6304

Ongoing Data Analysis

The concept of self-as-instrument may have been exotic at the beginning, and up to the point of analysis most students have felt it as an exciting and compatible vision, a breath of fresh research air. After all, self-as-instrument, rather than having the self dependent on other instruments, connotes personal control and personal responsibility and, therefore, personal creativity. But the start of analysis comes early, with the very first log notation. This is because analysis is part and parcel of the ongoing, intertwined process that powers data collection. For most people, analysis carries feelings somewhat different from assurance. The five of us have found that often the first analyses create a place where reality hits, where doubts, fears, and avoidances begin, where the theory and philosophy of qualitative research are put to a reality test. We have also found that this is a place of great value and rededication and personal joy. When the researcher gets right to it, it is an awesome, even frightening responsibility to bow to the fact that 'self-as-instrument' inevitably means one must create ongoing meaning out of the evolving and evolved data, since raw data alone have little value. But it gives tremendous elation to know it can be done. Thus, the naturalistic researcher must come to rely on his/her own talents, insights, and trustworthiness and, in the end, go public with the reasoning that engendered the results, while accepting with equanimity that other people may make different meaning from the same data.

Joan Giansante discusses the problems associated with getting started on the analysis:

> I am always putting things in order.
>
> On my desk, I have paper clips in one dish, rubber bands in another, and stamps in another. I even have my writing instruments in two jars — pens and pencils in one jar and colored felt markers in another.
>
> When I work as a consultant in corporations, I can easily classify and categorize everybody else's business. Once I was hired to improve a long-winded manual that desperately needed reorganization and editing. After reading the forty-five-page draft, I immediately told the writer, 'You have three different publications here: a brochure to inform management about your service, a user's manual for those who will use your service, and a job manual for your employees'. It seemed so obvious!
>
> Why was I having so much difficulty finding order in the chaos?
>
> The answer seems to have three aspects.
>
> 1 Like the writer I was hired to edit, I was overwhelmed by the data.
> 2 I had great difficulty getting started with analysis. I hesitated

> to select a focus, waiting for something to emerge, or hit me over the head. When I settled on three main issues, I could not come up with subcategories. Which brings me to the third aspect of the problem.
>
> 3 I skipped a step in the analysis procedure. I tried to go from identifying the main issues to making conclusions about those issues. I had skipped the important step of beginning with descriptive categories.

The categories Joan mentions begin with the smallest, most literal descriptions of the unfolding words and events. Creating categories triggers the construction of a conceptual scheme that suits the data. This scheme helps a researcher to ask questions, to compare across data, to change or drop categories, and to make a hierarchical order of them. At its most useful, the process of establishing categories is a very close, intense conversation between a researcher and the data that has implications for ongoing method, descriptive reporting, and theory building. In the foregoing statements, we have been deeply influenced by the work of Glaser and Strauss (1967) and Strauss (1987). Indeed, apropos of this section, we recommend Anselm Strauss's (1987) detailed, cogent treatise on his sense of the discovery of grounded theory via qualitative analysis. This includes several chapters on coding strategies for a variety of purposes that seem pertinent, whether or not a researcher wants to construct theory.

Establishing categories from qualitative data seems rather like a simultaneous left-brain right-brain exercise. That is, one job is to distill categories and the other is to keep hold of the large picture so that the categories are true to it. Experts (for example, Bogdan and Biklen, 1982; Giorgi, 1989; Goetz and LeCompte, 1984; Lofland and Lofland, 1984; Miles and Huberman, 1984; Tesch, 1990) have explained how they go about establishing categories. The teaching team has worked out its way which amalgamates many of the proposed ideas:

1. Start by reacquainting yourself deeply with what you are about to begin to categorize. Choose one entry in your log. Read and think about those pages several times until you feel you have caught their essence. Don't stint here. The team members do at least four readings for each entry.
2. Next, write some notes in the margins of those pages. 'Free think' your ideas. You might remark on lines that interest you — and why, your question about the text, your insights about your frameworks for observation and interview, and any topics that seem to be cropping up. These notes are useful for the next step, as well as for later when you are working to describe the meaning of your data.

3 Because '... nobody can do an analysis without some kind of breaking into parts' (Giorgi, 1989, p. 4[illegible]), this is now your job. We've concluded that applying the concept of creating 'meaning units' works well here. This occurs as one reads the narrative and divides it in some way that makes sense. Giorgi (1989) describes that since he is interested in understanding the meaning of experience, every time he notes a change in meaning in the text he puts a slash there, and continues in that manner until he has segmented the entire piece. He says, '... I let my spontaneity operate while constructing meaning units; "Something important is happening here", or "There's a change here", or "Something interesting is going on here"' (p. 49).

