Finally, in our experience, one of the joys that qualitative inquiry affords individual researchers is in the leeway to create and choose organizing sys-

Developing Themes

Up to this point we have discussed the making of categories as tools to help us organize our data. Categories, however, can serve another function, and that is to help us tease out the meaning of our findings as we consider the supporting evidence in each category and as we determine how categories may be linked. This section about themes and the next one about vignettes/constructs describes how categories served researchers in broader meaning-making and in how the results were communicated.

One widely used approach to final analysis is the search for themes. A theme can be defined as a statement of meaning that (1) runs through all or most of the pertinent data, or (2) one in the minority that carries heavy emotional or factual impact (Ely, 1984). It can be thought of as the researcher's inferred statement that highlights explicit or implied attitudes toward life, behavior, or understandings of a person, persons, or culture. Many of us first became aware of the concept of theme as students of literature. Here we became accustomed to analyzing the 'culture' of a novel by looking for the underlying ideas about human existence with which it is concerned. When expressing the themes of literary works, we often use phrases or even single words: *The Hobbit* is a 'quest' story; it celebrates the strengths and values and decencies of ordinary people. In qualitative analysis it is customary to express themes in the form of a statement.

Margot used a thematic analysis to present her findings gleaned from ethnographic interviews of young people from a culture of poverty who appeared to be 'making it' in *Beating the Odds*, (1984). Here is her description of the process:

> I am heavily influenced by the work of Bussis, Chittenden, Amarel, and Carini (1978) in how I develop themes. In brief outline, this is the process I apply:
>
> 1 Study and re-study the raw data to develop detailed, intimate knowledge.
> 2 Note initial impressions.
> 3 List tentative categories.
> 4 Refine categories by examining the results of steps 2 and 3 and returning to the entire database of step 1.
> 5 Group data under the still-tentative categories and revise categories if needed.

6 Select verbatim narrative to link the raw data to the categories.
7 Study results of step 6 and revise if needed.
8 Write theme statements for each participant from my best attempt to speak from her/his point of view by linking data in and across categories.
9 Integrate findings about each person.
10 Compare findings for commonalities or patterns, differences, and unique happenings.

Quite frequently in qualitative work the categories that were created to organize data help the researcher to discover themes by highlighting some relation between them. This is certainly true in my case. For example, in the study, out of the many categories I established, were these three:

José's relationship to mother (Relm).
José's planning strategies (Pl).
José's financial goals (Fin).

After studying the supporting data for these categories, I wrote the following themes as if José were talking:

> My mother supports me and helps me to clarify plans for the future.
>
> I sometimes give up immediate positive experiences for what I feel I must do to establish later financial security.

These themes, among others, held strongly for the data about José.

Could I have established the themes without first building a set of categories? Some people might do that. But I feel most grounded in the data when I progress from category to theme. Could other researchers have discovered different categories and themes from the same data? Yes. And this would probably happen. The important process here is to be able to explain one's reasoning for whatever one created. On the other side of the picture, however, is the fact that I am often stunned by how similar the analytic categories and themes are when an entire group of people — a class — is given the same field log pages to analyze. That speaks to an aspect of the topic of trustworthiness, to be discussed later.

Teri writes about finding her themes in the following ways:

> My system of creating themes might be considered a 'bottom up' system. I first divided my transcripts into categories. For example, whenever interviewees would discuss interactions with fellow officers, I would put in the code word 'relationships' on the side margin. With the help of a computer program, I then put all the material coded with the word 'relationships' into a separate file. 'Relationships' became a category. I initially had 31 such categories. When the categories were examined for overlap, redundancy, and

> significance, they were compressed into seven final categories that were judged to encompass the material sufficiently.
>
> Within each category, I reviewed the data for statements that were particularly revealing or expressive or somehow stood out as potential themes. I listed each such statement on a separate index card. (I color coded my index cards — for each different category I had a different combination of colors for the ink and the card.) I grouped together statements that were similar or related. For example, one woman stated,
>
> > We were so visible — we had to outperform; we weren't supposed to make mistakes; we were not supposed to fail.
>
> Another woman stated,
>
> > The propaganda value of one woman screwing up is incredible — by that night the whole borough will know and ... reinforce the suspicion that you're no good.
>
> These two quotes were written on one index card, along with several similar ones sprinkled throughout the transcripts. I wrote the heading on the top of the card, 'Because I'm so visible, I feel heightened pressure not to screw up'. I wrote such statements in first person in order to capture the phenomenological quality — these themes describe women's experience of being police officers — of the research. I discovered that the majority of women had made a related statement, and based on the context in which the statement was made, I assessed that they felt strongly about it. I had defined major themes as those that apply to more than half of the interviewees. I therefore defined this statement as a major theme about 'Relationships'.

Belén Matías analyzed teacher and student directives in a Puerto Rican pre-school setting that seemed to be at work in 'getting things done' in that particular classroom. Here are a few of the thematic social rules that she derived from the data in her categories. They are presented as though they were spoken by the children:

> We must bid for a turn to speak one at a time and listen to others quietly during a presentation of a material or while the teacher gives instructions.
>
> When the teacher asks questions during circle time, more than one can answer at the same time.
>
> Most often I am in charge of what I do in the time of entrance/transition to work.

The presentation of each of the social rules was bolstered by data from videotaped and in-person observations as well as information from teacher interviews over one year.

Developing Vignettes/Constructs

During the writing of my dissertation (1986) about the play styles of four kindergartners I developed vignettes about each child. I called these vignettes 'constructs'.

> A construct is an inferred soliloquy based on the content of repeated observation and an interpretive composite of one child's seemingly characteristic thought and behavior. The construct contains statements which are considered central to the way the child perceives his or her experience during a play period. (p. 22)

I used a construct to introduce the readers to the child and to foreshadow trends in the information about each child that followed next. This is Chris's introductory construct:

> I'm looking at these black droppings of this caterpillar I brought into school. I've been doing this for about twenty minutes, and I suppose there are other things going on in the classroom, but I haven't noticed. I'm really intensely into doing this and I guess I haven't moved much. Some people might say the droppings mean the bug went to the bathroom, but I've decided they are eggs, and I might flick one in your eye! I could even make them tiny bullets in my stick gun! What do you think of that? In fact, I think I'll speed across the room to the class wagon, which is mine most of the time, and set up my gun. A lot of kids follow me, and I'll let them see the droppings. I want them to feel them, too. Boy, they feel mushy. After that I'm off on my own to watch for enemies from my wagon. I'm smiling, but those kids bother me sometimes.
>
> When I get home from school, I like to go off on my own and really work at making all the stuff around my house into things I can use for building and fighting. It's okay sometimes to have kids around while I'm doing this. I like Alice, in fact I love her. She's a girl and she does girl things, but she's impressed with the neat things I do. Mrs Garner is pretty impressed too, but she's harder to shock than my mom. I told Mrs Garner I would blow her butt off! Funny, huh?! I did something even better. I took my pants off right in front of her. Boy, I'll bet she was shocked! Course there's Jill, my sister, and Alice's sister Nicola. They get me crazy. I have to get crazy to get my way in things I'm doing. Nicola is really stupid even if she is Alice's sister. When I do things my way, I feel terrific. My dad is a good person to do things with; he's like me and he understands.

