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Strategy for Analyzing

In virtually every chapter of the book we have made some reference to analysis, but more in terms of how it is linked to other strategies than as a process with its own distinctive properties. Particularly, we have emphasized its simultaneity and continuity with other strategies; also its self-corrective and cumulative character. We require, now, a more pointed discussion of qualitative analysis itself as a set of strategies.

Right off, we need to caution the reader against his own expectations that he may learn from us standard procedures for handling this task of analysis. Qualitative analysts do not often enjoy the operational advantages of their quantitative cousins in being able to predict their own analytic processes; consequently, they cannot refine and order their raw data by operations built initially into the design of the research. Qualitative data are exceedingly complex, and not readily convertible into standard measurable units of objects seen and heard; they vary in level of abstraction, in frequency of occurrence, in relevance to central questions in the research. Also, they vary in the source or ground from which they are experienced. Of course, data also differ according to substance, and, coupled with the ways data are gathered and the forms in which they are apprehended, may lend themselves to different sorts of operations. Little wonder, then, that field researchers cannot predesign their analytic operations with exactness; probably most do not even try.

Not only are the data variable and complex, but so are the analysts. They have had different training and subsequent experiences which, along with their variable temperaments and interests, have produced many analytic styles. Some researchers are satisfied to deal with uncodified, anecdotal data and depend almost entirely upon the fortuitous development of insight; at the other end of the spectrum are those who laboriously codify their data and apply more systematic analytic techniques, including statistical ones, to arrive at social theory.

What Is Analysis?

Our purpose in the present chapter is to help the novice redefine somewhat the concept of analysis—so that he might find it more comfortable to deal with—as *the working of thought processes* rather than as a formidable, academic abstraction. We hope to alter some of the imagery commonly associated with that concept. Our experiences with students lead us to believe that the two most dominant images they associate with the concept of analysis are Science and Insight. Neither is a very comfortable term with which to work. The first bears upon the status of the researcher as "scientist" rather than upon the character of the data and the pragmatic ways that he might work with them. The second bears upon the "genius" of the researcher through which analytic processes take on mystical quality; it offers little room for understanding the craftsmanship involved in the production of theory.

For the novice who is confronted with a mass of heterogeneous data, and who is trying to make sense of them, there is little advantage to be gained from discussing Science and Insight. A discussion of thought processes as strategies is more helpful; for once the components of any craft are understood, then the genius of even its most expert practitioners loses its mystical quality. Their skills may be recognized "simply" as a variation of ordinary thinking and ordinary skills. After all, to be told immediately that $9 \times 9 = 81$ is an act of genius and a mystery, until one comes to know of the existence of the multiplication table. Analytic thinking is not different from ordinary (but yet complex, logical and purposeful) thinking. As with all other aspects of the research process, analyzing data involves thinking that is self-conscious, systematic, organized, and instrumental. It is thinking, objectified and operationalized. Above all, it is extremely active—better still, an interactive process between the researcher and his experience or data—and it is sustained rather than intermittent or casual, as in ordinary thinking.

We need, parenthetically, to develop further a point made earlier bearing upon how one prepares for analyzing and when it may be done. Theoretically or arbitrarily, analyzing can begin when the first data are obtained, or it can begin after much or all of the data are obtained. In one sense, the option may be regarded as a *work strategy,* a matter of convenience in pacing or sequencing one's total work. On the other hand, we have already noted that our model researcher starts analyzing very early in the research process. For him, the option represents an *analytic strategy:* he needs to analyze as he goes along both to adjust his observational strategies, shifting some emphases towards those experiences which bear upon the development of his understanding, and generally, to exercise control over his emerging ideas by virtually simultaneous "checking" or "testing" of these ideas. This is why he prepares TN's as he goes along; for he will not only have a chronicle of his thoughts and some checks bearing upon their usefulness and validity, but will have saved himself from an otherwise crushing task of sorting out a mountain of data without benefit of "preliminary" analysis. Any given TN is—or potentially is—a "mini-proposition" that may even form the core of an analytic scheme; therefore, the systematic development of TN's can be thought of as preliminary analysis.

Discovering Classes and Their Linkages

Probably the most fundamental operation in the analysis of qualitative data is that of discovering significant *classes* of things, persons and events and the *properties* which characterize them. In this process, which continues throughout the research, the analyst gradually comes to reveal his own "is's" and "because's": he names classes and links one with another, at first with "simple" statements (propositions) that express the *linkages,* and continues this process until his propositions fall into *sets,* in an ever-increasing density of linkages. This, at least, is the operational model the analyst will use when he is attempting to encompass or account for the greater part of his data. Whether his objective is straight description,[1] analytic description,[2] or substantive theory,[3] the task of establishing and linking classes is mandatory.

[1] In *straight description,* the analyst accepts and uses theory and organizational schemes that are extant in the discipline: he simply finds classes in the data which correspond with those commonly utilized in the discipline or in more common parlance, and he arranges them accordingly; that is, he links his classes in ways suggested by received classificatory schemes.

[2] In *analytic description,* the organizational scheme is developed from discovered classes and linkages suggested or mandated by the data. Considerable novelty in description is thereby achieved, and with some further development in the analytic process, substantive theory can be made evident.

[3] *Substantive theory* is at least implicit in any description, even in straight description, but there it bears upon received theory. For new, grounded theory to be made evident, the analyst must reveal the metaphor or scheme he has worked with in the analysis. Then, he must transform the metaphor into sociological language, which, though it relates the analyzed object or process to traditional formulations, nevertheless establishes its own identity.

Key linkage

The analyst need not, however, link every class to every other, although he will probably have to perform this operation until a guiding metaphor or general scheme emerges in his thinking as he interacts with the data. For once the analyst gains a *Key Linkage*—that is, a metaphor, model, general scheme, overriding pattern, or "story line"—he can become increasingly selective of the classes he needs to deal with: classes to look for, to refine further, or to link up with other classes. The principal operational advantage to the researcher of creating or finding a key linkage is that, for the first time, he has the means of determining the significance of classes. Without it, he must give relatively equal attention to a vast number of the more obvious classes, and consequently will never feel comfortable enough to implement a closure process.

Without the key, one obvious alternative is to gather data until virtually nothing new seems to be coming in. Another alternative is to utilize fully a starting framework and any received theory as a key. Then he will have known all along which classes are significant and, having "located" rather than "discovered" them, he will have concluded his data gathering and most of his analysis rather quickly—and without much sweat (or profit). However, if he is intent upon developing theory, he will have to take the "long route" until he discovers a *grounded* key—one that is both original to him and faithful to his data.

Perhaps at this point we are a bit ahead of ourselves and must return to the model analyst quite early in his research, while he is still working with a starting framework. That framework, or any subsequent experience, may suggest substantive models or organizing schemes, but he makes no commitment to them. Such linkages as he imagines to be suitable will appear implicitly or explicitly in his TN's; these linkages are feelers towards working models suitable to all or portions of the data. (He has yet to enter into a kind of dialogue with his data about their efficacy and validity. Shortly, we shall discuss some of the nature of this dialogue.)

At this time we need to ask, How does the analyst arrive at his classes? We might try to answer this question in part by indicating some sources from which classes (often only as elements in classification systems) come to the attention of the researcher. For practical purposes, we shall identify a few bearing most directly upon social science field research.

First are the *Common Classes* of the culture generally, which are available to most anyone of a given society to help distinguish between and among the varieties of things, persons, and events. These classes, as names, provide discriminations that lay persons use in their thinking and communication, and that largely define a common or shared reality.

Second are the *Special Classes,* which persons within selected areas of interest or study utilize to distinguish among the things, persons, and events within their own province. We have in mind here two sub-varieties: (1) those inherent in the researcher's collegial group and (2) those inherent in the host group. Thus, the researcher as a social scientist has available to him from his discipline a discriminative array of class categories (in part serving as conceptual framework), which, at least initially, allow him to see and to organize the events at the site—for example, classes of charismatic and bureaucratic leadership, formal and informal structures as classes of organization, and so on.

In contrast to these "collegial classes" are the "host classes," which are also special but which provide, probably, some distinctive differences in discrimination—connotative if not denotative. In medicine are class names for wards, departments, and equipment (things), class names for echelon, specialists, and offices (persons), and class names for procedures, conferences, and work arrangements (events). Many names for special classes are also in common use outside particular areas of discourse, sometimes borrowed from, sometimes introduced into, the common fund of language. However, the ways in which they tie in with still other class terms, particularly as conceptual frameworks, often make them quite different connotatively from the same terms when in common use. The researcher is aware of this, also that special class terms are not necessarily used and understood universally within any specialized area.

