

Generalizations from experiences also tend to be context dependent because they depend on weightings of what may be discordant observations. A question like "Is your relationship with your sister good or not so good?" can have many true responses, including those indicating that the two get on well enough, that the two have a barely submerged rivalry, and that the two stopped talking for a year after the sister disputed a will. If we are seen as a friendly stranger, we may be told that the relationship is amicable, which is true. If we are seen as a sympathetic listener, interested in the respondent's disappointments, we may hear that the sister is not to be fully trusted, also a true response.

While questions about concrete incidents—such as "What happened when you and your sister were last together?"—may be answered from more than one perspective, they are less likely to be modifiable by the interviewing context. Thus, we will obtain more reliable information and information easier to interpret if we ask about concrete incidents than we will if we ask about general states or about opinions.

Despite all the ways in which interview material can be problematic, richly detailed accounts of vividly remembered events are likely to be trustworthy. Nor does apparent inconsistency always demonstrate invalidity. After all, people can act in inconsistent ways or maintain inconsistent feelings. Business partners, for example, can be both grateful and resentful. A respondent who in a second interview describes an attitude toward a partner contradictory to an attitude described in a first interview may in both interviews have been telling the truth.¹¹

Sometimes we can check on the validity of a respondent's account by interviewing other respondents. Occasionally, there are records we can look to for corroboration.¹² But for the most part we must rely on the quality of our interviewing for the validity of our material. Ultimately, our best guarantee of the validity of interview material is careful, concrete level, interviewing within the context of a good interviewing partnership.

CHAPTER 6

ANALYSIS OF DATA

Most investigators let analysis slide until the advent of an "analysis phase." Anselm Strauss, in *Qualitative Analysis for Social Scientists*, his manual on analysis in the style of "grounded theory,"¹ and Miles and Huberman, in their more eclectic *Qualitative Data Analysis*, consider this bad practice. They urge that analysis begin as soon as there is data collection. Miles and Huberman observe that the more investigators have developed understandings during data collection, the surer they can be of the adequacy of the data collection and the less daunting will be the task of fully analyzing the data.²

Despite the unquestionable merits of this view, a conspiracy of forces regularly impedes early analysis. During the interviewing phase the investigator must deal with all the demands of obtaining the data: recruiting the respondents, conducting the interviews, getting them transcribed, deciding whether the right information is being collected, and returning to conduct more interviews. Nor can the investigator escape awareness that when the interviewing is finally over, not only will all the data be at hand but there will be uninterrupted weeks or months available for their analysis. Undoubtedly the investigator will develop insights, speculations, and small-scale theories beginning with the first pilot interview or before. But it is likely to be only after interviewing has ended that the investigator can give full attention to analysis and writing.

Although analysis and writing can be separated from data gathering, they cannot be separated from each other. The kind of report that will be written and the kind of analysis that will be done must be decided jointly. Each implies the other. If the report is to be a set of case studies of respondents, then each respondent's material must be organized and interpreted separately; conversely, if each respondent's materials are organized and interpreted separately, the only report possible is a set of case studies.

We can identify four different approaches to analysis and reporting. The four approaches represent the different combinations of decisions regarding, first, whether the report will focus on issues or on cases and, second, what is to be the report's intended level of generalization.

Analysis and reporting can deal either with issues as they are learned about from respondents or with the respondents themselves: they can, for example, deal with the effect being forced to retire has on feelings of worth, using relevant materials from all respondents; or with particular respondents and how they happened to retire, and what were their subsequent experiences, perhaps including their feelings of worth. We can call the first approach "issue-focused" and the second "case-focused."³

Analysis and reporting can also either remain on the level of respondents' reports or be generalized from their reports: they can, for example, deal with the effects of retirement on the particular administrators and professionals who were respondents in the study, or they can use what was learned in the study to consider the effects of retirement on all or most former administrators and professionals. We can call the first the "level of the concrete," the second the "level of the generalized."

When the report is issue focused and generalized, we have the usual sociological account. When the report is issue focused and concrete, we have the usual historical or journalistic account. When the report is case focused and concrete, we have the usual case studies of individuals. When the report is case focused and generalized, we have typological description. Later in this chapter I give descriptions of reports in each of these modes.

No matter the level or focus of the final report, there is no single tried-and-true method of analysis or strategy for presentation of findings. Investigators have different styles, studies different requirements, audiences different needs. Consider only the extent to which the investigator's voice is present. Studs Terkel's books present respondent's voices, one after the other, without editorial interruption,⁴ Daniel Levinson's *Seasons*

of a *Man's Life* presents theory and generalized observation interrupted by heavily interpreted case materials.⁵ In some chapters of Elliot Liebow's *Talley's Corner* the only voice presented is the author's; in others, passages of interview, of dialogue, and of group conversation provide a sense of the reality that underlies Liebow's observations.⁶ Any of these might serve as a model, but they would be different models.⁷

In one respect all good reports, despite wide variation in style, are similar: they tell a coherent story. They provide a line of argument, or an image of how it all works, such that material presented early in the report prepares the reader for material that will appear later and later material draws on the earlier. The reader, at the end, can grasp the report entire.

The kind of story the investigator can tell must be consistent with the kind of data that has been collected. As I noted in chapter 3, a coherent story may be achieved either by the diachronic strategy of describing movement through time or by the synchronic strategy of describing the integration of elements at a moment in time. A report whose subject is single parents might begin with the way in which people become single parents, go on to describe how they manage to meet the problems they encounter, and then assess the benefits and stresses of single-parent life. Alternatively, it might move from sector to sector within single-parent life, describing first the way single parents organize their households and then their relationships with their children and so on. One of these strategies of presentation will almost surely have been anticipated when the interview guide was developed, and it would be difficult now to move to the other. In other respects, however, the investigator is likely to be able to choose how the data should be presented, and so how it should be analyzed.

In the following pages I shall discuss first what might be involved in issue-focused analyses and then what might be involved in case-focused analyses. In my discussion of issue-focused analyses I will begin with the generalized level, because the analytic issues are clearer there; in my discussion of case-focused analyses I will begin with concrete description, because this provides an introduction to the problems of generalizing from individual cases.

ISSUE-FOCUSED ANALYSIS

What I believe is the most frequent social science approach to presenting the findings of a qualitative interview study is to describe what has been learned from all respondents about people in their situation. An analysis

whose aim is issue focussed would concern itself with what could be learned about specific issues—or events or processes—from any and all respondents. Some respondents might contribute more to the analysis, others less. The analyst would become like the medical specialist who is interested in livers, not in particular patients, except that the analyst would be concerned with more than a single set of issues.

An issue-focused description is likely to move from discussion of issues within one area to discussion of issues within another, with each area logically connected to the others. A report about people who retired might begin with a discussion of how the retirement occurred (an area). In the discussion it might consider the different impacts of being pushed out of one's job, retiring to gain freedom from the job, and retiring for reasons of health (specific issues). The next area to be discussed might be early reactions to retirement. This area would, of course, have its own issues. Then would follow still other areas, again each with its own issues. The whole would together describe the retirement experience in a number of areas for the group the respondents represented.

