strategy as “not especially original.” True, the general notion of comparative analysis was developed by our sociological forefathers—Weber, Durkheim, Mannheim—and by social anthropologists. We can only trust that our readers will absorb enough details of comparative analysis as rendered in this book to be able to spot the advances in the strategy that should make a world of difference in its use.

Purposes of Comparative Analyses

The distinction made earlier between relative emphasis on generating and verifying can be illuminated further by considering the typical uses of evidence obtained through comparative studies.


Generating Theory

Accurate Evidence

On the factual level, evidence collected from other comparative groups—whether nations, organizations, counties, or hospital wards—is used to check out whether the initial evidence was correct. Is the fact a fact? Thus, facts are replicated with comparative evidence, either internally (within a study), externally (outside a study), or both. Sociologists generally agree that replications are the best means for validating facts.

Although this use of comparative analysis is not, of itself, our goal, it is definitely subsumed under our goal. Naturally we wish to be as sure of our evidence as possible, and will therefore check on it as often as we can. However, even if some of our evidence is not entirely accurate this will not be too troublesome; for in generating theory it is not the fact upon which we stand, but the conceptual category (or a conceptual property of the category) that was generated from it. A concept may be generated from one fact, which then becomes merely one of a universe of many possible diverse indicators for, and data on, the concept. These indicators are then sought for the comparative analysis. (See Chapters III and IV.)

In discovering theory, one generates conceptual categories or their properties from evidence; then the evidence from which the category emerged is used to illustrate the concept. The evidence may not necessarily be accurate beyond a doubt (nor is it even in studies concerned only with accuracy), but the concept is undoubtedly a relevant theoretical abstraction about what is going on in the area studied. Furthermore, the concept itself will not change, while even the most accurate facts change. Concepts only have their meanings respecified at times because other theoretical and research purposes have evolved.

For example, one theoretical category related to the care of dying patients is their social loss—loss to family and occupation. This category clearly affects how nurses care for dying


patients. The category of "social loss" can be generated from either the observation that VIP's receive special care on intensive care units or that lower-class Negroes often are neglected on city hospital emergency wards. Even if the evidence changes (or is different in other hospitals for various other reasons), we can be sure that social loss is a category related to nursing care, and we can make predictions on its basis. We can predict that patients who have high social loss will receive better care than those who have low social loss. If that prediction proves incorrect, then we are likely to find out next what structural conditions have tended to negate this relationship; for example, how the medical staff has overcome this socially induced tendency in one type of hospital. In short, the discovered theoretical category lives on until proven theoretically defunct for any class of data, while the life of the accurate evidence that indicated the category may be short.

Empirical Generalizations

Another standard use of comparative studies is to establish the generality of a fact. Does the incest taboo exist in all societies? Are almost all nurses women? Is basic research the most revered goal of scientists in all research organizations? Accuracy is not at stake so much as establishing the structural boundaries of a fact; where is the fact an accurate description? For some sociologists and anthropologists this purpose becomes a quest for "universals"—facts and their explanations by other facts—that apply to all men irrespective of their society or culture.

Our goal of generating theory also subsumes this establishing of empirical generalizations, for the generalizations not only help delimit a grounded theory's boundaries of applicability; more important, they help us broaden the theory so that it is more generally applicable and has greater explanatory and predictive power. By comparing where the facts are similar or different, we can generate properties of categories that increase the categories' generality and explanatory power.

For example, dying of cancer in America can be characterized as occurring in a "closed awareness context"—while the hospital staff does, the patient does not know he is dying. Most doctors do not tell their patients that their illness is terminal, and patients find that cues that might alert them that they are dying are vague and hard to read until the last stages of their dying. In a Japanese hospital we once visited, cancer patients typically know they are dying (an "open awareness context"). Why? Because the hospital ward is openly labeled "Cancer." The patient entering the ward reads a clear cue that makes him aware that he is dying. While in America the cues tend to be vague and fleeting, we discovered through the Japanese example that they can be clear even at the beginning stage of a long term of dying. Until then, we had not realized that cues can vary in clarity at the beginning of such a disease as cancer. We had thought that clear cues emerged only during the final stages; for example, when the priest arrives, or the patient's pain is beyond endurance, or massive bodily degeneration occurs.

This comparative data from Japan stimulated us to find locations in America where clear cues are provided at the start of dying. We found that in a veterans' hospital and in a prison medical ward, patients from the outset were given clear cues that they had cancer. Thus we discovered that under the structural condition of being a captive patient in a government hospital, one tends to die in an open awareness context. But most patients in America do not die under such circumstances.

Specifying a Concept

Another (usually detailed and painstaking) use of comparative data is to specify a unit of analysis for a one-case study. This is done by specifying the dimensions of the concept designating the unit. To make certain the reader understands what a given monograph will be about, in comparison with seemingly similar units, the author compares his unit for analysis with these other units. His comparison brings out the distinctive elements or nature of the case he has studied. For instance, Cressey painstakingly compared taxi-dance halls with all other forms of dance halls before proceeding with his analysis. Lipset,

5. Glaser and Strauss, Awareness . . . , op. cit., Chapters 3 and 8.
Trow and Coleman compared the distinctive political nature of the ITU with the characteristic political structure of other unions to establish their “deviant” case study. Wirth compared the Chicago ghetto with the European to establish distinctive changes in the new-world ghetto. Coleman, with the aid of IBM equipment, carefully distinguished between types of high schools on three dimensions, themselves checked out empirically to assure us that they are different in more than script.

This standard, required use of comparative analysis is accomplished early in the presentation of a study for the purpose of getting the ensuing story straight. This use is, of course, subsumed under the purpose of generating theory. However, when the analyst’s purpose is only the specifying of a unit of analysis, he stifies his chances for generating to a greater degree than with any other use of comparative analysis. The distinctive empirical elements distinguishing the units of comparison are kept on the level of data, to insure clear understanding of differential definitions. As a consequence, the units’ general properties in common, which might occur to the analyst as he compares, are carefully unattended. No ambiguity of similarity, such as a general underlying property pervading all of them, is allowed between the competing units. Comparative analysis, then, is carefully put out of the picture, never to “disrupt” the monologue again.

Verifying Theory

When the analyst turns to theoretical concerns, evidence is invariably used as a test of his hypotheses—and thereby of the relevance of his categories; comparative data give the best test. Both implicitly and explicitly, the analyst continually checks out his theory as the data pour in. Explicit verification beyond testing his hypotheses may lead to establishing major uniformities and universals, to strategic variations of theory under different conditions, and to grounded modifications of theory. A touch of generation may be included, but the researcher’s focus is on verifying; he generates theory only in the service of modifying his original theory as a result of the tests. And most of this work is done with existing theories; for example, Blauner’s work with Marxian theory or Lipset’s work with Michel’s theory.

Some analysts focus on verifying the new theory that emerges in their data. Thus, in their work, theory is generated, but its emergence is taken for granted; what is intentionally worked for is the verification of this emergent theory. The analysts are preoccupied with “checking out” the “emergent set of propositions.” Their favorite technique is looking for negative cases or setting out deliberately to accumulate positive ones to gain further evidence for their hypotheses. And while, as in Dalton’s research, great trouble may be taken in actively seeking comparative groups, other analysts may use comparative groups incidentally or even implicitly.

These researchers in specific studies do not seem to have focused directly on how their theory emerged; as a result, they have not explored how they could have generated more of it more systematically, and with more conceptual generality and scope. A focus on testing can thus easily block the generation of a more rounded and more dense theory (see Chapter VI). Ordinarily, we are presented with well-tested theory fragments, which can only partially account for what is happening in the researched situation. Also, we are presented with plenty of evidence, coupled with at least implicit assurances that there were mountains more for verification—because evidence is still most important to the analyst as the means for testing how he

12. See Blauner, op. cit. and Lipset et al., op. cit.
knew his theory was "right." This focus on evidence paradoxically allows cantankerous colleagues, with their own different comparative evidence or personal experience, to "pooh-pooh" his theory, wholly or in part.

Generating Theory

While verifying is the researcher's principal and vital task for existing theories, we suggest that his main goal in developing new theories is their purposeful systematic generation from the data of social research. Of course, verifying as much as possible with as accurate evidence as possible is requisite while one discovers and generates his theory—but not to the point where verification becomes so paramount as to curb generation. Thus, generation of theory through comparative analysis both subsumes and assumes verifications and accurate descriptions, but only to the extent that the latter are in the service of generation. Otherwise they are sure to stifle it. To be sure, the urge to generate is normal; and sociologists, students and professors alike, if they are not "hooked" on verifying, tend to give themselves enthusiastically to generating. But when generating is not clearly recognized as the main goal of a given research, it can be quickly killed by the twin critiques of accurate evidence and verified hypotheses. This happens especially when the critiques are made by an influential colleague or professor. The analyst's confidence is destroyed because everyone involved fails to realize that accurate description and verification are not so crucial when one's purpose is to generate theory. This is especially true because evidence and testing never destroy a theory (of any generality), they only modify it. A theory's only replacement is a better theory.

When the vital job of testing a newly generated theory begins, the evidence from which it was generated is quite likely to be forgotten or ignored. Now, the focus is on the new evidence that will be used for verifying only a part of the theory. Furthermore, sociologists will find it worthwhile to risk a period in their careers in order to test grounded theories, since these theories are certain to be highly applicable to areas under study. This situation is in contrast to the risk of testing a logico-deductive theory, which is dubious related to the area of behavior it purports to explain, since it was merely thought up on the basis of a priori assumption and a touch of common sense, peppered with a few old theoretical speculations made by the erudite. The verifier may find that the speculative theory has nothing to do with his evidence, unless he forces a connection.

Generating theory carries the same benefit as testing theory, plus an additional one. Verifying a logico-deductive theory generally leaves us with at best a reformulated hypothesis or two and an unconfirmed set of speculations; and, at worst, a theory that does not seem to fit or work (and perhaps the uncomfortable feeling that some "thinker" might have been playing with us). A grounded theory can be used as a fuller test of a logico-deductive theory pertaining to the same area by comparison of both theories than an accurate description used to verify a few propositions would provide. Whether or not there is a previous speculative theory, discovery gives us a theory...
that "fits or works" in a substantive or formal area (though further testing, clarification, or reformulation is still necessary), since the theory has been derived from data, not deduced from logical assumptions.

Since accurate evidence is not so crucial for generating theory, the kind of evidence, as well as the number of cases, is also not so crucial. A single case can indicate a general conceptual category or property; a few more cases can confirm the indication. As we note in the next chapter, generation of theoretical sampling requires by comparative analysis requires a multitude of carefully selected cases, but the pressure is not on the sociologist to "know the whole field" or to have all the facts "from a careful random sample." His job is not to provide a perfect description of an area, but to develop a theory that accounts for much of the relevant behavior. The sociologist with theoretical generation as his major aim need not know the concrete situation better than the people involved in it (an impossible task anyway). His job and his training are to do what these laymen cannot do—generate general categories and their properties for general and specific situations and problems. These can provide theoretical guides to the layman's action (see Chapter X on practical applications). The sociologist thereby brings sociological theory, and so a different perspective, into the situation of the layman. This new perspective can be very helpful to the latter.

Sociologists who conceive of this task as their job are not plagued (as are those who attempt to report precise description) by thoughts such as "everybody knows it, why bother to write a book"; or feelings that description is not enough: a good sociologist from Chicago must do more, but what? Sociologists who set themselves the task of generating theory from the data of social research have a job that can be done only by the sociologist, and that offers a significant product to laymen and colleagues alike. Research sociologists in their driving efforts to get the facts tend to forget that, besides methodology, the distinctive offering of sociology to our society is sociological theory, not only researched description. Indeed, the market, corporate, and government fact-finding agencies can easily outdo any sociologist in researched descriptions through sheer resources, if they care to. Where the sociologist can help these agencies is by providing them with theory that will make their research relevant. And, as a brief reading of typical fact-finding and market-research reports indicates, sociological relevance is sorely needed both for understanding the "dust heap" of data piled up by agencies and for correcting the conventional ideology that guides this piling up of data.

What Theory Is Generated

This book is about the process of generating grounded theory, and so our polemic is with other processes of arriving at theory, particularly the logico-deductive. Grounded theory, it should be mentioned, may take different forms. And although we consider the process of generating theory as related to its subsequent use and effectiveness, the form in which the theory is presented can be independent of this process by which it was generated. Grounded theory can be presented either as a well-codified set of propositions or in a running theoretical discussion, using conceptual categories and their properties.

20. We are in complete agreement with Zetterberg on this issue of whether sociology will advance more by concentrating on theory or on methodology. But we feel that a methodology of generating it is needed for theoretical advance. See Hans L. Zetterberg, On Theory and Verification in Sociology (Totowa, N.J.: Bedminster Press, 1963), Preface.
21. A good instance is the sociological relevance of vast amounts of governmental statistics on the differential medical care of socioeconomic strata in America. The common-sense meaning of these statistics is almost self-evident, but deeper sociological significance neither guides these governmental surveys nor much affects agency policies. What sociologists know about socioeconomic life styles and about the organization of medical facilities can easily be brought to bear upon government data. See policy paper on medical care by Anselm Strauss, written for the Institute for Policy Studies (Washington, D.C., July, 1965).
22. This choice is not news, since most theory is written this way, whether grounded or logico-deductive. But we have noted this decision, on the request of several colleagues, to fend off the critique that the only true theory is the one written, by the numbers, as an integrated set of propositions. The form in which a theory is presented does not make it a theory; it is a theory because it explains or predicts something.
We have chosen the discusional form for several reasons. Our strategy of comparative analysis for generating theory puts a high emphasis on theory as process; that is, theory as an ever-developing entity, not as a perfected product. (The reader will see further what we mean in Chapters III and IV.) To be sure, theory as process can be presented in publications as a momentary product, but it is written with the assumption that it is still developing. Theory as process, we believe, renders quite well the reality of social interaction and its structural context.

The discusional form of formulating theory gives a feeling of “ever-developing” to the theory, allows it to become quite rich, complex, and dense, and makes its fit and relevance easy to comprehend. On the other hand, to state a theory in propositional form, except perhaps for a few scattered core propositions, would make it less complex, dense, and rich, and more laborious to read. It would also tend by implication to “freeze” the theory instead of giving the feeling of a need for continued development. If necessary for verificational studies, parts of the theoretical discussion can at any point be rephrased as a set of propositions. This rephrasing is simply a formal exercise, though, since the concepts are already related in the discussion. Also, with either a propositional or discusional grounded theory, the sociologist can then logically deduce further hypotheses. Indeed, deductions from grounded theory, as it develops, are the method by which the researcher directs his theoretical sampling (see Chapter III).

Substantive and Formal Theory

Comparative analysis can be used to generate two basic kinds of theory: substantive and formal. By substantive theory, we mean that developed for a substantive, or empirical, area of sociological inquiry, such as patient care, race relations, professional education, delinquency, or research organizations. By formal theory, we mean that developed for a formal, or conceptual, area of sociological inquiry, such as stigma, deviant behavior, formal organization, socialization, status congruency, authority and power, reward systems, or social mobility. Both types of theory may be considered as “middle-range.” That is, they fall between the “minor working hypotheses” of everyday life and the “all-inclusive” grand theories.23

Substantive and formal theories exist on distinguishable levels of generality, which differ only in terms of degree. Therefore, in any one study, each type can shade at points into the other. The analyst, however, should focus clearly on one level or other, or on a specific combination, because the strategies vary for arriving at each one. For example, in our analysis of dying as a nonscheduled status passage, the focus was on the substantive area of dying, not on the formal area of status passage.24 With the focus on a substantive area such as this, the generation of theory can be achieved by a comparative analysis between or among groups within the same substantive area. In this instance, we compared hospital wards where patients characteristically died at different rates. The substantive theory also could be generated by comparing dying as a status passage with other substantive cases within the formal area of status passage with other substantive cases within the formal area of status passage, whether scheduled or not, such as studenthood or engagement for marriage. The comparison would illuminate the substantive theory about dying as a status passage.

However, if the focus were on formal theory, then the comparative analysis would be made among different kinds of substantive cases which fall within the formal area, without relating them to any one substantive area. The focus of comparisons is now on generating a theory of status passage, not on generating theory about a single substantive case of status passage.

Both substantive and formal theories must be grounded in data. Substantive theory faithful to the empirical situation cannot, we believe, be formulated merely by applying a few ideas from an established formal theory to the substantive area. To be sure one goes out and studies an area with a particular sociological perspective, and with a focus, a general question, or a problem in mind. But he can (and we believe should) also study an area without any preconceived theory that dictates, prior to the research, “relevancies” in concepts and hypotheses.