The following vignette was developed by Margot from her category files in a study of a project to serve teenagers considered homeless, or in danger of being so, jobless, and probable high school dropouts.

Introducing Trina: My Life Is So Dangerous

I'm real shy and I can't speak right. What's that word? Oh, yes! I stutter. I just feel [illegible] happening. Maybe it's because I just came from Haiti a few months back. Maybe it's because I got a big problem with my mother and I can't seem to pay much attention to anything else.

I'm seventeen and I could have stayed in Haiti. Why did she act like she wanted me to come?

My mother don't like me to stay into her house, I don't know why. She only likes my little brother and she doesn't like half of us (8 children). She's just mean. Especially to me. My mother, she wants to put me out.

When I got to New York I went to Catholic school for a few weeks and then to a public school. The day my brother Joe brought me to this high school, I had a problem ... with my mother and Amy [Project Supervisor] brought me here to the project office. That was a couple of weeks ago and it's a good thing too. Amy and Dan [Project Supervisor] try to help me out. Sometimes if I'm hungry, he [Dan] feeds me. He give me the money to my Aunt and that's because my mother don't feed me. He wanted me to get a letter from my mother. A letter to put me in my own house, to live by myself. And my mother did not want to give it to me.

I come down to talk with Dan every day. He always pays attention to us. To the students. But I still have trouble in classes. These kids bother me in my class.... I don't want to leave this school too. I want to finish school here. I've got a lot of credits to go. Dan will help. I hope other people can help me with school.

Sometimes I dream of what I'll be when I grow up. I want to be a fashion model. But, you have to go to a training for that and you've got to have money too, to get any. I don't know how I'll get the money. I used to work but now I can't.

You want to know what I'm going to do with my life? I don't know. My life is so dangerous. (Ely, 1989, pp. 59–60)

You may have noticed that often these themes, social rules, and vignettes/constructs are stated in the first person. When possible, they include the actual words of the participant(s). Otherwise, they are distilled from the data in as close a likeness as possible of the participant's mode of expression. The intention is to present in miniature the essence of what the researcher has seen and heard over time. Not every researcher opts to use first person, although we are of the mind that this brings readers closer to the people who were studied. That is a personal choice. In the final chapter, you will notice that Margaret uses thematic statements in third person to introduce the topics she discusses.

It stands to reason that themes, social rules, and constructs/vignettes do not stand alone. They are devices that are established through analysis and offered to provide meaning, cohesion, and color to the presentation. They

of overabstracting by anchoring the findings firmly in the field that gave rise to them. Other presentation devices that have been developed from the analysis of qualitative researchers are discussed as part of making the story (pp. 167–75).

Including Numbers

One bane of my qualitative researcher existence is the assumption held by some that this research must be number-free; that it is not appropriate to distill numbers, and certainly not appropriate to apply statistical treatment. Ostensibly, this assumption is laid to rest by the availability of at least one volume about qualitative data analysis that involves among other aspects, those authors' versions of how numerical data from qualitative research may be compiled, treated, and presented (Miles and Huberman, 1984). So, why this assumption?

The answer may lie in several possibilities — for a start. First, some people seem to believe that the difference between positivist and naturalistic research lies in the absence of numerical data in the work of the latter persuasion. This group includes some beginning doctoral students. A second possibility that may give rise to the assumption is the fact that many qualitative reports do not include visible numbers.

> Many researchers who write case studies use qualitative data because they believe them to be richer, more insightful, and more flexible than quantitative data. They believe that the meaning of an event is more likely to be caught in the qualitative net than on the quantitative hook (US General Accounting Office, 1987, p. 55)

Many researchers, however, do include numbers. For example, in my study on children's play styles, I judged it informative to include a rather complex table that presented numerical results about each style category as observed over time. I presented frequency counts in order to compare quantitative loadings in play-style categories, as well as consistencies/inconsistencies over observations and contexts. These data were also used to produce a quantitative play-style category profile for each child and for the entire group. While I chose not to do further statistical treatment, the numerical data about play style and contexts were such that I might well have done so had I considered it useful in the spirit of my work. Researchers who do semantic analysis often present more complex statistical treatments. While they may at times include numbers, 'Phenomenology, hermeneutics, psychological case studies, anthropology, critical sociology, and linguistics all have long traditions of non-numerical research' (Tesch, 1987, p. 3).

On perhaps a more important level, every category, every theme, every finding, whatever its form, arises from the fact that it exists in the data and as [illegible] even though the researcher may choose not to do so. For example, in this book we hold that a theme may be established (1) because it appeared many times and/or for the majority of people who were studied, or (2) it appeared once or a very few times but carried important analytical impact. (Margot is known to say that she has to be slapped hard only once to know something meaningful has happened.) Other researchers present only majority themes. In both cases, themes arise because the support for them is evident to the analyzer. The responsibility of a qualitative researcher in the final report is to bring public spotlight on her/his decision-making process in establishing findings. When this holds, readers have the information with which to judge for themselves whether the findings are reasonable.

Return to Trustworthiness

We pondered at some length where to place this section. Discussions of the 'quality control' of qualitative research traditionally occur toward the end of books and articles on research methodologies. It is difficult to choose the correct place for talking about trustworthiness because, as we emphasized in previous chapters, a qualitative researcher pays continuous, recursive, and, we dare say, excruciating attention to being trustworthy. This concern begins before the first word is written and does not end until the research is completed. The quest is to make the research project credible, produce results that can be trusted, and establish findings that are, to use Lincoln and Guba's phrase, 'worth paying attention to' (1985, p. 290). We have, as you see, negotiated a solution by presenting issues about trustworthiness in two places: in Chapter 3 and here.

The topics in this section deal with aspects of trustworthiness that seem particularly pertinent during the final stages of a research project. Here, as throughout the book, we focus on the facets of establishing credibility because, to us, this is the bedrock of trustworthiness.