There are four distinct analytic processes involved in producing an issue-focused analysis of interview material. These are coding, sorting, local integration, and inclusive integration. Early in the analysis, coding is likely to predominate; later, local and inclusive integration may absorb the most energy and attention. But all analytic processes occur throughout the analysis. Coding is intended to provide the materials for sorting and integration, but sorting and integration can also raise questions for further coding.

CODING

The idea in coding is to link what the respondent says in his or her interview to the concepts and categories that will appear in the report.

Suppose that in analyzing data on retirement we read in the transcript of an interview with a retired surgeon the following comment: "I started the process of retiring several years ago when I began winding down my practice and cut out the major surgery and went just to doing minor surgery." We could code this—that is, link it to concepts and categories—by noting that it is an instance of (1) preparation for retirement in which there is movement to the periphery of the occupation; (2) withdrawal from tasks of high responsibility, tension, skill, and visibility;

(3) unclear regarding the source of the pressure to retire; and (4) more a change in work routine than a change in identity as a physician. How we code it depends on our theoretical assumptions and the research interests we bring to the project.

Some coding categories we bring to our studies before ever knowing what the interviews will produce. We plan to use them because they are related to the problem we hope to study, as an aspect of the problem or an explanation for it or a consequence of it. Others we bring with us as readiesses to interpret respondents' comments in one way or another. The readiesses come from our training, our reading, our life experiences, and our general understandings. When I code a respondent's description of night sweats as an expression of stress, my coding comes from my having learned to attribute a particular meaning to this particular symptom. Many codings seem to be immediately plausible abstractions from respondents' statements. For example, the respondent statement "We don't agree on the children's bedtimes, but I'm the one who has to deal with them" might be coded "conflict over children resolved by reference to domain responsibility."

I find myself coding automatically as I read interview transcripts. The code categories I use, at least initially, are efforts to capture the interview material. I don't try to make sense of every "meaning unit"—every utterance that provides a complete thought—nor of every sentence or paragraph. But as I go through the material I do ask myself what I am seeing instances of, what I am learning about, and what questions the material raises.

I generally write my codings, along with formulations or speculations, in the margins of the transcripts I am working with. Later, I go back over my marginal notes. If they still make sense, I dictate them into a tape recorder, together with the interview excerpts on which they are based, to be transcribed as background notes. I am likely to use the formulations in these notes as I develop chapter drafts, but I may not. However, once I have dictated the notes, I remember their formulations at least in outline and can carry them in my mind and review them and think about how to test them or develop them. I am likely, too, to return again to the interview excerpts in the notes to reconsider whether they truly support the codings I have made.

Fairly early in a study, if I am doing issue-focused analysis, I shift from reading and coding transcripts of interviews to reading and coding "ex-

cerpt files." These are collections from many interviews of excerpts dealing with the same issue. I say more about excerpt files in the next section.

As I make decisions regarding whether instances fit my coding categories, I find it necessary to further specify what I mean by the categories. Thus, the coding category is developed and defined through interaction with the data. Take the category "shares responsibility for home maintenance," which was one of the categories in my study of occupationally successful men. One man said that he fully shared household responsibility and performed many of the household tasks, except at those times when his professional work load became heavy. When his professional work load became heavy, he would ask his wife to take over the entire responsibility of the home, and he believed it was her place to do so. I then realized that I meant by "shared responsibility" a responsibility for home maintenance that was understood by the couple to be shared not just when other responsibilities permitted it but always.

Coding of this sort cannot be delegated to a research assistant. Coding is thinking about the material, and the thinking should be that of whoever will be responsible for the report. This is not to say that others cannot help. It is almost always useful to be able to talk about codes, and having others code the same material can be a good basis for such talk.

When do you stop coding? Only when you stop reading transcripts—and that may not be until the final report is completed. But there is a change in the character of coding as a study progresses. By the end of the analysis phase the investigator should more and more see the data fitting into already established codes. While the point is never reached where new material can no longer enrich codings, toward the end of the analysis there should be few important phenomena that significantly extend or deepen or qualify codings.

SORTING

Before the advent of computers and word-processing programs there was a standard approach to the analysis of interview transcripts and other qualitative data: Make second copies of transcripts and notes. Put the first copies away, to serve as an archive. Label file folders with the titles of the sections of the report, the chapters, or chapter sections. Cut up the second copies of the material into topical units corresponding to the labels on the

file folders. File each excerpted topical unit in its proper folder. If folders become too full, divide the materials by subtopics and sort into new folders.

The result is a set of folders containing excerpts from cases, each folder holding a category of material. What happens next with the material is, first, "local integration," in which the material of the file folder is interpreted, and, second, "inclusive integration," in which the collection of file folders is organized into a coherent sequence.

The cutting and sorting into file folders, which is the traditional approach to qualitative analysis, can now be done by computer word-processing programs. Instead of literally cutting up interviews and putting the snippets into appropriate file folders, the analyst at a computer electronically moves material from an interview file or a file of notes into an appropriate excerpt file. Then the excerpt file can be printed out as needed.

As I noted, in doing issue focused analysis I find it more satisfactory to code from excerpt files than from full interviews. If an initial excerpt file becomes too bulky, I can break it into smaller excerpt files dealing with narrower topics. If it is too narrow to start with—this doesn't often happen—I can amalgamate it with other excerpt files.

In analyzing material from the study of occupationally successful men for one section of a chapter on fathering, I began with an excerpt file of expressions of paternal investment in children. That turned out to be too thick to deal with. So I divided it into several more narrowly focused excerpt files: one dealt with indications of pride in the children, another with instances of dealing with crises, another with instances of acting in a paternal role in sports or discipline or character formation, and so on. I then coded each of these files (by writing in the margins of transcript pages) and dictated the codes and exemplary excerpts for later transcription.

I find it most efficient to produce excerpt files only as I need them. If the interviews provide a great deal of material regarding an issue, I will not require that the excerpt file contain it all. An excerpt file that runs to 80 pages of single-spaced text, with wide enough sampling from the interviews to make me confident the interview materials are adequately represented, strikes me as good enough and about all I will want to work through.

While I haven't found it possible to have others help me in coding, I have found that others can help in making up an excerpt file. Sometimes

the decisions they make about what belongs in a file aren't exactly the decisions I would make, but ordinarily they are good enough. I can always add to or subtract from the file if I want to.

The use of a computer in making up excerpt files makes for greater flexibility in managing data. It is easier with a computer file than with sheets of paper in an actual file folder to break a too inclusive file into narrower files or to amalgamate too thin files. Furthermore, the computer excerpt file can be printed out to provide a set of passages from interviews to be read and coded, formatted in any way you like. Computer programs exist whose aim is to facilitate the coding and sorting of interview material. While their use is not without problems—including the need to invest time in mastering them and the risk that, once mastered, they will dictate the direction of the analysis—they can further facilitate analytic work.⁸

LOCAL INTEGRATION

The codings of the material in an excerpt file constitute a statement of what we believe the material tells us. We now need to find a way of organizing and integrating our observations and understandings in each section of the report. This I call local integration. I find myself beginning to do this as soon as any interview materials show up and continuing it all the way through the study, although it becomes a focus of my effort only when I am involved in producing the final report.

One way to achieve local integration is simply to summarize the excerpt file and its codings: Here is what is said in this area, and this is what I believe it to mean. We might first summarize the main line of the material: what most respondents say. Then we could turn to the material that does not fit in the main line, the variants, and summarize them. If there are too many variants, our definition of what constitutes the main line is unsatisfactory and we need to move to a more inclusive formulation.

