Data Analysis in Qualitative Research: A Plea for Sharing the Magic and the Effort

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Why am I writing a paper about data analysis in qualitative research? What could I have been thinking when decided I would do this? I am not an expert (it certainly doesn't come easy for me), and a plethora of books detail the process and how to carry it out using different traditions and methods (simply or with complexity, depending upon your preference). But I worry about where qualitative research is going in occupational therapy literature, and I think part of my worry is a response to the manner in which data analysis is described. This paper is my attempt to shed some light on the issue of qualitative data analysis, or at least raise some of my concerns about the way qualitative data analysis is handled in our literature. The paper will echo concerns of previous helpful and cautionary treatises on the subject of qualitative research in occupational therapy literature (Banning, 1997; Frank, 1997; Hasselkus, 1991, 1995). I believe the issue today is not about qualitative research gaining recognition and acceptance as a legitimate and useful scientific endeavor, but rather the need to recognize the many different legitimate forms of qualitative research and insist on excellence within the particular tradition a researcher follows.

I will argue that there is a problem with trying to write qualitative research papers within the accepted tradition of quantitative works, a problem that may be particularly evident in the data analysis section. Neither the orderly progression of topics of the traditional quantitative research paper nor the setting aside of data analysis in a “method” section is a good fit for some qualitative research. Nevertheless, we may try to fit qualitative research reports into the format established by quantitative research in order to meet explicit or perceived standards of colleagues, publications, and reviewers.

Some Context

This is a very personal essay grounded in my desire to see qualitative research thrive as the “practical science” that exists along side the “theoretical science” of our discipline (Schwandt, 1997). I am an anthropologist and I “do” ethnography. I con-
fess to liking numbers and often make use of descriptive statistics to gain some early and broad understanding of phenomena I observe. My focus, however, is on the particular, on people and their occupations in a specific context. Schwandt (1997) cites Aristotle’s notion of phronesis as a “practical–moral” form of reasoning, “intimately concerned with the timely, the local, the particular and the contingent” (p. 125). It is this phronesis that I seek—the knowledge from the particular situation that lets me see other situations in a new way. This sense of using the particular to jostle our notions of what is “normal” or “given,” is, to me, the essence of a good qualitative research report. To the extent that a paper pushes me to view some piece of the world differently and convinces me that there is reason to do so, then I think the paper is successful. And while this may indeed be practical science (phronesis), for me this new view of the world informs and transforms theoretical science as well. Just as phronesis confronts my views of the world, it can also challenge or support existing theory. This is the bias I bring to my evaluation of qualitative research and to my own work. I do not see my research as necessarily atheoretical, but I am not setting about my task with a goal of building theory.

The State of Affairs

In order to begin this paper, I went back to the data—in this instance, qualitative studies for the past 5 years in The American Journal of Occupational Therapy and the past 10 years of The Occupational Therapy Journal of Research. This return to the literature was an attempt to discover the sources of my unrest with the state of things. Many of the papers were compelling reading whereas some failed to capture my attention. Sometimes the findings of studies seemed too obvious, and other times too far-fetched. And sometimes I experienced a new awareness and sense of possibilities because of the manner in which authors crafted their analyses and discussions. Some papers were clear about theoretical stances that guided the studies, while others directly or indirectly purported to be atheoretical. For the most part, I came to the conclusion that my pleasure and displeasure with the data analysis processes in this body of research had more to do with the manner in which they were (or were not) written, rather than shortcomings in the processes themselves (although there were some of those). Furthermore, my reaction to the data analysis portion of a paper was almost always congruent with my reaction to the paper as a whole.