Back to the Field

You've just left the field! Why, then, this section about returning? Because, in the final analysis process, so many people experience the need to check again, to ask another question, to jump back in for a very specific purpose. Sometimes the reason to return to the field lies in an anomaly in the data. Sometimes just a bit more information is needed. Sometimes it seems vital to

observed. Sometimes it is the nagging thought that one was not as fair as possible in collecting data. Here Beverly Rosenthal, who did a pilot study on the impact of computers on writing in a classroom, tells why she decided to return to the field after she thought she was through:

> Trustworthiness became an even greater issue when it came time to analyze the data and report the findings. My concern was that perhaps I had overemphasized the negative and had not seen enough of the positive aspects of the implementation. I was struggling for a balanced and fair perspective.
>
> The observations confirmed as well as conflicted with my personal experiences, and with my reading and research experiences. For example, I had uncritically accepted the notion that the networked computers would automatically and conveniently facilitate peer response. The literature, course announcements, the program director, and the computer coordinator presented enthusiastic claims for the technology. However, after observing several classes, it became apparent, much to my consternation, that there was a discrepancy between the expected uses and outcomes and the real uses and outcomes of implementing the technology.
>
> I had not considered that in reality the use of the networked computers was fraught with difficulties. I was compelled to find out more. I returned to a critical examination of the literature on computers and composition, the factors affecting composition instruction and innovation and organizational change theory. I also spent some time critically reflecting upon myself as researcher. Was it consternation or interest I should have been expressing when I found the discrepancy between the expected and real?
>
> When I returned to the field, this reflection helped me to discover some possible factors that related to the discrepancies: lack of specific instructional and implementation guidelines, inadequate software, incomplete installation of necessary hardware, no teacher training in the technology, high turnover of teachers, no full-time faculty devoted to teaching the course, no evaluation procedures, inadequate student training, inadequate access to the technology for both student and instructor, and inadequate resources allocated to the program. Returning to the field was not only satisfying but a relief.

Beverly's account illuminates one fact we all know only too well: the broad and general plans we laid at the start in order to establish credibility will almost certainly be redefined and augmented as the real thing comes along. As with every other phase of qualitative research, so even more here, the researcher struggles with the demands of emergent design. While Beverly's return to the field occurred during her final analysis, this can and often does happen at any time in the research process. It is to her credit, however, that

she did not decide to ignore the whispers of her own voice at that particular point near the very end of her work.

When Is Enough Enough?

One criterion for being credible is to engage in collecting data for such duration and in such ways that these are sufficient to help us understand what we set out to study. What 'sufficient' means is often perplexing. Near the completion of my analyses. I began to hyperventilate with fears that I had not collected all the necessary data. I returned to the field briefly to continue observing. In less than a week, I experienced a weary sense of relief, knowing my data were sufficient because they were repeating themselves.

Margot (Ely *et al.*, 1989, pp. 30–1) tells her own story:

> My sabbatical project was to work on a piece of research I titled *Beating the Odds: An Ethnographic Interview Study of Young Adults from the Culture of Poverty*. After a time, I left for an island hideaway laden with the voluminous transcript of the eight interviews that had been carried out. My task was to analyze. Well, I did. I categorized, cut, pasted, reorganized, categorized again, in cycles, while the island breezes blew my little pieces of paper merrily around the room. Yes, I am one of those strange folks who need to cut and paste. I was bewitched! No beach, no time off, no Sunday brunches, just trying to make sense. After two and a half months I arrived at some analytical insights that seemed reasonable. Well, almost. I went about with this terrible feeling that maybe I had missed something. Suppose I had not asked the right questions? Suppose I should have probed more? Less? Suppose the people were putting me on? Suppose they were so stunned by the lengths of the transcripts I had shared with them that they carelessly agreed that what was there was the story as they lived it?
>
> After worrying about this for some days, I packed up my by now even more huge pile of notes, papers, and files and flew back home. I got in touch with the eight people and did a lengthy follow-up interview with each. Again, I spent months analyzing, cutting, pasting. Same results! Not one new category. Not one new theme. A few more examples.
>
> What to learn from all this? I don't quite know. Perhaps to be a bit more confident when I follow what I consider decent methodology? But then again, suppose some really new, terrific findings had come from the second round? Perhaps to exult that the findings were so similar? Perhaps to be leery about just that? Perhaps to learn to let go, in the knowledge that ethnographic research often means letting go to come back? Certainly, I now see things in a different light when I work with doctoral students who grapple with the demons of 'When is enough enough?'

It seems that every qualitative researcher fights those demons. Some of us cling hopefully to the criterion of redundancy; of trusting that when data repeat themselves, it is time to stop. But then, there is always the spectre of another interesting person to interview, another observation that may unearth shatteringly different and useful information, another event to experience that will just clinch things beautifully. May you fight your demons successfully. Let us know please.

Sometimes, returning to the field has less to do with establishing credibility than with providing a very seductive strategy to avoid what we feel is that awesome final analysis and write-up. Some of us return to the field once too often, and we know it. Others labor mightily to fulfill the impossible dream of 'getting it all'. Somewhere between 'barely sufficient' and 'any number of cycles of the recursive qualitative research process' lies that place where enough may not be all, but enough is enough. Finding that place and then having the confidence and skill to define it and to use it well are key to qualitative researchers. That place can be found.

It is far easier but perhaps more unsettling to know when enough is *not* enough. Essentially, this is a personal conviction which transcends time limits, family concerns, job deadlines, and plans for vacation. I believe we've all been through that also. Even when it looks sufficient to all the relatives who have wondered when you'll ever be finished, even when the support group says to stop, even when your dissertation committee argues that this is it, there are instances, small as they may be, when the researcher knows well that some thing or things need to be done. If this is so, and if you can stand it, do what needs doing. You've come this far and you live with yourself.

Negative Case Analysis

Surely this process is not new at this stage of the game. The search for disconfirming evidence to help us check our in-progress conclusions, to reconceptualize our categories and themes, and/or to point us to minority subthemes, has been part and parcel of the ongoing analysis and collection up to this point. Sometimes, however, unearthing evidence that does not support the emerging findings and deciding how to handle that situation take on special meaning at final analysis time.

Ruth Alperson shares how her findings about 4-year-old Mary served to broaden her outlook and, in the end, reshape her vision about humor in the final analysis phase:

> I feel that I have focused on Mary here because I have learned most about humor, in a way, from her. This is ironic, for I concluded, at

> first, that I would probably get very little from Mary that was humorous. What happened, in final analysis, however, was that my very narrow appraisal of what belonged in the category of the humorous became transformed and broadened by what I saw, unexpectedly, within the workings of Mary's various behaviors. Mary finally confounded and corrected my assumptions.
>
> Although I had concluded otherwise, I learned very near the end of my analysis that Mary, who had a nervous demeanor, and who I would characterize as a tense, worried child, had her own brand of humor, which is not particularly funny, but I would term it humor, nonetheless. Kay, the assistant teacher, called Mary's joking 'pathetic', and described one instance of this in her interview with me. Mary was particularly anxious that her babysitter be on time to pick her up from school. Lia was always on time, and very indulgent with Mary, but Mary worried nonetheless. When Lia appeared at the school, Mary liked to 'tease' her. Kay said that one day Mary hid from Lia after Lia had started to leave with her. Lia could not find Mary for a short time, and really became concerned. Mary popped out of her hiding place, laughing heartily. I felt, on hearing this, that Mary was setting up a situation in which she could vicariously experience her own fear of not finding Lia, of being left alone, through Lia. I categorized this act as humorous, as in black comedy, because in Mary's eyes, it seemed she was, ostensibly, playing a joke, and because she laughed so at its conclusion. It seemed like humor used as a defense, or a cover; it was funniness creating its own flip-side, fear in the guise of funny. I wanted to check this out so I went back to observe Marry. This aspect of her humor reappeared with sufficient impact to allow me to have some faith about my insight.