An example of summarizing the main line and its variants might be taken from bereavement project material dealing with widowers who were parents of small children. Here we could summarize material dealing with the fathers' reliance on their mothers or their sisters or on female housekeepers or, in a few instances, their mothers-in-law. We could then summarize as cases that did not fit the main line the stories of fathers who did not at all rely on women to help them care for their children. We

would try to provide enough material about the main line so that our summary made sense to the reader, and it also made sense that this should be the most frequent development. We would also try to explain why cases that did not fit the main line were different. We might note, for example, that the fathers who did not rely on women to help them care for their children had older children or had decided to stay home from work for a time.

A theory that makes sense of the materials is preferable to a descriptive summary of main lines and variations. I find myself developing, from the very beginning of the study, "minitheories," hypotheses whose aim is to make sense of material dealing with specific issues or material within a particular sector. As the study progresses I retain those that are supported by the data and drop those that are not, almost without noticing that I am doing so.

Early in my study of occupationally successful men I found myself developing a minitheory about the meaning for the men of challenge at work. The theory was attractive to me because it made a connection that I thought valid between two things that weren't obviously connected: the men's zest for challenge and their frequent stress at work. The minitheory was that the men viewed challenge at work as a test of their worth and responded by mobilization of their energy and attention. They relaxed their energy and attention if they met the challenge successfully—indeed, they then felt good about themselves. If they didn't meet the challenge successfully, however, they remained mobilized. This persisting mobilization was exactly work stress. Further, the men's jobs consisted of meeting challenges, one after another. This was what the men wanted. Such a job gave them maximum opportunity to test themselves and gain the pleasure of self-approbation. But it also made it likely that sooner or later they would run into a challenge they could not meet, and so experience work stress.

I liked this minitheory because it explained a lot, fit with the interview material, and had an aesthetically pleasing neatness. Almost all the excerpted materials corroborated it. A few men did seem to have nonchallenging work, but they didn't like it or didn't respect it. So even material off the main line could be assimilated to the minitheory. I began extending the minitheory, adding to it ideas such as that successfully met challenge could augment self-confidence and so make the next challenge less daunting.

But while this minitheory offered one basis for the integration of a

good deal of material in the area of work, it didn't deal with other material adequately, or at all. This included, for example, material on the importance of teamwork, on work as a kind of play, and on the importance of recognition. In these other sectors I had to develop other minitheories about men's investments in work.

When I turned to still other sectors of men's lives, including their marriages and their relationships with their children, I had to develop new bases for local integration. This meant working with new excerpt files and new codings, from which I could develop new summaries and try to develop new minitheories. When I came to write chapters for the book that was the eventual project report the summaries and minitheories became the armatures of the chapters.

If a minitheory seems both plausible and potentially useful, I try to verify it. If there is interview material I haven't included in the excerpt file, I might now consult it. If I know of relevant literature, I without fail examine it. But if, after having done this, the truth value of a particular minitheory seems to me high, that is, if it is well supported by the interview material and in addition makes sense, then I go on to the next excerpt file. My anticipation is that as minitheories become organized into more embracing theories, the truth value of the minitheories will be enhanced.

INCLUSIVE INTEGRATION

Local integration brings coherence and meaning to excerpt file materials and their codings. Inclusive integration knits into a single coherent story the otherwise isolated areas of analysis that result from local integration.

The problem in inclusive integration is to develop a framework that will include all the analyses the investigator wants to report, moves logically from one area to the next, and leads to some general conclusion. An initial framework for the report is likely already to have been outlined in the course of developing the interview guide. But that first framework will probably now require further development, or even fundamental revision, given the need to incorporate within it the understandings realized by the interviewing and analysis.

It may be useful for me to review how I came to the framework I used in a report on single parents.⁹ My aim in that study was to describe the life experience of custodial parents after the turmoil of separation and divorce had subsided. I anticipated a report in which I would describe the structure and functioning of the single-parent household, with the idea that this

would help make sense of the problems single parents encountered. The interview guide, in consequence, focused on the tasks of daily life in single-parent households and how single parents managed them, the single parents' relationships with their children and with others, what the single parents saw as the problems of their lives, and how they coped with those problems.

In analyzing the interview material, I focused first on what seemed to me the two core areas of my intended report. One was the parent's relationships with the children, which provided the "structure" of the single-parent household. Here I noted, among other things, that in single-parent households children were likely to be treated as junior partners in household management. My other core area of analysis was how things got done in the single-parent household—how the household functioned. I described here, among other things, how employed parents might have even quite young children do some household chores.

At this point it seemed to me that the report in which I presented these analyses should begin with how single-parent households developed. I now did further analysis, comparing the experiences with marital dissolution of my largely divorced sample with the experiences of bereavement of a small number of widowed respondents. I hadn't initially intended to include widowed single parents, but had interviewed a few anyway, and now I found I could use the materials of their interviews. It also seemed to me that I needed to follow descriptions of my core areas with material describing the personal life of the single parent, including relationships with other adults and issues of loneliness and of sexuality. The interviews had collected information on these issues, and I now proceeded with their analysis.

It was only when I had produced about half the report that I worked out how the report should conclude, although that was something I had worried about as soon as I began to write. What seemed an appropriate conclusion was to identify what were the problems faced by single parents and how they coped with them. I found that the interviews had collected the needed information, and again I was able to proceed with analysis in the area.

As I look back, it seems to me that much of the work developing the report's story line took place away from my desk, as I repeatedly considered what would constitute a satisfactory report. This strikes me as the way I have customarily developed the framework for a report. Very early in the study I had a general idea of what issues I would eventually discuss.

This sense of the issues to be discussed gave shape to the interview guide and later suggested the core areas for analysis. On achieving local integration in the core areas, I could then think further about what might be the shape of the final report and what materials should precede and follow the core areas. But the final framework wasn't entirely in place until I had finished the report.

Other investigators with whom I have worked seem to have proceeded in roughly the same way I did. They, too, began with a rather sketchy framework, developing it further as they went along, but did not have everything in place until they had their report nearly done.

VISUAL DISPLAY

Many researchers find visual display of the elements of their story a valuable means of achieving both local integration and inclusive integration. Miles and Huberman recommend diagrams on paper, with lines linking related issues, to display graphically the conceptual framework of the final report.¹⁰ Becker suggests putting data and memos about the data on file cards that can then be spread on a large flat surface and arranged and rearranged until they achieve a logical sequence.¹¹ Agar suggests finding an empty classroom full of blackboards on which can be drawn maps of concepts and their interrelations.¹²

I don't much use visual display myself, although I do often make tentative lists of chapters or chapter sections in what I believe to be logical order. But visual display as a way of achieving local or inclusive integration is so often recommended that researchers who are just beginning to work with qualitative interview material might want to see whether this approach works for them.