23. See Merton, op. cit., pp. 5-10.
Indeed it is presumptuous to assume that one begins to know the relevant categories and hypotheses until the “first days in the field,” at least, are over. A substantive theory generated from the data must first be formulated, in order to see which of diverse formal theories are, perhaps, applicable for furthering additional substantive formulations.

Ignoring this first task—discovering substantive theory relevant to a given substantive area—is the result, in most instances, of believing that formal theories can be applied directly to a substantive area, and will supply most or all of the necessary concepts and hypotheses. The consequence is often a forcing of data, as well as a neglect of relevant concepts and hypotheses that may emerge. Our approach, allowing substantive concepts and hypotheses to emerge first, on their own, enables the analyst to ascertain which, if any, existing formal theory may help him generate his substantive theories. He can then be more faithful to his data, rather than forcing it to fit a theory. He can be more objective and less theoretically biased. Of course, this also means that he cannot merely apply Parsonian or Mertonian categories at the start, but must wait to see whether they are linked to the emergent substantive theory concerning the issue in focus.

Substantive theory in turn helps to generate new grounded formal theories and to re-formulate previously established ones. Thus it becomes a strategic link in the formulation and development of formal theory based on data. For example, in our theory bearing on “awareness contexts” relevant to dying, two important properties are cues leading to awareness and the personal stakes involved in the various parties’ becoming aware. Currently, in generating a formal theory of awareness contexts, we are developing the generalities related to stakes and cues by studying such groups as spies and building subcontractors. A dying patient or a spy has a great stake in any type of awareness context, and a subcontractor has a quantifiable or monetary stake. In Chapter IV, we shall discuss more fully the generation of grounded formal theory. Suffice it to say that we use the word grounded here to underline the point that the formal theory we are talking about must be contrasted with “grand” theory that is generated from logical assumptions and speculations about the “oughts” of social life.

Within these relations existing among social research, substantive theory and formal theory is a design for the cumulative nature of knowledge and theory. The design involves a progressive building up from facts, through substantive to grounded formal theory. To generate substantive theory, we need many facts for the necessary comparative analysis; ethnographic studies, as well as direct gathering of data, are immensely useful for this purpose. Ethnographic studies, substantive theories and direct data collection are all, in turn, necessary for building up by comparative analysis to formal theory. This design, then, locates the place of each level of work within the culmination of knowledge and theory, and thereby suggests a division of labor in sociological work.

This design also suggests that many ethnographic studies and multiple theories are needed so that various substantive and formal areas of inquiry can continue to build up to more inclusive formal theories. Such a call for multiple theories is in contrast to the directly monopolistic implications of logico-deductive theories, whose formulators claim there is only one theory for an area, or perhaps even one sociological theory for all areas. The need for multiple theories on the substantive level may be obvious, but it is not so obvious on the formal level. Yet multiple formal theories are also necessary, since one theory never handles all relevancies, and because by comparing many theories we can begin to arrive at more inclusive, parsimonious levels. The logico-deductive theorist, proceeding under the license and mandate of analytic abstraction, engages in premature parsimony when arriving at his theory. (In Chapters III, IV and V we shall discuss in more detail the relations of research to the generation of substantive and formal theory.)

**Elements of the Theory**

As we shall discuss and use them, the elements of theory that are generated by comparative analysis are, first, conceptual categories and their conceptual properties; and second, hypotheses or generalized relations among the categories and their properties.

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Categories and properties. Making a distinction between category and property indicates a systematic relationship between these two elements of theory. A category stands by itself as a conceptual element of the theory. A property, in turn, is a conceptual aspect or element of a category. We have, then, both categories and their properties. For example, two categories of nursing care are the nurses' "professional composure" and their "perceptions of social loss" of a dying patient that is, their view of what degree of loss his death will be to his family and occupation. One property of the category of social loss is "loss rationales"—that is, the rationales nurses use to justify to themselves their perceptions of social loss. All three are interrelated: loss rationales arise among nurses to explain the death of a patient whom they see as a high social loss, and this relationship helps the nurses to maintain their professional composure when facing his death.

It must be kept in mind that both categories and properties are concepts indicated by the data (and not the data itself); also that both vary in degree of conceptual abstraction. Once a category or property is conceived, a change in the evidence that indicated it will not necessarily alter, clarify or destroy it. It takes much more evidence—usually from different substantive areas—as well as the creation of a better category to achieve such changes in the original category. In short, conceptual categories and properties have a life apart from the evidence that gave rise to them.

The constant comparing of many groups draws the sociologist's attention to their many similarities and differences. Considering these leads him to generate abstract categories and their properties, which, since they emerge from the data, will clearly be important to a theory explaining the kind of behavior under observation. Lower level categories emerge rather quickly during the early phases of data collection. Higher level, overriding and integrating, conceptualizations—and the properties that elaborate them—tend to come later during the joint collection, coding and analysis of the data.

Although categories can be borrowed from existing theory, provided that the data are continually studied to make certain that the categories fit, generating theory does put a premium on emergent conceptualizations. There are a number of reasons for this. Merely selecting data for a category that has been established by another theory tends to hinder the generation of new categories, because the major effort is not generation, but data selection. Also, emergent categories usually prove to be the most relevant and the best fitted to the data. As they are emerging, their fullest possible generality and meaning are continually being developed and checked for relevance. Also the adequacy of indicators for emergent categories is seldom a problem.

By contrast, when we try to fit a category from another theory to the situation under study, we can have much trouble in finding indicators and in getting agreement among colleagues on them. The result is that our forcing of "round data" into "square categories" is buttressed by a long justificatory explanation for the tentative relationship between the two. Forcing data to apply to categories or properties is sure to arouse the disbelief of both colleagues and laymen from the start. Working with borrowed categories is more difficult since they are harder to find, fewer in number, and not as rich; since in the long run they may not be relevant, and are not exactly designed for the purpose, they must be respecified. In short, our focus on the emergence of categories solves the problems of fit, relevance, forcing, and richness. An effective strategy is, at first, literally to ignore the literature of theory and fact on the area under study, in order to assure that the emergence of categories will not be contaminated by concepts more suited to different areas. Similarities and convergences with the literature can be established after the analytic core of categories has emerged.

While the verification of theory aims at establishing a relatively few major uniformities and variations on the same conceptual level, we believe that the generation of theory should aim at achieving much diversity in emergent categories, synthesized at as many levels of conceptual and hypothetical generalization as possible. The synthesis provides readily apparent connections between data and lower and higher level conceptual abstractions of categories and properties.

This position on the diversity of conceptual level has impor-


27. See the case history on this problem confronted by Reisman and Watson, op. cit., pp. 305-49.
tant consequences both for the sociologist and for sociology. As the sociologist uses standard sociological concepts, he soon discovers that they usually become very differently defined, dimensioned, specified, or typed. Typical boundaries of the standard concept become broken. Furthermore, the boundaries of the established battery of sociological concepts are also broken. As he discovers new categories, the sociologist realizes how few kinds of behavior can be coped with by many of our concepts, and recognizes the need to develop more concepts by straying out of traditional research areas into the multitude of substantive unknowns of social life that never have been touched —to give only a few examples, building subcontracting, auctioneering, mortgaging, or the producing of plays by amateur theater groups.

As one thinks about the broad spectrum of social life, one realizes that sociologists (with the focused aid of foundations) have really worked in only a small corner of it when posing the larger questions of deviance, social problems, formal organizations, education, mental health, community government, underdeveloped countries, and so forth. One also realizes that a great many more formal theories of sociology have yet to be generated about such additional areas as loneliness, brutality, resistance, debating, bidding systems, transportation, mail-order distribution, corporate collusion, financial systems, diplomacy, and world interdependence through business systems. One strategy for bringing the generation of theory to greater importance is to work in non-traditional areas where there is little or no technical literature. Finding non-traditional areas is also a strategy for escaping the shackles of existing theory and contemporary emphasis. The sociologist who does so can easily find himself not merely generating a new theory but also opening a new area for sociological inquiry—virtually initiating a new portion of sociology. Whether he studies less or more traditional areas, however, the first requirement for breaking the bounds of established sociology is to generate theory from data.

The type of concept that should be generated has two, joint, essential features. First, the concepts should be analytic—sufficiently generalized to designate characteristics of concrete entities, not the entities themselves. They should also be sensitizing —yield a “meaningful” picture, abetted by apt illustrations that enable one to grasp the reference in terms of one’s own experience. To make concepts both analytic and sensitizing helps the reader to see and hear vividly the people in the area under study, especially if it is a substantive area. This perception, in turn, helps the reader to grasp the theory developed for the area. To formulate concepts of this nature, bringing together the best of two possible worlds, takes considerable study of one’s data and requires considerable data collection of incidents bearing on a category. If, when a category is but scarcely established, the sociologist turns to collecting data for another potential category, slighting the newly established one, the latter is likely to lack development both in sensitizing and in some of its analytic aspects. A balance must be struck between the two lines of effort in accordance with the theoretical saturation of categories (a strategy we shall discuss in Chapter III).

Hypotheses. The comparison of differences and similarities among groups not only generates categories, but also rather speedily generates generalized relations among them. It must be emphasized that these hypotheses have at first the status of suggested, not tested, relations among categories and their properties, though they are verified as much as possible in the course of research.

Whether the sociologist, as he jointly collects and analyzes qualitative data, starts out in a confused state of noting almost everything he sees because it all seems significant, or whether he starts out with a more defined purpose, his work quickly leads to the generation of hypotheses. When he begins to hypothesize with the explicit purpose of generating theory, the researcher is no longer a passive receiver of impressions but is drawn naturally into actively generating and verifying his hypotheses through comparison of groups. Characteristically, in this kind of joint data collection and analysis, multiple hypotheses are pursued simultaneously. Some are pursued over long periods of time because their generation and verification are linked with developing social events. Meanwhile, new hypotheses are continually sought.

Generating hypotheses requires evidence enough only to
establish a suggestion—not an excessive piling up of evidence to establish a proof, and the consequent hindering of the generation of new hypotheses. In field work, however, general relations are often discovered in vivo; that is, the field worker literally sees them occur. This aspect of the “real life” character of field work deserves emphasis, for it is an important dividend in generating theory. (We shall say more about this point when discussing the credibility of analyses of qualitative field data in Chapter IX).

In the beginning, one’s hypotheses may seem unrelated, but as categories and properties emerge, develop in abstraction, and become related, their accumulating interrelations form an integrated central theoretical framework—the core of the emerging theory. The core becomes a theoretical guide to the further collection and analysis of data. Field workers have remarked upon the rapid crystallization of that framework, as well as the rapid emergence of categories. When the main emphasis is on verifying theory, there is no provision for discovering novelty, and potentially illuminating perspectives, that do emerge and might change the theory, actually are suppressed. In verification, one feels too quickly that he has the theory and now must “check it out.” When generation of theory is the aim, however, one is constantly alert to emergent perspectives that will change and help develop his theory. These perspectives can easily occur even on the final day of study or when the manuscript is reviewed in page proof: so the published word is not the final one, but only a pause in the never-ending process of generating theory. When verification is the main aim, publication of the study tends to give readers the impression that this is the last word.

Integration. Integration of the theory—which takes place at the many levels of generality that emerge—does not necessitate a distinction between “working” (or “ordinary”) and theoretical hypotheses. Our emphasis on integration takes into considera-

29. Our colleague, Leonard Schatzman, has called this the “momentum effect.” The emergence of categories and theoretical perspective gains such momentum that a researcher must usually retire from the field after the first few days to appraise the data and establish an order for what is happening. He stops being drowned by the flood of data and starts to plan his theoretical sampling.

30. Zetterberg, op. cit., p. 21, and passim.

For example, our integration of substantive theory on the
social loss of dying patients—under the major categories of calculating social loss, social loss stories and the impact of social loss—includes the effect of the social loss of dying patients on nurses’ attitudes and behavior. We cannot say whether or not this same scheme of interrelations would apply to other substantive theories that deal with the social value of people served by experts. Our substantive integration, however, would provide a useful beginning for integrating a formal theory about the distribution of services as affected by the social value of the people. The move from substantive to formal levels of theorizing is referred to in Chapter III and will be explicitly discussed in Chapter IV.

Paying heed to these strictures on emergence and the application of integrative schemes, as well as to strictures on the emergence of concepts can insure that substantive and formal theories will correspond closely to the “real” world. These rules are beginning descriptions of a process—which we cannot emphasize too strongly—whereby substantive and formal theories that “work” (predict and explain—and do not sound “windy”) are generated from data.

The following chart provides examples of elements of the two kinds of theory that we have discussed:

<table>
<thead>
<tr>
<th>Elements of Theory</th>
<th>Type of Theory</th>
</tr>
</thead>
<tbody>
<tr>
<td>Category</td>
<td></td>
</tr>
<tr>
<td>Social loss of dying patients</td>
<td>Formal</td>
</tr>
<tr>
<td>Properties of Category</td>
<td></td>
</tr>
<tr>
<td>Calculating social loss on basis of learned and apparent characteristics of patient</td>
<td>Social value of people</td>
</tr>
<tr>
<td>Hypotheses</td>
<td></td>
</tr>
<tr>
<td>The higher the social loss of a dying patient, (1) The better his care, (2) The more nurses develop loss rationales to explain away his death</td>
<td>Calculating social value of person on basis of learned and apparent characteristics</td>
</tr>
</tbody>
</table>

33. The way we have integrated a theory of dying as a non-scheduled status passage—legitimating, announcing and coordinating the passage—would provide a useful beginning to the study of status passage in general, “Temporal Aspects of Dying as a Non-Scheduled Status Passage,” op. cit.
Theoretical Sampling

Theoretical sampling is the process of data collection for generating theory whereby the analyst jointly collects, codes, and analyzes his data and decides what data to collect next and where to find them, in order to develop his theory as it emerges. This process of data collection is controlled by the emerging theory, whether substantive or formal. The initial decisions for theoretical collection of data are based only on a general sociological perspective and on a general subject or problem area (such as how confidence men handle prospective marks or how policemen act toward Negroes or what happens to students in medical school that turns them into doctors). The initial decisions are not based on a preconceived theoretical framework.

The sociologist may begin the research with a partial framework of “local” concepts, designating a few principal or gross features of the structure and processes in the situations that he will study. For example, he knows before studying a hospital that there will be doctors, nurses, and aides, and wards and admission procedures. These concepts give him a beginning foothold on his research. Of course, he does not know the relevancy of these concepts to his problem—this problem must emerge—nor are they likely to become part of the core explanatory categories of his theory. His categories are more likely to be concepts about the problem itself, not its situation. Also, he discovers that some anticipated “local” concepts may remain un-
used in the situations relevant to his problem—doctors may, for the problem, be called therapists—and he discovers many more structural and processional "local" concepts than he could have anticipated before his research.

The sociologist should also be sufficiently theoretically sensitive so that he can conceptualize and formulate a theory as it emerges from the data. Once started, theoretical sensitivity is forever in continual development. It is developed as over many years the sociologist thinks in theoretical terms about what he knows, and as he queries many different theories on such questions as "What does the theory do? How is it conceived? What is its general position? What kinds of models does it use?" Theoretical sensitivity of a sociologist has two other characteristics. First, it involves his personal and temperamental bent. Second, it involves the sociologist’s ability to have theoretical insight into his area of research, combined with an ability to make something of his insights (see Chapter XI).

These sources of developing theoretical sensitivity continually build up in the sociologist an armamentarium of categories and hypotheses on substantive and formal levels. This theory that exists within a sociologist can be used in generating his specific theory if, after study of the data, the fit and relevance to the data are emergent. A discovered, grounded theory, then, will tend to combine mostly concepts and hypotheses that have emerged from the data with some existing ones that are clearly useful. We have put most emphasis on the emergent concepts—those coming from the data. Still, whether the theoretical elements are emergent or already exist with fit and relevance that emerges, the strategies of comparative analysis presented in this and the next two chapters apply.

Potential theoretical sensitivity is lost when the sociologist commits himself exclusively to one specific preconceived theory (e.g., formal organization) for then he becomes doctrinaire and can no longer "see around" either his pet theory or any other. He becomes insensitive, or even defensive, toward the kinds of questions that cast doubt on his theory; he is preoccupied with testing, modifying and seeing everything from this one angle. For this person, theory will seldom truly emerge from data. In the few instances where theory does emerge, the preconceived theory is likely to be readily dropped or forgotten because it now seems irrelevant to the data.

Beyond the decisions concerning initial collection of data, further collection cannot be planned in advance of the emerging theory (as is done so carefully in research designed for verification and description). The emerging theory points to the next steps—the sociologist does not know them until he is guided by emerging gaps in his theory and by research questions suggested by previous answers.