In Ruth's example, her finding served both to challenge an emerging conclusion about Mary and to broaden the meaning of a category. Teri recounts how she discovered a negative case theme and presented it to stand separately, in juxtaposition to a major theme.

> In my dissertation on women police officers, I included a category entitled 'Behind the Blue Wall: Professional Relationships within the Precinct'. Within that category, I discussed several themes that pertained to the interviewees' experience of professional relationships between male and female officers. Some of these themes highlighted: the pervasive undercurrent of hostility that women experience as directed towards them when they are promoted or given a good assignment; increased pressure not to make mistakes because of their small percentage in the department and heightened visibility; the conflict they feel between trying to integrate into a man's world while not wanting to become 'one of the boys'.
>
> One theme was very strong for the majority of women. This was, 'I found a home here. It's family'. This theme describes their

perception that, despite the obvious ambivalence that characterizes many of their professional relationships with male officers, they feel that they have been accepted into the police family. Interestingly, however, this theme applied to all the women but one. Delores felt strongly that she had never been accepted into the police family. Moreover, she felt that no women were, and that those who felt they had been accepted were deluding themselves:

> You're an anomaly. And there is no real place for you. I think all the girls know it at a certain level.
>
> No matter how much you fit in, *per se*, you don't — it's superficial. Even the girls that think they fit in, they don't.
>
> Some of the men are decent human beings — intelligent, articulate, with something to bring to your daily professional life. But the vast majority aren't. So you're constantly out of synch.

Her acute sensitivity to sexism in the police force, with a strong sense of self-worth and perception that she was never duly credited for her performance, created for her an overriding bitterness and loneliness that has colored the whole tenor of her police experience. She was the only woman I interviewed who clearly felt her career choice was a grave mistake. She alone stated she would never do it agin. She counted the days towards retirement.

In-depth analysis of Delores' interviews revealed that her experience provides an exception to the theme, 'I found a home here. It's family'. I therefore constructed a second theme that would describe her unique experience, and include it here as an example of the result of negative case analysis.

Some researchers (Goetz and LeCompte, 1984) distinguish between negative case — an exception to the emerging rule — and discrepant case — a variation of the emerging rule. A negative case refutes a construct while a discrepant case refines a construct (pp. 188–9). In this light, Teri's example is about a negative case and Ruth's is about a discrepant one. Most often in our work, the line between negative and discrepant blurs. We worry less about what to name the 'differing instances' than to use them to shape and refine the findings. The essential idea is that qualitative researchers go through an active process of confirmation and are willing to change their minds about findings when the data so dictate.

Peer Involvement in the Research Process

Peers can play several vital roles in helping each other to be credible. We have discussed the formation and function of peer support groups in Chapter 2, and their important contributions during the research process in Chapter 3.

Here we present some ideas about peer involvement during the final stage of the process.

Peer Support Groups. Lincoln and Guba (1985) suggest peer debriefing as one technique for maintaining credibility:

> . . . the process helps keep the inquirer 'honest', exposing him or her to searching questions by an experienced protagonist doing his or her best to play the devil's advocate. (p. 308)

With much less emphasis on playing devil's advocate, we call the vehicle of that process peer support groups. In our experience, support groups have been extremely important in helping people establish and maintain credibility. Belén Matías shares one way in which her support group served to force her to face the need to check her findings with data from yet another source:

> I had promised in my proposal that my major data would come from a micro-analysis of some classroom communication events. Actually, I did everything but that. The observational data were so fascinating, so magnetic that I was drawn — not against my will — to describing classroom life first in very broad strokes. Next I established and supported a series of social rules from and with the data. These gave me ground to make some tentative generalizations about the wielding of power in that classroom. I was planning to write three case studies to illustrate how the social rules worked themselves out in the school lives of individual children when a member of my support group put an end to that. 'What if the results of your micro-analysis go against the statements your social rules are making? Suppose you can't write the case studies in the way you now envision? I think you need to get those micro-analysis data now!' I saw the sense to that and what's more, I feel that without my support group I would have plowed ahead with my own plan, much to my disadvantage.

In my own dissertation fieldwork, I did not have a support group. It was a lonely experience not to share with peers the extraordinary happenings I had documented. I had to find other ways to check my insights about the children who had provided me with such rich details. Particularly in the final stages, my doctoral committee came the closest to being a support group, but a doctoral committee is quite different from a group of peers who are going through the same thing. I missed that.

Nancy Biederman sees similarities in the ways professional support systems serve therapists and ethnographers:

> As a therapist, it is imperative to achieve the equivalent of Guba and Lincoln's trustworthiness. How do you check yourself as an accu-

rate instrument in the understanding and exploration of someone else's reality? In much the same way that ethnographers do. I have been in individual and/or group supervision consistently throughout my practice. It is here that one triangulates — checks feelings and perceptions, gets feedback and fresh perspectives. Support groups and meetings with experienced ethnographers can function in much the same way. Clients, too, can be a source of verification.

Several experiences in my support group, and meetings with Margot, very much remind me of the supervisory process. Both kinds of work are in many ways introspective and solitary. It felt tremendously helpful and reassuring to get other people's perspectives and support. I remember feeling frustrated and despairing about my project and, after discussing it, taking heart and feeling hopeful. This was also very true, especially in my first years as a therapist, in the supervisory process.

Nancy's contribution highlights the fact that some qualitative research processes are akin to those in many human service professions. Could this help to explain why those of us who wrote this book feel so related to the qualitative paradigm?

We have found the peer support network exceedingly useful in establishing credibility. It is necessary, however, to remember that a group can be just as blind as the researcher alone, unless the members strive to counter that. Janis (1973) writes about this in his provocative look at 'groupthink' during what he labels some of our historical fiascos (p. 407); those of the Bay of Pigs invasion, Pearl Harbor and escalation in Vietnam. Janis reminds us that a group may value uniformity more than variety. Indeed, group members may find it much easier to agree, to be nice, than to make possibly upsetting waves or to take different stands. Members of some groups strive more for social acceptance than they do to grapple with the tasks for which the group was formed.

This danger sounded, this team has found qualitative research support groups to be amazingly and refreshingly helpful, supportive, forthright, and dedicated. Support groups have differed when it was easier not to do so, but the ways in which they have differed and faced other difficult moments with each other have been constructive down the line. One of the most potent reasons for such cohesiveness has been the dedication of the members to help each other produce the most substantial and credible work possible and to do this both through their own eyes and through the imagined eyes of others who would read and assess the final product.

Peer-Checking. A few qualitative researchers believe that one aspect of being credible is to obtain statistical agreement about some of their findings with at least one other person. Often this person is a member of the peer

support group or a colleague on the job. The process is called establishing inter-rater reliability, a phrase that is borrowed from the traditional research paradigm. All five of us have established inter-rater reliability at various points in our research careers. We do not do so now.