A GENERALIZED ISSUE-FOCUSED REPORT

Most studies of family life have been based on interviews. Other methods don't work nearly as well in capturing family life: adequate observation of families is intrusive and time consuming, and family records such as letters, diaries, and photographs tend to be sparse and to emphasize the ceremonial. Interviews, however, can obtain information not only about family events but also about the ordinarily unvoiced perceptions and emotions that underlie family dynamics. Mirra Komarovsky's *Blue-Collar Marriage*¹³ is among the earliest of the studies to ask how a type

of family or type of marriage worked, although still earlier interview-based studies of families had examined how families dealt with the Great Depression and how men and women responded to divorce.¹⁴

Komarovsky interviewed husbands and wives in 58 blue-collar marriages. Her aim was to learn how blue-collar marriages were different from other marriages and thus how social class affects marriage. Her eligibility criteria for respondents required that, in addition to being married, they be white native-born Protestants, without any college education, and with the husband in a blue-collar occupation. She also wanted respondents to be currently raising children. She obtained the names of her couples from a city directory, from membership lists of Protestant churches, and, in two instances, from recommendations by other respondents.

Two 2-hour interviews were held with wives, and a third 2-hour interview with the husband. The interviews were for the most part qualitative, "within a carefully worked out guide," but also included questions and checklists to be asked of everyone. Komarovsky did not do much tape-recording: her study was conducted before tape recorders were common household equipment, and she thought that her respondents would be made uncomfortable by them. Instead, her interviewers were required to take detailed notes.

Some of the codings of responses appear to be immediately plausible categorizations of respondents' comments. For example, husbands were asked "How does marriage change a man's life?" and wives were asked the parallel question "How does marriage change a woman's life?" Komarovsky used the coding "escape from home" to characterize responses like these:

He's got his own place and he's boss. With my mother, I had to come home when she said or I wouldn't get anything to eat. Now I can come and go whenever I like. [*A husband's comment*]

One reason I got married at sixteen was to get away from home. . . . They would tell us kids that they stayed together for our sakes—that made us feel real good. [*A wife's comment*]¹⁵

Other codings seem to be expressions of interest Komarovsky brought to the study. Thus, couples were assessed on the extent to which their marriage was close or distant and on the extent to which the spouse was a friend, even when closeness and distance weren't directly discussed by

the spouses. In addition to "closeness," codes dealing with adequacy of marital communication and with level of marital happiness appear to have been brought to the study by Komarovsky.

Still other codings seem to be neither *a priori* categories nor immediate inferences from responses. These are possibly the most interesting. They include "the pull of the male clique," which in Komarovsky's view "retarded the domestication or the marital socialization of the husband." This coding, and others like it, must have come from a good deal of thought about the meaning of responses.

The sequence of material presented in Komarovsky's book begins with a description of the respondents and proceeds to the respondents' motivations for marriage, the division of labor in their marriages, their marital communication, and the relative power of the two spouses in the marriage. It then proceeds to kin ties, economic and occupational issues, the couples' use of leisure, and, finally, the implications of the findings. Sorting of materials was undoubtedly done within this structure and probably was straightforward.

Local integration may well have absorbed most analytic time and energy. Each chapter seems to have been treated almost as a separate study. Chapter 5, "The Marriage Dialogue: Expectations," begins by setting out the problem to which it will attend: Do working-class husbands and wives share the desire and expectation of middle-class husbands and wives that their marriages will be relationships of communicative intimacy? The data used to answer this question came primarily from respondents' reactions to vignettes describing problems in marital communication and answers to questions about the qualities of a good husband and wife.

The chapter reports that couples who shared middle-class ideals regarding intimacy were likely to be better educated. If not, the wives were better educated and the husbands aspired to upward social mobility. Other blue-collar couples, however, are presented as happy despite little talk that could be called intimate. To show how such a marriage might work, Komarovsky presents a case study of a husband and wife who were happy although noncommunicative. Komarovsky's conclusion is that while working-class marriages of a certain sort display the communicative ideals of middle-class marriages, other working-class marriages do not—and they aren't necessarily unhappy.

Komarovsky achieves local integration in this chapter by posing and then resolving the question of intimacy in working-class marriages. She

first proposes a minitheory about which of her respondent couples would display communicative intimacy and then presents material that illustrates and supports the minitheory. She then presents further material that suggests necessary qualifications in the minitheory. Other chapters display the same movement from problem statement to minitheory, to supporting data, to qualifications, and finally, to conclusions. Most chapters could stand alone as sector reports.

Komarovsky accomplishes inclusive integration by leading the reader from the early days of a couple's marriage and the couple's division of marital labor to what appears to be her primary concern, the closeness of the present marriage. That occupies the bulk of the book. At the end, chapters are added on economic and occupational issues as they may affect a couple's marriage and on how couples use leisure. A final chapter, "Theoretical and Methodological Conclusions," is intended to wrap up the whole thing.

How good was Komarovsky's analysis? It was certainly good enough to enable her to influence thinking about working-class marriage and about the problems, as well as benefits, of presumably supportive relationships with friends and kin. (Komarovsky calls the wife's continued closeness to her mother "the working-class marriage triangle.")

Maybe more could have been learned about the lives of the husbands and wives had there been less focus on marital communication and less reliance on checklists. There is almost nothing on courtship. Sex is discussed, but there is little discussion of love and affection. There is surprisingly little about children and the problems of parenting: about all there is appears in a section where Komarovsky notes that parents sometimes quarreled over how the children should be raised.

More than the most authors of qualitative studies, Komarovsky is concerned with quantification. She regularly reports the proportion of cases that had particular characteristics: that, for example, exhibited communicative distance. Because the sample was not representative of any meaningful universe, the proportions cannot be taken as estimates of anything; they are only descriptive of Komarovsky's sample. Still, reporting proportions does suggest the strength with which the sample supports generalizations, and it gives the reader reason for confidence in the probity of the analysis.

In a few respects, the study's methods place the study in an earlier era of qualitative work. Quotations have the choppy quality that comes from being based on notes rather than tape recordings. There is a pervasive

concern with demonstrating that there is legitimacy in qualitative work even though it may appear less scientific than large-scale surveys. And the data gathered included, at least for my taste, too much material regarding expectations and reactions to hypothetical vignettes and not enough regarding ongoing life. But the analysis is thoughtful and thorough, and if the resulting picture is not as vivid as it might be, it is thoroughly credible.

AN ISSUE-FOCUSED REPORT AT THE LEVEL OF CONCRETE MATERIAL

An example of how concrete excerpts can be organized to tell a story is provided by Ronald Fraser's *Blood of Spain*. Fraser's aim was to weave a history of the Spanish Civil War not from the perspective of its leaders but, rather, from the perspective of the ordinary people swept up in it. He conducted more than three hundred interviews in order to, in his words, "describe the major contours of the war through eye-witness narration."¹⁶

Although Fraser does not tell us his analytic methods, he must have coded his interview responses for the time in the war they referred to, the particular region they gave information on, and whether their perspective was Republican or Nationalist. His sorting would then have been straightforward: collect materials by time and, within time, by event. Local integration would require that he work out what descriptions he would give of the historical context and how he would introduce the interview materials; how much space he would give particular perspectives; which interview excerpts he would quote and which he would paraphrase; and when he would enter the report in his own voice and when he would have respondents speak. Inclusive integration would have been a matter of organizing events temporally.

Fraser begins his book with interview excerpts describing the years before the war, when Spain was governed by the Left. He sketches the opposition to the government by business leaders, the military, and the church and shows the hatred and fear that existed between the classes. Then he tells us how the war started, through interviews with those who had participated in the uprising of army units in Morocco in July, 1936. He uses further interview excerpts to display the reaction in mainland Spain to the news of the uprising.