The basic question in theoretical sampling (in either substantive or formal theory) is: what groups or subgroups does one turn to next in data collection? And for what theoretical purpose? In short, how does the sociologist select multiple comparison groups? The possibilities of multiple comparisons are infinite, and so groups must be chosen according to theoretical criteria.

In actuality, many sociologists escape this problem of selecting groups by studying only one group during a given research, with some slight effort at delineating subgroups, and with occasional references (usually in footnotes) to comparative findings on another group, typically followed by a brief description of differences, but not by a theoretical analysis. In other studies, particularly survey research, comparisons are usually, and quite arbitrarily, based on only one different substantive group (such as natural scientists compared with social scientists, or scientists with engineers); or the comparisons are based on several subgroups within the substantive group. And in "comparative studies" of more than two groups, the sociologist usually tries to compare as many as he can of the groups for which he can...
obtain data within the limits of his own time and money and his degree of access to those groups. The resulting set of groups is then justified by citing common factors and relevant differences, stating that this constitutes all the available data anyhow. Further comparisons are left to future researchers.

Although these methods of choosing groups yield worthwhile research, they do not employ the criteria for theoretical sampling that we shall discuss in this chapter. Our criteria are those of theoretical purpose and relevance—not of structural circumstance. Though constrained by the same structural circumstances of research, we do not base research on them. The criteria may appear flexible (too much so for validity, one critic has said), but the reader must remember that our main purpose is to generate theory, not to establish verifications with the “facts.” We trust that these criteria will also appear to create a more systematic, relevant, impersonal control over data collection than do the preplanned, routinized, arbitrary criteria based on the existing structural limits of everyday group boundaries. The latter criteria are used in studies designed to get the facts and test hypotheses. One reason for emphasizing this difference in control is immediately apparent. The criteria of theoretical sampling are designed to be applied in the ongoing joint collection and analysis of data associated with the generation of theory. Therefore, they are continually tailored to fit the data and are applied judiciously at the right point and moment in the analysis. The analyst can continually adjust his control of data collection to ensure the data’s relevance to the impersonal criteria of his emerging theory.

By contrast, data collected according to a preplanned routine are more likely to force the analyst into irrelevant directions and harmful pitfalls. He may discover unanticipated contingencies in his respondents, in the library and in the field, but is unable to adjust his collection procedures or even redesign his whole project. In accordance with conventional practice, the researcher is admonished to stick to his prescribed research design, no matter how poor the data. If he varies his task to meet these unanticipated contingencies, readers may judge that his facts have been contaminated by his personal violation of the preconceived impersonal rules. Thus he is controlled by his impersonal rules and has no control over the relevancy of his data, even as he sees it go astray.

Selecting Comparison Groups

In this section we focus on two questions: which groups are selected, why and how?

Which Groups?

The basic criterion governing the selection of comparison groups for discovering theory is their theoretical relevance for furthering the development of emerging categories. The researcher chooses any groups that will help generate, to the fullest extent, as many properties of the categories as possible, and that will help relate categories to each other and to their properties. Thus, as we said in Chapter II, group comparisons are conceptual; they are made by comparing diverse or similar evidence indicating the same conceptual categories and properties, not by comparing the evidence for its own sake. Comparative analysis takes full advantage of the “interchangeability” of indicators, and develops, as it proceeds, a broad range of acceptable indicators for categories and properties.

Since groups may be chosen for a single comparison only, there can be no definite, prescribed, preplanned set of groups that are compared for all or even most categories (as there are


in comparative studies made for accurate descriptions and verification). In research carried out for discovering theory, the sociologist cannot cite the number and types of groups from which he collected data until the research is completed. In an extreme case, he may then find that the development of each major category may have been based on comparisons of different sets of groups. For example, one could write a substantive theory about scientists' authority in organizations, and compare very different kinds of organizations to develop properties associated with the diverse categories that might emerge: authority over clients, administration, research facilities, or relations with outside organizations and communities; the degree or type of affiliation in the organization; and so forth. Or the sociologist may wish to write a formal theory about professional authority in organizations; then the sets of comparison groups for each category are likely to be much more diverse than those used in developing a substantive theory about scientists, since now the field of possible comparison is far greater.

Our logic of *ongoing inclusion* of groups must be differentiated from the logic used in comparative analyses that are focused mainly on accurate evidence for description and verification. That logic, one of preplanned inclusion and exclusion, warns the analyst away from comparing "non-comparable" groups. To be included in the planned set, a group must have "enough features in common" with the other groups. To be excluded, it must show a "fundamental difference" from the others. These two rules represent an attempt to hold constant strategic facts, or to disqualify groups where the facts either cannot actually be held constant or would introduce more unwanted differences. Thus in comparing variables (conceptual and factual), one hopes that, because of this set of "purified groups," spurious factors now will not influence the findings and relationships and render them inaccurate. This effort of purification is made for a result impossible to achieve, since one never really knows what has and has not been held constant.

To be sure, these rules of comparability are important when accurate evidence is the goal, but they hinder the generation of theory, in which "non-comparability" of groups is irrelevant. They prevent the use of a much wider range of groups for developing properties of categories. Such a range, necessary for the categories' fullest possible development, is achieved by comparing *any* groups, irrespective of differences or similarities, as long as the data apply to a similar category or property. Furthermore, these two rules divert the analyst's attention away from the important sets of fundamental differences and similarities, which, upon analysis, become important qualifying conditions under which categories and properties vary. These differences should be made a vital part of the analysis, but rules of comparability tend to make the analyst inattentive to conditions that vary findings by allowing him to assume constants and to disqualify basic differences, thus nullifying their effort before the analysis.

It is theoretically important to note to what degree the properties of categories are varied by diverse conditions. For example, properties of the effect of awareness contexts on the interaction between the nurse and the dying patient within a hospital can usefully be developed by making comparisons with the same situation in the home, in nursing homes, in ambulances, and on the street after accidents. The similarities and differences in these conditions can be used to explain the similar and diverse properties of interaction between nurse and patient.

The principal point to keep clear is the purpose of the research, so that rules of evidence will not hinder discovery of theory. However, these goals are usually not kept clear (a condition we are trying to correct) and so typically a sociologist starts by applying these rules for selecting a purified set of groups to achieve accurate evidence. He then becomes caught up in the delights of generating theory, and so compares everything comparable; but next he finds his theory development severely limited by lack of enough theoretically relevant data, because he has used a preplanned set of groups for collecting his information (see Chapter VI). In allowing freedom for comparing any groups, the criterion of theoretical relevance

used for each comparison in systematically generating theory controls data collection without hindering it. Control by this criterion assures that ample data will be collected and that the data collection makes sense (otherwise collection is a waste of time). However, applying theoretical control over choice of comparison groups is more difficult than simply collecting data from a preplanned set of groups, since choice requires continuous thought, analysis and search.

The sociologist must also be clear on the basic types of groups he wishes to compare in order to control their effect on generality of both scope of population and conceptual level of his theory. The simplest comparisons are, of course, made among different groups of exactly the same substantive type; for instance, federal bookkeeping departments. These comparisons lead to a substantive theory that is applicable to this one type of group. Somewhat more general substantive theory is achieved by comparing different types of groups; for example, different kinds of federal departments in one federal agency. The scope of the theory is further increased by comparing different types of groups within different larger groups (different departments in different agencies). Generality is further increased by making these latter comparisons for different regions of a nation or, to go further, different nations. The scope of a substantive theory can be carefully increased and controlled by such conscious choices of groups. The sociologist may also find it convenient to think of subgroups within larger groups, and of internal and external groups, as he broadens his range of comparisons and attempts to keep tractable his substantive theory's various levels of generality of scope.

The sociologist developing substantive or formal theory can also usefully create groups, provided he keeps in mind that they are an artifact of his research design, and so does not start assuming in his analysis that they have properties possessed by a natural group. Survey researchers are adept at creating groups and statistically grounding their relevance (as by factor analysis, scaling, or criteria variables) to make sure they are, in fact, groups that make meaningful differences even though they have been created: for example, teachers high, medium, and low on "apprehension"; or upper, middle, and lower class; or local-cosmopolitan. However, only a handful of survey researchers have used their skill to create multiple comparison subgroups for discovering theory. This would be a very worthwhile endeavor (see Chapter VIII on quantitative data).

The tactic of creating groups is equally applicable for sociologists who work with qualitative data. When using only interviews, for instance, a researcher surely can study comparison groups composed of respondents chosen in accordance with his emergent analytic framework. 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, already concentrated in the library (see Chapter VII). As in field work, the researcher who uses library material can always select additional comparison groups after his analytic framework is well developed, in order to give himself additional confidence in its credibility. He will also—like the field worker who sometimes stumbles upon comparison groups and then makes proper use of them—occasionally profit from happy accidents that may occur when he is browsing along library shelves. And, again like the researcher who carefully chooses natural groups, the sociologist who creates groups should do so carefully according to the scales of generality that he desires to achieve.

As the sociologist shifts the degree of conceptual generality for which he aims, from discovering substantive to discovering formal theory, he must keep in mind the class of the groups he selects. For substantive theory, he can select, as the same substantive class, groups regardless of where he finds them. He may, thus, compare the "emergency ward" to all kinds of medical wards in all kinds of hospitals, both in the United States and abroad. But he may also conceive of the emergency ward as a subclass of a larger class of organizations, all designed to render immediate assistance in the event of accidents or break-

8. In fact, in backstage discussions about which comparative groups to create and choose in survey analysis, the answer frequently is: "Where the breaks in the distribution are convenient and save cases, and among these choose the ones that give the 'best findings.'" Selvin, however, has developed a systematic method of subgroup comparison in survey research that prevents the opportunistic use of "the best finding" criteria. See The Effects of Leadership (Glencoe, Ill.: Free Press, 1960).
downs. For example, fire, crime, the automobile, and even plumbing problems have all given rise to emergency organizations that are on 24-hour alert. In taking this approach to choosing dissimilar, substantive comparative groups, the analyst must be clear about his purpose. He may use groups of the more general class to illuminate his substantive theory of, say, emergency wards. He may wish to begin generating a formal theory of emergency organizations. He may desire a mixture of both: for instance, bringing out his substantive theory about emergency wards within a context of some formal categories about emergency organizations.9

On the other hand, when the sociologist’s purpose is to discover formal theory, he will definitely select dissimilar, substantive groups from the larger class, while increasing his theory’s scope. And he will also find himself comparing groups that seem to be non-comparable on the substantive level, but that on the formal level are conceptually comparable. Non-comparable on the substantive level here implies a stronger degree of apparent difference than does dissimilar. For example, while fire departments and emergency wards are substantially dissimilar, their conceptual comparability is still readily apparent. Since the basis of comparison between substantively non-comparable groups is not readily apparent, it must be explained on a higher conceptual level.

Thus, one could start developing a formal theory of social isolation by comparing four apparently unconnected monographs: *Blue Collar Marriage, The Taxi-Dance Hall, The Ghetto* and *The Hobo* (Komarovsky, Cressey, Wirth, Anderson).10 All deal with facets of “social isolation,” according to their authors. For another example, Goffman has compared apparently non-comparable groups when generating his formal theory of stigma. Thus, anyone who wishes to discover formal theory should be aware of the usefulness of comparisons made on high level conceptual categories among the seemingly non-comparable; he should actively seek this kind of comparison; do it with flexibility; and be able to interchange the apparently non-comparable comparison with the apparently comparable ones. The non-comparable type of group comparison can greatly aid him in transcending substantive descriptions of time and place as he tries to achieve a general, formal theory.11

### Why Select Groups

This concern with the selection of groups for comparison raises the question: Why does the researcher’s comparison of groups make the content of the data more theoretically relevant than when he merely selects and compares data? The answer is threefold. Comparison groups provide, as just noted, control over the two scales of generality: first, conceptual level, and second, population scope. Third, comparison groups also provide simultaneous maximization or minimization of both the differences and the similarities of data that bear on the categories being studied. This control over similarities and differences is vital for discovering categories, and for developing and relating their theoretical properties, all necessary for the further development of an emergent theory. By maximizing or minimizing differences among comparative groups, the sociologist can control the theoretical relevance of his data collection. Comparing as many differences and similarities in data as possible (as mentioned in Chapter II) tends to force the analyst to generate categories, their properties and their interrelations as he tries to understand his data (see Chapter V also).

Minimizing differences among comparison groups increases the possibility that the researcher will collect much similar data on a given category while he spots important differences not caught in earlier data collection. Similarities in data that bear on a category help verify its existence by verifying the data behind it.

The basic properties of a category also are brought out by similarities, and by a few important differences found when minimizing group differences. It is helpful to establish these properties before differences among groups are maximized. For

11. This statement is made in implicit opposition merely to “writing” one’s theory in a general formal manner, on the basis of sheer conjecture or on the basis of one group, as is typical of journal articles.
example, the basic property of calculating the social loss of dying patients is their age, as was discovered by observation on geriatric and nursery wards. It was important to establish this property before going on to establish other properties of social loss by studying dying on other kinds of wards.\textsuperscript{12}

Minimizing differences among comparison groups also helps establish a definite set of conditions under which a category exists, either to a particular degree or as a type—which in turn establishes a probability for theoretical prediction. For example, "open awareness contexts" about dying—where the patient and the staff are aware that he is dying—are expectable whenever patients are held "captive" in a government hospital (whether national, state, or county). "Captive" patients may be convicts, veterans, or research patients.\textsuperscript{13}

The other approach, maximizing differences among comparison groups, increases the probability that the researcher will collect different and varied data bearing on a category, while yet finding strategic similarities among the groups. The similarities that occur, through many diverse kinds of groups, provides, of course, the most general uniformities of scope within his theory. As the analyst tries to understand the multitude of differences, he tends to develop the properties of categories speedily and densely and, in the end, to integrate them into a theory that possesses different levels of conceptual generality, thereby delimiting the theory's scope. The sociologist does not merely look for negative cases bearing on a category (as do others who generate theory); he searches for maximum differences among comparative groups in order to compare them on the basis of as many relevant diversities and similarities in the data as he can find.

When beginning his generation of a substantive theory, the sociologist establishes the basic categories and their properties by minimizing differences in comparative groups.\textsuperscript{14} Once this

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basic work is accomplished, however, he should turn to maximizing differences among comparison groups, in accordance with the kind of theory he wishes to develop (substantive or formal) and with the requirements of his emergent theory. When maximizing differences among comparative groups (thereby maximizing differences in data) he possesses a more powerful means for stimulating the generation of theoretical properties once his basic framework has emerged.\textsuperscript{15} Maximizing brings out the widest possible coverage on ranges, continua, degrees, types, uniformities, variations, causes, conditions, consequences, probabilities of relationships, strategies, process, structural mechanisms, and so forth, all necessary for elaboration of the theory.

As the sociologist maximizes differences by changing the scope of his research—for example, by going to different organizations, regions, cities or nations—he discovers more startling differences in data. His attempts to understand how these differences fit in are likely to have important effects on both his research operations and the generality of scope of his theory. These differences from other organizations, regions, or nations will make him wonder where he could have found the same differences at original research sites. And how can he continue his theoretically focused research along this line when he returns to home base?

At the same time the scope of his theory is broadened, not qualified. For example, one of us once noted that in Malayan hospitals families work in caring for dying patients. This observation was interesting because up to this point we had considered the family member, in the United States, as either being treated as another patient (sedated, given rest) or just ignored as a nuisance. Reviewing our American data, though, we discovered that the family is used in several ways for the care of dying patients. We had failed to focus on this not-so-observable occurrence. Thus, we discovered a cross-national uniformity—not a difference—by noting abroad what we had missed in America. We then proceeded to study it at our home base, where we had more time for the inquiry. We had similar experiences when comparing hospitals in various regions of the United States with those closer to home, in San Francisco.

\textsuperscript{12} See Barney Glaser and Anselm Strauss, "The Social Loss of Dying Patients," \textit{American Journal of Nursing}, Vol. 64, No. 6 (June, 1964).

\textsuperscript{13} See Glaser and Strauss, \textit{Awareness of Dying}, op. cit., Chapter 6.

\textsuperscript{14} Good substantive theory can result from the study of one group, if the analyst carefully sorts data into comparative subgroups. For example, see Evans-Pritchard, \textit{Witchcraft, Oracles and Magic Among the Azande} (Oxford, England: Clarendon Press, 1937), and our discussion of this book in Chapter VI.

\textsuperscript{15} Shils, op. cit., p. 25.
Chart 1 presents the basic consequences of minimizing and maximizing groups in generating theory.