Then there are the *Theoretical Classes,* those discovered by the researcher as observer and analyst that are his own constructs, whether their nomenclature is borrowed from other sources or invented. What makes these a very special source is that the classes developed in this process are grounded in the experience of observation in this specific substance, and are demonstrably applicable and useful to its analysis. For example, recall the field notes in Chapter 6 which suggested a class of "professional patients," and a "calculus of patienthood" suggesting a class name for the way any psychiatric hospital may distribute or "dispose" of patients. Since these classes are not available to the researcher until he has observed in the field for some time, he requires the use of common and collegial classes to gain physical and conceptual entrée, and to locate and establish the general boundaries of his field. In time, through observation, newly discovered and conceptualized classes replace the initial ones.

Perhaps we can illustrate with a simple example this process of shifting

grounds: In the study of a medical system, the researcher initially expects or is prepared to find evidence of doctors, nurses, hospital, clinic, and so on, and probably he has some model for how these fit together. When he gets to the site and observes for a brief time he finds, indeed, evidence for these classes. However, he learns that this particular hospital is not what he had imagined it to be: it is an "emergency receiving center" and has "contractual arrangements" with other hospitals for long-term treatment. Then, he learns that ideologically and operationally the core of the medical system within which the hospital is embedded is a series of store-front clinics distributed among strategic neighborhoods. Thus, the terms "hospital" and "clinic" are something different from his original expectation and require new conceptualization.

Upon visiting several of the clinics, he finds that each is manned by only one nurse who is assisted by several Licensed Vocational Nurses—a category of nurse with whom he was unfamiliar. Further, he finds, the physicians are not in evidence at the clinics, but are "on call."

We probably need not go any further with this illustration to make the point that the researcher must shift his grounds to accommodate both some changed and new class categories: the hospital is different, there is a new variety of clinic, he has discovered the LVN, and even the physician looks a bit different. Perhaps most important, he has discovered what appears to be an operational philosophy about health delivery that suggests that he should change his original model.

Thus, we can anticipate *the researcher will continue shifting his grounds as he creates or changes his classes, until all his presumed classes are displaced by those based upon observation,* whether his presumptions were essentially correct or not. He will then have a set or sets of theoretical classes, tested in experience and amenable to linking and to theory construction.

An Illustration of an Analysis

Certainly, it is not enough simply to discover classes, although this is a difficult task in itself, since it requires that their properties and boundaries also be ascertained before propositions bearing upon how they link to each other can be hypothesized and validated. What we require now is a more complicated and detailed illustration of how the linking process might be handled, and how the analyst arrives at his key linkages. The example we use here is a reconstruction of processes engaged in by the authors in a research venture a few years back. Since we must be brief, to indicate the many classes with which we were working, we refer the reader to *Psychiatric Ideologies and Institutions* (see bibliography). Here, we shall attempt to reconstruct some of the processes leading to the discovery of the conceptual scheme around which the book was organized.

Our illustration involves the study of a relatively small (80 bed), private psychiatric hospital consisting of five wards; also a relatively large complement of attending physicians, residents, nurses and nurses' aides.

The field researchers consisted of a team of three persons, the two present authors and one other colleague. We worked much in the manner outlined in the chapters on watching and listening. We divided our labor, prepared and shared our respective field notes, and frequently met to tell each other of our experiences. We had had practically no direct experience with psychiatry—and very little with medicine generally—prior to undertaking the research. With the very barest of frameworks, organized essentially around a general understanding that there were several different treatment philosophies at the hospital, we set about to discover how the many professionals there managed to organize and carry out their respective and collective tasks. Naturally, as social scientists, we were prepared to find that this social order was governed mainly by rules and norms. Yet, we held this hypothesis most tentatively to maximize the possibility of discovering a social order developed along different lines.

True to our own research philosophy, we hesitated making early commitments to conceptualization about the structure of relations among the hospital staff; consequently, our early field notes were exceedingly rich in detailed vignettes of encounters between and among the staff of the several echelons working there. In a sense, each vignette was a discrete story; and sometimes several vignettes constituted a longer story. Their early examination revealed among whom these encounters occurred—just about everyone there, within and between all echelons—among attending men, between attending men and residents, residents and nurses, nurses with attending men, and so on. Indeed, we discovered frequent encounters among all the logical possibilities for encounter. Not so incidentally, we were thereby able to discover the classes and sub-classes of personnel at the site. Then, an examination of the observational notes revealed that many, if not most, of the encounters occurred "incidentally," that is, at the point —and around the time—of some new or problematic incident or happening. Since we had observed that the staff encountered each other at still other occasions, such as at scheduled meetings, we were able to establish an "incidental" class of encounter.

Encounter content: rules versus informal agreements

What were these encounters all about? By simply scanning the vignettes we were able to classify varieties of content: transfers of patients from one ward to another; controls over patients through the use of drugs (including discussions or arguments over whether drugs should be used for this purpose); the "privileges" that patients might be granted (to use the phone, leave the ward unescorted); and who among the staff was to do what, and how, with the patients. There were many other substantive types of encounter besides these. Especially noteworthy and interesting were those involving special arrangements or agreements among personnel; for example, the head nurse balking at the assignment to her ward of a patient deemed "inappropriate" to that ward, and the physician asking her to "take him just for a few days, until. . . ." Among other things, an incident such as this allowed us to create such classes as "patient fit" and "misfit"; "overt" and "covert" agreements; also special classes of agreements, whereby certain nurses and physicians—or any jurisdictional combination —had long histories of such arrangements with consequences for who would side with whom when even more crucial issues arose.

As sociologists—and even as laymen—we were often surprised to find so many private agreements, so many expectantly (by us) ruled procedures become the subject of so many encounters. We began to ask the staff about the rules and norms governing the institution. To our surprise, we found very few able to list more than one or two beyond those governing fire and flood. We knew there were formal agreements among the staff because these were established at scheduled meetings that we also attended; yet, we also saw private agreements, which would bend or break the more formal ones, being made constantly. At about this time we, the researchers, began to use such terms as "pacting," "forming alliances," and "special agreements." Also, we noted that the alliances or pacts bearing upon virtually any agreement were often very short-lived, since any professional, at most any time, could make a new agreement or alliance with someone which frequently forced a revision or breakage in some previous agreement made with someone else. The frequent cries of "betrayal" added evidence for the many broken compacts.

It seemed that we were "on to something," but we still weren't certain of what this was; so we continued to pursue the general phenomena of "pacting" and "agreement-making." Continued examination of the data helped raise additional questions bearing upon why the staff seemed to find it so difficult to reach agreement on what seemed to us such ordinary problems as patient privileges, transfers, and general control of patients.

Our starting framework gave one clue to this question, namely, *differential treatment ideology.* We had originally planned to administer a detailed questionnaire to all the staff bearing on treatment philosophies. This was done, and in addition the fieldworkers interviewed staff on their philosophies while also observing them at work. As expected, we found some major differences among them, and we were able to attribute many of the pacts, agreements, broken agreements and misunderstandings to differential treatment ideologies.

But the staff did not exactly argue ideology in an abstract sense; they argued operations bearing upon real patients in real situations. It was then that we discovered another concept that seemed to explain why even ideologues do not always carry on their work according to ideological dictates. These professionals developed *operational philosophies* as a median ground between pure idea and the pragmatic necessities of collaborating with others of different ideological faith.

Indeed, their work had to do with treating and caring for patients, although it often seemed to us that the staff was far more concerned with each other than with the patients. What was now most important to us was that we could begin to explain *why* this concern with each other was so important to the staff. The work that these professionals had to do by way of treatment and care had to be operationally manifest through agreements bearing upon such mundane but necessary matters as the ward placement and transfer of patients and patient control. To do all of this in the absence of hard-and-fast rules and norms—which could hardly be implemented, much less written down—the staff had to reach agreements, day-to-day, on large numbers of very concrete issues bearing on the handling and disposition of patients.

Meanwhile, we were raising questions about where these treatment ideologies had come from. Quite empirically, we related the various ideologies to the *professional careers* of the professionals and to the *career models* which they were developing and living out. Data for these concepts and relationships came in copious amounts from our interviews with most of the staff. Widely different backgrounds in training and other experiences in psychiatry were evident.

By this time we had assembled many propositions about careers, and an equally cogent set of propositions about treatment ideologies. We had long since done the same for institutional structure through observations of the intricately complex operations of staff on the five wards. It did not, then, require a giant step for us to reach our concept *arena* which helped us link —locatively and situationally—our developed sets of propositions on professions and careers, ideologies, and institutions. Now we were able to view each ward as a location and arena where varying professionals could be found at different stages in their respective careers, adhering to varying ideologies, and implementing ideologies and career models through their development of operational philosophies that were compatible with institutional structures and requirements.