4 Now, label your meaning units. That is, in the margin next to each unit, label what that unit is about. Try to use one word, or at least as few words as possible. Use your own labeling words here unless the right label jumps out at you from the text. In this step, make the labels descriptive. Even though they were in their final analysis stage, the labels of Ronna Ziegel and Jim Hinojosa on pp. 146–7 are good examples because the process of creating categories is quite similar at any stage.

5 After you've studied and marked several log entries, make a list of all your labels. Group the ones that seem to fit together and try to find one label that will do for each entire group. Group the ones that don't fit together and let each stand separately. Look for links between labels. Compare. Contrast. Move labels around if that is sensible. Play a bit. Remember, the test for a useful label is that it describes what the meaning unit is about, not that it has occurred many times. Although that happens, a label that occurs once only can be very important to your research.

6 Analyze the next few log entries by applying the labels you've decided upon in Step 5. Be careful here not to force labels that don't fit well into meaning units. This indicates that other labels are needed. If so, make some other labels and try them out, yes, from the very beginning to the end of the pages you have already coded once. Do this as many times as you need until you judge that your categories suit the data. Don't try for perfection. Not everything will fit. Try for a sensible organizing scheme. At that point you have succeeded with a valuable qualitative analysis technique. When your labels fit well, they may be considered tentative titles for categories. Understand that sometimes the same meaning units can be an example of more than one category, and, so, can be coded in more than one way. Our categories often overlap.

7 Write analytic memos as you go along. These will help you at this point and in final analysis.

These steps apply particularly to making beginning categories, and I feel it is best not to move too quickly to other meaning constructions. Because of this, the topic is continued in Chapter 5. Two more suggestions may serve. First, if you are having trouble starting the process, take a cue from what we do in class. Margot hands out a transcript — any transcript provided by a person who is not a member of the class — and we construct tentative labels in the ways I have described. It works every time. Find a piece of log narrative, perhaps even a published one, and begin. Second, the section about thinking units on pp. 143–5 may also help you to get started.

The next two quotations are fine examples of how methodology and the creation of categories influence each other in process. Jo Ann Saggese tells how interviewing created difficulties with her categories.

> I began interviewing and analyzing. Things started looking very different. All the observations that fit nicely into categories began to bulge and pucker and made packaging very difficult. For some reason I had a strange notion that if I put my log away, when I finally decided to retrieve it, everything would be analyzed, synthesized, and computerized. I tried it. It doesn't work. In the field of occupational therapy, a learning through doing theory exists. Case study exemplifies this theory. I let go of my categories and observed some more. This time my new categories started to make sense.

Marcia Kropf planned to study how students learned computer programming, but as she analyzed her log, she found that what most drew her attention was human interaction:

> My logs began to be filled with the comings and goings of students, the ways in which they were welcomed in the class and asked to participate (or were not), and the ways in which they attempted to participate (or did not). I found myself fascinated by the interactions between these students and the teacher.

Because Marcia stayed in touch with her log through rereading, she was able to identify a new category she tentatively labeled 'interactions' that seemed important to her. This helped her to become a more focused observer in the ensuing sessions. Staying close to the log through repeated rereadings helps the researcher find new directions, refine questions, develop emergent meanings, and hone a conceptual scheme.

The timing of discovering useful categories is not the same for everyone. Sometimes a great idea comes along late in analysis, thereby causing a review of all the preceding data in light of the current discovery. For other researchers, the process from descriptive categories to linked categories and finally to establishing overall meaning follows an even progression without major detours. Most of us, however, take a number of detours before we make final meaning. Chapter 5 will continue this discussion by focusing on analysis of data once the researcher has left the field.

Computers as Aids in Analysis

One of the exciting advances in qualitative data analysis is the continuing development of various computer software packages to assist in the tagging and retrieval of data. Although computer programs for text analysis have been around since 1966 (Tesch, 1990), it is only since the mid-1980s that easy-to-use software for the two most common varieties of computers has been widely available. Many word processing packages and database managers have advanced features that allow the user to manipulate data onscreen. WordPerfect allows the user to create a second document into which text can be moved while the user moves back and forth between the two documents. Other software allows text to be dragged to a 'window' for storage and retrieval. Both methods will speed the laborious process of cut-and-paste, but if the original context is to be preserved, each segment must be identified individually. As of 1990, six packages have been developed by researchers in the social sciences to overcome this limitation: ETHNO, TAP, QUALPRO, The Ethnograph, TEXTBASE ALPHA, and HyperQual. These 'Analysis programs perform only *two basic functions*: They allow you to attach codes to segments of text, and they will, according to your instructions, search through your data for segments that were coded in a certain way, and assemble them' (Tesch, 1990, p. 150).