**Chart 1. Consequences of Minimizing and Maximizing Differences in Comparison Groups for Generating Theory**

<table>
<thead>
<tr>
<th>Differences in Groups</th>
<th>Data on Category</th>
<th>Diverse</th>
</tr>
</thead>
<tbody>
<tr>
<td>Minimized</td>
<td>Maximum similarity in data leads to: (1) Verifying usefulness of category; (2) Generating basic properties; and (3) Establishing set of conditions for a degree of category. These conditions can be used for prediction.</td>
<td>Spotted fundamental differences under which category and hypotheses vary.</td>
</tr>
<tr>
<td>Maximized</td>
<td>Spotted fundamental uniformities of greatest scope</td>
<td>Maximum diversity in data quickly forces: (1) Dense developing of property of categories; (2) Integrating of categories and properties; (3) Delimiting scope of theory.</td>
</tr>
</tbody>
</table>

**How To Select Groups**

Part of the sociologist's decision about which groups to select is the problem of how to go about choosing particular groups for theoretically relevant data collection. First, he must remember that he is an active sampler of theoretically relevant data, not an ethnographer trying to get the fullest data on a group, with or without a preplanned research design. As an active sampler of data, he must continually analyze the data to see where the next theoretical question will take him. He must them systematically calculate where a given order of events is—or is not—likely to take place.16 If ongoing events do not give him theoretical relevance, he must be prepared to manipulate events by words or actions in order to see what will happen.

The following memo from our research for Awareness of Dying describes how the active search for data occurs as the researcher asks himself the next theoretically relevant question, which, in turn, directs him to seek particular groups for study:

Visits to the various medical services were scheduled as follows: I wished first to look at services that minimized patient awareness (and so first looked at a premature baby service and then at a neurosurgical service where patients were frequently comatose). I wished next to look at dying in a situation where expectancy of staff and often of patients was great and dying was quick, so I observed on an Intensive Care Unit. Then I wished to observe on a service where staff expectations of terminality were great but where the patient's might or might not be, and where dying tended to be slow. So I looked next at a cancer service. I wished then to look at conditions where death was unexpected and rapid, and so looked at an emergency service. While we were looking at some different types of services, we also observed the above types of service at other types of hospitals. So our scheduling of types of service was directed by a general conceptual scheme—which included hypotheses about awareness, expectedness and rate of dying—as well as by a developing conceptual structure including matters not at first envisioned. Sometimes we returned to services after the initial two or three or four weeks of continuous observation, in order to check upon items which needed checking or had been missed in the initial period.17

And in connection with cross-national comparisons, here is another research memo which shows how groups are selected:

The emphasis is upon extending the comparisons made in America in theoretically relevant ways. The probability of fruitful comparisons is increased very greatly by choosing different and widely contrasting countries. That is, the major unit of comparison is the country, not the type of hospital. The other major unit


17. "Once the theoretical gap is identified, it leads almost as a matter of course to further questions, each with its distinctive rationale." "The objective typically requires a search for empirical materials through which the problem can be investigated to good advantage." We have detailed these general comments of Merton on developing theory by linking them to comparative analysis and its specific strategies. (Ibid., pp. xxii-xxiv.) See also Dalton's discussion of using the "next question technique" to guide his comparative analysis of industrial organizations, in Melville Dalton, "Preconceptions and Methods in Men Who Manage," in Hammond, op. cit.
of comparison, as we have seen in our own hospitals, is the type of hospital service, since what ensues around the terminal patient depends on how he dies and under what circumstances. In each country, therefore, I shall attempt to maximize the kinds of dying situations which I would see. I know, for instance, that in some Asian countries many hospitals consist of only one large ward, and this means that I will have to visit hospitals in contrasting regions of the countries. But in the cities, even in Asia, the same hospital may have differing services; and, as in Malaya, there will be hospitals for Chinese and hospitals for mixed ethnic groups right within the same city.

The selection of hospitals and services at which I would observe overseas will be guided, as in the current terminal study, by the conceptual framework developed to date. I will want to observe at hospitals, to begin with, where [four important] structural conditions we have noted are different than in America. I will observe, where possible, in hospitals (or on wards) where all four conditions are maximally different from the usual American conditions; also where three are different, where two are different, and one. I shall also choose wards or services which will maximize some of the specific conditions studied in the United States: namely, wards where dying is predominantly expected by staff and others where dying is relatively unexpected; wards where patients tend to know they are dying, and ones where they do not; wards where dying tends to be slow, and wards where predominant mode of dying tends to be relatively rapid. I hope to observe on various of those wards patients who are of high as well as low social value, and will try to visit locales where conditions are such that very many patients tend to be of low social value, as well as where there would tend to be many patients of high social value.

Degree of Theoretical Sampling

When choosing groups for theoretical relevance, two strategic questions of degree of sampling arise: How many groups should one choose? To what degree should one collect data on a single group? Answering these questions requires discussions on theoretical saturation, "slice" of data, and depth of theoretical sampling.

Theoretical Saturation

As we have said, the sociologist trying to discover theory cannot state at the outset of his research how many groups he will sample during the entire study; he can only count up the groups at the end. Since data for various categories are usually collected from a single group—although data from a given group may be collected for only one category—the sociologist usually is engaged in collecting data from older groups, or returning to them, while simultaneously seeking new groups. Thus he continually is dealing with a multiplicity of groups, and a multiplicity of situations within each; while absorbed with generating theory he would find it hard to count all these groups. (This situation contrasts with that of the researcher whose study involves verification or description, in which people are distributed throughout various categories, and he, therefore, must state the number of groups that will be sampled, according to rules of evidence governing the collection of reliable data.)

Even during research focused on theory, however, the sociologist must continually judge how many groups he should sample for each theoretical point. The criterion for judging when to stop sampling the different groups pertinent to a category is the category's theoretical saturation. Saturation means that no additional data are being found whereby the sociologist can develop properties of the category. As he sees similar instances over and over again, the researcher becomes empirically confident that a category is saturated. He goes out of his way to look for groups that stretch diversity of data as far as possible, just to make certain that saturation is based on the widest possible range of data on the category.

One reaches theoretical saturation by joint collection and analysis of data. (See Chapter V for a discussion of saturation during analysis of data.) When one category is saturated, nothing remains but to go on to new groups for data on other categories, and attempt to saturate these new categories also. When saturation occurs, the analyst will usually find that some gap in his theory, especially in his major categories, is almost, if not completely filled. In trying to reach saturation he maxi-
mizes differences in his groups in order to maximize the varieties of data bearing on a category, and thereby develops as many diverse properties of the category as possible. The criteria for determining saturation, then, are a combination of the empirical limits of the data, the integration and density of the theory, and the analyst's theoretical sensitivity.

Saturation can never be attained by studying one incident in one group. What is gained by studying one group is at most the discovery of some basic categories and a few of their properties. From the study of similar groups (or subgroups within the first group), a few more categories and their properties are yielded. But this is only the beginning of a theory. Then the sociologist should try to saturate his categories by maximizing differences among groups. In the process, he generates his theory. For example, from studying one incident in one group we might discover an important property of nursing students' perspectives about course work is their assessment of the differential importance of certain kinds of course work to the faculty; but this discovery tells us almost nothing. To find such properties as when and how an assessment is made and shared, who is aware of given assessments, and with what consequences for the students, the faculty, the school, and the patients whom the students nurse, dozens and dozens of situations in many diverse groups must be observed and analyzed comparatively. 

Theoretical and Statistical Sampling

It is important to contrast theoretical sampling based on the saturation of categories with statistical (random) sampling. Their differences should be kept clearly in mind for both designing research and judging its credibility. Theoretical sampling is done in order to discover categories and their properties, and to suggest the interrelationships into a theory. Statistical sampling is done to obtain accurate evidence on distributions of people among categories to be used in descriptions or verifications. Thus, in each type of research the "adequate sample" that we should look for (as researchers and readers of research) is very different.

The adequate theoretical sample is judged on the basis of how widely and diversely the analyst chose his groups for saturating categories according to the type of theory he wished to develop. The adequate statistical sample, on the other hand, is judged on the basis of techniques of random and stratified sampling used in relation to the social structure of a group or groups sampled. The inadequate theoretical sample is easily spotted, since the theory associated with it is usually thin and not well integrated, and has too many obvious unexplained exceptions. The inadequate statistical sample is often more difficult to spot; usually it must be pointed out by specialists in methodology, since other researchers tend to accept technical sophistication uncritically.

The researcher who generates theory need not combine random sampling with theoretical sampling when setting forth relationships among categories and properties. These relationships are suggested as hypotheses pertinent to direction of relationship, not tested as descriptions of both direction and magnitude. Conventional theorizing claims generality of scope; that is, one assumes that if the relationship holds for one group under certain conditions, it will probably hold for other groups under the same conditions. This assumption of persistence is subject only to being disproven—not proven—when other sociologists question its credibility. Only a reversal or disappearance of the relationship will be considered by sociologists as an important discovery, not the rediscovery of the same relationship in another group; since once discovered, the relationship is assumed to persist. Persistence helps to generalize scope but is usually considered uninteresting, since it requires no modification of the theory.

Furthermore, once discovered the relationship is assumed to persist in direction no matter how biased the previous sample of data was, or the next sample is. Only if the hypothesis is disproven do biases in the sample come under question. For generating theory these biases are treated as conditions changing the relationship, which should be woven into the analysis as


such. Thus, random sampling is not necessary for theoretical sampling, either to discover the relationship or check out its existence in other groups. However, when the sociologist wishes also to describe the magnitude of relationship within a particular group, random sampling, or a highly systematic observation procedure done over a specified time is necessary. For example, after we discovered the positive relationship between the attention that nurses gave dying patients and the nurses' perceptions of a patient's social loss, we continually found this relationship throughout our research and were quick to note conditions altering its direction. But we could never state the precise magnitude of this relationship on, say, cancer wards, since our sampling was theoretical.

Another important difference between theoretical and statistical sampling is that the sociologist must learn when to stop using the former. Learning this skill takes time, analysis and flexibility, since making the theoretically sensitive judgment about saturation is never precise. The researcher's judgment becomes confidently clear only toward the close of his joint collection and analysis, when considerable saturation of categories in many groups to the limits of his data has occurred, so that his theory is approaching stable integration and dense development of properties.

By contrast, in statistical sampling the sociologist must continue with data collection no matter how much saturation he perceives. In his case, the notion of saturation is irrelevant to the study. Even though he becomes aware of what his findings will be, and knows he is collecting the same thing over and

20. We have taken a position in direct opposition to Udy, who says: "Any research of any type whatsoever which seeks to make generalizations beyond the material studied involves problems of sampling.... [The researcher] is implicitly identifying a larger population, of which his cases purport to be a representative sample, and contending that certain relationships observed in his sample could not have occurred there by chance. It is simply not true that one can avoid sampling problems by proceeding in words instead of numbers or by avoiding the use of statistical techniques, though it is unfortunately true that by avoiding such methods one can often keep sampling problems from becoming explicit." Udy's gross, categorical position could be modified to compatibility with ours, we believe, if he thought rather in terms of diverse purposes of research and the degree to which each purpose requires a relationship to be described in terms of its various properties: existence, direction, magnitude, nature, and conditions, etc. In any event, a few lines later he then admits that "one cannot really solve them" (problems of representativeness). Udy, op. cit., pp. 169-170.

over to the point of boredom, he must continue because the rules of accurate evidence require the fullest coverage to achieve the most accurate count. If the researcher wishes to diverge from his preplanned research design because of conceptual realizations and implicit analyses, he must hold his wish in abeyance or laboriously integrate his new approach into the research design, to allow a new preplanned attack on the total problem. He must not deviate from this new design either; eventually it leads him back into the same "bind." 21

Slice of Data

In theoretical sampling, no one kind of data on a category nor technique for data collection is necessarily appropriate. Different kinds of data give the analyst different views or vantage points from which to understand a category and to develop its properties; these different views we have called *slices of data*. While the sociologist may use one technique of data collection primarily, theoretical sampling for saturation of a category allows a multi-faceted investigation, in which there are no limits to the techniques of data collection, the way they are used, or the types of data acquired. 22 One reason for this openness of inquiry is that, when obtaining data on different groups, the sociologist works under the diverse structural conditions of each group: schedules, restricted areas, work tempo, the different perspectives of people in different positions, and the availability of documents of different kinds. Clearly, to

21. For example, Udy says, "The coding operation proved to be very tedious 'dog work' in the worst sense of the term. I . . . was now attempting to resist, rather than encourage flights of imagination. I had to accept the fact that there were gaps in the data about which I could do nothing" (op. cit., pp. 178-79). To avoid this bind, many sociologists hire data collectors and coders in preplanned research for description and verification. Then, however, discoveries are made too late to effect changes in data collection. See the tug-of-war waged between Riesman and Watson on this bind: Riesman continually wanted to break out and Watson wanted to maintain tight control; David Riesman and Jeanne Watson, "The Sociability Project: A Chronicle of Frustration and Achievement," in Hammond, op. cit., pp. 269-84.

22. For examples of multifaceted investigations, see in Hammond, op. cit.: the research chronicles of Renee Fox, "An American Sociologist in the Land of Belgian Research"; Dalton; and Seymour M. Lipset, "The Biography of a Research Project: Union Democracy."
succeed he must be flexible in his methods and in his means for collecting data from group to group.\textsuperscript{23}

The result is, of course, a variety of slices of data that would be bewildering if we wished to evaluate them as accurate evidence for verifications. However, for generating theory this variety is highly beneficial, because it yields more information on categories than any one mode of knowing (technique of collection). This makes the research very exciting to the sociologist, providing motivation to keep him at his task. The different ways of knowing about a category virtually force him to generate properties as he tries to understand the differences between the various slices of data, in terms of the different conditions under which they were collected.\textsuperscript{24} But it must be remembered that this comparative analysis of different slices of data should be based on the researcher's theoretical understanding of the category under diverse conditions, not on methodological differences and on standard problems of the diverse techniques he has used.

Among the many slices of data that may be collected, which one is the best to obtain? The answer is, of course, the collection technique that best can obtain the information desired, provided that conditions permit its use in some manner.\textsuperscript{25} For an extreme example, Dalton had to bribe a secretary in order to see secret personnel records so that he could find out the ethnic composition of an executive hierarchy, rather than trying to guess its composition from names.\textsuperscript{26}

Most often, however, the sociologist's strategy will be constrained by such structural conditions as who is available to

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\textsuperscript{23} Compare the flexibility in ethics of Dalton, \textit{op. cit.}, pp. 59-62, with the ethical problems of Riesman and Watson, \textit{op. cit.}, pp. 280-89.

\textsuperscript{24} Lipset said he wished to test his theory of union democracy by a survey of the International Typographers' Union. What actually happened when he compared this new slice of data to the formed theory was not testing but coming to terms with differences. Thereby more theory on union democracy was generated. See Seymour M. Lipset in Hammond, \textit{op. cit.}, pp. 107-119.

\textsuperscript{25} Thus, any discussion about whether survey data are better or worse than field data is usually meaningless. Often the researcher is forced to obtain only one kind—and when theory is the objective, both kinds are useful. Only under particular conditions of a group which allows both does the question arise: which method would give the best data on the information desired? The answer is technical, not doctrinaire.

\textsuperscript{26} Dalton, \textit{op. cit.}, pp. 68 and 67.

be observed, talked with, overheard, interviewed, or surveyed, and at what times. He should realize that no matter what slices of data he is able to obtain, comparing their differences generates properties, and most any slice can yield the same necessary social-structural information. For example, no matter whom the sociologist observes or talks with in a situation where someone is dying (patient, nurse, doctor, chaplain or family member), he will soon know what type of awareness context is operating. Possibly his theory will receive considerable development from any information that happens his way; even substantively "trivial" data can help, if it yields useful information on a relevant category. For example, one can gain useful data on the life styles of professionals by examining, for this group, a national market-research survey about meat consumption (done for the meat-packing industry). The data need not be important in themselves; only the category which they indicate must be theoretically relevant. Similarly, a down-to-earth article on illness and pain by a nurse or patient may yield very useful information to a researcher who is studying the management of pain in hospitals.

Another slice of data that should be used is the "anecdotal comparison." Through his own experiences, general knowledge, or reading, and the stories of others, the sociologist can gain data on other groups that offer useful comparisons. This kind of data can be trusted if the experience was "lived." Anecdotal comparisons are especially useful in starting research and developing core categories. The researcher can ask himself where else has he learned about the category and make quick comparisons to start to develop it and sensitize himself to its relevancies.