From this vantage point, we were able to reach our prime linkage—

negotiated order—which allowed us to "cross-cut" every one of the subsidiary conceptual links, and relate to each our major classes. Our analysis, then, was essentially complete: We were confident that our major and minor classes, concepts and linkages had a maximum of explanatory power. Also, we believed we had discovered a theory—a class of social institutional order—grounded in primary, empirical data.

A final point on the above description of our analysis: given the limitations of writing as a form of communication, and the practically insurmountable task of showing *exactly* how we applied complex and varied analytic procedures to highly complex data, we can only hope that the reader now, very generally, understands how we worked in that particular research situation. By no means have we attempted a full description. We do not wish to leave the reader with the impression that our analysis of our data was the only one or the only legitimate one.

But that in general is how we proceeded, moving back and forth between gathering and analyzing the data. The two processes sometimes are virtually simultaneous, although more often they are separate in time: sometimes separate by some days or weeks, sometimes occuring during the course of the same day or even hours. At any event, the analytic processes are "grounded" in the data—where "grounded" means *both* interpretation of the data and checking upon that interpretation by the gathering of more data.

Conceptual Levering

In preparing this chapter, we had in mind two beginning researchers—one who had done most of his data gathering with care given to its organization and ideation, and one whose data are not only poorly organized but exciting to him only in an intuitive sense. The former novice is well along with his analysis, having prepared TN's and Memos that give him some analytic advantage over his experiences; he is probably headed towards the kind of structural analysis just depicted.

The latter novice poses a more serious problem, mainly because he had no clear analytic goal or research model when he began to gather his data. Perhaps he expected that his data and his "genius" would eventually determine the outcome of his study: ethnographic description, substantive theory, formal theory, or "merely" a few cogent concepts that might shed some light on some especially interesting processes he had observed. Now, surrounded by data, he is probably at the mercy of whatever form and content they present to him. What is he to do? He may not understand or appreciate the kind of structural analysis we have proposed; he may feel his data warrant another kind of treatment.

It may help now to discuss a number of techniques the analyst might use to gain conceptual leverage on his data, either as a preanalytic strategy or as analysis itself, depending upon the state that the data are in. By "lever" we mean any thinking device that both distances the analyst from his data and provides a new perspective on them, so that he may enter into a new relationship with his data. A major problem is that the data do not "speak for themselves"; they barely hint at something, and then only if someone is able to hear. "Hearing" in this sense is an active pursuit of meaning, but only if the listener has some conceptual apparatus to begin with. Unfortunately, data do not leap off the pages to provide the analyst with the insight or genius he needs to "carry it off." This suggests the need for an active discussion, in the context of a triad, among the analyst, an audience, and the data. There are two general possibilities: one where the analyst attempts to gain leverage over his data by communicating them to an audience; the other where the analyst more directly interrogates his data in preparation for later communication.

Communicating the Data

Assuming that the data are more scattered than organized, that the form they are in does not tell a straight nor consistent "story" nor offer a thematic representation of the research experience, then probably the best first step is to develop an elemental description of what was observed. By "elemental" we mean a straightforward, detailed "laying-out" of the significant classes and their properties of the scenes observed: the people there, their understandings, and their activities. Many practiced ethnographers do as much, and make contributions to knowledge in precisely this way.

Lest the researcher get bogged down trying to write for a conjured audience of social science sophisticates, he might try first conjuring a more mundane audience, to pry loose a "good story." "Audience conjuring" often proves effective as a levering process. Since one can hardly write or say anything without there being some real or imagined audience to receive it, any description necessarily will vary according to the audience to which it is directed. Audiences "tell" what substances to include, what to emphasize, and the level and complexity of abstractions needed to convey essential facts and ideas. Since the researcher is party to this dialogue (and audience to himself), he will probably get to know his data better—that is, get to know the data in new ways and thereby discover new properties and linkages in them.

The logic of this procedure, given this stage of data development, is to make it necessary for the analyst to invent an organizational scheme for his data. For in the process of organizing a description, the analyst will be unable to escape having to provide some of the more cogent categories he will need: classes and names for them; relationships among these classes, and names for them too. Moreover, he will not be able to do all this without also providing connective categories necessary to even the most ordinary communication: locative (in time and space), sequential, causal, correlative, consequential. How can anyone describe a complex, ongoing scene without coming to terms with process, or describe social relations—other than in list form—without developing a sense or model of structure?

Moreover, the analyst will necessarily be forced to create priorities for his experience, selectively ignoring some—he cannot include every one—relegating some to the status of context or background, and placing others into the foreground of his description. In this process, he will at least have implicitly established a number of propositional statements and, if the least bit inventive, he will have coined terms or phrases which may constitute key concepts for any later analysis he may wish to pursue.

In addition to audience conjuring (for purposes of writing description), the researcher might also try telling his story to a live audience, especially an interested and sympathetic colleague. There is, we think, a qualitative difference between writing and telling; for many of our readers this may be painfully true as they "tell it beautifully" and then block at writing the same representation. Likewise, there is a qualitative difference between talking to a person and to a tape recorder. We suggest, then, that some researchers might seek out live audiences, and make telling the story a lever. Live interaction exhibits important properties, and more than ordinary "feedback" is implied here. When one speaks to another about something, one also speaks to oneself; hence, the speaker may be a greater stimulant to himself than is the (other) listener.

What are the possibilities here? The listener's comments may "catalyze" the data for the speaker; the listener's questions may "catalyze" the speaker; the speaker may "catalyze" the data in the process of telling it without much overt help from the listener. If any of these consequences of communication occur, the researcher will have levered himself into a new relationship to his own data.

Interrogating the Data

Whether or not the analyst achieves leverage on the data by communicating them to others, and if so, whatever the outcome, he must finally consult his data directly. He must put to the test whatever ideas he may have developed about what the data have to say. If, until then, he has developed no exciting ideas, he must tease them out of the data, and when he gets them feed them back for a test—that is, search for supporting and negative evidence. The analyst cannot *tell* the data what to say. However, he may question them, or pose as queries the cogency and the validity of models, general concepts and metaphors. In reply the data might answer "yes," "no," "maybe"—the latter reply signalling there is too little supporting evidence for the idea, or possibly that the idea, though supportable, is not particularly cogent.

What to ask of data

But how does the analyst know what to ask the data: what models to pose, and operations to perform? We suggest he make use of two mutually supporting sets of levers—one substantive, one logical—for gaining the distance and the variability in perspective that will provide the questions and the models. The *substantive* set is made up of the special, abstract vocabulary of the analyst's own discipline—in the social science, such concepts as institution, ideology, work, career, collective behavior, social movement, and charisma. These concepts are often clustered and provide frameworks that will help the analyst *start* his questioning. We say "start" only to alert the novice to the dangers of using these received concepts *finally* to define and to organize the reality with which he has been dealing. They are, however, perfectly good and useful levers for preliminary analysis; they provide perspective. Consider the concepts "institution" and "social movement": Although they both deal with the affairs of people working in concert, they nevertheless evoke very different kinds of imagery and ideation. In short, to apply either to the data gives one a different perspective or angle of "vision" from the other.

The second set of levers is primarily the *logical, operational armamentarium of science,* for example, experimental, comparative, historical, analogical thinking and working processes. All of these—and still other processes, such as setting up polarities—provide considerable differences in perspective as well as of operation and so help produce the ideas that link datum to datum in various configurations. Most analysts develop skills and styles around these operations, and probably select problems and data that "lend themselves" to favored analytic operations: some analysts use the comparative method and are virtually ahistorical; others think experimentally and some even confine their research to this form; still others work most effectively through analogy and metaphor. However, these are not mutually exclusive forms of operation, and certainly not of speculation and thought.

Ordinarily, any analyst will utilize at least one component in each array as a set of levers for analyzing his data. Unless he has made a commitment to a particular analytic combination sometime before, or early in the research, the analyst will think about and test various combinations once he has most of his data before him. If his data are substantively rich and varied, he may find that certain operations will work better for some portions of the data than others.

To illustrate the utility of a substantive-logical lever combination: imagine observing a city substantively through the eyes (perspective) of a lay homeowner, a realtor, an urban planner, and an urban historian; then vary the position of observation, logically, by walking along the streets, by bicycling and motoring through it and then flying just above it in a helicopter. Assuming one were able to take these perspectives, in combination, the city as "data" would naturally present itself in a variety of conceptual patterns.

Working with abstract forms

Novices occasionally, if not characteristically, bog down in their attempts to utilize substantive levers because they view them as real forms. Experienced researchers and scholars more often see through these abstract devices to the ordinary, empirical realities they represent; they are thereby capable of considerable conceptual mobility. Thus, we urge the novice in analysis to convert relatively inert abstractions into stories—even with plots—in order to induce themes and models that link datum to datum.