When I was conducting my ethnographic study, I had not quite loosened my bonds with the experimental research community, and was fearful that my research might not be looked upon as acceptable if someone else couldn't duplicate my findings. For this reason, I established inter-rater reliability in my analysis. In this case, this meant that a second observer, the teacher's aide in another classroom, would join me in observing the four children. During four practice observations, this person was instructed in identifying specific play-style features and providing descriptions for each feature observed. Immediately following each practice observation, I met with her to discuss the features that were identified and described by me in the same sessions. We also viewed videotapes together and wrote and compared field notes. Following the training period, I asked this colleague to observe each child for a half-hour once a month and to keep a written record of the observations. As we had done during the training period, we met after each observation and compared the play-styles we identified in our written records. We reached a 98 per cent level of agreement for the four months in which this was done.

Another sort of peer-checking is that which occurs in the support group. One example is that one member shares sections of a field log, asks people to create categories and even themes, and then compared the individual results with those that the researcher had established. If this last process does not result in substantial agreement, it is the researcher's job to seek understanding about the analytical reasoning of the group members and either agree of disagree, accept or not accept the ideas proffered.

It is probably evident that I find this last example of checking emerging results most sensible in qualitative research. In fact, all five of us on this writing team have given up establishing statistical inter-rater reliability via the training-retraining model because it does not serve our understanding of being credible in naturalistic research. However, we all endorse the idea of checking and honing our findings with a support group.

Within the range of qualitative researchers, there are groups who do not depend on any sort of confirmation by and with others. Those who do reflective phenomenology and heuristic research are two such groups (Tesch, 1990, p. 70). They hold the position that analysis is their idiosyncratic creation which can only be internally confirmed by them, and if they do a good job of communicating their reasoning, perhaps their results can be understood and even supported by others. This is a good example of the fact that the epistemological underpinnings of one's research shape every part of the method, and that includes how one establishes and reports the findings.

Member-Checking

There is no question but that we are wholehearted about another aspect of being credible: checking our interpretations periodically with the very people we are studying. This is called member-checking by Lincoln and Guba (1985), and it is, in their view, at the heart of establishing credibility:

> Credibility is a trustworthiness criterion that is satisfied when source respondents [like people who provided the information] agree to honor the reconstructions; that fact should also satisfy the consumer. (p. 329)

During the final months of my study I interviewed parents, siblings, and other teachers of the children I studied. As part of this process, I also shared some of my emerging findings about each child's play-style for their consideration. I felt I was at this time closer to recognizing the consistent patterns of each child's style, and more equipped with knowledge that allowed me to elicit detailed descriptions and comparisons from the informants by introducing explicit examples of what had happened. Obtaining feedback about my findings in these interviews helped me to establish credibility, but it also deepened and substantiated data gathered in other ways.

Because of the nature of my study, I did not verbally check findings with the 5-year-old children whose play styles I documented. However, when it is appropriate, most of our colleagues check directly. Matt Foley shared his findings at several points with the young people whose coping strategies in slum streets were the focus of his study. Don McGuire checked his categories and supporting evidence with his participants, young adults who had attempted suicide during ages 5 through 9. In both of these cases, the people who were studied agreed with the findings and interpretations. In one case, several people gave more descriptive examples for some of the categories. In the other, two young adults provided more detailed specifics for the case study that was written to describe their lives.

Steve Spitz's thesis was about the experience of fathers who were major caregivers to their children. Steve writes:

> As part of member-checking, I sent profiles that had been developed to the participants for their review. Barry received his just prior to Father's Day. He called me to tell me that his profile was the best Father's Day present he had ever received and that he had read it to his entire family. Mark wrote back that he had showed the profile to his wife and parents who found it to be quite moving.

Not every case of member-checking is a constructive event, however. The outcome depends on a variety of factors that include what is studied,

what data are provided as feedback for the checking, the relations of the researcher to the people who are studied, and the context in which the [illegible] provided a rich description of just such an interplay of factors when he attempted to check with the faculty and administration of a British comprehensive school in which he carried out a sociological study of mixed ability grouping (Burgess, 1984, pp. 65–96). Ball, who had been a participant observer in this school for three years, provided in-progress chapters as well as a final draft to the people who were involved. He found that '... many of the staff had apparently read my chapter solely in terms of what it had to say about them in their subject' (p. 86). Further, he discovered that despite his numerous explanations, the people involved '... clearly had little idea of what sociological research was and the sort of outcomes it might produce' (p. 87). In this case, member-checking was made even more thorny because several administrators were put off by the possibility that the school would somehow be publicly recognized, judged, and found wanting. At that point, he was met with a challenge:

> The head of science continued with queries about the 'scientific' adequacy of my work. He asked what 'measures' I had used and what 'tests of significance' were employed, adding '... well, I think it comes back to what I said originally. Your sample is so tiny that you can't hope to make comments like that'. The remainder of the session consisted of my attempts to explain the epistemological basis of ethnographic research and the conflicts between positivist and interpretive sociologies. The head of science remained quite unconvinced by my arguments and finally dismissed my account as 'honestly, absolute drivel'. (p. 88)

Ball pointed out that at times the political realities are such that arriving at consensus may not be in the real interest of participants. At such times, member-checking may well fail in its ideal purpose and alternate solutions for establishing credibility must be relied upon.

We have had no such troubles with member-checking. Guba and Lincoln (1989) add:

> We should note here that in no research or evaluation project we have ever worked on or conducted has any participant or stakeholder asked to have a direct quotation removed from the final case study — even though it has become clear that others in the setting can identify its 'voice'. Few of our graduate students ever have to remove quotations from their dissertations, either, in the member-check process. (p. 133)

It is entirely possible that member-checking will create problems in some studies. Nevertheless, it seems to us that the effort to check and to discuss

can do to inform and to establish collegiality.

Making the Story

You're ready to tell it. Your job here is to create a text in which the person or persons you have learned about come to life. This means that you have a tremendous responsibility to be true to their meanings. The written presentation is of crucial importance: in a deep sense, what one writes *is* what happened and what was learned. The presentation, not what is held privately in the head and heart of the researcher, is what exists about the research. What one writes can make what was studied tangible, compelling, credible, or flat, uninteresting, questionable.

Nervous? I'm with you. I suddenly have an awful urge to clean the fingerprints I just noticed on the wall above my computer, but delving right into writing is the better action. The first draft is not the final one after all. This is a time of individual and lonely expression. Your support group and other sources of inspiration help, but in the end only you can put it all together and expose the meaning. I lost ten pounds, lived in a single room so I would not be distracted, and thanked God I had the summer off from my elementary teaching in order to give my all to the perilous journey of organizing the chaos, final analysis, and writing.

Little demons came to me, too. I would be working steadily, finally, and up would pop one of my demons with a question: Are you absolutely sure you have the facts as they existed for the children you observed? Other questions: Are you positive that all your data are here in this room? Suppose the house burns down? Why didn't I transfer everything to a computer instead of insisting I couldn't afford one and could depend more on my typewriter? One summer did not bring me to the completion of organizing the chaos, much less the final analyses, but my room remained spotless and I remained contentedly hopeful.