Throughout the book Fraser places his interview excerpts and paraphrasings within their historical context. The historical context, in turn, is illuminated by the interview excerpts. His style may be suggested by the

following paragraph from the book, in which he describes the resignation of one leftist government and the formation of another:

In the evening the government resigned. A new government . . . was formed. To a Left Republican school teacher, Regulo Martinez, the new government seemed a prudent move. . . . But the people took the news badly. "Even members of my own party—Azana's party—began tearing up their membership cards. The masses wanted revenge, revolution. They threw caution to the wind; courage and resolution were the order of the day . . ."

Fraser's style provides a "you are there" sense of the disorganization that accompanied the early days of the war, the breakdown in civil order, the excitement and hope of participating in heroic action, and the pervasive blindness on both sides to the misery the war would bring.

Fraser goes on to describe, in the same way, the early days of the war, in which battles sprang up between isolated units distant from the larger forces and everywhere people acted on thin information and often mistaken surmise. As his story moves on to the full-scale war, Fraser presents excerpts from respondents that describe the callousness of leaders, the advent of atrocity and counteratrocity, the many casually ordered deaths, and the fewer narrow escapes. In passing, Fraser reproduces published materials of the time, including a full-page announcement from a right-wing newspaper pleading for the confidence of readers. Finally, Fraser describes the end of the war and the advent of the Franco government.

The book does wonderfully well in providing a sense of the experience of the Spanish Civil War for a reader removed from it in time and space. It may, perhaps, give disproportionate space to the vivid. This bias is understandable; the ordinary does not hold our attention. Nevertheless, there is enough in the book to make evident the torn texture of daily life and the numbed reality of a daily round carried on against a backdrop of catastrophe.

CASE-FOCUSED ANALYSIS

CONCRETE CASES

Feature stories in newspapers and magazines regularly begin with an account of a particular case. Our interest is much more likely to be attracted by the concrete and particular than by the abstract and general.

And we understand the concrete more easily: we can identify with actual people and immediately grasp real situations whereas the abstract and general requires us to understand in a more secondary way.

What concrete case studies of individuals do superbly is make the reader aware of the respondents' experience within the context of their lives: this is what it is like to be this person in this situation. Case studies take readers into respondents' lives.

In addition, case studies regularly imply generalizations, although without assurance of their validity. Readers take from case studies a sense of the case as exemplary, with general lessons to teach. They believe themselves to be learning not just about particular people but about people who are like them, not just about particular situations but about a class of situations.

Because investigators, as well as readers, grasp concrete cases more easily than abstract models, constructing case studies can be useful even in an issue-focused analysis. I have found that when I am unable to work out a persuasive conceptual scheme or unsure of what to emphasize in achieving local integration or inclusive integration, I am helped by putting together the materials of an individual case. I gain from the experience of case construction a more immediate understanding of the situation of respondents, and I can then more easily imagine how that situation might be presented and explained. For example, my first step in the analysis of data dealing with single parents' lives was to construct two extensive case studies. With them at hand, I could see how central was the issue of vulnerability to overload.

Producing a case study begins with sorting. Material that deals with the same issue must be brought together no matter where the material originally appeared in the interview transcripts. The material is then organized into a story of that issue. In the single-parent study, "fights with children" was one grouping of material. Integrating is likely to be done in two stages: a first stage of bringing together material into coherent scenes—the case-study parallel to the local integration of an issue-focused analysis—and a second stage of inclusive integration, in which scenes are linked into a coherent story. The investigator may also want occasionally or regularly to introduce a "voice-over," that is, a paragraph or page of commentary in which the reader is told what the case materials mean or what general processes they illustrate.

It takes me several drafts to arrive at an organization of interview material I feel presents a case properly. In my first drafts of a case study

I include too much; I want to preserve respondents' words, as if I were a curator of language. Only as I read and reread the material can I see where points are being made, what is superfluous, and where the argument has begun to veer off the track. New drafts are likely to suggest additional voice-overs, or better ones. At the end I will have to do a final editing to eliminate redundancies, straighten out the story line, and ensure that the case reads as well as it can. A good case study may make a reader believe that putting it together was easily done, but I have not found this to be true.

An interesting question is the extent to which the investigator who constructs a case study intends from the beginning of the analysis to illustrate a particular conception or provide support for a particular theory. My guess is that most often the investigator's understanding and the developing organization of the case material interact. The investigator's preconceptions help to decide the initial organization of the material, but then the material forces modification of the investigator's thinking, which then leads the investigator to revise the organization of materials, and so on for several iterations.

In the two examples given here of the case-study approach, the investigators make different choices regarding explicit generalization. Oscar Lewis, in an introduction to his cases, proposes that from them one can infer the grand, inclusive hypothesis that the lives described in the book were shaped by a "culture of poverty." Ronald Blythe is more circumspect in his treatment of case material. But he too seems to have a more general story in mind for which he intends his study to provide an instance: that the organization of society structures and constrains individual lives, but the human spirit, given the least opportunity, will break through the limitations that social organization imposes.

TWO CASE-FOCUSED REPORTS AT THE LEVEL OF CONCRETE MATERIAL

In *Children of Sanchez: Autobiography of A Mexican Family*, Oscar Lewis presents the autobiographies of the four children of Jesus Sanchez, a restaurant worker in Mexico City.¹⁷ Lewis edited and rearranged segments of his interview transcripts so that the lives of his respondents are told by the respondents themselves. He begins the book by having Jesus Sanchez describe coming to Mexico City to make a life for himself. He ends the book with Jesus Sanchez speaking of his present life as an older

man who has succeeded, finally, in obtaining title to a dwelling of his own. Between this beginning and end, Lewis has the children describe their lives—in the first section, their childhoods; in the second, their early adulthoods; in the third, their current lives.

Lewis's editing is intentionally novelistic. The first of the children to be presented is Manuel, the oldest. The excerpt from Manuel's interviews that Lewis chooses as Manuel's introduction has the quality of an early memory, like the beginning of Joyce's *Portrait of the Artist As a Young Man*. But it also sets the stage for what follows.

I was eight years old when my mother died. I was asleep on a mat on the floor next to my brother Roberto. My little sisters, Consuela and Marta, slept on the bed with my Mama and Papa.

Here we are introduced all at once, in the space of three sentences, to the main characters, to the poverty that defines them, and to the mother's death that was the tragic beginning of their lives with a father we later see to be brusque and resentful. We are also introduced to what will be the dominant tone of all the narratives: a flat, matter-of-fact description of intensely emotional events.

The last sentence of this first section of Manuel's account is a statement of the event that ended his childhood: "That's how we got married: I had just turned fifteen and she was nineteen." By his marriage, Manuel established himself as an adult despite his age. Consider how much control of his material Lewis needed to be able to find just the right quotations with which to begin and end this section.

Lewis next introduces Roberto, the younger brother, who will describe recurrent problems with the law. The quotation with which Lewis has him introduce himself is "I started stealing things from my own house when I was small." The theme of thieving is presented at once.

The end-of-childhood statement that Lewis gives Roberto, with which Lewis ends the first section of Roberto's narrative, is "Then I joined the army, first because I wanted to be a soldier, but most of all, because it was getting impossible for me at home." And so we learn of another way of establishing adulthood.