Better still, he might best go directly to the data to *discover* "institution" or "social movement"; they evoke different kinds of stories. This way, the analyst escapes the formal stereotype inherent in the concepts; he deals with very human and live phenomena that are amenable to story-making and probably productive of new constructs. The story line can always, later, be reconverted to formal terminology, should the analyst find it necessary. In the meantime, he deals comfortably and naturally with what appears only as description and illustration, but which is but a short distance, conceptually, from generalized social process.

Combining levers

A similar process can be applied to the logical levers as well, although here the questions take different form. Rather than deal separately with substantive and logical questions, let us illustrate their combined uses—again, bearing in mind that we are dealing with starting levers that stimulate thinking. Any valid idea, worth a few minutes of thought, should be carried forward to the limits of its conceptual usefulness; it may become a central or sub-theme, or simply function catalytically for still another idea.

Imagine now that the researcher has his data before him and is in a quandary on how to proceed. Since the data are already gathered, and unless he can return to the site for an extended period, he cannot work the experimental lever. Practically, the options left to him are *comparative* and *historical* levers.

Let us suppose the researcher has some very rich data on a service institution. The institution is organized into several segments—wards, services, or offices, all of which are organizationally and structurally similar and therefore suggestive of comparative analysis. He decides to use the comparative lever; the data allow him to do so. Then he searches for substantive handles and comes up with *leadership, communications,* and *division of labor*. He works with these for a time, and finds it rather discouraging that, though his data offer many suggestions, they are too thin for direct development.

Then, are these data really that "rich" after all? The analyst may raise the question on what substances the data *do* offer him analytic possibilities; and he may, for a time, suffer through having to construct new substantive rubrics to find out what he does have. In this process, he discovers that, at the time he was observing, there was much going on at the institution having to do with some personnel leaving the institution and new ones being hired. He thereby finds himself a *substantive* lever—something to do with "succession." That he has this data in considerable abundance is a function in part of his having exercised relatively little control over his observations; but it is also due to a historical (temporal) condition of the institution at the time he was there, whatever his original intention may have been. He then begins to see (perspective) that the succession of personnel at the institution provides him with a lever on much of his other data: succession activity highlights many structural and organization properties of the several services that he now knows in new ways. As a matter of fact, by virtue of perspective it now becomes clear how these other data can be used—they can be built around his central substance.

Now the questions that he poses to the data can give the affirmative answers he needs, since the data are rich enough for development along the above lines. What about "succession"; how would he define it? Perhaps he has some difficulty here, and decides he can wait until he is more familiar with his data on succession. Looking at the data from this perspective he finds that succession occurred on three of the services, and in two of them not at all; further, that where it occurred, it involved three distinct echelons of personnel. There is, he now sees, an obvious opportunity to compare succession with nonsuccession, and succession in one echelon with two others.

Then it may occur to him that he has two general problems: the first one having to do with the *process* of unseating and seating personnel, and the second with the differential *consequences* of these processes—per service, per echelon—for leadership, communication, and division of labor as these reflect off the succession process.

Indeed, now that he has looked over his data from this perspective, he finds that what he has on leadership, and the other two "aborted" levers deals mainly with the succession problem in the institution studied. Now the analyst is relatively secure, both in his understanding of the data and how he must now proceed. He can now begin to ask about, and search for answers to, differential mechanisms (per service, per echelon) for unseating one person and for seating his replacement. If some had quit with "regret" and with due notice, and others were fired and left in haste, then the analyst has still other comparative possibilities.

Of course, it may occur to him that he does not have a sufficient number of cases to do a "proper" comparative analysis along several variable lines. Yet, this is the data he has; it is at least suggestive if not definitive of any generalizations he may develop around the problem of succession. He can think of his generalizations as hypothetical and promise himself to do another related study in another similar institution, or farther afield in institutions that are organized differently and do different kinds of works. In that event, he would be reaching out from *substantive* theory bearing upon a given institution or type of service to more *formal theory of a social process* applicable to many kinds of institutions and associations.

But to return to the question that the analyst may pose: What do the data tell about mechanisms for replacement? About how loyalties to the displaced persons are handled? About processes of disengagement on the part of those leaving and those being left behind? About the interim period? Then, there are questions about the selection of replacements: who is involved, what sorts of negotiation go on and expectations developed by negotiators; also, differentially depending upon the level of the replacement, how are the new people ushered in, how are they socialized, and who does the coaching? Also what sorts of claims are made by the new people? How are these expressed and dealt with by the old-timers? Finally, what happens to old agreements and rules that bound those who left and now confront the successors with their different identities?

Throughout this entire interrogation, the novice can call upon his extensive experience, wherever it may have been—to conjure a story line of how this process gets worked out anywhere, for *past experience is also a lever*. The analyst will not, of course, inject into his data something experienced earlier or elsewhere, but he would be foolish not to look for events that are suggested by experiences. For example, he may at one time have been party to a "messy situation," wherein a colleague of his was fired from a job, and he remembers many consequences of that event. From that experience he may even be able to work out a model of "cooling out" the displaced person and of his allies who were left behind, and then search his own data for parallels and differences. This may lead him to a subsidiary perspective on his data bearing upon the tactics and countertactics of firing and hiring, of alliances, of processes of exacting concessions in exchange for "keeping the peace."

As to the analyst's other discovered problem (the differential consequences to the various services and to the entire institution of the succession process), he has still other questions to ask: What changes occurred in the division of labor, differentially, in the various services? How about differential impact depending upon the echelon which had the replacement? The analyst traces or tracks down consequences of the events for leadership and for communication; but he may also, by this time, have discovered new classes he did not know existed before as some persons at the site "lined up" pro and con in new combinations.

He may also have discovered that the consequences of succession for the clients being served by the institution were not considerable. But he may have too little data on this aspect of the research to make valid propositions. In that event, he must suffer the consequences of his own failure—while in the field—to have asked the kinds of questions he is now belatedly posing. He may now understand why he can evaluate his data as good or bad, for they are either only to the extent they help him answer his present questions.

Our model researcher—the first novice—had been raising these questions all along during his research, and had been guided by them to build density into his ON's and TN's. He had developed a language and a grounded framework: claims, negotiations, tactics, consequences, and the like—some newly coined, some borrowed from other special languages. With cogent as well as copious data, he is better able than his counterpart (to whom we have been addressing ourselves) to find answers to his questions. However, each novice in his own way had come to realize that in working particular data he had discovered both *general processes* and *general questions* applicable to other fields.

Another lesson learned is that not every bit of data need be included in the final scheme; that is, except for negative evidence that may invalidate the final statement, the researcher need not pursue his original intention to prepare a description of his whole experience. Researchers frequently "spin off" pieces of a whole for publication and later, if not satiated, go on to other segments of the field. The "whole," after all, is a construct; and the "part" dealt with may later be redefined as the whole. This is but another illustration of how new perspective and discovery may alter "reality."

Historical questions

Let us now shift our attention to historical questions to show how ideas may be generated and analysis sparked. We shall use a social movement as our illustration. As with the data on the service institution, there is an apparent richness, but again the analyst cannot gain sufficient or significant leverage. He has spent several months somewhat systematically visiting among locales identifiable (in common parlance) as "hippie-type" communes. Common and special vocabularies describe members of this movement as having become "alienated," "turned off," "anti-establishment," and now in their new life as "tuned in" and "doing their own thing." Most of the data tell about relationships among members living in these communes; lesser amounts tell of the work they do there, of their ceremonies and rituals; and many interviews offer data on the beliefs these people hold about themselves and about the "established" society they despise.

Of course, the analyst may do some comparative analysis, since he had been to many communes that exhibit strikingly similar as well as dissimilar attributes. He also has data, based primarily upon interviews, which tell how persons in the movement became involved in it. In examining these data, the analyst is drawn to two kinds of historical levers: the first is based upon the longitudinal and career models embedded in the empirical data that he has; the second lever is more social-philosophical, bearing upon the model of social forms emerging out of general social-cultural properties characteristic of a time or era in the life of a society. On the latter, the analyst has no ready data nor ideas grounded in his current research experience. Yet he may raise questions prompted by this model to help him see the theoretical possibilities in the data which he does have.

Historical versus comparative analysis

A word first about the relationship between historical and comparative analysis; they are not in all respects distinctive. If our analyst were to compare a social form with itself at an earlier stage in its own history, he would be simultaneously thinking historically and comparatively. He would also be doing so if he were to examine his own movement against a backdrop of readings on the "Bohemiam" and "Beat" movements of some decades past. Imagine a chess board with the horizontal squares providing comparative social forms, and the vertical squares offering temporal stages in their development. If our analyst were to "place" his movement in a center square, and then imagine expressive movements in the squares to the left and political reform movements to the right of it, he would then have prepared a *comparative base* across the theoretical board of social movements. He would also have a basis for comparing historically the various stages of development of each movement. Even if he were to have no primary data on the other movements, some readings on them would probably spark many questions bearing directly upon his own data. For example, from what strata of the general population did these movements spring? (social class, age, sex, region and so on). What were these people doing occupationally prior to joining the movement? To what extent did joining the movement constitute a total commitment? What sorts of social organization evolved among members of each? What kinds of rituals, ceremonies and norms of interpersonal conduct developed and at what stages?