Although writing the completed account is usually discussed as a culminating activity, as we are doing here, there are those who advise researchers to begin written drafts while still in the field just as analysis is begun during these early stages. Wolcott (1988, p. 200) draws on his own experience in suggesting strategies for beginning the narrative:

> It is splendid indeed if one is able to follow the advice to prepare a first draft while fieldwork is still in progress. In attempting to set down in writing what you understand, you become most acutely aware of what you do *not*

> understand and can recognize 'gaps' in the data while time remains to make [illegible] for drafting a full account, one can nonetheless begin to 'think' in chapters, sections, or expanded outlines, and thus keep tuned to the difficult task sometimes dismissed as simply 'writing up one's notes'.

Wolcott goes on to suggest that 'wherever and whenever the task of writing begins ... begin at a relatively easy place'. Most of us and many colleagues have started with a description of our entry into the field and a 'grand tour' (Spradley, 1980) account of our fieldwork as a whole. Margaret, whose observations spanned two school years, used the summer between them to start writing her 'overview' chapter and to work on the presentation of one approach to classroom interactions. So, if we are fortunate, we will begin with chunks of writing already in draft form. But however we begin, it is with the purpose of shaping a complete descriptive account that meshes our fieldwork with the interpretations that have emerged from our analyses.

Finding a Voice

Among our basic tasks in making a story are cultivating personal style and finding a voice. These are choices that we must make as writers and ideally we do so consciously. When we undertake academic writing, however, we often let that choice be made for us. 'Academics-in-training ... adopt what they see around them, the style professional journal articles and books are written in, as an appropriate signal of guild membership' (Becker, 1986, pp. 39–40).

Becker tells the story of one of his doctoral students whose initial drafts of dissertation chapters were written in the academic conventions she considered 'classy', what we sometimes term 'dissertationese'. When Becker pressed her to remove 'redundancy and academic flourish', she wrote in reply:

> While I personally find scholarly writing boring and prefer to spend my time reading novels, academic elitism is a part of every graduate student's socialization. I mean that academic writing is not English but is written in a shorthand that only members of the profession can decipher.... I think it is a way to maintain group boundaries of elitism.... Ideas are supposed to be written in such a fashion that they are difficult for untrained people to understand. This is scholarly writing. And if you want to be a scholar, you need to learn to reproduce this way of writing.

We wish we could give more space to Becker's student and her story in our book. Since that cannot be, we refer all who are concerned with the issues of 'dissertationese', or 'classy' scholarly writing to Becker. Fortunately for us all,

we are working at a time when even in academic circles scholars are expressing themselves in clearer, more direct, and personal voices. We recognize also that each one of us may go about actualizing our own voice differently.

Creating a Narrative

The writing of a qualitative research report demands the creation of a narrative. Although concerned specifically with case studies, a United States General Accounting Office booklet (1987) includes the following about the narrative heart of qualitative writing:

> Case studies are usually reported as narratives that read like chronologies of what led up to an event and what happened during and after it.... In this respect, the narrative mode is not a stylistic choice, it is inherent in the purpose of case studies and the nature of their inquiry. (p. 59)

The writing of a narrative is the telling of a tale after all. As such, and with important differences, the process has much in common with all other literary undertakings. Some of the students we worked with were highly conscious of the similarities with fiction. Raimundo Mora was particularly intrigued by the artistic nature of ethnographic writing:

> When I read the most recent novel by Gabriel García Márquez, *Amor en los Tiempos de Colera*, I found that its structure was similar to an ethnographic study. This novel describes a love affair from the point of view of five different characters. Each of these characters belongs to a different geographic and socioeconomic world. Each version of the events reveals a different psychological and social interpretation.

Laura Berns undertook a study of a computer lab. A student of literature, she had a heightened awareness of the process of composing in 'a whole new genre'. Although speaking here of her log, the comparisons she draws are even more relevant to writing the final report.

> As something of a closet novelist, I took delight in the unfolding of this 'story' in a computer lab. A log, though, is clearly a different genre. In writing fiction, the details — the single gold earring, the sigh, the 'Oh, shit!' — are put there intentionally because they contribute to the writer's preconceived notion of character or plot or theme. But this careful selection, this filtering of details to fit a 'theory', is anathema for case study research, which encourages an open mind, an inductive strategy in which data are first collected and only later analyzed for categories, patterns, themes. Yet, even in the

> 'real' world, we are of necessity always selecting tidbits of data from ... satisfaction, with the result that my log seemed at times to be a random assortment of details and at other times to be leading covertly to an implicit 'conclusion'.
>
> In fact, on one gloomy February morning, I was suddenly struck by one of those insights that makes up a bit for the grubbier parts of being a graduate student. I was struck by how dependent I was upon my prior knowledge in making sense of the situation and how inextricably external 'facts' are bound up with interpretation. I was aware of how my interpretation of Bill's saying, 'How do I get it to double-space?' was dependent upon my prior understanding of computers, of the writing course, of learning theories, of human motivation. Indeed, even my 'knowledge' that Bill was a first-year college student enrolled in a computer section of Writing 2 — not an instructor or a freeloading senior writing a paper for Economics 453 — was based upon interpretation.
>
> This relativistic viewpoint was not intellectually novel, as it is a theme that surfaces insidiously in nearly every graduate course I take. I read Bruner's *In Search of Mind* (1983) and stumble over his New Look revelation that there is no 'immaculate perception'. I read literary theory, to find a shared assumption, even among the warring camps of structuralist, deconstructionist, and reader response critics, that the interpretation of a text is dependent upon the reader's prior knowledge. But what was new that February morning as I struggled to write my log was the experiential, pre-verbal revelation of the pervasiveness of interpreting in routine situations.

The point for us to remember, of course, is that the ongoing mental act of interpreting is here consciously harnessed in the service of presenting the context we have studied as fully and richly as possible. If in our logs we have learned to include every detail we can manage, always remembering, as Laura expressed it, that 'we are of necessity selecting tidbits of data', when we compose our final reports we become even more selective. Although our aim is to portray natural settings and phenomena, the writing is crafted. It is a construction by an author.

> Description should transport the reader to the scene, convey the pervasive qualities or characteristics of the phenomenon, and evoke the feeling and nature of the ... experience.... The use of [linguistic] devices should create a description so vivid the reader can almost see it and hear it. (Ross, 1988, p. 166)

John Van Maanen (1988) has spotlighted three distinct styles of ethnographic writing which he categorizes as realist tales, confessional tales, and impressionist tales. He characterizes the three forms of tale as follows:

> culture, unclouded by much concern for how the field worker produced such a portrait.... Confessional tales focus far more on the fieldworker than on the cultures studied.... Impressionist tales are personalized accounts of fleeting moments of fieldwork cast in dramatic form; they therefore carry elements of both realist and confessional writing. (p. 7)

We find that we are most familiar with realist tales, and Van Maanen presents them as the most prevalent and familiar form of ethnographic writing (1988, p. 45). In them, the researcher 'vanishes behind a steady descriptive narrative'. In some realist tales, the narrator adopts the device of allowing various informants to speak for themselves. John Devine did a preliminary study of the experience of Haitian students in an urban high school. Here is how he handled the weaving together of voices he had documented:

> At the most basic level, one must clarify who the 'I' of the researcher is for each successive narrative or story. This past year, I found that at times, the 'I' of the storyteller was myself because it was I who had interacted with the Haitian student. At other times, I was recounting a tale told me by a Haitian teacher who had in turn interacted with a student; in this case, the teacher, my informant, was the 'I'. In a third case, it was the principal telling me how he had handled a suicide threat, and in a fourth, it was a tutor from the university explaining how overjoyed a student was in getting an award.