As everyone knows, different people in different positions may offer as "the facts" very different information about the same subject, and they vary that information considerably when talking to different people. Furthermore, the information itself may be continually changing as the group changes, and different documents on the same subject can be quite contradictory. Some sociologists see these circumstances as presenting an unbounding relativism of facts—no data is accurate. Since such a situation is unbearable to those who wish to verify or describe, they tend to claim that only their method can give the "accu-
rate" evidence. Other methods that they might use only yield biased or impressionistic data, and so can be discounted.\textsuperscript{27} Using this argument, they take only one slice or mode of knowing as giving the "facts." Since they do not seek other modes, they remain untroubled. For example, in one noted study of adolescents in high schools, only the adolescents were surveyed; and in a study of workers in a factory, only workers were observed and interviewed.\textsuperscript{28}

But when different slices of data are submitted to comparative analysis, the result is not unbounding relativism. Instead, it is a proportioned view of the evidence, since, during comparison, biases of particular people and methods tend to reconcile themselves as the analyst discovers the underlying causes of variation. This continual correction of data by comparative analysis gives the sociologist confidence in the data upon which he is basing his theory, at the same time forcing him to generate the properties of his categories. The continual correction of data also makes the sociologist realize clearly an important point: when used elsewhere, theory generated from just one kind of data never fits, or works as well, as theory generated from diverse slices of data on the same category. The theory based on diverse data has taken into consideration more aspects of the substantive or formal area, and therefore can cope with more diversity in conditions and exceptions to hypotheses.

If the sociologist has two slices of data (such as field and survey data), but does not engage in comparative analysis, he will generate his theory from one mode of collection and ignore the other completely when it disproves his theory—although he may selectively use confirmatory pieces of the other data as supporting evidence. Thus, when no comparative analysis is done, different slices of data are seen as tests of each other, not as different modes of knowing that must be explained and integrated theoretically. The result is that, without comparative analysis, even men who generate theory tend to use and fall into the rhetoric of verification.\textsuperscript{29} They miss out on the rich diversity of modes of knowing about their categories. And they fail to tell their readers of their other data, since they believe, quite wrongly, that it disproves their theory, when it would have actually enriched it immensely.

**Depth of Theoretical Sampling**

The depth of theoretical sampling refers to the amount of data collected on a group and on a category.\textsuperscript{30} In studies of verification and description it is typical to collect as much data as possible on the "whole" group. Theoretical sampling, though, does not require the fullest possible coverage on the whole group except at the very beginning of research, when the main categories are emerging—and these tend to emerge very fast.\textsuperscript{31} Theoretical sampling requires only collecting data on categories, for the generation of properties and hypotheses.

Even this kind of selective collection of data, however, tends to result in much excess data, from which new and related categories emerge. For example, after a full day in the field, when the field worker is tired and jammed with dozens of incidents to report in his field notes, he need only dictate data about his categories. Going through his categories also helps him to remember data he may have forgotten during his full day. With these categories firmly in mind, directing his attention, the field worker can focus on remembering the details of his day's observations with the confidence that the notes will be implicitly

\textsuperscript{27} For example, "The significance of the quantitative case study, then, is (1) that it stimulates the kind of theoretical insights that can be derived only from quantitative analysis as well as the kind that results from close observation of an empirical situation, and (2) that it provides more severe checks on these insights than an impressionistic study and thus somewhat increases the probably validity of conclusions," Peter Blau, "The Research Process in the Study of the Dynamics of Bureaucracy," in Hammond, op. cit., p. 20.


\textsuperscript{29} These same sociologists tend to be debunkers who try to dig up something out of their own reading to disprove the theory presented by their colleague. They do not understand they are merely offering a new slice of data that under comparative analysis would enrich his theory by providing or modifying properties and categories.

\textsuperscript{30} See the instructive discussion on "depth" by Udy, op. cit., pp. 164-65.

\textsuperscript{31} For examples on the quick emergence of relevant categories see, Blanche Geer, "First Days in the Field," in Hammond, op. cit.; and Blau, op. cit., pp. 33-34. Blau discovered the significance of the "consultation" pattern within a week after starting his field research.
guided by his categories. Any additional information he decides to note afterwards is "gravy" for theoretical consideration, not a required chore for the fullest coverage. Theoretical sampling, therefore, can save much time in note-taking.

It is not too difficult to compare as many as forty groups on the basis of a defined set of categories and hypotheses (not on the basis of the "whole" group), and when 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, to check out major hypotheses before too much theory is built around them. Without theoretical sampling, the field worker, or the writer of a survey questionnaire, collects as much data as he can and hopes that this full coverage will "catch enough" that later will prove relevant. Probably, though, it will prove too thin a basis for a developed theory. Theoretical sampling reduces the mass of data that otherwise would be collected on any single group. Indeed, without theoretical sampling for categories one could not sample multiple groups; he would be too bogged down trying to cover just one.

The depth to which a category should be sampled is another matter. The general idea is that the sociologist should sample a category until confident of its saturation, but there are qualifications. All categories are obviously not equally relevant, and so the depth of inquiry into each one should not be the same. Core theoretical categories, those with the most explanatory power, should be saturated as completely as possible. Efforts to saturate less relevant categories should not be made at the cost of resources necessary for saturating the core categories. As his theory develops and becomes integrated, the sociologist learns which categories require the most and least complete saturation, and which ones can be dropped. Thus, the theory generates its own selectivity for its direction and depth of development.

In actual practice, even the saturation of core categories can be a problem. In field work especially, the tendency always is to begin collecting data for another category before enough

32. For example see Riesman and Watson, op. cit., p. 295.
33. See Shils, op. cit., p. 17.

Temporal Aspects of Theoretical Sampling

When generating theory through joint theoretical collection, coding, and analysis of data, the temporal aspects of the research are different from those characteristic of research where separate periods of work are designated for each aspect of the research. In the latter case, only brief or minor efforts, if any, are directed toward coding and analysis while data are collected. Research aimed at discovering theory, however, requires that all three procedures go on simultaneously to the fullest extent possible; for this, as we have said, is the underlying operation when generating theory. Indeed, it is impossible to engage in theoretical sampling without coding and analyzing at the same time.

Theoretical sampling can be done with previously collected research data, as in secondary analysis, but this effort requires a large mass of data to draw on in order to develop a theory of some density of categories and properties. The sociologist engages in theoretical sampling of the previously collected data, which amounts to collecting data from collected data. Also, he is bound to think of ways to make quick, brief data-collection forays into other groups, to find additional relevant comparative data. Therefore, in the end, theoretical sampling and data collection for discovering theory become simultaneous, whether the sociologist uses collected data or collects his own data, or both. How much time and money are available is important in deciding to what degree the data to be sampled will have been collected previously by the researcher or anyone else who compiles data.
All studies require respite from data collection for the relief and health of their personnel. Generating theory by joint collection, coding and analysis requires such respite for additional, obvious reasons. The sociologist must engage continually in some systematic coding (usually just jotting categories and properties on the margins of his field notes or other recorded data) and analytic memo writing (see Chapter V). He must be looking for emergent categories, reformulating them as their properties emerge, selectively pruning his list of categories while adding to the list as the core of his theory emerges, along with developing his hypotheses and integrating his theory—in order to guide his theoretical sampling at each step of the way. If he does not take respite for reflection and analysis, he cannot avoid collecting a large mass of data of dubious theoretical relevance.

Most generating of theory should be done in uninterrupted quiet, away from the field or the machine room. This is true especially during earlier stages of the project, when more time is needed for careful formulation. At later stages, the sociologist will find that analysis can proceed more easily during moments of data collection. When his categories are firmer in integration and development, he usually can spot what he is doing in theoretical terms while collecting data. At this time, he may observe in a few minutes all that he needs to know about a group with reference to a given theoretical point. However, actually generating theory at the moment of collecting data is never easy; usually it takes reflection afterward to discover what one has actually found. In addition, if one has colleagues on the same project, they all must have respite from data collection to discuss what they are doing and should do next. Such discussion is difficult or impossible in the field because they are either scattered in different places or cannot talk freely in other people’s presence.

The sociologist eventually learns to pace the alternating tempo of his collecting, coding and analyzing in order to get each task done in appropriate measure, in accordance with the stage of his research and theory development. At the beginning, there is more collection than coding and analysis; the balance then gradually changes until near the end when the research involves mostly analysis, with brief collection and coding for picking up loose ends. To pace the alternating tempo of these three operations, the sociologist soon learns that analysis can be usefully accomplished at various times: immediately after leaving the field; during the evening between successive days of data collection; and during two- or three-day, or weekly, respite from data collection. However, the systematic formulation of the core structure of his theory may take considerable time, though it need not. In either event, the sociologist should be very flexible about timing his work. He should not be afraid to take, literally, months off his data collection, if necessary (and if possible), to think through his emergent theory before returning to the field.

The continual intermeshing of data collection and analysis bears directly on how the data collection is brought to a close. A researcher can always try to collect more data for checking hypotheses or for generating new properties, categories and hypotheses. When writing is done in or near the field, the temptation to go back is especially strong. These final searches for data tend to be for either specific confirmation (the researcher moving now with considerable sureness and speed) or elaboration (the researcher wishing to round out his work by exploring some area that was previously untouched or even unconsidered).34 They can be strongly tempting if personal relations formed in the field are satisfying or if exciting new events are developing there. However, collection of additional data can be a waste of time for categories already saturated or for categories not of core value to the theory.35 Sometimes there is a tendency to wait in the field just in case something new should happen, but often it does not—and the study is prolonged unnecessarily. This tendency may be related to the researcher’s anxiety to “know everything,” which is not necessary for theoretical saturation.


35. 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 it.
The tempo of the research is difficult to know beforehand, because it is largely contingent on the tempo of the emerging theory, which may come quickly at some points and at others involve long periods of gestation. This difficulty raises a problem: in presenting proposals for research grants, how does the sociologist who intends to generate theory anticipate the amount of time necessary, for data collection and for the whole project? This is a question that review boards want answered—but it is difficult to answer for studies focused on generating theory, while relatively easy for those devoted to verification and description, which require preplanned schedules.

Because the sociologist who wishes to generate theory cannot state beforehand how many groups he will study and to what degree he will study each one, he cannot say how much time his project will take. But he can state the type of theory, substantive or formal, that he wishes to generate, and give the geographical areas where he will study certain kinds of groups. Specifying the kinds of groups will indicate the range of types necessary to achieve the desired scope and conceptual generality and to maximize differences for developing properties. In field and survey research, rough estimates can be given of how many large units (such as number of cities, regions, and countries) will be sampled. In library research, the sociologist can talk of the different caches of material to be used (see Chapter VII). From these descriptions, he can estimate the time necessary for completion of his project, allowing ample time at least for the data collection, and realizing that the final theoretical analysis and writing can continue for years.

Detailed breakdowns of the timing of research (number of situations to be observed in one group, hours of observation, numbers and positions of people to be interviewed or surveyed, amount of time necessary for respite) are also difficult to give in a research proposal designed for discovering theory, since they depend on the directions the emerging theory takes, and on the temporal open-endedness of theoretical sampling. However, after describing the kinds of groups to be studied, the researcher can sometimes describe structural conditions that surely will affect the detailed timing of his project.

For example, when and how often do situations for routine sampling occur (what shifts, lunches or staff meetings)? What

are the best hours, days of the week, or times of the year to meet the people to be sampled, or to get the kind of data necessary? What kinds of encapsulated periods of data collection are there, such as training periods, seasons, job periods (as time for building a house in order to study subcontractors), or periods of waiting for unscheduled situations to occur (as with suicides)? How long does it take to follow the course of action in situations occurring over time (such as recovering from polio)? The researcher might find it worthwhile to explore his groups briefly for some of these structural contingencies that affect timing before he writes his anticipated timing of research into a proposal. Since the core theory would begin to appear during even this exploratory period, he might gain a clearer visualization of how long he will need to fill out the theory. Colleagues who have had experiences in similar research and/or groups can also help in judging temporal contingencies.

Finally, another time-consuming aspect of data collection is establishing rapport with the people who are to be interviewed or observed. To establish rapport quickly is, of course, sometimes difficult. Particularly in field studies on one group in depth, the sociologist may spend weeks or even months getting people to allow him to study them at will. Theoretical sampling could also require this amount of time too, though establishing rapport is often not necessary. In later stages of the research, when sampling many comparative groups quickly for data on a few categories, the sociologist may obtain his data in a few minutes or half a day without the people he talks with, overhears or observes recognizing his purpose. He may obtain his data before being shoed off the premises with interfering with current activities; and he may obtain his data clandestinely in order to get it quickly, without explanations, or to be allowed to obtain it at all.

In field studies, theoretical sampling usually requires reading documents, interviewing, and observing at the same time, since all slices of data are relevant. There is little, if any, systematic interviewing of a sample of respondents, or interviewing that excludes observation. At the beginning of the research, interviews usually consist of open-ended conversations during which respondents are allowed to talk with no imposed limitations of time. Often the researcher sits back and listens while the
respondents tell their stories. Later, when interviews and observations are directed by the emerging theory, he can ask direct questions bearing on his categories. These can be answered sufficiently and fairly quickly. Thus, the time for any one interview grows shorter as the number of interviews increases, because the researcher now questions many people, in different positions and different groups, about the same topics. Although the time taken by most interviews decreases as the theory develops, the sociologist still cannot state how long all his interviews will take because a new category might emerge at any time; this emergence will call for lengthy open-ended conversations and prolonged observations within some groups. Also, theoretical sampling aimed at following an incident or observing over a period of time requires sequential interviews, with no clear notion of when the sequence will be terminated.

Conclusion

Theoretical sampling, then, by providing constant direction to research, gives the sociologist momentum, purpose and confidence in his enterprise. He develops strong confidence in his categories, since they have emerged from the data and are constantly being selectively reformulated by them. The categories, therefore, will fit the data, be understood both to sociologists and to laymen who are knowledgeable in the area, and make the theory usable for theoretical advance as well as for practical application. The sociologist will find that theoretical sampling, as an active, purposeful, searching way of collecting data, is exciting, invigorating and vital. This point is especially important when one considers the boring, dull, and stultifying effects on creativity of the methods involving separate and routine data collection, coding and analysis which are used frequently in descriptive and verificatory studies. Conventional field

36. Theoretical sampling would have avoided the dilemma facing Watson and Riesman (op. cit.) in their study of sociability. Watson feared the loss of her detailed, preconceived code when starting to collect data, since Riesman lacked confidence in it and wanted to change it completely. If they had undertaken an active theoretical search for categories that worked and fit, then the preconceived code could have been selectively reformulated with the approval and confidence of both researchers.

research is also exciting work but, as we have detailed, it lacks the more extensive commitment to discovery of theory displayed by research utilizing theoretical sampling.

One final and important point: since each researcher is likely to encounter special conditions in his research, he will inevitably add to the discussion of theoretical sampling as outlined in this chapter. We would scarcely wish to limit this type of comparative analysis to what we can say about it, from either our own research or our knowledge of others' research. We have merely opened up the topic. The motto should be: the more studies are based on theoretical sampling, the more effective should future theoretical sampling and comparative analyses become—provided researchers write about their strategies and techniques.
IV

From Substantive to Formal Theory

Since substantive theory is grounded in research on one particular substantive area (work, juvenile delinquency, medical education, mental health), it might be taken to apply only to that specific area. A theory at such a conceptual level, however, may have important general implications and relevance, and become almost automatically a springboard or stepping stone to the development of a grounded formal theory.¹

As we remarked in Chapter II, substantive theory is a strategic link in the formulation and generation of grounded formal theory. We believe that although formal theory can be generated directly from data, it is most desirable, and usually necessary, to start the formal theory from a substantive one. The latter not only provides a stimulus to a "good" idea, but it also gives an initial direction in developing relevant categories and properties and in choosing possible modes of integration. Indeed, it is difficult to find a grounded formal theory that was not in some way stimulated by a substantive theory. Often the sub-

¹ For example, one author of this book received the following note from a colleague: "Thanks very much for your article on comparative failure in science. The notion of comparative failure would seem to have application in many areas of life." Other colleagues wrote letters detailing the relevance of "comparative failure" to religion, marriage, social class, and political behavior. Others phoned to give their ideas about comparative failure, and still others sent theory and research references. Though not using this term, these references provided immediate material for a comparative analysis that would facilitate generating a formal theory of comparative failure.
stantive and formal theories are formulated by different authors. Sometimes in formal theory the substantive theory is implicit, having been developed previously by the author or another writer.

In this chapter we shall only begin the discussion of the processes by which a substantive theory is advanced to a formal one. We should emphasize that, since our experience and knowledge are least extensive in this area, most of our discussion will be concerned with general rules, positions, and examples of initial efforts at generating formal theory. More specific procedures await the time when enough sociologists will have generated grounded formal theory that their procedures can be codified. Although we lack many specific examples, we feel certain of our general position on the ways that formal theory should be generated. Near the end of the chapter, we shall discuss the closely related questions: "Why go on to formal theory?" and "What are its uses?"