While maintaining the autobiographical form, Lewis was able to introduce descriptions of the social institutions that structured low-income Mexican life. Here, for example, is Manuel on seeking admission to the United States to work as a migrant laborer:

We got in line [in front of the U.S. Customs House] and waited. There were people from all ends of the Republic, all dirty, in rags, and starving. . . . Everybody was anxious to get through; I understood their desperation because I felt the same way.

Then the squeezing and pushing began. . . . The shoving got worse. I was standing between two big guys, and when I was being smothered and felt myself walled in, I grabbed the two of them around their necks and pulled myself up. . . . The Immigration Office was at the height of a flight of stairs. Well, these guys began going up the stairs with me hanging from them; otherwise I wouldn't have made it . . .

When I reached the office, I got nervous. We were all convinced that the Immigration Officer knew who was lying and who wasn't. . . . Then, "Thank God and the Holy Mother," I said to myself, "I think they are going to let me in." . . .

Always, in reading autobiographical accounts, we give some attention to the credibility of the narrator. Especially do we do this when narrators talk about themselves. Because Lewis presents the views of four siblings, we hear not only the self-appraisals of each but the appraisals made of them by their siblings. After hearing Roberto deprecate himself, it is instructive to have his brother, Manuel, tell us, "Roberto was really very noble, the most noble in the family."

The last words of the book are the ruminations of Jesus Sanchez about the meaning of his life. Lewis proposes in his introduction that the message of his book is the way the culture of poverty works to prevent people from rising. But the final statement of Jesus Sanchez suggests that his story may have a different message. Here are the words with which *Children of Sanchez* ends:

I struggled and worked day and night to establish my home, a poor home, as you can see, but I have my happy moments with my grandchildren. It is first for God and then for my grandchildren that I'm on my feet, plugging away. . . . I hope God will allow me to be with them until they can earn their own living. . . . I want to leave them a room, that's my ambition; to build that little house, one or two rooms or three so that each child will have a home and so they can live there together. . . . It will be a protection for them when I fall down and don't get up again.

This final speech of Jesus Sanchez suggests that the book is not about poverty as much as it is about fathers and children. The book's title—*The*

Children of Sanchez, not *The Lives of Members of a Poor Family*—suggests that Lewis may have recognized this. Although many stories may be found in the book, the one that is for me the most compelling is Sanchez's story of how he came to Mexico City as a young man in search of a better life, rapidly became the father to four children by his wife and to other children by another woman, provided for his children and raised them—while bitterly resenting having to do so—and discovered at the end that his children's lives, filled with troubles about which he knew little and yet in some fundamental way shaped by him, and the lives of his children's children, had become the ultimate justification for all his struggles.

Ronald Blythe, in his *Akenfield: Portrait of an English Village*, presents many more voices than does Lewis, none of them extensively and some only briefly. He is concerned less with the events that made up the lives of his respondents than with their personalities and their place in the life of his village.¹⁸ In a long excerpt, for example, one of the village's bell ringers talks about his obsession with ringing: "You have to be bitten by the bug. You have to be smitten. If you are a real ringer you think about bells morning, noon and night, and you only live for the next time you can have a go at them."

Early in the book, a man, aged 71, remembers leaving the village for the battles of World War I: Gallipoli and the battles in France. Another man, aged 80, talks about the Armistice. Both had been underpaid farm workers before they enlisted, and both returned to find conditions no better. There was a strike, bitter and inconclusive. A bit later in the book, retired officers and gentlemen are introduced to give their different views, and the class structure emerges. This is the way Blythe shows us how the personal and the social intermesh.

Blythe frequently introduces other material in addition to the interviews: he offers his own observations of a pub evening; he gives excerpts from the log book of the village school, beginning in 1875; he gives the local version of the Rumpelstiltskin legend, transcribing it in the local dialect; and he offers brief essays as introductions to interviews. He is willing to include anything—whatever scenes caught his eye, whatever thoughts came to his mind—that might deepen the picture of the village.

The result, one reviewer said, is "sociology written by a poet." Blythe offers a few generalizations about English life, but these are context for the lives he presents rather than the point of his work. His aim is to tell his readers what life is like in the small and, by extension, in the large.

The impulse seems to be like that underlying Thornton Wilder's *Our Town*: to capture the tragedy of the ordinary. At the very end, after having considered the many rhythms of the village, Blythe allows its grave digger to muse:

I have no family, none at all. No one in all the world is my relation. I never did read a lot. I never could give my mind to it. I talk too much, that is my failing. I come into contact with many people at a serious time, so I have picked up serious conversation. What most folk have once or twice in a lifetime, I have every day. I want to be cremated and my ashes thrown in the air. Straight from the flames to the winds, and let that be that.

These final words of the grave digger are also Blythe's final words: "Straight from the flames to the winds, and let that be that." So very many things can be done with a concrete case study—even the composition of a long prose poem.

Because Blythe says nothing about his analytic methods, we must guess how he put this case study together. My guess is that he sorted, arranged, and rearranged his case materials, probably several times, until he found a sequence that made a compelling story. He may have done this in his mind, but I think it more likely he shuffled pages on a table. Levi-Strauss is reputed to have said that if he had a card table big enough he could figure out all of France.¹⁹ There's much to be said for a large flat surface when trying to generate a coherent story from case material.

TYOLOGIES AS GENERALIZATIONS FROM CONCRETE CASES

A way of generalizing from concrete cases while yet retaining their holistic character is to introduce types. Description of a type suggests that a common core has been identified within a set of particular cases and now is being named. The investigator, in presenting the type, is saying to the reader, "Here is the underlying structure, the skeleton of the case, unencrusted by the idiosyncrasies of specific instances, and here is a model of the dynamic that sets its parts in motion."

The type embraces many cases in a single framework. By invoking a type, the investigator need not describe first this particular case of, say, a young upwardly mobile couple of middle income and then that particular case of a young upwardly mobile couple of middle income and then the next, but can instead describe, all at once, "The young upwardly mobile couple of middle income."

Investigators often tell readers what they think is essential in a type by the name they give it. Suppose we believed that what was essential about young upwardly mobile couples of middle income was that they were doing everything in their power to achieve social success. We might communicate this by naming them "young strivers." We could then propose that this essential characteristic was responsible for much of the behavior of our respondents. We could therefore call on it to explain, among other things, the frequency with which they maintained dual careers and their determination to send their children to the best schools.

This view of types is close to the biological idea of species. One type is different from another in the same way an oak is different from an elm; there is a difference in kind. If it is a single type that is being described—the suburban family or the American housewife—the underlying idea is of a single inclusive species. But often a typology made up of several types is identified. Cuber and Harroff, for example, identified five types of marriage among the affluent: "conflict-habituated," "devitalized," "passive-congenial," "vital," and "total." Each of these is equivalent to a subspecies within the inclusive species of marriage of affluent couples.²⁰

We can use our model of the type to report, in a fashion similar to the local integration of issue-focused data analysis, the main line of our interview material and the variations from the main line. We would first describe what we believe to be true of the group as a whole and then turn to its usual expression and variations from that. For example: "While all young strivers are limited in the time they can give to being with friends, their responses differ. Most accept that they will see friends less often than they would like. But one couple arranged with two other couples to have potluck dinner together, children included, every other weekend, with each couple serving as hosts in turn. And a second couple each summer arranged to take a vacation home with another couple, not always the same one."