But wait, you may be breathing too easily. Text analysis packages do not do the analysis for the researcher. The user must still create the categories, do segmenting and coding, and decide what to retrieve and collate. If, however, the researcher is reasonably comfortable with a computer, the analysis packages can remove most of the drudgery from the cut-and-paste process. The main objection to the process, especially from non-computer users, is that the computer and the software put distance between the researcher and the material. Whether or not this happens depends on the researcher's work style, not the computer. People who have used the packages are often amazed that this kind of work, with its thousands of pages of data, could ever have been conducted by hand.

Indeed, because of the ease with which data can be organized and [illegible] pair of scissors and tape. The savings in time and energy may motivate the researcher to reconsider data that might previously have been put aside or neglected, to place data in more than one category to test new relationships, and, in general, to be more creative. It is our intention merely to alert the beginning researcher to the availability of these packages. For a more detailed examination of these matters, you might consult Renata Tesch's 1990 book, *Qualitative Research: Analysis Types and Software Tools.*

Leaving the Field

> It's time to analyze and present data — and I'm still anxious. Do I have enough data? Do I have the 'right' data? Are my data presented properly? Should I leave the field, or should I stay? How do I know when enough is indeed 'enough?' (Rena Smith)

Knowing when to leave the field is a judgment based on the researcher's sense that substantial amounts of data have been gathered on both the initial questions and the questions that have emerged during the study. Courage, insight, and, for many of us, emotional sturdiness are required to decide that observation has been sufficiently persistent and prolonged; all of us, though, have lived through it, and there is always the comforting thought that we can return to the field if that is necessary.

While this judgment must be faced personally and often alone, there are some criteria for when it is time to leave. Being in the field for a sufficient time should create a feeling of immersion. The researcher should know that he or she can talk for the participants — as the participants — in a legitimate way. If the researcher can't speak for the participants, then more time in the field is necessary. In addition, if there are too many unanswered questions pertaining to the questions asked in process, more time must be spent in the field. Remember, however, that most often there will be more data in the log than can be shared in a research report. Fight the tendency that many of us have to want to include everything in the final report. Useful data can sustain other studies, other articles. Since you are the only expert about your data, your judgment is required to know when your important questions can be answered and, therefore, to know when you have gathered sufficient data. Fight also that old siren song that tells you to stay in the field because it's such fun, because you are needed. Those issues may need to be considered, but they are not sufficient reasons by themselves to stay yet another week, another month:

> Leaving the field can also be a time of strong feelings. Who wants to leave doing something that has been so much fun, where people have responded in such a positive fashion? Qualitative researchers have a wonderful time collecting their data. The hard part begins when they have to stop collecting. So when I had to leave the field, there were two feelings. One was the sadness of leaving people who have been fun and interesting and helpful. And the other was the anticipation of the agonizing amount of work that lay ahead. (Leslie Rice)

In deciding when to leave the field, time is relevant, but the significant factor is what is done with that time. A micro-analysis of a series of videotapes may require relatively few hours to tape, but many hours to analyze. When documenting the social interactions among group members, prolonged face-to-face contact is necessary. You are the best judge. So, time alone is not a reliable indicator of when to leave the field. Other elements are also not reliable: 2000 pages in your log, a log too heavy to lift, a log that occupies more than one cardboard carton, forty interviews, exhaustion. Well, if these factors are of no help, what is? Redundancy and a feeling of completion. Lincoln and Guba (1985) tell us that when the data repeat themselves, when the researcher has confidence that themes and examples are repeating instead of extending, it may be time to leave the field. It is time to go when the researcher feels he or she has accomplished what needed to be done (Bogdan and Biklen, 1982). In addition, taking a break and leaving the field can bring unexpected revelations:

> I wanted to abandon qualitative methodologies because of inner conflicts. I was able to leave the field for two weeks, and put aside all my work for the study, including the rereading of the logs. After two weeks, the logs beckoned. As I reread what I had written, my doubts began to fade. My log convinced me that qualitative research was for me. (Barbara Gagliardi)

Keeping the Door Open

Qualitative researchers are deeply immersed in the analytical process all through the data-gathering stages, but analytical surprises frequently emerge when the researcher is out of the field and looking again at all the data. It is important, then, to leave the participants in the field with the understanding that the researcher might want to return for another visit, another interview, or a few phone calls. If the researcher has promised to feed back some information, for example, this would be an opportunity to plan that event. Often, the sharing session is an appropriate time to do some final clarifications.

As we have noted, when the research has been a happy experience, the researcher and the participants may be sad about parting company. It's wonderful to make friends, but people generally understand [illegible] self-limiting relationship. Sometimes the relationship continues, but in a different way. Laura Lee Lustbader had this to say about her study of a 17-year-old woman labeled schizophrenic:

> My case study for my graduate work is completed now; however, the relationship between Eileen and myself, which is a by-product of the class assignment, is by no means a *fait accompli*. When I wrote my final field notes, I had the overwhelming sense that I was a rat! Here I have a project. I like the project and believe I learned a lot from the doing of it. What happens to Eileen? What does she get out of it? I believe she has a friend. No longer do I need to run to my desk when I finish spending time with her, nor do I have to see her weekly. But, I can see her as she and I wish, and as time permits, to be her friend and sounding board.