If the analyst were to do this kind of semisystematic reading and questioning, he would have attained both comparative and historical perspective, and the distance necessary for a new look at his own direct experience. Where was this movement some two or three years ago in organization, membership, beliefs? Are the same kinds of people still being recruited into it now? How did the relations between the sexes evolve? It is our guess that in this process of questioning, the analyst will have discovered distinctions not only between his own movement and others but also within the various segments of his own. Thus, he might further subdivide the square within which he was working and sort out several sub-typical segments. Once again he can apply the same kinds of questions to each of these.

The comparability of data

An important implication of what we just wrote is that, in the social sciences, one never really studies even a single case of some social phenomenon without at least implicitly making internal distinctions that are amenable to comparison. Also, it is difficult to imagine the study of a case without suggesting how it relates to *other* realities of like and different kind on the same plane. Otherwise, would our analysis be social science? How, also, would anyone read the case without temporal-spatial reference and without social-cultural context that might suggest *general conditions* of which this case is a single instance.

We need make a distinction between the operations of our two novices bearing upon the ideas just presented. In the case of the second novice, who is not likely to return to the field, the reading and the thinking about comparison groups functions primarily as a *stimulant* to operations on the data he has. For our model researcher, the operational possibilities are greater, since he can build the product of his comparative thinking directly into his own data as they are discovered, as well as have it help with observation in the field.

Some contemporary methodologists insist that the only valid data are those which the researcher himself gathers. We think not. *Concepts, models, and even data drawn from examinations of secondary sources can be utilized directly and openly by the researcher in any way which facilitates his understanding,* not only of his field, but of any other field that conceptually bears upon discovered generalizable processes.

Suggested Reading

BARTON, ALLEN H., and PAUL F. LAZARSFELD, "Some Functions of Qualitative Analysis in Social Research," *Frankfurter Beitrage zu Sociologie*, I (1955), 321–61. Also in McCall-Simmons, *Issues in Participant Observation: A Text and Reader*, pp. 163–96. Reading, Mass.: Addison-Wesley Publishing Co., 1969.

A lengthy article suggesting useful modes of analyzing qualitative data; some excellent suggestions (models) for levering data.

BECKER, HOWARD S., "Problems of Inference and Proof in Participant Observation," *American Sociological Review*, XXIII (1958), 652–60. Also in McCall-Simmons, *Issues in Participant Observation*, pp. 245–54.

A systematic discussion of basic analytic processes carried on in field work; consideration given to such important matters as the credibility of qualitative data and their frequency and distribution.

BECKER, HOWARD S., and BLANCHE GEER, "Participant Observation: Analysis of Qualitative Data," in R. N. Adams and J. J. Preiss (eds.), *Human Organization Research*, pp. 267–89. Homewood, Ill.: The Dorsey Press, 1960.

Excellent article supporting the logic of field work through a discussion of the organization and analysis of qualitative data.

GLASER, B., and ANSELM STRAUSS, *Discovery of Grounded Theory: Strategies for Qualitative Research*. Chicago: Aldine Publishing Co., 1967.

An influential work offering a distinct perspective on the analytic uses of qualitative data for the discovery and understanding of social processes. See especially Chapter 5, "The Constant Comparative Method of Qualitative Analysis," which may also be found in *Social Problems*, XII (1965), 436–45.

STRAUSS, ANSELM, et al., *Psychiatric Ideologies and Institutions*. New York: The Free Press, 1964. See especially Chapter 2, pp. 18–22.

A small list of items on organizing and analyzing qualitative data which complements our current discussion.

8

Strategies for Communicating the Research

We had originally planned to develop separate chapters for validating the research findings and for telling and writing about them. However, since research is ultimately addressed to audiences that selectively and variously judge its validity, these apparently disparate processes can logically be combined into a single discussion. One may argue that the researcher ought best to address himself to the canons of Science rather than to audiences, which, in contrast, are more ephemeral and often ideological. Yet, even the canons of Science are the product of human thinking, of human groups that define and sustain them. Besides, the researcher must very practically address himself to people, even if also to Science.

Then which people or groups does he tell of his work? Should he select only those who represent themselves as methodologically expert? What a pity, and a bore! Even among methodologists within the social sciences there are sharp differences in perspective on what is, and what is not, acceptable research. But perhaps this is a philosophical or ideological issue we had best not deal with here. We prefer simply to take a sociological, and practical, position on the question, and write mainly about the researcher communicating with audiences he is likely to encounter in the course of

and as a consequence of his research. Questions about the validity and reliability of the research can then be examined in the context of communications with those who would judge it.

We are aware that many of our readers are graduate students with pressing problems in writing up their research, bearing particularly upon methodological issues; therefore, we shall not neglect to discuss strategies for handling this problem. These students face faculty committees, which can be most exacting and critical. Because there are so few faculty on a committee, and because they play such a special role in that capacity, they tend to represent relatively narrow substantive and methodological interests. Yet they are so powerful! Little wonder, then, that so many students do their research exclusively for this audience and ignore any others.

It may be of some comfort for students to know that even accomplished researchers do not also escape close scrutiny and judgment: "readers" who help publishers decide whether to publish articles or books can be harsh indeed; and after books are published, writers can await very harsh critiques published as "reviews" in many different journals. However, it is not unusual to find both praising and damning reviews appear simultaneously in separate journals, attesting both to the politics of criticism and to the variability in criteria used in passing judgment. Yet, we suppose the established researcher still has considerable advantage over the student, since his reputation will probably help him find a publisher willing enough to give the writer access to multiple audiences with wide and compatible interests.

Multiple Audiences and Communication

During any stage of his study, the researcher is likely to be in communication with one or another audience about the substance or methods of his research. These audiences will vary widely in how they relate to the research as a project, to the research findings and operations, and even to the researcher as a person: They comprehend, selectively use, and judge the work from a variety of perspectives and interests. Some audiences are methodologically sophisticated and take an interest in the research almost solely in terms of the acceptability (to them) of the research procedures used; other audiences are interested primarily in gleaning substantive ideas, and validate or invalidate them informally according to their own experiences, intuitions, and logic. But this example—presented as a polarity—indicates only one dimension in the variability of audiences and their respective modes and criteria of judgment.

Before discussing particular audiences and ways of dealing with them, we shall tell in advance of two central points we wish to make in this concluding chapter. The first takes us back to what we had written earlier in the book about the nature of *reality:* that it is neither fixed nor finite, that it is infinitely complex, and that the observer holds the key to an infinitely varied relationship with "it." Well, audiences are observers too, and they, no less than the researcher, hold keys to understanding it. Therein lies the essence of a central problem in communication as it affects the researcher: he must make judgments on specific and general interests his audiences may have, on what sorts of information they might appreciate, need, or demand, and what their sense of credibility will allow them to accept.

This leads us to the second point: the researcher—just as he must decide on what to look at, listen for, and analyze—must likewise make decisions on whom to tell, what and how much to tell, and how and when to tell. Similarly for writing: when to write (during or after the research), for what audience(s), and how. Such decisions rest upon assumptions of the researcher on what his audiences will accept as important and valid; therefore, he needs to anticipate the kinds of questions that his varying audiences will pose and decide how he will later defend his data, his ideas, and his methods.

Now we have come "full circle" back to our proposition that the researcher will meet many audiences with varying expectations on substance, and with varying standards for establishing the credibility of what they read or hear. Of course, the number and range of audience types will differ for each researcher; his options to avoid, lightly entertain, or seriously meet "head on" these different audiences will vary, as will the risks entailed for avoiding or making contact. Also, the researcher will experience variable conditions for engaging with audiences in informal conversation, "informal" seminars, formal speeches and different orders of formal writing. If the reader were to take into account and "cross cut" the variables written into this paragraph, he would be well along towards developing a structural model for communication during and after a given species of research project.

Let us consider now what might be a typical course of audience encounters experienced by our model researcher. If he is a student, most of his work will have been preceded by a series of conferences with one or more professors; if he is a postdoctoral fellow or young professional, he will have played the joyous game of "grantsmanship" with a funding agency, and probably will have consulted with colleagues and "old pro's" at the game to help bolster his case with the granting committee of the agency. Already, he will have encountered several audiences to whom he has stated his problem and fashioned his research plan. Now at this stage, and with these audiences particularly, he has convinced very powerful others of his own abilities to fulfill the overall requirements of the research, to understand the relationship between what he is about to do and what his forebears have already done in that area, and to finish the task. All of this probably with little or no data.