Generating Formal Theory

One-Area Formal Theory

There are at least two "rewriting" techniques for advancing a substantive to a formal theory that is grounded in only one substantive area. The sociologist can simply omit substantive words, phrases or adjectives: instead of saying "temporal aspects of dying as a nonscheduled status passage" he would say "temporal aspects of nonscheduled status passage." He can also rewrite a substantive theory up a notch: instead of writing about how doctors and nurses give medical attention to dying patients according to the patient's social value, he can talk of how professional services are distributed according to the social value of clients. By applying these rewriting techniques to a substantive theory, the sociologist can change the focus of attention from substantive to formal concerns. He writes a one-area formal theory on the basis of a substantive theory; he does not generate the formal theory directly from the data.


A quick perusal of any sociological journal will demonstrate that almost all sociologists believe this is the way to write formal theory! For example, Selvin and Hagstrom recently have published an article entitled, "Two Dimensions of Cohesiveness in Small Groups," but this article does not offer the grounded formal theory its title implies, only a grounded substantive theory (about college women) written up a notch. At the close of the paper, some comparative speculation is offered about broader implications; there is no comparative research or analysis to establish formal theory.

Such rewriting techniques applied to a substantive theory produce only an adequate start toward formal theory, not an adequate formal theory itself. Probably the researchers are, as is typical, responding to the substantive stimulation with some general implications. All they have done is to raise the conceptual level of their work mechanically; they have not raised it through comparative understanding. They have done nothing to broaden the scope of their theory on the formal level by comparative investigation of different substantive areas. They have not escaped the time and place of their substantive research, though their formal writing of the theory may lead readers into thinking so. A classic example of this type of theory writing is Merton and Kitt's theory of reference group behavior.

We can only wonder what such theories might have looked like if their authors had done the comparative analyses implied by their writing.

Another danger of the rewriting technique as used on a single substantive area is that, for the reader, it tends to dissociate the data from the formal theory. When the theory is very abstract, it becomes hard to see how it came from the data of the study, since the formal theory now renders the data without a substantive theory intervening.

Also, the formal theory cannot fit or work very well when written from only one substantive area (and usually only one case of the area), because it cannot really be developed sufficiently to take into account all the contingencies and qualifications that will be met in the diverse substantive areas to which


it will be applied. All that happens is that it will be modified by other theories through the comparative method, since by itself it is too sparsely developed to use in making trustworthy predictions and explanations. Thus the one-area formal theory becomes, in actuality, treated as a substantive theory to be generalized by comparative analysis.5

Multi-Area Formal Theory

When advancing a substantive theory to a formal one, the comparative analysis of groups is still the most powerful method for generating core categories and their properties and formulating a theory that fits and works. The rewriting techniques are subsumed in the process. The logic used in discovering substantive theory, which provided an efficient guide to selecting multiple groups of one substantive area, also will provide a guide for obtaining more data from many kinds of substantive areas, in order to generate formal theory. While the process of comparative analysis is the same for generating either substantive or formal theory, it becomes harder to generate the latter because of its more abstract level and the wider range of research required. Yet the task can be done by one sociologist or a few collaborators. It need not be relegated to the distant future when the division of labor within sociology will have built the wall of formal theory from the research bricks of a multitude of sociologists. There are never enough bricks and there are too few good synthesizers who wish to search out the bricks and thus put the wall together.6 These worthy people are usually too busy working on their own data!

Two examples from our own work will suggest how one can begin to generate formal theory through comparative analysis. As we have discussed in a recent article, “awareness contexts” are not confined to situations in which people are dying, but


are found generally in all kinds of social interaction. Consequently, if we wish to develop a formal theory of awareness contexts, we are automatically led to analyzing data from many substantive areas. Here is how this might be done starting with our substantive theory of awareness contexts (in dying):

Awareness contexts. Situations where awareness contexts exist are, for instance, clowning at circuses, buying and selling cars, hustling in pool halls, comparative bidding, the passing of Negroes as whites, spying as a usual practice carried out by nations, and the mutual suspicion of prisoners of war in Chinese prison camps.

Quick scrutiny of these situations (as well as our earlier preliminary analysis of differences between some of them and the dying situation) suggests several categories in terms of which they can be usefully compared. The signs or indicators of an interactant’s status may vary in visibility to the other interactants. Different numbers of interactants can be involved (two, three, or more). Different numbers of groups can be represented by the interactants. The ratios of insiders and outsiders present during the interaction may vary (one patient and dozens of staff members; five cons and one mark; one Negro, five “wise” people who know his secret, and millions of white and Negro persons who do not). The positions of interactants may also vary hierarchically (same or different level of the hierarchy). And of course the stakes of the interaction may vary tremendously.

Comparisons of each category for diverse substantive groups quickly leads to the development of properties and the formulation of associated hypotheses. Suppose that one focuses, for instance, upon the identifying signs of status. Some signs are physical (skin color), some are behavioral (speech or gesture), some are marks of skill (the agility of the card shark), some are insignia (uniforms and clothing), and so on. For any given interactional situation, certain signs of status may be thought of as primary and others as secondary: in America, skin color is the primary indicator of “Negro” just as genitalia are of respective sex. The secondary signs—those that strongly suggest

status, especially when found in conjunction with primary signs—would be, for "Negro," "kinky hair" and perhaps "southern-style speech"; and, for sex, clothing, hair style, and gesture. The visibility of such signs depends on learned ability to recognize them; for instance, many people have never learned to recognize homosexuals, and others would not know an American Indian if they saw one.

Understandably, some interactants may not even recognize the signs of their own status; for instance, the dying person may be kept unaware of his own position (closed awareness context). Signs can be manipulated, both crudely and subtly. For instance, they may simply be removed from vision, as when stigmata are concealed. They can be disguised, as when kinky hair is straightened or, as John Griffin did when passing for Negro, skin color is changed temporarily with chemicals. Signs can also be suppressed, as when an interactant chooses not to indicate that he is really an American spy, or when a Japanese-American visiting Japan speaks Japanese at a department store so as not to be recognized as a "rich American." All these tactics, of course, are aimed toward minimizing potential recognition by other interactants.

Counter-tactics consist of eliciting important "give-away" signs, to avoid having to wait for signs and hoping to recognize them. Some counter-tactics for recognizing persons who are suppressing their identity depend on "passing" as a member of their group (an FBI man posing as a Communist), or on getting information from others within the group. Persons of similar status may use conventional signs to further recognition; the deliberate use of these signs will vary, depending on whether outsiders are present or absent, and whether they are "wise" (sympathetic to insiders) or not. Usually there are places where the gathered insiders can forgo their efforts to disguise or suppress identifying signs. But they may need (as with drug addicts) counter-tactics to avoid betrayal even in such secluded places.

It is worth emphasizing that identifying signs sometimes need to be rectified—as when a customer in a store is mistaken for a salesman, or a man mistaken for a thief must prove his


innocence to bystanders, or even to police and later to a court of law. Sometimes identifying signs are "rectified" falsely! The new signs are believed and accepted, even though the original indications were really true. In "mutual pretense" situations, the dying patient in some sense rectifies the notion that he is dying by acting very much alive; given the ambiguity of most signs, other people act up to his false rectification, until the signs are either so unambiguous that the game is hard to play, or until he drops the pretense and admits his real situation. A subjective and subtle variation occurs when an interactant's status is rejected and he himself begins to doubt who he is, as in Nazi Germany when gentiles with faint Jewish lineage came to doubt their true identities because their claims to be non-Jewish were denied.

Such comparisons of diverse groups in terms of identifying "signs" quickly lead to both useful properties and hypotheses about this facet of a formal theory of awareness context. Just as in the development of substantive theory, the hypotheses will be concerned with such matters as tactics and counter-tactics, as well as with their structural conditions, their consequences, and so on. But it is important to understand that this kind of inquiry can be furthered immensely by systematic analysis, not only of a single category but of combinations of categories: signs and stakes, for instance; or signs, stakes, ratios of insiders-outsiders, and numbers of group representatives present at the interaction. This kind of analysis becomes increasingly richer, because it leads the researcher to ask "Where can I find another comparison group that differs in one more specified respect?" When he finds that group, its examination leads him to further generation and qualification of this theory. By such means, exceedingly complex and well-grounded formal theory can be developed. It is precisely by such means that a substantive theory of awareness contexts can be extended upward in conceptual generality and outward in scope. In doing so, many more useful types of awareness contexts would be generated and related to interactants' behavior.

Status passages. Our second example is the initial generation of a formal theory of status passages, prompted by our substan-

tive theory on the status passage involved in dying. We have written about the "nonscheduled status passage" of dying; several other dimensions (properties) of status passage also arose from our study. One of these is whether or not a status passage follows an institutionally prescribed sequence of transitional statuses. For instance, many ethnographic descriptions of growing up and aging, and many descriptions of organizational careers, delineate prescribed passages. (Such passages may or may not be precisely scheduled.)

"Transitional status" is a concept denoting time in terms of the social structure. It is a social system's tactic for keeping a person in passage between two statuses for a period of time. He is put in a transitional status, or sequence of them, that determines the period of time that he will be in a status passage. Thus the transitional status of "initiate" will, in a particular case, carry with it the given amount of time it will take to make a non-member a member—a civilian is made a soldier by spending a given number of weeks as a basic trainee; an adolescent spends a number of years in "training" to be an adult.

A third dimension of status passage is the degree to which it is regulated; that is, to what degree there are institutionalized operations for getting an occupant in and out of beginning, transitional, and end statuses and for keeping others informed of the passage. Rites of passages are instances of such regulated operations. It is notable in our studies of dying patients that the nonscheduled status passage involved both fairly regulated and fairly unregulated temporal elements. One regulated aspect is that at certain points in the passage the doctor must announce the death to a family member. But less regulated is the typical problem: when (if ever) does the physician tell the patient that he is dying? The regulated and unregulated elements of the nonscheduled status passage together generate one structural condition leading to differential definitions among parties to the passage.

Further dimensions of status passages include to what degree the passage is considered undesirable; whether or not it is inevitable; and how clear are the relevant transitional statuses and the beginning and end statuses of the passage itself. Dying in hospitals can be located by all these structural dimensions in the following way: the status passage is nonscheduled, nonprescribed, undesirable, and, after a point, inevitable. The passage is sometimes regulated but sometimes not; and sometimes relatively unambiguous but (except for its end status) sometimes not.

The next step is to study different types of status passage in order to begin generating a formal theory. Various combinations of the above dimensions provide ways of typing different status passages as well as some of the conditions under which the passage is managed. Differences between two sets of these conditions will, therefore, tend to explain why two types of status passages are managed differently.

For example, in the United States the engagement status passage (between the statuses of being single and married) is usually institutionally nonscheduled, like dying, though unlike dying it is desirable to the parties involved. Because it is a status they have chosen, the status occupants themselves determine when they are in passage, what the transitional statuses will be, and for how long a period they will be in each one. In contrast, couples involved in personally undesirable or forced engagements, such as sometimes found in Europe and Japan, especially among the upper class, do not control their own transition.

A status passage that contrasts with both the engagement and dying is the defendant status passage, which links the statuses of citizen and prisoner. It is scheduled and undesirable. Commitment to a state mental hospital can be regarded as an instance of the defendant passage. In contrast to dying, while the legalized legitimator of the passage is a judge, the unofficial legitimator can be, in fact, a lawyer, a general practitioner, a psychiatrist, the family, or the "defendant" himself. Thus, anyone who would be an unofficial legitimator must develop tactics to make both his claim as such "stick" and his definition of the defendant's sanity status accepted by the court. Comparative analysis of the characteristic tactics in this situation with those used during engagement or dying passages can be useful for developing a formal theory.

Also, useful comparisons between the recovery and dying status passage are provided by a study of the polio patients who

recover from their acute attacks of polio but who suffer varying degrees of muscular impairment.\textsuperscript{12} This particular kind of recovery passage is non-institutionally scheduled or prescribed, undesirable, and, after a point, inevitable. One difference with dying is that the end status—where the passage will lead—is frequently unclear. The doctor is uncertain about the degree to which the patient will regain use of the affected muscles. As a result, the doctor as legitimator is often very chary with information to family and patient, both in the hospital and after discharge (even though after a time he may form a clear idea of where the patient will end up). This lack of clear announcements about the end status stimulates the patient and family to engage in vigorous searches for cues which might define just how much better the patient can be expected to get.

In Davis’s account of the polio recovery there is very little information or analysis concerning the coordination of people’s behavior that is obtained by defining statuses correctly. The reason is easy to find: while our study was focused upon medical personnel in the hospital, his study—especially in later phases of the passage to “getting better”—focused largely upon the family outside the hospital. The medical personnel would not be so concerned with coordinating a passage outside their organizational jurisdiction.

The above examples are taken from our research; however, as we noted earlier, anyone can begin generating formal theory directly from published theory. For instance, he might systematically extend Erving Goffman’s “On Cooling the Mark Out.”\textsuperscript{13} In this useful paper, Goffman focused on the type of status demotion that reflects on the incapacity of the demoted person. “Cooling out” means demoting him while simultaneously taking measures to minimize those of his reactions that would be most destructive to the institutional setting where the demotion occurs. Goffman’s theory of “cooling out” encompasses such matters as when this process occurs, what typical tactics are used in cooling out, and what happens when the demoted person refuses to be cooled out. The theory is built on Goffman’s reflections about various kinds of institutional settings (e.g., bureaucratic, small establishments) and situations (courtship, demotion).

An examination of his paper quickly shows that, in fact, Goffman begins by pointing to comparison groups that he does not later build systematically into his comparative analysis. He uses these initial comparisons to set his own point of view squarely before the reader (quite like Cressey in The Taxi-Dance Hall).\textsuperscript{14} Thus, “losing a role” may occur through promotion, abdication or demotion. Likewise, demotion may or may not involve reflection of the person’s capacities. Each of these comparisons, in fact, can be built into the emerging theory to give it much more scope and depth. Even if demotion alone is focused on, Goffman has offered useful cues for extending his analysis. Thus, what happens when demoters and demoted both agree he has been demoted, as over against when they define him as demoted but he does not? What about the reverse situation? What about when demoters (and bystanders) do not agree among themselves? And when they are differentially above or below him in status? And when there are variable dimensions of “awareness context”—whether “open,” “closed” or “suspicion”—concerning agreement or disagreement? Also, what about the distance that he is demoted? And when more than one person is demoted simultaneously? Other cues for theoretical sampling are offered in passing by Goffman. He remarks that criminal gangs sometimes can afford not to cool out the client, but department stores necessarily must be concerned. The implications of that important point—including when each party can or cannot afford to cool out—are not followed through.

We are told also, through a passing remark, that agents who cool out may themselves react (as with guilt) to their actions. But what different kinds of agents, under what conditions, react similarly or differently? Also, if we scrutinize what we are offered in the way of tactics for cooling out or situations where it typically occurs, then we find lists of tactics and situations that are related in the analysis only rather loosely to different types of organizations or situations. Systematic comparison of organizations—either through field research or, quite feasibly,

\textsuperscript{12} Fred Davis, Passage Through Crisis (Indianapolis: Bobbs-Merrill, 1963).

\textsuperscript{13} Psychiatry, 15 (1952), pp. 451-83.

\textsuperscript{14} See our commentary on this common practice in Chapter III.
through secondary analysis of published substantive research—will quickly begin to densify the emergent formal theory.15

This kind of scrutiny and illustrative extension of Goffman's theory suggests that an important strategy in generating formal theory through theoretical sampling is to begin with someone else's formal theory. That theory may be developed less abstractly than Goffman's, and may be tied much more closely to firsthand research.16 The strategy consists of asking, first of all, what comparisons the author has forgotten or "thrown away" because of his initial focus; second, what comparisons he has suggested in passing but has not followed up; third, what comparisons are suggested directly by his analysis; and fourth, what comparisons are suggested by one's own reflections on the theory. As these analyses feed into the development of another theory, further comparisons—directed by that theory—will occur to the analyst, just as if he were thinking about his own data. This strategy not only permits the efficient generation of grounded theory, but allows speedy incorporation and transcendence of other sociologists' theories.

Direct Formulation of Formal Theory

Formal theory formulated directly from comparative data on many substantive areas is hard to find, as we have noted earlier, since stimulation and guidance, even if unacknowledged, have usually come from a substantive theory. However, it is possible to formulate formal theory directly. The core categories can emerge in the sociologist's mind from his reading, life experiences, research and scholarship. He may begin immediately to generate a formal theory by comparative analysis, without making any substantive formulations from one area; though


17. For an example of a formal theory that sounds "nice" and "neat" but appears "useless" to us—because its relevance as an explanation of anything or its dubious fit to the real world has not been demonstrated but simply assumed out-of-hand—see Peter Blau, Exchange and Power in Social Life (New York: John Wiley and Sons, 1964).


referents, the borrowing is never done by asking the following question of logico-deductive theory: How do I know this theory is relevant to the data that my formal theory purports to handle, and that it will help formulate my theory? This question is easier to ask and answer with grounded formal theory.