Keeping the door open is the key to minimizing any reluctance about leaving the field. We discuss more about returning to the field in Chapter 5.

Establishing Trustworthiness

> Trustworthiness is a big issue; that is, it is big in scope. In fact, it is bigger than I ever imagined. I used to think trustworthiness was pervasive; but after reading Guba and Lincoln, I realize that it is more than pervasive. It's so big that it's bigger than the ethnographic study itself. Not bigger in the sense of how much time it takes. But bigger in the sense of how much it has to be thought about — before, during, and after the ethnographic study is done. (Joan Giansante)

Being trustworthy as a qualitative researcher means at the least that the processes of the research are carried out fairly, that the products represent as closely as possible the experiences of the people who are studied. The entire endeavor must be grounded in ethical principles about how data are collected and analyzed, how one's own assumptions and conclusions are checked, how participants are involved, and how results are communicated. Trustworthiness is, thus, more than a set of procedures. To my mind, it is a personal belief system that shapes the procedures in process.

Because concern for trustworthiness is part of every qualitative research activity, it is present in spirit in every one of our chapters. In two of them, however, trustworthiness is addressed directly. Here, in Chapter 3, we discuss

those issues that seem particularly germane to the processes of collecting data [illegible] analysis and writing the research report.

No matter what their situation, their past education, or their research experiences, qualitative researchers know that there is considerable concern about trustworthiness, that it comes from many quarters and in many voices, and that it is clothed in some controversy.

The Shock of Recognition

When we first came to consider research during our doctoral studies, all five of us, and we believe most others, came with 'quantitative heads'. We knew that issues about validity and reliability were paramount, and we knew that there were some tried and true ways to establish these. Many of us remember our days in a foundations of research seminar where questions regarding reliability and validity became staples of our weekly critiques of each other's first, stumbling efforts at research design:

> How can this be replicated? Is this a representative sample?

What we did not know at the time, even after we first began thinking about qualitative study, was that while issues about reliability and validity apply to both quantitative and qualitative work, they are conceived of and arrived at in different ways. When people inclined toward viewing the world with qualitative lenses recognize this fact and begin to understand the routes to doing acceptable research in that paradigm, it is often an intense and poignant event:

> I felt all those years of struggling to believe — and to get others to believe — that all those sampling procedures, those equations, those different forms of predetermined tests indeed measured whether what I was doing was truly reliable and valid, slip off me like the dead weight I finally accepted they were. Now I could ask questions such as, 'How adequately did I represent what I witnessed?' 'What's the match between my vision and those of the people I studied?'
>
> I didn't fool myself. There was plenty of hard work ahead, harder by far. But let me tell you, I was freed. I was breathing differently. (Margot Ely)

For many of us, the use of different terms for 'validity' and 'reliability' is a deliberate and liberating act. To speak of trustworthiness and its components of credibility, transferability, dependability, and confirmability (Lincoln

and Guba, 1985) or of authenticity criteria (Guba and Lincoln, 1989) is to [illegible] of the issues and processes that must weave their way through and beyond our qualitative research to keep it and us honest and believable. These terms are not just different ways for qualitative researchers to say the same things that positivists say. They indicate real differences, at least to us. Not all qualitative researchers, however, agree, and they continue to use some of the terms of positivist research in their work (Fetterman, 1989; Goetz and LeCompte, 1984; Miles and Huberman, 1984). These people may be doing the rest of us a real service by making a statement in familiar terms to the larger community that qualitative work can be reliable and valid. On the other hand, we have found that the language of positivistic research is not congruent with or adequate to qualitative work, and its use is often a defensive measure that muddies the waters.

To me, the fundamental differences between the naturalistic and positivistic paradigms became most clear in the encounters I had about trustworthiness with other doctoral students and faculty. It was in this arena that research issues went beyond the specifics of a particular piece of research to engender great interest, and heat:

> How do we know?
> What can we know?
> What is worth knowing?

Morgan, as quoted in Lincoln and Guba (1985, pp. 293–9), states: 'Different research perspectives make different kinds of knowledge claims, and the criteria as to what counts as significant knowledge vary from one to another'. In our experience, if one researcher sees 'reality' as single, fixed, and ideally 'objective' and another sees it as multiple, changing, and transactional, the latter position encompasses the former, sees it as one among many possible approaches. The former can encompass only the fixed view. The result is a standoff. This standoff, this clash in points of paradigmatic views, can often show itself in what people consider to be acceptable in their research. What seems important for researchers in any paradigm is to understand thoroughly what needs doing in order for their research to be trustworthy and to work to communicate that as clearly and as non-defensively as possible.