Types can be constructed of anything that could serve as the subject for case studies—not just individuals but also relationships, families, organizations, and more. Gerhardt, in a study of dialysis patients, found it useful to construct two idealized "patient careers," one leading to invalidism and the other to a return to normal or near-normal life.²¹

Types are effective ways of communicating images that then are easily incorporated into thinking. Indeed, types can be seductive in their capacity to simplify thought. It is easier to picture in one's mind *the* single

parent than the range of family arrangements and living situations that would be found by a survey of single parents—although it is the range of family arrangements that is the reality.

The Epistemological Status of Types

Where do types come from? Are they inferences from observations or are they conceptual inventions? Arguments can be made for each view.

When most people invoke a type, they imply that the type captures essentials shared by all the instances it embraces. People who talk about "deadbeat dads," for example, have in mind a way of acting toward a family left behind that they believe is common to a group of irresponsible divorced fathers. The essentials that are believed to mark the type presumably have been inferred from examination of instances. To the extent this is the case, types are anchored in reality.

What is often unclear, however, when a type is thought of as a classification based on essentials, is how one is to know in any particular instance whether the essentials are there. Is Madame Bovary an instance of the neurotic type or just a victim of romantic novels? How can we judge whether Madame Bovary's behavior is essentially neurotic or not? The difficulties of deciding in any given case on the presence of inferred essentials may suggest that types would be better defined by the observables themselves rather than inferences from the observables. Then we could decide whether Madame Bovary was or was not an instance of the neurotic type by considering whether she displayed such observables as nervous tics or unjustified fears.

A second approach to typing, then, would be to define a member of a type by a conjunction of one or more significant characteristics. For example, a "young striver" would be someone in the right part of the age range (between 25 and 35), with the right marital status (married), the right occupation (middle-class), and a high score on a rating scale of desire for upward mobility. We could leave open the question of whether the observables (between 25 and 35, married, middle-class occupation, rating scale score) are causes or expressions of some underlying essential. Some might hold that the conjunction of observable characteristics locates an unobservable essential (such as conformist striving) with more or less success. But others might hold that the conjunction *is* the type, period.

Defining a type in terms of observables can be helpful for the work of classification. (It is the approach taken by the American Psychiatric As-

sociation's *Diagnostic and Statistical Manual of Mental Disorders*.) Insofar as it is unsatisfactory, it is because there is something odd about proposing a special name and implying a special set of dynamics for individuals who differ only slightly from others. Could someone be almost neurotic but not quite? Once we admit that, yes, this is a possibility, we suggest that we are imposing typological thinking on what are actually continua.

This brings us to the third meaning of types, in addition to their identifying essentials and their representing a conjunction of characteristics: types, no matter their source, are conceptualizations, mental constructs. Whether they have been inferred from real instances or not, they are themselves only imagery, theories in the form of models of people or families or institutions.

This is the construction Weber would have us make of types. He proposed that we treat types as pure conceptualizations, with no necessary match in any real instance. Their purpose is to help us grasp our empirical cases by providing us with coherent models of what would exist if essential elements of the cases were in pure, idealized, form. We can then understand actual cases by comparing them with the conceptual template provided by the type. We note where the cases match the type and where they do not. And we can understand the dynamics of an individual case by understanding those of the ideal type and then considering how much the individual case is a modification of the ideal type.²²

The Weberian approach is appealing. And, yes, we do use types in our thinking and in that sense all types are conceptualizations. But beyond this the Weberian view has limited application. The types we use in the reports of our studies are almost always intended to be descriptive of our data. Even when they are presented as idealizations, they are idealizations of a state real instances are thought to approach. Consider the typology of the "other-directed" character type and the "inner-directed" character type in *The Lonely Crowd*.²³ Although the types were idealized, they were used descriptively to suggest how the America of its time had changed, and readers certainly interpreted them as descriptive of the characters of real people.

My own view of the matter is that types are almost always generalizations from observed instances or at least purport to be. If the instances have been well observed and the types capture important dynamics, they can be a powerful device for explaining how things happen. They also can offer vivid imagery even when there is no evidence for the theories they

imply. A commonly used typology of personal functioning classifies people, on the basis of their birth dates, as Libras or Geminis or any of ten other types. This classification is believed by many to make individual behavior understandable and to an extent predictable. There is, of course, no evidence whatsoever to support the theory. In general, types and typologies are a valuable resource in data analysis, but they should be treated with caution.

An Example of Generalized Case-Focused Analysis

In *The Gamesman: Winning and Losing the Career Game*,²⁴ Michael Maccoby attempted to present portraits of the managerial types who direct large American businesses. Working from structured and unstructured interviews with 250 managers from 12 major companies, Maccoby identified four character types: "gamesmen," "craftsmen," "jungle fighters," and "company men." The types differed in motivations, goals, resources, and ways of treating others, but what was most important for Maccoby was that they approached their managerial jobs in different ways. The gamesman treated work as a gamelike challenge, the craftsman as a set of technical problems, the jungle fighter as a conflict to be won, and the company man as an opportunity for service.

Maccoby describes in detail how each of his four types of business leader functioned in the business environment. He also provides case studies to suggest the kinds of people he classified into each of his types and the kinds of business environments in which each type might do well. Because he thought gamesmen would be the managerial style of the future, he gave a good deal of space to describing a respondent whom he characterized as a "creative gamesman."

Where did Maccoby's types come from? Maccoby reports that he based the typology on his study of the managers' self-descriptions, augmented by their responses to Rorschach ink blots and descriptions of them provided by their colleagues. Maccoby describes his approach in this way:

Once we had scored [the respondents] in terms of dynamic character traits, different types began to emerge as distinct from one another in terms of the individual member's overall orientation to work, values, and self-identity. We eventually came to name four main psychological types in the corporate techno-structure: the craftsman, the jungle fighter, the company man, and the gamesman. These are "ideal types" in the sense that few people fit the type

exactly and most are a mixture of types. But in practically every case, we were able to agree on which type best described a person, and the individual and his colleagues almost always agreed with our typing. (p. 37).

Maccoby alludes to three analytic processes in this paragraph. The first process was perceiving a typology among the different groupings of scores respondents had been given on such traits as aggression and intellectuality. Theories of personality may have played a role in the interpretation of the observed clusterings, but chances are a certain amount of inspiration also entered in. We have to guess, because we aren't told exactly how the typology was developed.

The second analytic process was the naming of the emergent types. Here Maccoby clearly sought words that would accurately communicate each type's essential nature. Implicit is a theory of individual functioning in which some single self-other outlook determines social behavior. Actually, the types proved too broad for many analyses, and at several points Maccoby had to introduce subtypes. Craftsmen were divided into the "dutiful," who were driven and hierarchical, and the "receptive," who were more life-loving and democratic. Jungle fighters were subspecialized into "lions," who terrorized, and "foxes," who manipulated. As I noted, one gamesman was a "creative" gamesman; presumably, others were not.