It should not be a secret that writing is critical to the success of qualitative research endeavors. Wolf (1992), in writing about ethnography, states:

The better the observer, the more likely she [sic] is to catch her informants’ understanding of the meaning of their experiences; the better the writer, the more likely she is to be able to convey that meaning to an interested reader from another culture. (p. 5)

Rowles (1991) extends this notion to the writing of descriptions of the research endeavor that establish the legitimacy and usefulness of the findings:

…the criteria for verification are intuitive: They rely on the degree to which a researcher can authentically convey the essence of the research experience, rather than on measures of statistical significance. The presentation of qualitative findings becomes a crucial determinant of their usefulness. Presentation is characteristically detailed and descriptive. It relies on the researcher’s ability to write, not only in a way that evokes the nuances of the research situation, but also in a manner that effectively conveys the environmental context and the process involved in arriving at the conclusions—the natural history of the project. (p. 267)

This tracing of the “natural history” is what should be conveyed in describing data analysis, and it rarely happens. Just as the account of the findings needs to tell a story, so does the account of the process itself. This account needs to go far beyond the use of terms such as “open coding,” “triangulation of data sources,” and “line by line reading.”

Writing a separate data analysis section in a qualitative study can too easily result in a cursory text composed more of labels than description. In part, this state of affairs is due to some of the inherent messiness of qualitative data and analysis, but it is also heavily influenced by postpositivist research traditions where the presentation of work takes place in an orderly and predictable manner and the labels convey accepted and substantive meanings. In an area of study where we may start with a loosely-defined research question and follow the data where they guide us, the order of a “traditional” research report lacks goodness of fit. A method section is usually appropriate, but description of the processes of both data collection and analysis may best be interwoven with the findings.

Labels Are Not Enough

Qualitative researchers in occupational therapy frequently reference one or more supporting texts in the methodology sections chosen from a relatively standard set. Lincoln and Guba’s Naturalistic Inquiry (1985) is the source for terms such as “trustworthiness,” “prolonged engagement,” “triangulation,” “member checks,” “peer debriefing,” and “audit trails.” Authors reference Strauss and Corbin (1990) for “grounded theory” and “open” and “axial coding,” and Bogdan and Biklen (1992) for the “constant comparative” method of analysis. Miles and Huberman (1994), arguably
one of the most “quantitative” of the “qualitative” methods of data analysis, and other texts such as Denzin (1989), Van Manen (1990), and Lofland and Lofland (1995) are cited as well. Although Spradley (1979) is often referenced for his interview methodology, his method of componential analysis and description of cultural domains is rarely cited. Some authors use an introductory research book such as DePoy and Gitlin (1993) or a comparative text such as Creswell (1998) as the only source to support their data analysis process.

When these references are used as if they give a recipe or an explanation, almost as if they are a statistical procedure, they fall short of being either descriptive of the analytic process or helpful to the reader. For example, the terms “open” and “axial coding” (Strauss and Corbin, 1990) are sometimes used as if they were an ANOVA or t-test. “Triangulation” and “member checking” are presented as prescriptions for trustworthiness of both data and analysis, without further description of what these processes entailed, or how discrepancies were resolved. Furthermore, much of the effectiveness of texts such as Strauss and Corbin (1990) lies in the use of examples from the authors’ studies to illustrate the processes they are describing, a practice their devotees could well emulate.

If an author writes of coding, I want to know something about the codes—what they were, perhaps; how they were selected; how the author used them. Codes may have “emerged” from the data, but I suspect they were more likely to “emerge” from the researcher’s mind and in either case some decisions were made about what mattered and what didn’t. I want to know about that process—what the researcher was thinking. Labels don’t convey this. This desire to know what the researcher was thinking is not to question the value of the findings, but rather to understand his or her thought processes and ways of viewing the world. I’m not even sure that we need to read about data being coded, and in fact this sort of information is not always evident in research presented in other disciplines. Sherry (1990) for example, an anthropologist and professor of marketing, devotes considerable space to a description of his methodology in a study of a flea market, talking about the data, his time in the field, and his member checking. He presents a framework of marketplace structure and function “to alert the reader to a fundamental bias that pervades this study” (p. 15). But even though he talks about his interpretations of what he observed, the photographs he made, and the hundreds of interviews he conducted, he never talks about data analysis, per se. He presents his understanding of the flea market interwoven with rich and evocative descriptions, and he links his interpretation to theory. The authenticity of his study lies in the story he tells, a story that relates his experiences integrated with vivid descriptions of the context and interactions as well as quotes of people participating in the flea market experience.