In the Act: Working toward Credibility

The words sound fine. Who could be against Lincoln and Guba's (1985) advice that to increase the chance of doing a credible research job, one that can be believed by the people who were studied as well as by the readers of one's report, a researcher must:

have prolonged engagement in the field;
do persistent observation;
triangulate;
search for negative cases;
determine referential adequacy;
experience peer debriefing;
check with the people one studied.

It is the doing of these, the actions, however, that establishes credibility. This is not simple or neat. In this section we address the first three of these topics. Chapter 5 considers the rest.

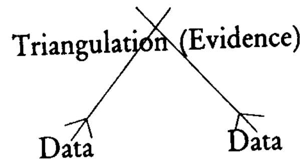
Prolonged and Persistent Observation. Joan Giansante tells of the value of prolonged engagement and persistent observation to the credibility of her work:

> Prolonged engagement enabled me to ask interview questions that followed up on the decisions I had observed Pam make earlier. After all, it's one thing to say you are going to do something, and quite another to actually do it. My perceptions about the professional demeanor a business must maintain in order to succeed faded over time. And time also played a role in gaining Pam's trust. Because I had enough time, I could devote major portions of the interviews to what Pam wanted to discuss.
>
> Persistent observation forced me to look at the big issues. I tried to clarify what they were and how activities impacted on them. Then I observed them deeply.

And, you ask, how long is prolonged? How much of my life needs to be given to this research? I know two things: this is a meaningful question and there is no one answer. Margaret did her fieldwork over two years, albeit she held a full-time position for the same span. Teri collected data for one and a half years. I did that job in two years. Many of our doctoral students collect data in one semester of six months or less. Prolonged engagement depends so much on your research questions, your data-collection methods, your available time, and the time the research needs. In her experience as mentor to doctoral students, Margot has found that often people give similar time to qualitative and quantitative studies, but they apportion this time in different ways. In her view, qualitative researchers spend more time in the field and in data analysis.

Triangulation. In previous sections of this chapter, we discussed a number of ways to gather data and several approaches to ongoing data analysis. These are more fully described in Chapter 5. Checking data obtained by a variety of

methods is one way of contributing to trustworthiness. Watching for the [illegible] of data, for triangulation of findings, can be as suspenseful as it is important.



Many experts indicate that triangulation characteristically depends on the convergence of data gathered by different methods, such as observation and interview. We have found that triangulation can occur with data gathered by the same method but gathered over time. This has been true, for example, of observational notes in field logs written over some months. In addition, triangulation can be based on different reports about the same event by two or more researchers who are studying the same phenomenon. Not every log entry requires a search for triangulation. Guba and Lincoln (1989) suggest that researchers seek only to triangulate, to cross check, '... specific data items of a factual nature' (p. 241) and to check insights, results, conclusions, and presentations with the people they studied and with their peer support group. These latter checkings are discussed in Chapter 5.

Patricia Cobb puns about her rather traumatic introduction to the importance of triangulation. She had barely begun to study a colleague's classroom:

> I experienced a sense of being overwhelmed by all the different measures that must be taken into account to continuously establish credibility. During one of my case study classes, Margot talked about an assumption I had made in the field notes in my log about the teacher I was observing. She asked had I established prolonged engagement, done persistent observation, and where was my strangulation? Oops, I mean where was my triangulation to support this assumption.

Suzy Hahn studied the experience of several women of different ages who worked as grocery clerks. Suzy shares what she did to triangulate:

> In reviewing field notes, writing 'thick' descriptions, and composing analytic memos, I checked for triangulation vs. 'seeing what I wanted to see'. I went back to conduct one long and several short interviews to see if the data would repeat. Many did.

While it sounds relatively straightforward, I have found that communication about the idea of triangulation is really rather complex. Some authorities [illegible] purpose of that process.

> [Triangulation is] basic in ethnographic research. It is at the heart of ethnographic validity, testing one source against another to strip away alternative explanations and prove a hypothesis. (Fetterman, 1989, p. 89)

Mathison (1988) cautions that triangulation may not only confirm, it may also have a number of other useful purposes:

> In practice, triangulation as a strategy provides a rich and complex picture of some social phenomenon being studied, but rarely does it provide a clear path to a singular view of what is the case.
>
> Because of the predominance of the assumption that triangulation will result in a single valid proposition, we look for the convergence of evidence and miss what I see as the greater value in triangulating. More accurately, there are three outcomes that might result from a triangulation strategy ... convergence, inconsistency, and contradiction. (Mathison, 1988, p. 15)

In introducing this quotation, I used the word 'caution' deliberately. Mathison serves to alert us to the danger of throwing out useful information if we focus too tightly only on the purpose of finding convergent evidence. Inconsistencies and contradictions may help us to refine and revise our framework and findings; but they may be just what they seem: inconsistent and contradictory findings that must stand as they are and be reported as such.