Host audiences

In addition to the aforementioned classes of audience, the researcher will have confronted varieties of hosts in the field setting. This set of encounters has already been dealt with at some length in an earlier chapter. It should be borne in mind that the hosts have their own particular identities (real or self-styled) as administrators, policy-makers, practitioners, "activists," theorists, and even researchers. These same identities are also found outside the research field: leaders of other, but similar, organizations and movements may be no less interested in the substance of the research findings, in solutions to very practical problems affecting themselves, and in the implications of the research for broader policy development and organizational work. Thus, once the researcher is well along, or finished with his data gathering and preliminary analysis, he can begin to map out some of the available options concerning whom to write to, whom to speak to, and what to say.

Even if he were doing the research as a thesis necessarily addressed in part to his faculty committee, he will still have many other audiences with whom to communicate: his closest colleagues, his own more general colleagueship or profession, the larger group of which his hosts in the field are only a segment, varieties of lay audiences with real and sustained interests in the field he has researched, and so on.

Levels of Publication

Social psychologically, it is the multiplicity of audiences and the researcher's awareness of others' perspectives and information needs that account for much of the complexity in the researcher's thinking; in turn this accounts for the complexity of the data which he will or has gathered. Therefore, the researcher may simultaneously prepare several presentations directed at many audiences: a written piece for the "house organ" of the hosts, one for a popular magazine answering to a general public interested in social commentary, and another for one of the researcher's own professional journals. There are many other possibilities: a talk or an article on his methods of field research, on the development of his theory, on the implications of his findings for policy-making, and several different presentations bearing upon different topical aspects of his research.

For each of these presentations, the proffered data, and the researcher experience generally, are shaped to an audience; the form of the presentation is also audience directed: general essays, polemical articles, social commentaries, scientific tracts, descriptive monographs. The language used and the topics emphasized will differ according to the audience in order to effect not only good, but comfortable and mutually interesting communications. This way, the data can be "mined" for several years in a multitude of ways and for many audiences—and many a researcher has done just that.

In preparing for any telling or writing, and in imagining the perspective of his specific audience, the researcher is apt to see his data in new ways: finding new analytic possibilities, or implications he has never before sensed. This process of late discovery is full of surprises, sometimes even major ones, which lead to serious reflection on what one has "really" discovered. Thus, it is not simply a matter of the researcher writing down what is in his notes or head; writing or telling as activities exhibit their own properties which provide conditions for discovery. Once the products of these unintended consequences are apprehended, they are generally incorporated into still later speeches and writings, and in the "final" writing.

Likewise, once the researcher has told or written his account—whatever its content and form, and for whatever audience—thoughtful criticism and questions from any audience will suggest further reflection, and possibly some revisions in thinking about the data or about the ideas generated in preparation for the communication. As a communicator the researcher takes criticism in stride, and selectively deals with it. Some criticism goes directly to any weakness the research may have had; some is "misplaced," possibly because of failure in the communication itself, or because the content or style of the communication was directed at the "wrong" audience. Any journal or magazine, for example, will "turn down" a submitted article which does not meet its requirements for form or content; it, too, has audiences. But this does not necessarily reflect upon the validity or usefulness (to still others) of the communication. If the research itself is an honest work and the findings grounded and original, the persistent researcher will surely find or create his audience. Quite possibly, the researcher will speak with or write for an audience just to "try out" his ideas, and simultaneously to get some feedback.

Establishing Credibility

For graduate students and impecunious researchers, shopping for audiences is no easy matter; faculty committees and research-grant review committees are like company stores to which most shoppers are committed by debt or

contract. However, insofar as these committees bear serious responsibilities to institutions of higher learning, to their fields, and to their conception of science, they will probably not make their judgments merely according to the "feel" of validity or of its "ring of truth." It is "natural" for committee members to examine research problems in terms of how the problems (and they themselves) tie in with existing knowledge and theory, and to examine the research methods—intended or accomplished—according to how they (and themselves) relate to established operations for determining the validity of findings. It is not an easy decision for the judges, since to some extent and in some ways the success of the research (its completion and its validity) reflects upon them: they, too, are subject to group-defined norms. Hence, the questions they raise and the criticisms they offer tend to be typical of those found within their colleagueship or institution.

Although so-called *methodologists* tend to be more exacting and demanding than other audiences concerning how validation and reliability are assured, the logic underlying their expectations is not qualitatively different from that of other audiences whom the researcher is likely to encounter. Methodologists' logic is simply better articulated and grounded in conventional research procedures. The "ring of truth" is the same for all these audiences, except that the methodologists can identify the bells and have themselves been bell ringers. Those who have done field research in the manner we have described find that their validating procedures are not always or easily recognized by certain audiences. This may be as much the fault of the researcher in not making his procedures explicit as it is for an audience that may expect his procedures to be similar to those used by quantitative analysts.

An essential prerequisite to establishing credibilty with any audience is the researcher's conviction that what he is saying or writing is so. And this conviction rests upon necessary and credible procedures performed, as well as upon the sense of certainty that the observer did in fact see what he says he saw.

But what does this mean? It means that every proposition uttered—indeed, every declarative sentence—is a datum or a derivative of data, that the data are demonstrably empirical, and that they are empirically and logically related to the propositions stated. Even if the propositions are not particularly brilliant, they are grounded and the researcher has found no negative evidence bearing directly upon them. On this, at least, the researcher can rest his case. However, some audiences will not let him rest here; they require evidence or explicit affirmation of "validating procedures."

Some audiences must be fully assured that the researcher did pinpoint or check out every major proposition, that is, that each was derived from original field experience and from the data, was tested again with the data or with additional experience, and was also tested for logical consistency with every other major proposition. Quantative researchers characteristically demonstrate the validity of their findings through statistical tables and measures, and they therefore are likely to feel comfortable when defending their observations. Yet, if one were to ask them to defend the validity of the categories, one by one, upon which the tables and measures rest, they would be in essentially the same boat with the field researcher. Fortunately for them, most audiences do not question that far, conditioned as they are to the persuasive power of quantified evidence. The field researcher, however, may encounter some skepticism in those audiences who expect much quantified evidence from him.

Host verification

Credibility may be established with some audiences by showing or simply stating that at least the major propositions were tested or checked against the experiences and understandings of the hosts. If it was found that the propositions offered to the hosts did not empirically contradict their own understandings of their situation, then the researcher may convince audiences that he has a measure of validity—possibly a large measure. This mode of validating one's work does not require that the hosts actually concur in the propositions themselves, but that they recognize rather the validity of the grounds (events) upon which the propositions rest. But this procedure for achieving credibility with given audiences leads to another question which audiences may raise, bearing upon the repeatability or reliability of the work. Would another independent observer have seen or heard the same events, and reached the same conclusions?

For the field researcher whose view of social reality is one of infinite complexity, the only germane question is, *Would an independent observer make conceptual discoveries that empirically or logically invalidate his own?* That another observer—with or without the same general framework or perspective—might develop a very different analytic scheme, conceptual model, or metaphor is to be expected. Perceptual and conceptual selectivity must be taken for granted. Some identical and some different events would become data for other field observers; therefore, all independently developed data and analyses would necessarily be different. One or another analysis may be conceptually superior, but if any fails to contradict the original research, it must be regarded as supplementary or complementary.

A subsidiary question can also be posed: *Would another social science analyst, examining only the actual raw data (the ON's only) reach the same conclusions?* Here too, the answer would be the same. It is only when an independent analyst is given the original researcher's categories and propositions that he can possibly arrive at the same conclusions. Without these categories and linkages, another analyst—even if trained in the original researcher's own tradition—would create his own leads to follow and to develop.

Phenomenon recognition

The same or other audiences may pose a different order of questions not directly concerned with internal validation or with formal tests of reliability. They are knowledgeable about the phenomenon researched and themselves have had direct experiences with "it"—but elsewhere and under different circumstances and with different perspectives. These, too, may be difficult audiences with which to establish credibility, even if they are not methodologically sophisticated. They have their own direct and "real" experiences against which to test the validity of what our model researcher has said or written; they have worked—or still do work—in similar institutions or have had experiences as members of social movements. Now the researcher as communicator must rest his case upon the generality or universality of his propositions: *Do these people recognize the phenomenon?* Does what the researcher tells them call out in them a common experience? Even more important: *Does the researcher's analysis, which was probably based upon a different perspective or framework from theirs, actually help the audience explain—albeit in a new way—their own experiences?* If so, the researcher is virtually assured of credibility with this audience; for in a special sense, predictability and control, as well as generality are thereby indicated to them.