The third analytic process to which Maccoby refers in the quoted section was the assignment of individuals to one of the four types. Although Maccoby first notes that few people precisely fit a single type, he then says that there was agreement by respondents and their colleagues regarding the type to which respondents were closest. It seems to me that Maccoby is waffling a bit on the epistemological status of his types. On the one hand he describes the types as inferential, developed from a study of diagnostic material. He asserts that in practically every case managers could be fit into one type or another and that there was agreement regarding the typing. But in introducing the types, Maccoby described them as ideal types, which real instances might approach but should not be expected to match.

One test that Maccoby's typology passed with flying colors was acceptance by its readers. *The Gamesman* made the best-seller list. Most reviews were positive. Those that were not were critical of Maccoby's view of the managers rather than of the methods he used in his study. One

reviewer, for example, speculated that, given the time they were able to take from their work to participate in the study, Maccoby's managers were second-stringers rather than real leaders. Another reviewer thought the managers banal and uninteresting. But all reviewers accepted Maccoby's typology as valid. For a time, at least, the book was influential in business circles.

THE DEMONSTRATION OF CAUSATION

In qualitative interview studies the demonstration of causation rests heavily on the description of a visualizable sequence of events, each event flowing into the next. In *Children of Sanchez* we can see how the tensions in the Sanchez home, together with the values of the society in which he lived, pushed Manuel into independent adulthood at age 15. In *Blood of Spain* we can follow the widening impact of garrison revolts into first skirmishes and then war. In *Blue-Collar Marriage* we can see the ways that mutual trust and respect, plus a secure marital partnership, can produce contentment in a marriage in which there is little apparent companionship. And in *The Gamesman* we can see how managerial styles affect organizational cultures.

The description of a visualizable sequence of events differs from the approach of quantitative studies as a way of demonstrating causation. Quantitative studies support an assertion of causation by showing a correlation between an earlier event and a subsequent event. An analysis of data collected in a large-scale sample survey might, for example, show that there is a correlation between the level of the wife's education and the presence of a companionable marriage. In qualitative interview studies we would look for the process through which the wife's education or factors associated with her education express themselves in marital interaction.

A search for the visualizable process that led from one event to the next can be one of the directing forces in an interview. At the end of many interviews the investigator can offer, with some confidence, a description of how some aspect of the respondent's life or situation came to be. If the same sort of description fits a series of respondents, the investigator can feel justified in proposing a more general statement as a hypothesized minitheory. Each new interview can then be a test, the results of which will support the minitheory, discredit it, or, most likely, require that it be augmented or qualified.

One idea of how to establish causal connections using qualitative interview material no longer has much currency. This was the idea that an invariable sequence must be found in the material, that is, that determinant A must always be followed by consequent B and consequent B must always be preceded by determinant A. Robert Cooley Angell, in *The Family Encounters the Depression*, and Donald Ray Cressey, in *Other People's Money*, were among those in the social sciences who tried to find a process in their materials that was present each and every time. Angell hoped to show that income loss in families that were initially poorly integrated would always end in familial disintegration whereas in better-integrated families income loss could be taken in stride. He ultimately gave up on this hypothesis; some families that looked bad to begin with were nevertheless able to pull themselves together. Cressey did find an invariant association between certain potentially causal elements, including the embezzler's ability to find some way of justifying what he was doing, and ultimate embezzlement. But, by and large, invariable associations aren't to be found, not in interview data any more than in survey research.

However, a process does not have to be invariable in order to be causal. There are always extraneous factors that can modify the direction of a particular process. All that is necessary to show that a process may be causal is to make evident how it moves from cause to consequence. *Blood of Spain* shows that news of the garrison revolts in Morocco led to battles elsewhere in Spain, although such battles did not happen everywhere. It is a distinct problem—albeit a most important one—to identify the factors that can impede, redirect, or facilitate a causal process.

In chapter 2 I pointed out that qualitative interview studies regularly lack comparison groups; it's hard enough to study the group of interest without taking on the study of another group. I also suggested that it is often desirable nevertheless to do some limited interviewing of comparison respondents. Here let me note that we quite often can make comparisons within the sample we are studying. Komarovsky had within her study the comparison groups of couples who had completed high school and couples who had not. And although she had limited her study to people who were raising children, she could ask her respondents to tell her about their preparental lives. Her respondents could, in a way, serve as their own comparisons. On the other hand, if Komarovsky had wanted to consider the possible effects of the working-class status that all her

respondents shared, she would have needed at least a few comparison cases from the middle class.

Respondents often have beliefs regarding causal processes, but they must be treated cautiously. The returned veterans of Blythe's *Akenfield* blamed the landowners for the hard times they experienced on their return from war, but the landowners too were caught in an economic downturn that affected them all. Potential causal factors outside respondents' observational fields cannot be referred to by them.²⁵

Caution in the treatment of respondents' causal explanations should be extended to respondents' explanations of their own behavior. People tend not to be fully aware of their emotions and their motivations. This does not mean that their self-reports have to be disregarded. Rather, they should be treated as likely to be incomplete.

Nevertheless, we do well to attend to respondents' descriptions of causation in social developments as well as in themselves. Often, they have identified at least part of the dynamics that have produced events. Some respondents may be especially worth attending to because they were close to the decision-making process or to some other development that set events in motion, or are especially perceptive or thoughtful. We will, of course, have to treat what these respondents tell us as suggestions to be investigated further, but they may point us in the right direction.

I think we ultimately have to judge the validity of our identification of a causal process by considering all of our evidence together. We have to weigh the nature and density of the interview information supporting the identification; consider whether we have negative cases and, if we do, whether we have plausible explanations for them; and determine whether there are other bases for support or, on the other side, reasons for doubt.

SUMMARY

Certain analytic processes show up in every analysis of the data of qualitative interview studies: sorting the data, achieving local integration, achieving inclusive integration. In analyses intended to generalize regarding issues, coding is also important. In a typological analysis, the characterization of types is crucial.

Each analytic approach has strengths and drawbacks. The generalized approaches, both issue-focused and case-focused, contribute to our understanding on the level of theory. The approaches that provide us with

concrete materials, again both issue-focused and case-focused, enlarge our experience and provide us with a basis for developing our own inferences.

Causal explanation can be found in the products of every analytic approach, issue-focused and case-focused, concrete and generalized. Its demonstration rests on empirical and logical grounds that are different from those of a survey or an experimental study, but it need be no less a contribution to our understanding for that.

CHAPTER 7

WRITING THE REPORT

GETTING STARTED

By the time a study has reached the phase in which writing is its investigator's primary concern, it will have amassed a great deal of material. The study's file cabinets are likely to contain—in addition to the interview transcripts and excerpt files—codings of transcripts, descriptions of minitheories, data supporting the minitheories, summaries of the literature, and lists of questions to be answered, together with some answers to the questions. There may also be outlines and drafts of chapters or papers. Despite all this—maybe because of all this—it can be hard for the investigator to know where to start the report.

A first effort might be to reconsider the questions that motivated the study and how they have been answered. This may in itself provide an outline for a brief report. But it may be that answering the study's initial questions will use only a small part of the information that has been gathered. Or the study's initial questions may no longer seem of central importance, after all that has been learned.

If the investigator has already given a good deal of effort to analysis, the results of the analysis should provide the material for a report. All that may be necessary is their organization, development, and presentation. On the other hand, the investigator may have postponed full scale analysis until a decision was made on what was to be reported. In either event, but espe-