Data that stand out like sore thumbs are sometimes called negative cases. Negative case analysis is the search for evidence that does not fit into our emergent findings and that leads to a re-examination of our findings. This will be discussed in Chapter 5 as a factor in final analysis. However, because this process helps to shape the emerging picture, it also has a place here. Negative case evidence can be extremely helpful in guiding qualitative researchers to '... make data more credible by reducing the number of exceptional cases....' (Lincoln and Guba, 1985, p. 312). It can help qualitative researchers shift their emerging understandings better to describe what they are studying and to be more certain that they have caught some of its essence.

Our colleague, Belén Matías, provides an example of how negative case analysis helped her to reshape her findings:

> In my beginning analysis of field logs and videotapes I had been struck by the number of choices the children had as they entered the classroom. Because of this, I developed a tentative social rule that read: *I [child] am in charge of entrance time.* As I continued to analyze data, this rule held almost to the end, until I realized that

> something contradictory had been happening. The teacher had begun ever so slowly to cut into the children's opportunities to make independent choices at entrance by moving teacher-directed academic activities progressively closer to the beginning of the school day. I did not like what I was seeing. I fought to justify and to keep my social rule intact until I had to face the facts of the matter and changed the social rule to: *The teacher can change my entrance routines*. This rule more accurately described what happened in that classroom.

Support Group: The Life Line

Support groups are so vital to every aspect of the 'doing' discussed in this chapter that they deserve special mention here. We described in Chapter 2 how support groups were established and some of the roles they played in the initial qualitative research forays. We return in Chapter 5 to the contributions of support groups during the final research stages. You may have concluded by now that we find support groups indispensable to qualitative researchers. We do. In the team's experience, it has been the support group that has most often been instrumental in helping its members to face possible bias and possibly painful insights with grace and empathy, and in ways that resulted in constructive moves forward. Support groups can consider each member's emerging findings, suggest alternate explanations, and act as auditors of the research process. While that is not all, it is certainly part of the process. There is nothing like being in the same boat and cheering on your fellow paddlers. We know, however, of a number of researchers who do creditable work without the assistance of a support group. Some people prefer this, and we salute them. Most do better in a support group. That has been true for the members of this writing team in our postdoctoral research as well.

Joan Giansante found the support group a comfortable place for airing gripes and frustrations as well as for testing new ideas:

> The support group provided a forum in which I could discuss worries, problems, and frustrations, including those brought on by my computer. It also gave me an opportunity to test out some of my hunches. The discussion about my participant's decision-making process is particularly memorable for me. Other members discussed what intuition meant to them, and consequently broadened my interpretation of it.
>
> Participation in a support group would be a very important consideration if I ever attempted ethnographic research again.

We found that the support group experience served as a model for the beginning researcher's ongoing scholarly work long after the class had

members to begin to establish their continuing support groups. Groups formed themselves on the basis of shared interests, congenial personalities, or geography. Often, the original support groups opted to stay together, although this meant wide travel and attention to a great span of research topics. In our experience, for many people the commonality of applying qualitative methodologies and group cohesiveness have been more important than doing research in a similar topic area. Teachers, artists, nurses, and people from many other academic interests have benefited from each other's vision and help. One of the pleasures in the support group comes from learning about research in other areas.

Support groups need to find their own rhythm, but one good plan is to meet every two to three weeks and to schedule one or two members to present their current work at each meeting. Many people arrange to give their material to the group members during a meeting before the scheduled discussion so that the material can be carefully considered before the next meeting begins. My support group arrived an hour early to find on-the-street parking spots. We then read the current papers in our cars until the spots became legal, and the meeting began. This, of course, was an adaptation to the complications of meeting in downtown Manhattan. The value of reading the material immediately before the meeting was that the content was fresh in everyone's mind. Further, we had a general understanding that we would each accomplish what we could in one hour. Since we were all working feverishly on our own projects, we respected and valued each other's time.

We called ourselves the 'Kitchen Group' because we met in an NYU room that had a stove and a small refrigerator. We lived up to our name by beginning each meeting with a light supper provided by two of us. Inspiration needs nourishment, and we didn't wish to be unkind to inspiration.