Another danger to beware of when directly generating formal theory from data is the tendency to slip from the true generation of formal theory to the simple ordering of a mass of data under a logically worked-out set of categories. The relative case of being logical with abstractness means that logic dominates the theory; the result is a growing love of one's "nice, neat" speculations, which one feels must be correct because they sound so logical. The data are then forcibly ordered by the conceptual framework, not used to generate properties and categories, and so have no disciplining effect on how the theory turns out. Again the result is not a grounded formal theory, but merely an orderly, "postal clerk" approach to sorting out facts.

On to Formal Theory?

Most sociologists unquestionably tend to avoid the formulation of grounded formal theory; they stay principally at the substantive level. In addition to the inherently greater difficulties in working with high level abstractions, and in feeling confident about broader generalities, we believe there are several other reasons for this avoidance.

First of all, a researcher tends to know one or two substantive areas well, and feels increasingly comfortable as he learns more about them over the years. The internal satisfactions and securities of such specialization are abetted by the further rewards of mature expertise in a specialized field, rewards that emanate from colleagues and the wider public. Furthermore, sociologists learn very early the dictum that there is a great difference between a dilettante and a true "pro." The latter knows his data inside and out. This conviction tends to keep sociologists from researching more widely, and certainly from working more abstractly, because they feel they must amass and comprehend great amounts of data before they can safely claim "findings."

Another reason for avoiding the generation of formal theory is its supposed depersonalizing effect. Formal theory is viewed as too abstract, too divorced from people and everyday life to seem real. Many sociologists resist and distrust the separation of formal theory from the time and place of specific social structures. They see conceptual level and scope of the theory as too unbounded, and the parsimony of its terms too limiting. Thus, although sociologists know a formal theory can help in a substantive area of interest for which they have no theory, nor much data, nor time for research, they do not actually trust its applicability and powers of explanation and prediction. One colleague wrote us, apropos his own area of specialization: "Also I suppose I am sufficiently offended by the airy assertions that pass as sociological theory to want no part of it." This colleague had just published a remarkably plausible substantive theory, but wished to go no further in generalizing it to a formal theory.

Other colleagues have told us that the future of sociology rests on theories of substantive areas (period!) and so proceed to generate them. This task is, of course, important for sociology's future, but so is formal theory—there will not always be a substantive theory to help those sociologists who need a relevant theory, say, for use in consultation or lectures, but who have neither time nor inclination to generate a theory from their own research.

The depersonalization of formal theory is most apparent in logico-deductive theories, for it is truly difficult to relate them to the real world. Depersonalization is minimized and minimal in grounded formal theory because this theory is based on the data from many substantive areas, and may lean heavily on a substantive theory for only one area. It is not really far removed from the real world. Those colleagues who do not see much future for formal theories are thinking almost exclusively of the logico-deductive ones. We are confident that many will change their minds if they focus rather on grounded formal theory and its two links with data: many substantive areas and


a substantive theory. A good example of grounded formal theory may be found in Becker's *Outsiders;* he carefully generates a formal theory about the social control and creation of deviance from the comparative analysis of his substantive theories on musicians and marijuana users.22

**Uses of Formal Theory**

Insofar as the sociologist does concern himself with formal theory, currently he tends to handle it in several alternative ways. First, he may set out to verify, in a given substantive area, some small portion of one or more formal theories, often derived from prominent theorists. Such verification studies are legion.

A second approach is to study with comparative research materials an important body of theoretical writing, as when Robert Blauner systematically scrutinized a number of industries with respect to their degree of "alienation." 23 This type of research is typically confined to careful variation and qualification of the central guiding theory, checking it under diverse conditions (see Chapter VI). This approach tends to block chances for development of new theory based on the comparative analysis, except insofar as the old theory seems to require qualification. It uses comparative analysis conventionally, to show and explain variations in an established general theory. In contrast, our use of comparative analysis generates and generalizes a new theory; variations and explanations became part of the process, not the product.

A third approach is to apply several formal theories to a substantive area that the sociologist already knows well, in an effort to give his materials greater meaning. He does this as a post-hoc enterprise in research after the data is collected; but sometimes the formal theories direct portions, at least, of his data collection. The sociologist also does this to order and prepare lectures.

Probably the most widespread use of formal theory, how-


ever, is this: when initiating specific researches a sociologist begins with a loose conceptual framework of formal ideas, hunches, notions, concepts, and hypotheses about the substantive area under consideration.24 This framework is often linked with and biased toward the researcher's graduate training in formal theory under a particular professor (Parsonians from Harvard, Mertonians from Columbia), as well as with his further experiences since graduation. Examples of this use of formal theory abound. However, the characteristic difficulties it can present when the formal theory is ungrounded are well illustrated in the following review (by Strauss) of William A. Rushing's *The Psychiatric Professions: Power, Conflict and Adaptation in a Psychiatric Hospital Staff:*

Designed primarily for sociologists and secondarily for people who are interested in psychiatric hospitals, this book can be read on two distinct levels: theoretical and descriptive. A sociologist can, indeed, engage in a very useful exercise by giving himself three separate readings. He can read the book first for its theory, then again for its description, and finally reread it for its descriptions but asking himself what is disappointing in the description because the theory is disappointing in some regard. This is how I read the book.

Rushing spent a number of months observing and interviewing professionals in a university (teaching) psychiatric hospital. Like other commentators on psychiatric hospitals, he was impressed by the general lack of clear-cut consensus about professional roles in the mental hospital setting. So he takes as a central thesis that the "modern mental hospital" is not "yet" fully institutionalized but is "in process of institutionalization." His problem is how to analyze this process, with particular focus on its social psychological aspects (the impact of the establishment on individuals who work there). For this analysis, he finds conventional role theory too static: its forte is to illuminate relatively institutionalized structures rather than those that are not very institutionalized. Role theory therefore needs supplementary concepts. Among the key concepts—derived, I gather, mainly from Thibaut, Homans, Merton and Parsons—are power (and power strategies), influence, cost (and cost inducing, preventing, reducing strategies), relative deprivation, reference group, and instrumental versus expressive activities.

Using qualitative analysis, abetted by frequent quotes from his fieldnotes and interviews, Rushing discusses chapter by chap-

ter the social positions, plights and strategies of various auxiliary personnel in the hospital: notably, social workers, recreational workers, clinical psychologists, and psychiatric nurses. The discussion turns around a systematic and step by step presentation of hypotheses, with qualitative evidence bearing upon them. Two quotes from the concluding chapter will convey the kinds of hypotheses which he presents: "the typology of power strategies: implementing cost-inducing, structural cost-reducing, and maintaining cost-preventing. . . We hypothesized that this typology is related to the institutionalization process: the character of the particular power strategy—its function for the actor—depends upon the degree to which social relationships have been institutionalized" (page 241).

The descriptive material offered throughout the book is ordered by the theoretical requirements of each chapter. Anyone who has observed psychiatric hospitals closely—including state hospitals where the winds of current doctrine happen to blow even softly—will recognize many features either explicitly discussed by Rushing or implicitly touched upon by his descriptions and by his interviewees' remarks. The book teems with illustrations of the ambiguity associated with auxiliary personnel's tasks, of conflict among these personnel and between them and the psychiatrists, of strategies for getting work done and professional interests accomplished.

Nevertheless my response to the book is that it is not successful in portraying—through joined description and analysis—a hospital that is very much "in process." I lay the blame on an unwillingness to abandon conventional role theory for something bolder, something more suited to, as Rushing aptly regards it, the non-institutionalized hospital. Rushing's assumption is that these hospitals are moving toward institutionalization—which is probably incorrect, and if so still raises questions as to the most fruitful ways of studying their institutionalization. Careful as is Rushing's development of social psychological theory, it suffers from the all too customary effort to fit combined bits of logical formal theory to a substantive area. Not much, I suspect, is really added to the formal theories other than indicating how portions of them can be applied in this particular substantive area. If I am incorrect in that assertion, then at least the book fails to indicate how those formal theories (bearing on power, influence, cost, reference groups, relative deprivation) were modified, qualified or extended.

As for the relationships among professionals in the hospital: immersed as I have been in similar hospital settings, I miss in his account a quality of ongoing development of relationships. He portrays very well the conflict and tension among personnel and touches occasionally upon outcome of conflict and tension; but there is conveyed hardly any sense of institutional or professional development. He has not especially caught development in his descriptions, which are relatively static, or in his theorizing, which is essentially non-processual. While the book is very useful for its descriptive materials and detailed quotes, I believe it is also useful as an object lesson about a type of prevalent research style in the use of logical formal theory.2)

The several uses of formal theory discussed in this quotation are enterprises quite different from the generation of grounded formal theory, accomplished through systematic study of multiple comparison groups and substantive theories. Perhaps the closest relative to such formulation is the kind of essay writing established many years ago by Georg Simmel, and nurtured by such contemporaries as Erving Goffman and David Riesman, in which the essayist—with or without systematic data before him—develops a series of general propositions of relatively high abstraction. Such writing can be criticized as being, at best, full of insights and, at worst, as pure speculation. (Some "insights" may later be "tested" by more rigorously minded sociologists.) From our viewpoint, such writing is exceedingly valuable, but as theory it lacks both integration of well-defined concepts and sufficiently credible grounding in careful comparative research.

The more prestigious style of logico-deductive, systematic "grand theorizing" is, in the hands of its most brilliant practitioners, more than merely esthetically satisfying; it also gives impetus to considerable useful, precise verification of hypotheses. But it provides no directive—any more than it did a century ago when Comte and Spencer were its spokesmen—to closing that embarrassingly noticeable gap between highly abstract theory and the multitude of miniscule substantive studies so characteristic of current sociology.25 It should be evident that we put greater faith in grounded formal theory to close that gap, for it readily fits "what's going on" in everyday situations. Possibly the main benefit yielded by grand theories is their use of abstract models (mathematical, process, system, functional, interdependences, equilibrium, etc.). The integration of formal


theory often requires more guidance from such explicit models than substantive theory does, because of a greater level of abstraction. However, as we stated in Chapter II, the integration of a formal theory can begin very usefully with the emerging integration scheme that was used for the substantive theory that actually stimulated the formal theory's generation.

Because grounded formal theory fits and works, we see its use in research and teaching as more trustworthy than logico-deductive theory, for the simple reason that the latter often requires forcing of data into categories of dubious relevance to the data's meaning. Grounded formal theory is also more trustworthy for sensitizing the researcher to the generation of new substantive theory and for helping him to formulate it.

Grounded formal theory is thus also highly useful in predictions and explanations when we are consulted about substantive areas where we have no theory, and no time or inclination to develop one. Explanations and predictions from logico-deductive formal theory are used mainly where they will do no harm; that is, in the classroom, as "tacked-on" explanations of accomplished research (as mentioned in Chapter I), and as hypotheses (prediction) in the service of the perennial testing of parts of a formal theory with the eternal hope that it can be modified to fit reality.

Grounded formal theory is more trustworthy for consultations because both laymen and sociologists can readily see how its predictions and explanations fit the realities of the situation. This is strategically important. While in research, predicting and explaining have few real risks (the researcher merely modifies the theory according to his findings), a layman does not trust a prediction of what will happen in his situation unless he can readily see how it applies. Similarly, he will not accept a theoretical explanation unless he can readily see how it explains his situation, and gives him a sound basis for corrections and future predictions. Grounded formal theory, like substantive theory, earns the trust of laymen and sociologists alike. Both consultant and consultee must have this trust in order to work together (see Chapter X).

As yet there is not much of this type of consultation in sociology. Seldom is such a general theorist (if you can find one) called in for consultation by other sociologists, laymen, organizations or governments. Most consultants are well known for their research and everyday experience in a particular area, and perhaps for a portion of their substantive theory if they have generated some. The transferability of formal theories to diverse substantive areas is seldom done in sociological consultation because most formal theories are ungrounded, and therefore not trusted by either sociologists or laymen when they face "real life circumstances."

Theoretical consultation is an area of sociological work that would be suitable for many sociologists, but cannot really be opened up until there are many more grounded formal theories. Then, for example, a general theorist, not only the well-known researchers, could be called in for consultation about juvenile delinquency because he is especially skilled at applying grounded formal theory to substantive areas. Sociology cannot reach this stage of development if we continue to plod on with grand logical theorizing and miniscule verifications. But this stage can be reached through the generation of grounded substantive and formal theories. Whether a substantive problem is theoretical or practical, and whether extensive research is called for or not, general theorists skilled at applying grounded formal theories are needed as consultants for making cogent predictions and explanations, and for helping decide the course of action for research or practical action.
The Constant Comparative Method of Qualitative Analysis*

Currently, the general approaches to the analysis of qualitative data are these:

1. If the analyst wishes to convert qualitative data into crudely quantifiable form so that he can provisionally test a hypothesis, he codes the data first and then analyzes it. He makes an effort to code "all relevant data [that] can be brought to bear on a point," and then systematically assembles, assesses and analyzes these data in a fashion that will "constitute proof for a given proposition." 1

2. If the analyst wishes only to generate theoretical ideas—new categories and their properties, hypotheses and interrelated hypotheses—he cannot be confined to the practice of coding first and then analyzing the data since, in generating theory, he is constantly redesigning and reintegrating his theoretical notions as he reviews his material. 2 Analysis after the coding operation

* We wish to thank the editors of Social Problems for permission to publish this paper as Chapter V. See Barney G. Glaser, Social Problems, 12 (1965), pp. 436-45.


2. Constantly redesigning the analysis is a well-known normal tendency in qualitative research (no matter what the approach to analysis), which occurs throughout the whole research experience from initial data collect-
would not only unnecessarily delay and interfere with his purpose, but the explicit coding itself often seems an unnecessary, burdensome task. As a result, the analyst merely inspects his data for new properties of his theoretical categories, and writes memos on these properties.

We wish to suggest a third approach to the analysis of qualitative data—one that combines, by an analytic procedure of constant comparison, the explicit coding procedure of the first approach and the style of theory development of the second. The purpose of the constant comparative method of joint coding and analysis is to generate theory more systematically, than allowed by the second approach, by using explicit coding and analytic procedures. While more systematic than the second approach, this method does not adhere completely to the first, which hinders the development of theory because it is designed for provisional testing, not discovering, of hypotheses. This method of comparative analysis is to be used jointly with theoretical sampling, whether for collecting new data or on previously collected or compiled qualitative data.

Systematizing the second approach (inspecting data and

tion through coding to final analysis and writing. The tendency has been noted in Becker and Geer, op. cit., p. 270; Berelson, op. cit., p. 125; and for an excellent example of how it goes on, see Robert K. Merton, Social Theory and Social Structure (New York: Free Press of Glencoe, 1957), pp. 390-92. However, this tendency may have to be suppressed in favor of the purpose of the first approach; but in the second approach and the approach presented here, the tendency is used purposefully as an analytic strategy.

3. Our other purpose in presenting the constant comparative method may be indicated by a direct quotation from Robert K. Merton—a statement he made in connection with his own qualitative analysis of locals and cosmopolitans as community influencers: "This part of our report, then, is a bid to the sociological fraternity for the practice of incorporating in publications a detailed account of the ways in which qualitative analyses actually developed. Only when a considerable body of such reports are available will it be possible to codify methods of qualitative analysis with something of the clarity with which quantitative methods have been articulated." Op. cit., p. 390. This is, of course, also the basic position of Paul F. Lazarsfeld. See Allen H. Barton and Paul F. Lazarsfeld, "Some Functions of Qualitative Analysis in Social Research," in Seymour M. Lipset and Neil J. Smelser (Eds.), Sociology: the Progress of a Decade (Englewood Cliffs, N.J.: Prentice-Hall, 1961). It is the position that has stimulated the work of Becker and Geer, and of Berelson, cited in Footnote 1.

The Constant Comparative Method of Qualitative Analysis

redesigning a developing theory) by this method does not supplant the skills and sensitivities required in generating theory. Rather, the constant comparative method is designed to aid the analyst, who possesses these abilities, in generating a theory that is integrated, consistent, plausible, close to the data—and at the same time is in a form clear enough to be readily, if only partially, operationalized for testing in quantitative research. Still dependent on the skills and sensitivities of the analyst, the constant comparative method is not designed (as methods of quantitative analysis are) to guarantee that two analysts working independently with the same data will achieve the same results; it is designed to allow, with discipline, for some of the vagueness and flexibility that aid the creative generation of theory.