Although this team and our students have all written in the realist mode, most of us find that there are confessional elements woven into our realist tales. This is particularly evident when we write about our reflections upon qualitative methods in the discussion toward the end of the research report.

As this writing team has discussed the role of the 'confessional tale', we have found ourselves uncomfortable with this term. Ann has written to this point in the postscript which appears at the end of this chapter. The term 'autobiographical account' is used by some of the writers in *The Research Process in Educational Settings: Ten Case Studies*, edited by Robert G. Burgess (1984), and we prefer that usage, just as we also concur with certain points made there. Burgess offers the following rationale for having presented a collection of autobiographical accounts of research studies:

> The accounts that have been provided in this book focus upon personal and professional issues in the research process as well as technical matters. While each account highlights different aspects of the research process, they all point towards several broadly similar lessons that can be learned. First, that research is a social process and as such is worthy of study in its own right. Secondly, that doing research is not merely about techniques of social

> investigation but about the ways in which studies begin and are funded, access [illegible] conducted. Finally, they also indicate the importance of examining the ways in which evidence is accepted, rejected, and received. In these terms, the accounts would seem to have much to offer. (pp. 251–2)

Paul Atkinson (Burgess, 1984), who writes about such issues as handling the 'familiar' and the 'strange' as researchers conduct studies in their own society, concludes by stating:

> . . . while we can often learn much from autobiographical confessionals, they should be used to develop more systematic perspectives on the conduct of ethnographic research.
>
> . . . We make some things 'strange' against a background of unexamined assumptions. The value of autobiography and reflection may be in helping us to conjure up and confront . . . personal and methodological failings. (p. 182)

We recognize that there are objections in the literature to autobiographical accounts (Burgess, 1984; Van Maanen, 1988). It is not our intent in this book to argue for more or less of these confessional tales. It is our intent to highlight the characteristics of qualitative work. One such is that most researchers in this mode bring to the reader their reasoning as well as their stumbles and reservations about their findings. For these people, this sharing is considered essential. What needs to be understood is that researchers in all paradigms have some joys, some doubts and reservations. Airing these is extremely valuable to understanding results as well as to fashioning future research projects. In our opinion, every report must carry at least some of this 'confessional' element, whatever it is labeled.

Van Maanen (1988) notes that impressionist tales 'are typically enclosed within realist, or perhaps more frequently, confessional tales' (p. 106). Perhaps it is here that our instincts as storytellers take over most strongly. Ellen Klohman describes the powerful pull of this creative urge:

> I should be writing this down. This is important and I'll forget it if I don't describe it. I should be writing and I'm not. This is an important story. It should be told, but I don't know how to start it. There's a novel here or at least a short story. How should it start? Can I possibly get the feel of it — the *angst*, the terror, the joy, the relief. Why can't I think about anything but writing?
>
> Never let it be said that the 'voice of the turtle' isn't heard by the ethnographer! In fact at times it seems that nothing else can be heard above the din of the ethnography muse urging that every incident, every utterance be captured and recorded. The artistic impulse that lurks just beneath the surface of every ethnographic study is a strong

scenarios that the ethnographer encounters each day.

What advice is there for those newly afflicted with the condition? Accept the creative urge and find out what it can teach is the counsel of some seasoned ethnographers.

> Social scientists probably have a lot to learn from novelists and essayists. They'd best not set themselves apart, but rather try to understand what it is that they can learn from them to improve their own trade. (Bogdan and Biklen, 1982)

Narrative Devices

We discussed the narrative and rhetorical devices of themes, social rules, and constructs/vignettes in the section of this chapter entitled 'The Final Analysis of Data' (pp. 140–56). Usually such formulations begin to take shape, at least in the researcher's mind, or as framework for analysis, long before making the final story. We have found that most researchers combine a number of approaches to presentation. We have also noted that each person brings a particular shade of newness to the writing. It may be in the color, or order of presentation, in a particularly vibrant way of using quotations, or in presenting a new narrative device.

Some other narrative devices that have served in telling the story are these:

- A case, or a story over time about a bounded system such as one person, one event, or one institution that was the focus of study, in order to illuminate important findings about that person or about the entire broader social unit. Oral histories sometimes take this form. Gail Cueto wrote several individual stories over time about the re-entry process of Latino youth offenders after being in prison.
- Composites which are similar to constructs/vignettes but which describe findings that apply to a group of people rather than to any one unique individual. Ann used this device to describe, for example, a group of teachers who changed their view of authority in their classroom.
- Descriptions of 'critical incidents' in the data. These are incidents that carried important meaning about what was being studied and, so, were considered hallmarks of the findings. Margaret used this device in her study about children's reponses to literature.
- Presentation of 'snapshots, moving pictures, and reruns' (Lofland, 1971). I used these devices. The 'snapshot' was a written description that attempted to paint a single picture which captured the totality of

one child's play style. In Chapter 2 we included a snapshot of the beginning of our class. The 'moving picture' was written to illuminate the dominant time-oriented sequences of a child's play style. The 're-run' was written to describe cycles of activity within one child's play.

- Studies in contrasts and contradictions. These are seen by Werner and Schoepfle (1987) as '... The secret of fertile ethnographic insights' (p. 61). Contradictions have been presented in a variety of ways: Debrah Goldberg offered side-by-side vignettes of an 'outsider' and an 'insider' in a kindergarten classroom, each of whom had a vastly different experience there. In that same study, Debbie highlighted the contrasting reports and observed behaviors of other teachers toward these same two children. Gail Cueto related the stories of their re-entry from prison to wider community of a small number of Latino youth by contrasting what these people said and what they did, both in and out of prison. Maryann Downing, Belén Matías, Margaret, and Margot illustrated four versions of the same classroom experience in what they called the Rashomon Syndrome by focusing on contrasts as well as agreements.