A real sense of family developed around that table. It was created by our united effort to fight a common enemy, which was sometimes our committee, sometimes our subjects, sometimes our families, sometimes our own flagging energy; it was created by our joy in each other's progress — we felt we would pull each other through, and we did; and it was created by our growing confidence in ourselves as ethnographers. Eating together was a time for catching up on the news of the week, for relaxing before the last big push of the day — critiquing each other's work — and for celebrating our pleasure in each other's company. Each of us put a little time and effort into making a pleasant meal, and it was easy to see that effort mirrored in the care we gave to each other's writings. Each support group will develop its own rhythms and rituals; dining together is one that ought to be considered.

The members of a support group can help each other in several key ways: offering emotional support, suggesting new points a view, and estab-

[illegible] While professors also offer emotional support, committee members cannot often achieve the level of intimacy that colleagues and fellow sufferers manage. It is important that the members of the group respect each other's work, and that most members like each other as well. Groups, though, can work even if not all parties like each other. Members must lift out of the level of personal relationships and focus on the research project itself. One of our colleagues compared the process of doing the dissertation to being on an emotional roller coaster. The support group can help the members smooth out the highs and lows. Members cheer each other on and have a stake in each other's progress. A member can try out new ideas in a supportive, non-judgmental forum; checking a novel idea with your group may be taking a much lower level of risk than trying that same idea on a committee member. Often members will be in different stages of dissertation refinement, and that can be a great comfort to someone who is just beginning the process.

Support group members need to be curious about each other's work. Sometimes a writing jam is relieved because a person in the group takes the outsider's point of view.

Nancy Montgomery did a study of how Jane, one of her freshman writing students, functioned within the structure of her classroom's cooperating writing groups. Nancy shares with us some of the pain and joy of being in a support group that asked her to re-examine her participant's behavior:

> . . . my support group helped me get outside myself as the teacher in the class and observe myself more objectively rather than staying inside my own perspective so much. Because of my investment as a participant in what I studied, it was easy to lose my neutral perspective. The support group helped me regain my observer's balance.
>
> If it had not been for the incredibly helpful, insightful, and sometimes upsetting feedback of my group leader and other group members, I might have missed important points for understanding and interpreting my data.

Another critical area of support is establishing deadlines. The Kitchen Group was particularly helpful at breaking down the enormity of the dissertation task into manageable bits. My internal goal changed from 'Write this dissertation in one year' to 'Describe Chuck's relationship with his family for next week'. The first goal paralyzed, the second goal energized. Further, each of us presented the written material orally at each deadline. The very act of saying things out loud is often a way to discover a different point of view, and the comments from the group sometimes challenge, sometimes corroborate the cherished ideas. The group has a way of keeping its members clear and honest. After having discussed the central issues of the dissertation with the

group for a year or more, the challenges of the final oral dissertation defense were manageable for me.

If at all possible, find a congenial group of strugglers, share good food and good ideas, and, as David Sternberg (1981) suggests in the title of his book, you will be able to complete your research and survive.

Postscript

In this memo I, Margot, would like to highlight and expand upon three themes that seem crucial to this chapter, indeed to the entire qualitative research endeavor.

Theme 1: Qualitative Research Is Powered by a Group of Disciplined Procedures That Must Be Studied, Practiced, Learned, and Relearned.

While it may seem anticlimactic to talk of this theme at the close of this chapter, I'd like to approach it with a slightly different twist. This theme speaks directly against a myth that is often proposed by people who either (1) have not studied the assumptions and methods of qualitative work, and/or (2) find such research unacceptable to their frame of values. The myth goes something like: 'Qualitative research is soft, unscientific, atheoretical, without substance. It is "touchy-feely" messing about, and at best seeks unsubstantiated opinion rather than facts. Anyone can do it'.

To a qualitative researcher such a myth could not be further from the truth. The myth is anathema to those of us who work with painstaking care: who hone our observational skills, who work as collaborators-in-research with the people whose lives we seek to describe, who engage in increasingly productive ethnographic interview techniques, who surmount the seemingly insurmountable tasks of in-process, recursive analysis, meaning-making, and reporting. While such a myth is anathema, it is also extremely powerful — powerful enough to jolt many advanced graduate students who suspect deep down — or further up — that qualitative study is easier than quantitative, and who find very soon upon engaging in study that qualitative work may be even more complex and difficult than other kinds of research.

What must be faced is that every research paradigm demands disciplined congruent methods. Easier or harder is not the issue when choosing which way to go. The issue lies in people's commitments, their personal styles, what sorts of assumptions they make about research. For qualitative researchers, their commitments include passion to document life in multi-layered, first-hand ways, deep social concern, trust in process, and interest in working with people as well as independently. Qualitative researchers accept in increasingly whole-hearted ways that they themselves create the boundaries of their research and that these evolve in response to

what they learn along the way. They delight in the fact that the boundaries of their research plan are begun but not ended before they commence studying. Concomitantly, these researchers depend on their own flexibility and humor in accepting the inevitable and sometimes discomfiting notion that things are not as they seemed when they were planned — even yesterday — and that change may be our only constant.