If a researcher using the first approach (coding all data first) wishes to discover some or all of the hypotheses to be tested, typically he makes his discoveries by using the second approach of inspection and memo-writing along with explicit coding. By contrast, the constant comparative method cannot be used for both provisional testing and discovering theory; in theoretical sampling, the data collected are not extensive enough and, because of theoretical saturation, are not coded extensively enough to yield provisional tests, as they are in the first approach. They are coded only enough to generate, hence to suggest, theory. Partial testing of theory, when necessary, is left to more rigorous approaches (sometimes qualitative but usually quantitative). These come later in the scientific enterprise (see Chapter X).

The first approach also differs in another way from the constant comparative method. It is usually concerned with a few hypotheses couched at the same level of generality, while our method is concerned with many hypotheses synthesized at different levels of generality. The reason for this difference between methods is that the first approach must keep the theory tractable so that it can be provisionally tested in the same presentation. Of course, the analyst using this approach might, after proving or disproving his hypotheses, attempt to explain his findings with more general ideas suggested by his data, thus achieving some synthesis at different levels of generality.

A fourth general approach to qualitative analysis is “analytic
induction,” which combines the first and second approaches in a manner different from the constant comparative method.\footnote{See Alfred R. Lindesmith, Opiate Addiction (Bloomington: Principia, 1947), pp. 12-14; Donald R. Cressey, Other People’s Money (New York: Free Press of Glencoe, 1953), p. 16 and passim; and Florian Znaniecki, The Method of Sociology (New York: Farrar and Rinehart, 1934), pp. 249-331.} Analytic induction has been concerned with generating and proving an integrated, limited, precise, universally applicable theory of causes accounting for a specific behavior (e.g., drug addiction, embezzlement). In line with the first approach, it tests a limited number of hypotheses with all available data, consisting of numbers of clearly defined and carefully selected cases of the phenomena. Following the second approach, the theory is generated by the reformulation of hypotheses and redefinition of the phenomena forced by constantly confronting the theory with negative cases, cases which do not confirm the current formulation.

In contrast to analytic induction, the constant comparative method is concerned with generating and plausibly suggesting (but not provisionally testing) many categories, properties, and hypotheses about general problems (e.g., the distribution of services according to the social value of clients). Some of these properties may be causes, as in analytic induction, but unlike analytic induction others are conditions, consequences, dimensions, types, processes, etc. In both approaches, these properties should result in an integrated theory. Further, no attempt is made by the constant comparative method to ascertain either the universality or the proof of suggested causes or other properties. Since no proof is involved, the constant comparative method in contrast to analytic induction requires only saturation of data—not consideration of all available data, nor are the data restricted to one kind of clearly defined case. The constant comparative method, unlike analytic induction, is more likely to be applied in the same study to any kind of qualitative information, including observations, interviews, documents, articles, books, and so forth. As a consequence, the constant comparisons required by both methods differ in breadth of purpose, extent of comparing, and what data and ideas are compared.

Clearly the purposes of both these methods for generating theory supplement each other, as well as the first and second approaches. All four methods provide different alternatives to qualitative analysis. Table I locates the use of these approaches to qualitative analysis and provides a scheme for locating additional approaches according to their purposes. The general idea of the constant comparative method can also be used for generating theory in quantitative research. Then one compares findings within subgroups and with external groups (see Chapter VIII).

### Table I. Use of Approaches to Qualitative Analysis

<table>
<thead>
<tr>
<th>Generating Theory</th>
<th>Provisional Testing of Theory</th>
</tr>
</thead>
<tbody>
<tr>
<td>Yes</td>
<td>Combing inspection for hypotheses (2) along with coding for test, then analyzing data (1)</td>
</tr>
<tr>
<td>No</td>
<td>Coding for test, then analyzing data (1)</td>
</tr>
</tbody>
</table>

**The Constant Comparative Method**

We shall describe in four stages the constant comparative method: (1) comparing incidents applicable to each category, (2) integrating categories and their properties, (3) delimiting the theory, and (4) writing the theory. Although this method of generating theory is a continuously growing process—each stage after a time is transformed into the next—earlier stages do remain in operation simultaneously throughout the analysis and each provides continuous development to its successive stage until the analysis is terminated.

1. Comparing incidents applicable to each category. The analyst starts by coding each incident in his data into as many categories of analysis as possible, as categories emerge or as data emerge that fit an existing category. For example, the category of “social loss” of dying patients emerged quickly from comparisons of nurses’ responses to the potential deaths of their patients. Each relevant response involved the nurse’s appraisal
of the degree of loss that her patient would be to his family, his occupation, or society: "He was so young," "He was to be a doctor," "She had a full life," or "What will the children and her husband do without her?"

Coding need consist only of noting categories on margins, but can be done more elaborately (e.g., on cards). It should keep track of the comparison group in which the incident occurs. To this procedure we add the basic, defining rule for the constant comparative method: while coding an incident for a category, compare it with the previous incidents in the same and different groups coded in the same category. For example, as the analyst codes an incident in which a nurse responds to the potential "social loss" of a dying patient, he also compares this incident, before further coding, with others previously coded in the same category. Since coding qualitative data requires study of each incident, this comparison can often be based on memory. Usually there is no need to refer to the actual note on every previous incident for each comparison.

This constant comparison of the incidents very soon starts to generate theoretical properties of the category. The analyst starts thinking in terms of the full range of types or continua of the category, its dimensions, the conditions under which it is pronounced or minimized, its major consequences, its relation to other categories, and its other properties. For example, while constantly comparing incidents on how nurses respond to the social loss of dying patients, we realized that some patients are perceived as a high social loss and others as a low social loss, and that patient care tends to vary positively with degree of social loss. It was also apparent that some social attributes that nurses combine to establish a degree of social loss are seen immediately (age, ethnic group, social class), while some are learned after time is spent with the patient (occupational worth, marital status, education). This observation led us to the realization that perceived social loss can change as new attributes of the patients are learned. It also became apparent, from studying the comparison groups, under what conditions (types of wards and hospitals) we would find clusters of patients with different degrees of social loss.


6. Thus we have studies of delinquency, justice, "becoming," stigma, consultation, consolation, contraception, etc.; these usually become the variables or processes to be described and explained.
have run across in their own coding and data collection, and crosscheck his points. They, too, begin to compare the analyst's notions with their own ideas and knowledge of the data; this comparison generates additional theoretical ideas. With clearer ideas on the emerging theory systematically recorded, the analyst then returns to the data for more coding and constant comparison.

From the point of view of generating theory it is often useful to write memos on, as well as code, the copy of one's field notes. Memo writing on the field note provides an immediate illustration for an idea. Also, since an incident can be coded for several categories, this tactic forces the analyst to use an incident as an illustration only once, for the most important among the many properties of diverse categories that it indicates. He must look elsewhere in his notes for illustrations for his other properties and categories. This corrects the tendency to use the same illustration over and over for different properties.

The generation of theory requires that the analyst take apart the story within his data. Therefore when he rearranges his memos and field notes for writing up his theory, he sufficiently "fractures" his story at the same time that he saves apt illustrations for each idea (see Step 4). At just this point in his writing, breaking down and out of the story is necessary for clear integration of the theory.

2. Integrating categories and their properties. This process starts out in a small way; memos and possible conferences are short. But as the coding continues, the constant comparative units change from comparison of incident with incident to comparison of incident with properties of the category that resulted from initial comparisons of incidents. For example, in comparing incident with incident we discovered the property that nurses constantly recalculate a patient's social loss as they learn more about him. From then on, each incident bearing on "calculation" was compared with "accumulated knowledge on calculating"—not with all other incidents involving calculation. Thus, once we found that age was the most important characteristic in calculating social loss, we could discern how a patient's age affected the nurses' recalculation of social loss as they found out more about his education. We found that education was most influential in calculations of the social loss of a middle-aged adult, since for a person of this age, education was considered to be of most social worth. This example also shows that constant comparison causes the accumulated knowledge pertaining to a property of the category to readily start to become integrated; that is, related in many different ways, resulting in a unified whole.

In addition, the diverse properties themselves start to become integrated. Thus, we soon found that the calculating and recalculating of social loss by nurses was related to their development of a social loss "story" about the patient. When asked about a dying patient, nurses would tell what amounted to a story about him. The ingredients of this story consisted of a continual balancing out of social loss factors as the nurses learned more about the patient. Both the calculus of social loss and the social loss story were related to the nurse's strategies for coping with the upsetting impact on her professional composure of, say, a dying patient with a high social loss (e.g., a mother with two children). This example further shows that the category becomes integrated with other categories of analysis: the social loss of the dying patient is related to how nurses maintain professional composure while attending his dying. Thus the theory develops, as different categories and their properties tend to become integrated through constant comparisons that force the analyst to make some related theoretical sense of each comparison.

If the data are collected by theoretical sampling at the same time that they are analyzed (as we suggest should be done), then integration of the theory is more likely to emerge by itself. By joint collection and analysis, the sociologist is tapping to the fullest extent the in vivo patterns of integration in the data itself; questions guide the collection of data to fill in gaps and to extend the theory—and this also is an integrative strategy. Emergence of integration schemes also occurs in analyses that are separate from data collection, but more contrivance may be necessary when the data run thin and no more can be collected. (Other aspects of integration have been discussed in Chapter II.)

3. Delimiting the theory. As the theory develops, various

THE DISCOVERY OF GROUNDED THEORY

delimiting features of the constant comparative method begin to curb what could otherwise become an overwhelming task. Delimiting occurs at two levels: the theory and the categories. First, the theory solidifies, in the sense that major modifications become fewer and fewer as the analyst compares the next incidents of a category to its properties. Later modifications are mainly on the order of clarifying the logic, taking out non-relevant properties, integrating elaborating details of properties into the major outline of interrelated categories and—most important—reduction.

By reduction we mean that the analyst may discover underlying uniformities in the original set of categories or their properties, and can then formulate the theory with a smaller set of higher level concepts. This delimits its terminology and text. Here is an illustration which shows the integration of more details into the theory and some consequent reduction: We decided to elaborate our theory by adding detailed strategies used by the nurses to maintain professional composure while taking care of patients with varying degrees of social loss. We discovered that the rationales which nurses used, when talking among themselves, could all be considered "loss rationales." The underlying uniformity was that all these rationales indicated why the patient, given his degree of social loss, would, if he lived, now be socially worthless; in spite of the social loss, he would be better off dead. For example, he would have brain damage, or be in constant, unendurable pain, or have no chance for a normal life.

Through further reduction of terminology we were also discovering that our theory could be generalized so that it pertained to the care of all patients (not just dying ones) by all staff (not just nurses). On the level of formal theory, it could even be generalized as a theory of how the social values of professionals affect the distribution of their services to clients; for example, how they decide who among many waiting clients should next receive a service, and what calibre of service he should be given.

Thus, with reduction of terminology and consequent generalizing, forced by constant comparisons (some comparisons can at this point be based on the literature of other professional areas), the analyst starts to achieve two major requirements of theory: (1) parsimony of variables and formulation, and (2) scope in the applicability of the theory to a wide range of situations, while keeping a close correspondence of theory and data.

The second level for delimiting the theory is a reduction in the original list of categories for coding. As the theory grows, becomes reduced, and increasingly works better for ordering a mass of qualitative data, the analyst becomes committed to it. His commitment now allows him to cut down the original list of categories for collecting and coding data, according to the present boundaries of his theory. In turn, his consideration, coding, and analyzing of incidents can become more select and focused. He can devote more time to the constant comparison of incidents clearly applicable to this smaller set of categories.

Another factor, which still further delimits the list of categories, is that they become theoretically saturated. After an analyst has coded incidents for the same category a number of times, he learns to see quickly whether or not the next applicable incident points to a new aspect. If yes, then the incident is coded and compared. If no, the incident is not coded, since it only adds bulk to the coded data and nothing to the theory. For example, after we had established age as the base line for calculating social loss, no longer did we need to code incidents referring to age for calculating social loss. However, if we came across a case where age did not appear to be the base line (a negative case), the case was coded and then compared. In the case of an 85-year-old dying woman who was considered a great social loss, we discovered that her "wonderful personality" outweighed her age as the most important factor for calculating her social loss. In addition, the amount of data the analyst needs to code is considerably reduced when the data are obtained by theoretical sampling; thus he saves time in studying his data for coding.

9. If the analyst's purpose, besides developing theory, is also to count incidents for a category to establish provisional proofs, then he must code the incident. Furthermore, Merton has made the additional point, in correspondence, that to count for establishing provisional proofs may also feed back to developing the theory, since frequency and cross-tabulation of frequencies can also generate new theoretical ideas. See Berelson on the conditions under which one can justify time-consuming, careful counting; op. cit., pp. 126-34. See Becker and Geer for a new method of counting the frequency of incidents; op. cit., pp. 283-87.
Theoretical saturation of categories also can be employed as a strategy in coping with another problem: new categories will emerge after hundreds of pages of coding, and the question is whether or not to go back and re-code all previously coded pages. The answer for large studies is "no." The analyst should start to code for the new category where it emerges, and continue for a few hundred pages of coding, or until the remaining (or additionally collected) data have been coded, to see whether the new category has become theoretically saturated. If it has, then it is unnecessary to go back, either to the field or the notes, because theoretical saturation suggests that what has been missed will probably have little modifying effect on the theory. If the category does not saturate, then the analyst needs to go back and try to saturate it, provided it is central to the theory.

Theoretical saturation can help solve still another problem concerning categories. If the analyst has collected his own data, then from time to time he will remember other incidents that he observed or heard but did not record. What does he do now? If the unrecorded incident applies to an established category, after comparison it can either be ignored because the category is saturated; or, if it indicates a new property of the category, it can be added to the next memo and thus integrated into the theory. If the remembered incident generates a new category, both incident and category can be included in a memo directed toward their place in the theory. This incident alone may be enough data if the category is minor. However, if it becomes central to the theory, the memo becomes a directive for further coding of the field notes, and for returning to the field or library to collect more data.

The universe of data that the constant comparative method uses is based on the reduction of the theory and the delimitation and saturation of categories. Thus, the collected universe of data is first delimited and then, if necessary, carefully extended by a return to data collection according to the requirements of theoretical sampling. Research resources are economized by this theoretical delimiting of the possible universe of data, since working within limits forces the analyst to spend his time and effort only on data relevant to his categories. In large field studies, with long lists of possibly useful categories and thousands of pages of notes embodying thousands of incidents, each of which could be coded a multitude of ways, theoretical criteria are very necessary for paring down an otherwise monstrous task to fit the available resources of personnel, time, and money. Without theoretical criteria, delimiting a universe of collected data, if done at all, can become very arbitrary and less likely to yield an integrated product; the analyst is also more likely to waste time on what may later prove to be irrelevant incidents and categories.

4. Writing theory. At this stage in the process of qualitative analysis, the analyst possesses coded data, a series of memos, and a theory. The discussions in his memos provide the content behind the categories, which become the major themes of the theory later presented in papers or books. For example, the major themes (section titles) for our paper on social loss were "calculating social loss," "the patient's social loss story," and "the impact of social loss on the nurse's professional composure."

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 that others going into the same field could use—then he can publish his results with confidence. To start writing one's theory, it is first necessary to collate the memos on each category, which is easily accomplished since the memos have been written about categories. Thus, we brought together all memos on calculating social loss for summarizing and, perhaps, further analyzing before writing about it. One can return to the coded data when necessary to validate a suggested point, pinpoint data behind a hypothesis or gaps in the theory, and provide illustrations.10

Properties of the Theory

Using the constant comparative method makes probable the achievement of a complex theory that corresponds closely to

the data, since the constant comparisons force the analyst to consider much diversity in the data. By diversity we mean that each incident is compared with other incidents, or with properties of a category, in terms of as many similarities and differences as possible. This mode of comparing is in contrast to coding for crude proofs; such coding only establishes whether an incident indicates the few properties of the category that are being counted.

The constant comparison of incidents in this manner tends to result in the creation of a “developmental” theory. Although this method can also be used to generate static theories, it especially facilitates the generation of theories of process, sequence, and change pertaining to organizations, positions, and social interaction. But whether the theory itself is static or developmental, its generation, by this method and by theoretical sampling, is continually in process. In comparing incidents, the analyst learns to see his categories in terms of both their internal development and their changing relations to other categories. For example, as the nurse learns more about the patient, her calculations of social loss change; and these recalculations change her social loss stories, her loss rationales and her care of the patient.

This is an inductive method of theory development. To make theoretical sense of so much diversity in his data, the analyst is forced to develop ideas on a level of generality higher in conceptual abstraction than the qualitative material being analyzed. He is forced to bring out underlying uniformities and diversities, and to use more abstract concepts to account for differences in the data. To master his data, he is forced to engage in reduction of terminology. If the analyst starts with raw data, he will end up initially with a substantive theory: a theory for the substantive area on which he has done research (for example, patient care or gang behavior). If he starts with the findings drawn from many studies pertaining to an abstract sociological category, he will end up with a formal theory pert-