Yet, these audiences may find that what the researcher is saying—in part or whole—contradicts their own experiences and understandings. A lively dialogue may ensue with the audience offering negative evidence as a counterargument. But is it genuinely negative evidence? Evidence is negative only when it contradicts a hypothesis or proposition; otherwise it is, like any other data, positive for possibly another proposition or evidence of a sub-class or variant of the proposition stated in the first place but not accounted for in the researcher's analytic scheme. If the latter, the researcher will have learned something: most serious audiences make good teachers. In any event, if the researcher is secure in his own internal validation, and if he had done his comparative and historical analysis well, he will find little difficulty in defending his own propositional scheme. An additional reason for presenting to these audiences, then, is that they may greatly stimulate analysis, particularly on later phases of the research.

Letting Go: A Comment on Closure

Career exigencies and work styles, as well as research requirements figure prominently in the sequencing of research presentations, including the final presentation. Whether his current research is part of a larger endeavor or a highly circumscribed, one-time effort, the researcher will want to put final closure to it. If he is on an intellectual career course, other tasks will beckon him, although as an expert of sorts he may be called upon months and even years later to tell again, or anew, of his research. Whether undertaken hopefully, fearfully, or with a sense of boredom, the final writing poses a common problem: it relates to competence and to identification with some community of scholars.

There are researchers, on one hand, who literally rush into print, thinking they are ready despite cautionary cues signalled by their peers. For them, perhaps, publication *per se* is the name of the game, and the more the better. Or they publish because of or *in spite of* criticism. On the other hand, there are those who feel they are not intellectually ready, sometimes despite protestations of their colleagues to the contrary. They may put off closure for months—not stewing around, but doing other things; then later, having slowly digested audiences' comments, do their final writing. In these instances the structure of identification is different, as are the criteria for measuring competence. But in either case—the rush or the pause—the action is not a neurotic one. Somehow, each type is prepared to accept the consequences of his actions, including the possibility of having to blush years later when he is quoted for a work which now in his greater wisdom he would just as soon forget. Yet, at the time of final publication each had established credibility for himself and had "let go" of the work.

There are still other persons—not only researchers—for whom writing is a major problem, either because they are lacking in writing skills or because in some exaggerated sense they see the written word as a final, ineradicable fixing of their own intellectual identities; whereas in face-to-face talking about their work there is much room for immediate and tailored qualification and talking has an "off-the-record" quality. They find it extremely difficult to create options for themselves on when and how to end the work. Students, particularly for structural and career reasons, feel the weight of criticism from too many audiences, and in trying to satisfy all of them block their own efforts at closure. Indeed, some of their audiences appear to them so very knowledgeable and powerful on substance or method as to create a situation of coercion. Even their own colleagues can retard the necessary commitment, however altruistic their intentions. But in the end, it is the researcher—new or experienced—who must be his own judge on when and how to bring closure. To do this, he must himself be critically selective of criticism and then take a stand on his findings and methodology. Then he will "let go" and write what he understands to be reality, but even then, probably for limited audiences who will appreciate it—perhaps not all of it, but enough to establish a link with that community whose interests are met by his work.

A final word for purposes of emphasis: Having written his final report on his work, the researcher makes a commitment to the validity of the reality he created. He will have to stand by it even though later he will probably change, as will his conception of the reality he researched. But if he understands this, then he can also see final closure to his current work not as an end, but as a single bench mark in an intellectual career course. Also, then will he be able to smile when he reads Omar Khayyam's:

> The moving finger writes, and having writ moves on . . .

Suggested Reading

Much of the literature on the communication of research is concerned with the ethical consequences of disclosure.

BARNES, J. A., "Some Ethical Problems in Modern Fieldwork," in William J. Filstead, *Qualitative Methodology: Firsthand Involvement with the Social World,* pp. 235–51. Chicago: Markham Publishing Company, 1970.

BECKER, HOWARD S., "Problems in the Publication of Field Studies," in Arthur J. Vidich et al., *Reflections on Community Studies,* pp. 267–84. New York: John Wiley & Sons, 1964. Also in McCall-Simmons, *Issues in Participant Observation: A Text and Reader,* pp. 260–76. Reading, Mass.: Addison-Wesley Publishing Co., 1969.

RAINWATER, LEE, and DAVID J. PITTMAN, "Ethical Problems in Studying a Politically Sensitive and Deviant Community," *Social Problems,* XIV (1967), 357–66. Also in McCall-Simmons, *Issues in Participant Observation,* pp. 276–88.

Our discussion on communicating the research is best illustrated through a bibliography which shows how essentially the "same" data (and some additional data and thought) were selectively written for different audiences.

GLASER, B., and A. STRAUSS, "The Social Loss of Dying Patients," *American Journal of Nursing,* LXIV (June 1964), 119–21.

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———, "Temporal Aspects of Dying as a Nonscheduled Status Passage," *American Journal of Sociology,* LXXI (1965), 45–59.

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Discusses bringing the research to a close, including the issue of conveying credibility and also the reader's responsibilities for judging credibility. All these topics bear on our discussion of presenting materials to audiences.

Epilogue

For Whom This Book Was Written, and Why

We have written this book for all students and professionals regardless of field, whose interest in social science has brought them to the point of wanting to do research themselves—not just any kind of research but that which naturally leads them to inquire into social events exactly as they are encountered. Although most students may be familiar with the findings of research gained through the field method, relatively few know much about the ways in which this form of inquiry is, or may be, conducted. Even many graduate students in the social and behavioral sciences who have taken courses in research methodology know little specifically of the operations involved in field research—little beyond knowing generally that such techniques as direct observation and interviewing are customarily employed.

In part, this lack of knowledge is due to the failure of most field researchers—mainly anthropologists and sociologists—to tell precisely or enough about how they work; in part, it is due to the failure of those who teach research to provide adequate instruction in the logic of this method and opportunity for students to work in this way. Hence, far from developing an affinity with research, those students who wish to be field researchers often come to wonder how the research models and processes they read about, or are taught, relate to their own observations and understandings of social life, and to their careers as well.

In attempting research while at school, these students have frequently had to compromise or abandon natural intellectual interests and skills in order to define problems and implement inquiries that are compatible only with more methodologically orthodox research models. It is for these persons that we have written, as well as for those who are new to research; also for instructors in research so that they may be encouraged to offer students equal opportunity to use field techniques in their term and thesis projects.

The authors have been teaching field research for many years on a general level in relatively large classrooms and more intensively for selected students. Additionally, we have been continually involved in our own joint and separate research projects. From these experiences, but mainy from the intensive coaching of graduate students, many of whom have had prior training in other research approaches, we have learned much about how students initially view research, and from whence their views originate.

Thus, we have learned that many graduate students sense a discrepancy or discontinuity between the established methods taught them and those they would normally use themselves, at least as applied to their own day-to-day observations of people and of ordinary human events. These students are certainly aware that formal methods of research are frequently the only, or the most suitable, ways of handling certain kinds of research problems. However, they wonder whether other problems—especially their own—can find equal operational expression in other research modes. Also, they are aware that their own informal methodological skills are relatively undeveloped and unsystematic; yet, somehow, the methodology presented to them appears *different in kind* from their own, rather than consisting simply of more sophisticated operations. What they wish and expect to learn are not only the orthodox methods of social research but operational skills which constitute elaborations and extensions of those they already have.

The authors take the view that the informal methods that students are inclined to use are not so different from the formal ones, though the ways in which the latter are taught and written about often make them appear quite different. Theoretically, all social science methods reflect the general requirements of Western science and cannot differ logically in kind from each other. Yet, the appearance of so great a difference can be traced to different perspectives taken by many methodologists both on the nature

of human activity and on that of sciencing. These perspectives are reflected pedagogically in emphases given to three very closely linked components of contemporary thinking—substantive and methodological—in the social and behavioral sciences: mechanism as a model for human activity, standardized instrumentation in the operations of inquiry, and linearity in the design of research.

Mechanism and Instrumentation

For the the sake of brevity, we combine our discussion of the first two components. Mechanistic thinking about human action and human events generally, became fully established in modern times with acceptance of the Darwinian proposition that man is a species of animal. A number of logical implications drawn therefrom led not only to new ways of thinking about man but also to new ways of studying him. Man was linked to the "natural order" and viewed conveniently as subject to the same "natural laws" as those governing other natural objects. By discovering what "governing" meant, one was presumably discovering natural laws as these applied to man and to human events quite as to other objects and events. In keeping with the science of the time, these laws were to be viewed as determining, causal systems; therefore, human motions or actions were understandably made equivalent to determined behavior. This world view made it possible to objectify man in a new way and to model man after a machine or organism.