Some assumptions about research that qualitative researchers abide by are that they can never and should never attempt to remove themselves from what they are studying, that they are interested in documenting as much as possible of the whole of the phenomenon they study rather than its fragmented parts, that they will have more questions as they go along and at the conclusion, that they strive for insight and understanding rather than prediction and control.

These descriptions of some of the stances of qualitative researchers may sound high-handed and unobtainable. That is my problem when I attempt to describe in a linear way what is really a whole, a way of looking at life. Actually, many people who begin to learn about qualitative research experience a flash of understanding, a feeling of rightness. Ann called it a 'shock of recognition' in this chapter. Usually, for these people, anything else can be learned. They are the ones who come to understand, almost from the start, the discipline required of qualitative researchers. In a most human and humane way, they come through.

Theme 2: Qualitative Research Depends on the Researcher-as-Instrument

When all is said and done, the actuality of this theme is often both exhilarating and difficult to accept. In presenting the pros and cons of their work, qualitative researchers cannot point to the test, the sampling procedures, the statistical treatment, the outside expert. They can only point to themselves and to how they decided to sample, to treat data, to work with others, to confer with experts, to carry out their research, and to share their findings. This is so because they are their own most important instrument.

The measure of a successful ethnographic interview, for example, rests on the knowledge, sensitivity, and skills of the qualitative researcher. It is that person's skill to 'listen with the third ear', to let the quiet ride, to come in with just the right probe, and to understand and use judiciously the literature in the field that are at issue rather than following prescripted questions in a prescripted order. The sum total of what people are will shape them as qualitative research instruments. These researchers put themselves into the service of their search, analysis, and presentation.

Before all of this becomes too awesome, it's good to remember that, thank heavens, we need not be alone on the journey. This is true even though the researcher-as-instrument is, in a deep sense, responsible for both process and product. Just as instruments can be made better, sharper, more useful, so can researchers-as-instrument.

For the latter, however, it is each person's choice, and no one else's, to work, for example, in a support group, or to consult with a colleague, or to read for inspiration, or to take a rest away from everything smacking of qualitative research. It's a comfort to me to keep learning that there's a possibility that Margot-as-instrument really can get better with age.

Theme 3: Qualitative Research Honors Tacit Knowledge

In one sense, this theme relates to the previous one on researcher-as-instrument, but I feel it deserves its separate place. This statement emphasizes that people know a great deal from their own past and present experiences, from how their vision has been honed, from their evolving insights and hunches. Much of what we know is unspoken, inside us. These same people as qualitative researchers do not attempt to separate themselves from that they know tacitly or, for that matter, openly. Indeed, they use their tacit knowledge in important ways. They listen to their hunches. They attend to the seemingly unrelated sense of direction that pops into their heads at odd moments. They heed their own feelings that this log entry, not that one, carries relevant meaning.

It is essential for qualitative researchers to understand that such hunches, insights, directions do not arise out of nothing and that, often, they are the results generated from meaningful lived experience. What is equally important is that the process of working with such hunches, insights, directions must be one that attempts to lift the tacit from an unspoken to a voiced level; one that can be checked out in many, but not all, cases. Not every hunch is valid; not every insight is borne out by the data. Automaticities, even ones that bear much fruit, such as heeding one's tacit knowledge, are put to the test by qualitative researchers because their paradigm is characterized by a striving for increased reflection and awareness.

I am so often struck in ethnographic interviews by how people seem to depend on tacit knowledge as a road map. 'Why the shadow in his face at just that angle?' 'It felt right'. 'Why three codas for your symphony?' 'Because that's how I saw it!' 'Why the repetitions in this story?' 'I like it that way. It says something to me'. Sometimes, with probes, people give richer explanations, Often not. I wager, though, that in other ways people do examine their own hunches as they move through life, and that this is an essential aspect of their development.

The theme of tacit knowledge nudges at my tacit knowledge to remind me that there is a variety of ways of knowing: complex, not so easily pigeon-holed, important. In that message and its promise, it is a very hopeful theme.

Coda

There are plenty of other possible themes about Chapter 3, all of which are important. But I am aware that once the process of

studying any of these ideas is begun, the circles ripple outward to touch all the essentials. It hardly matters where one starts. That's one more fine thing about qualitative work. That is, the undergirding ideas that power this research paradigm are part of the seamless whole.

There is one more message to people who are considering becoming qualitative teachers-as-researchers. Not only the books and courses and lectures teach what needs to be known. The most vital part of that learning is one's self in action: one's self beginning to ask questions, to observe, to share with a support network, to take time, to try something out, to err, to study that again, and to become increasingly courageous and reflective. That's research with a professional kick! We continue this thought in Chapter 6.