Revising

In spite of our best efforts, sometimes our narrative devices can fall flat. It is often frustrating as well as depressing to read one's own text only to know that it does not do justice to the study. Courage! There is always the fact that writing means continual revision:

> Revision is commonly regarded as a central and important part of writing. It may powerfully affect writers' knowledge. Revision enables writers to muddle through and organize what they know in order to find a line of argument, to learn anew, and to discover what was not known before. (Fitzgerald, 1987, p. 480)

Revision may be onerous, but it is a very necessary element in completing the analysis and telling the story. Strauss (1987) adds another twist to the meaning of revision as he relates how the writing process and the research process intertwine, even at this final juncture:

> Ideally all of the integration, or at least its major features, should have been accomplished by the time that actual writing for publication takes place. Yet even when a first draft of a monograph (or the initial articles) is written, understandably, researchers always find themselves discovering something

> that tightens up or extends the total analysis. When the first draft is reviewed and revised one or more times — sometimes many more — then additional integrative details may be added. Even entire integrative steps may be taken, since further data collecting or at least further coding may be deemed necessary, not only to add more detail but to add to the final integration of the analysis. (p. 212)

Indeed, Strauss's 'rules of thumb' for final analysis and writing (pp. 213–14) have been a valuable resource to many of us. The following advice seems particularly germane:

> ... you must fight the temptation to be finished, to wash your hands of this project once and for all. You are finished, when finished! On the other hand, do not go to the other extreme ... and spend years tinkering with the product long after it could have withstood public scrutiny, and perhaps long after its effective impact might have been made. (Strauss, 1987, p. 214)

Personal Work Strategies

Finally, it benefits each one of us to honor our personal work strategies. Margot must write in longhand, insists that she solves writing snags while she sleeps, and writes in spurts until the time when she slaves demonically away, eighteen hours at a stretch. Ann writes only and directly on her computer, must sometimes find a place away from her home to write, and works still to overcome her feeling that she really cannot write, all the while she is writing. I myself run hot and cold. For example, for this book I was the first person to write a draft because I knew I'd need numerous revisions. I write copiously about my travels, while for other purposes I sometimes pull a dramatic farewell to the whole business, only to return to writing when I've cooled off.

It may help all of us to know that many of our strategies and feelings are shared. It may also help to take a break.

> It is wise to lay your report aside for a while after having written it: for a few days, a week, months, or even a year or more. We all tend to develop a mental attitude toward a piece of work we are involved in — a particular 'locked-in' view of what we are doing. It may well happen that your attitude is a good and insightful one. Then again, it may not. By backing off from a piece of writing for a while, forgetting and losing your commitment to what you had in mind, you can later come back with a fresh view. (Lofland and Lofland, 1984, p. 150)

In terms of dissertation writing, people have found that an exceedingly long break is not helpful. There are often prices to be paid for such long breaks like the 'cooling' of one's feel for the data. Try a vacation, not a divorce.

Postscript

This postscript [illegible]
Each person speaks in her own voice.

Ann

I'd like to share with you why I take exception to Van Maanen's use of the word 'confessional' in his analysis of types of naturalistic writing. My two objections are both on the grounds of the unfortunate connotations of the word. For Catholics, and many others, 'to confess' means to unburden one's soul of transgressions. The confessional is a place for seeking forgiveness from God. Naturalistic writing may relieve some participants of the burden of long-kept secrets, but it is not the intention for the writer to be forgiven, nor is it a forum for forgiveness.

Journalism is another context that suggests inappropriate connotations for the use of this word in naturalistic inquiry. In journalism, confessional writing connotes some of its worst excesses of emotionalism. The 'sob sister' story is one type of journalistic confessional writing, and much of what appears in tabloids, when not accusatory, purports to be confessional in nature.

Perhaps a better term for this sort of naturalistic writing might be 'revelatory'. It is characteristic that qualitative researchers reveal. Revealing one's research thoughts and actions is both an inherent imperative and a strength of the methodology. The public sharing of insights, reasoning, emotions, changes, consistencies — whatever — is done in the service of helping the readers to 'be there' with the researcher, to understand, and to make their own assessments of the research in informal ways. That is a far cry from the commonly understood purposes of confessing.

Diane

I was profoundly engaged in making final meaning of my study; so much so that it took me a while to get the distance I needed to be a bit more clear about what it was that touched me so deeply. I believe it was this: in studying the children, I connected to the child in me, the child I had not forgotten, the child I came to again when another child mirrored my own face of long ago.

Upon becoming aware of this, I took special care in forming categories and developing themes so that it was not the child in me who powered analytical decisions. But it was the child in me who could compare experiences. For example, my story of Chris and his need to gain approval of others while maintaining his independence resonated strongly with my own memories. Chris and I walked

similar paths. But it also became clear that the paths were not the same. My study of Chris provided both similarities and contrasts to my own experience. I feel that my understanding of Chris was substantially heightened by actively reflecting on my own life.

If my insights hold for others, then the task of searching for meaning as one writes begins with the crucial task of discovering ourselves, and our understanding of others in the final analytic presentation can only be as profound as the wisdom we possess as we look inward upon ourselves. Forgive me if I preach. Perhaps it was Arthur Jersild in his *When Teachers Face Themselves* (1955) who inspired me in my final reflections, but then I myself experienced that one can profit from trying to catch the meaning of one's own life when one strives to catch the meaning of the lives of others.

As you write, keep yourself actively up front. To be personally passive in ethnographic research may be dangerous to the health of what you are doing.

Margot

In this chapter (p. 145), we mention that we applied three criteria in order to select topics about final analysis. The third criterion states that we focus on those characteristics of analysis that are common to a large number of qualitative research approaches. I believe these commonalities deserve special mention because they are essential to understanding those analytical methods and some other major messages in this book.

In her 1990 work, Tesch discusses the results of analyzing texts on qualitative analysis for common characteristics of analytical principles and procedures (pp. 95–7). While at first no universal characteristics held, when Tesch omitted both extremes — the formal, most quantitatively based types of analysis, and the most obscure, unspecified types — she did develop ten principles and practices that hold true for the remaining types of analysis, from ethnomethodology to phenomenology (in addition, of course, to the usual principles of good scholarship such as honesty and ethical conduct) (p. 95)

In a condensed version, here are the ten common characteristics.

1 Analysis is not the last phase of the research process; it is concurrent with data collection or cyclic. Both analysis and data collection inform each other.
2 The analysis process is systematic, but not rigid. The analysis ends when new data no longer generate new insights.
3 Attending to data includes a reflective activity that results in a set of analytical notes that guide the process.

4 Data are 'segmented', i.e., divided into relevant meaning 'units', yet the connection to the whole is maintained. The analysis always begins with reading all data so as to provide
5 The data segments are categorized according to an organizational system that is predominantly derived from the data themselves.
6 The main intellectual tool is comparison. The goal is to discern conceptual similarities, to refine the discriminative power of categories and to discover patterns.
7 Categories for sorting segments are tentative and preliminary in the beginning; they remain flexible.
8 Manipulating qualitative data during analysis is an eclectic activity; there is no one right way.
9 The procedures are neither 'scientific' nor 'mechanistic'; qualitative analysis is 'intellectual craftsmanship'. (Mills, 1959)
10 The result of the analysis is some type of higher level synthesis. (Tesch, 1990, pp. 95–97)

Although this list is presented here, in Chapter 5, the essence of these ten items applies to various sections throughout the book. You might examine our chapters in terms of these ten characteristics. For example, where and how do we talk of concurrent data collection and analysis? How do we treat each of the other commonalities?

Here is one good test for this book. How does it measure up?