From these ideas came the development of comparative and physiological psychologies, and significantly, a stimulus-response framework to help explain human actions. Consistently and expectedly, there arose, without loss of mechanism as a model, an organismic sociology with emphasis upon the forms and functions of social relations. The stage was now set within the behavioral and social sciences for an effort to locate the "mainsprings" of human behavior—for psychology, generally within the organism; for sociology and anthropology, generally within the social-cultural environment. This type of thinking was explicitly or implicitly mechanistic, leading quite logically to the translation of "forces" into factors and variables to which "governing" or "responding" responsibilities might be ascribed.

Once established, this orientation led to a search for instrumentation to help discover and measure the stimulating forces and intervening mechanisms which determine response. Over time, a formidable array of instruments was developed. This effort was made understandable and acceptable, if not mandatory, in the context of a developing methodology which required accuracy, reliability and validity in the observation, control, measurement and analysis of variables.

Yet some deleterious consequences flowed from this effort, particularly as it affected generations of students who, driven by the logic and requirements of a "behavioral science," learned to define scientific problems appreciably in terms of the availability and capability of instruments favored or mandated in their time. The instruments—indeed virtually the entire process of thinking about research—rather quickly took on formidable qualities independent of the persons using them. Many students accepted the mandate and fused their own ideas with prevailing thought on the nature of sciencing. And why not, considering the relatively systematic discrediting, since the 1920s, of man's ability to make valid observations and inferences of his own? Was it not established that man was subject to error, bias, irrationality; and that his performance through "insight" was no match for independent, highly reliable instrumentation?

For an indeterminate number of other students, the pathway to "hard social science" was neither exciting nor tempting: they were reluctant to transform, or even abandon altogether, their research interests simply because they were "not amenable" to what had become orthodox instrumentation. However, all too frequently, they discovered it was expedient in the context of graduate or professional education to prepare research primarily as an exercise in the demonstration of competence with instruments. Indeed, it became understood that competence in a given substantive area was demonstrable through competence in specified operations. (We are reminded of the visiting graduate student who, when clearly asked about the substance of his dissertation, replied "I'm doing an analysis of variance.")

Linearity in Research Design

In the social and behavioral sciences, the most commonly used model for telling and writing about research takes the form of a narrative—a linear series of thoughts, operations, and outcomes—beginning with a statement of the problem, followed by a description of procedural design as intention, then by a description of actual operations, and ending with an itemization and discussion of findings. The operational portion of the narrative is also linear or sequential: sampling first, then data gathering, followed by data analysis. The linearity and the categories of the narrative suggest which of the total events experienced in the actual execution of the research are to be included in the final telling of the research story: events from among the countless acts comprising the research, from among the many "factors" taken into account, and from the myriad contingencies which impinged upon the research throughout its course. The narrator orders the selected events sequentially, more or less in terms of the linear model. While we have suggested that the research may not at all have gone as described, there is yet another position from which the research narrative can be viewed.

Aside from its apparent consistency with the mandates of science, the research narrative is important as a piece of communication. Addressed to future and contemporary audiences, this form is convenient for researchers' use. Moreover, the narrative is useful because it provides order and parsimony: it identifies problem, method and findings conveniently and provides a form for creating the credibility of a linear, logical, and causal relation among them. For these reasons the model is deeply embedded in the practices of educational institutions and professional research organizations. Schools teach that scientific work proceeds in such a fashion; scholarly journals favor research writing that follows this form; major funding agencies practically demand that proposals be prepared according to the requirements of the model (often requiring "anticipated" results in place of actual "findings").

We suggest that research itself involves a different organization of activity than research writing, and has a different social locus. Its locus is "in the field," in the special relation of the researcher to his object of inquiry, whatever his method. Research has mainly to do with the process of inquiry; research writing has to do with the process of communication—and for all practical purposes each has its own "field." This distinction is important because our writing has been concerned with a mode of work grounded in research experience rather than in the experience of writing about research.

Sociologically put, it is not the research which confers upon the researcher his status as a scientist; status primarily comes from the groups and institutions to which the researcher addresses his publications. This derivation of status helps account for the durability of the narrative model; but it does not fully account for the researcher's actual performance in the field. There, he finds or constructs his method as required by the peculiarities of his specific inquiry, and the conditions of the research field. Later, in describing what he did, he finds or constructs another method as required by professional communication and by the special features of his audiences. This suggests that "two methodologies" are linked through the researcher's performance in each field. If the reader cannot quite accept the idea of two methods then perhaps he can accept the following: One method is addressed to the object of inquiry as guided by the requirements of the audience to whom the findings later will be reported; the second (narrative model) is addressed to professional audiences, guided by the events of the inquiry already concluded. The two representations of method simply alternate as background and foreground depending upon time, location, and audience, or point of reference.

No necessary conflict is involved in dealing with these two methods, although students and other novices in research may sense a discrepancy or experience conflict when taking linearity literally, or when teachers of methodology insist upon linearity as a practice. There are two reasons for the lack of conflict between acts of inquiry and acts of writing.

First, the audiences which read research do not require full disclosure of all research actions nor of their true ordering. They are concerned only with the disclosure of those activities which could plausibly be related to the attained results, and which other researchers would need to know to perform work yielding similar results. That the next researcher probably could not get the same results from *only* the same actions, and in exactly the *same* order, causes no apparent concern.

A second reason for lack of conflict is that, aside from communication about what he will do or has done, the researcher has very few field requirements with which he need be concerned, the linear model notwithstanding. The order and types of activities to which he must attend are simply those that facilitate his inquiry. These need have neither direct relation to the future constructed narrative nor to the design he may have developed to obtain funds or institutional license for doing the research.

The requirements of research as they relate to science are simple enough: the researcher must deal with phenomena which have empirical referents; he must provide evidence for whatever constructs are developed about the phenomenon, and the evidence must be empirically and logically related to the operations performed upon the object of inquiry. The operations need *not* follow any given model: a maximum of operational maneuverability is fully available to the researcher. Some research operations occur in linear, progressive fashion; many occur simultaneously; while others occur "regressively" as when someone towards the end of his study discovers his "true" problem and its associated hypotheses. This may not be how methodology is taught or written about, but *it is how original nonreplicative research takes place.* Originality has no absolute, programmatic model to work from; it has its "own ways" and a logic necessarily consistent only with the general requirements of order and communication.

Having said the foregoing, we are tempted to undertake field studies of ongoing social science research projects. We imagine that many of our findings would resemble the kinds of dialogue which occur when "true colleagues" swap yarns about their research. Such insiders tell of casual observations that proved more critical or fruitful than the planned, systematic ones; of carefully prepared "designs" and expectations mutilated by unforseeable events; and of initial hypotheses that subsequently proved to be too foolish for later disclosure. Surely, even the most skilled and experienced researchers would tell how "disorderly" some research operations can be; indeed, how often certain "operations" are little more than random motion in search of meaning.

Our efforts in this book were intended to alert the novice in research to these very probabilities and to indicate the kinds of thinking he might engage in to make of this mode of research a positive experience despite its difficulties. Many novices do not see these difficulties until their data have piled up quite beyond their control. Then they begin to realize that the field research they "knew" through reading alone was deceptively simple. Only much later, when the novice has achieved an acute self-consciousness about his actual research performance in the field—when he has made his work systematic, organized, and sustained, and particularly when he has developed an analytic style—he will see that these properties of field research are a quantum jump beyond the informal inclinations and skills with which he began.

Strategy, Common Sense, and Ethics

A researcher's interest in some social phenomenon, and even his theoretical framework and perspective, will give him little or no understanding as to how he may proceed to study it; these provide him only with a measure of conceptual order. While the field method does not require operational design in the same sense as it may in other research methods, it nevertheless requires sets of strategies and implementing tactics to meet the requirements of getting data and of analyzing them. Otherwise, there are considerable waste of time and energy and probably some fateful errors in conduct. By "strategy" we mean recognizing, planning, and organizing ways of dealing with the major requirements—seeing, hearing, understanding—of the research. Since the object of inquiry is simultaneously substance and host, a multitude of tactics are necessary to implement strategic decisions. The tactics of conduct take many forms, sometimes differing little from common sense and good manners. At times, in the classroom, we blush at finding it necessary to tell our fully grown students to say "please" and "thank you," and not to "come on too strong." Indeed, if common sense and good manners can "go without saying," then we need not have written as much as we have; they are *not* so common, and they are assuredly vital to field research.

In writing about strategies and tactics as conscious and organized activities, they may appear quite Machiavellian in the sense of appearing manipulative. Yet, unquestionably, we want our hosts to do exactly what we wish them to do, and the tactics we use make it possible for them to do it. However, also unquestionable is the moral requirement to maintain the relative comfort and security of the host. Therefore, if his means to research are benign and his purposes good, the researcher can regard himself as expressing both intelligence and human concern. He needs both strategy and morality. The first without the second is cruel; the second without the first is ineffectual.

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