Letting the Reader Into the Process

In A Thrice-Told Tale, Wolf (1992) presents three different texts, each telling of an event that happened during a fieldwork experience in Taiwan in 1960. The first is a short story written soon after the event but never before published. The second is the ethnographic field notes written by Wolf and others at the time of the event, with minimal subsequent editing. The last is a scholarly analysis of the event, written and published in a peer-reviewed journal 30 years later. There is also a brief fourth account—a simple telling of what happened. Each of these texts yields knowledge, as does Wölfl’s discussion of them in the context of feminist theory and postmodernism. Each involves analysis and interpretation of data, data that are already a level of abstraction beyond the “real life” occurrence. Wolf’s account illustrates how the analytic process stretches from decisions about what the data will be to the nature of the text that results. Each of the accounts is essentially “true.” Each grants knowledge to the reader. Each truth is compelling. And yet Wolf never speaks of data analysis, per se. Furthermore, she acknowledges that others who were involved with the same situation and helped to collect the data have a different understanding of what occurred and what it meant.

What Wolf and many other effective writers of qualitative research do is tell the reader about the thinking that went on during the process of transforming data into a coherent description and sound interpretations. For example, Mattingly (1998) interweaves her text with stories, theory, and her own questions and the puzzles that led her to understand the occupational therapy she observed as a process of narrative construction or therapeutic story making. When I read her text I felt that I was part of the analysis, trying to make sense out of actions and words that often seemed on the surface to be insignificant. Mattingly describes contexts, actions, and words, but she also describes her own experience of being drawn into the stories that were taking place. In telling this story of her research experience, she draws us into the drama. To the extent that she makes the reader share her experiences, the reader also shares her conclusions.

Others develop questions that drive their analyses. For example, Wolf describes the incident of a woman in the village who is acting very strangely and reporting visions; her scholarly text focuses on the question of why this individual did not become a shaman (1992). In other classic exam-
ple, Whyte (1984) discusses research where finding an
answer to one question then leads to others, until a situ-
ation is ultimately understood. In a study of labor organizing
and personal choices in Oklahoma City in 1942–1943, the
orienting question was “who goes labor and why?” (p. 234).
Research with waitresses in the Stouffers Restaurant in
1944 identified the question, “why do waitresses cry?” In
each case, the “question” guided observations that raised
questions that led to more questions, and finally to com-
plete stories that made sense of the situations he studied.

Finding the questions to guide the analysis is critical to
good qualitative research. This is the aspect of the analysis
process where I think accounts of coding procedures fall
short. It isn’t so important to know that someone coded fol-
lowing the procedures outlined by a particular authority,
but rather to know what questions—what confusions—led
the researcher in the direction taken. What aspects of the
data became the puzzle, and how did the pieces finally fit
together? How can we convey this process, I ask myself.
What would an accurate, absolutely honest account of how
I go about conducting data analysis look like? Lump-
 ing together data management, analysis and interpretation, the
story might read something like this . . .

My Story: Sharing the Angst

It all begins when I write up field notes—a process that
requires a little distance from the experience, but not too
much—a few hours, or an overnight if the participant
observation took place late in the day. The authorities say
one should write the notes as soon as possible after leaving
the field, but I have to leave mentally as well as physically. I
need to get some distance from the end of the time in the
field in order to see the beginning hours of the fieldwork
with clarity. Participant observation and writing up the field
notes and interview notes are the easy part for me, easy in
the sense that they are more procedural than cognitively
challenging. Likewise, it takes little effort to accumulate
documents and artifacts, and it is sometimes tedious, but
not difficult, to record historical information.

As pro forma as this sounds, doing these acts of research
constitutes an early and essential step of analysis—sorting,
choosing, ordering data and converting actions, sights, and
sounds into words and texts. These are like housekeeping
activities—necessary to do, comfortable in their routine
and almost ritualistic performance, and satisfying as they
bring about order. But even though they can be labeled
“housekeeping,” they shape what happens afterward—the
nature of the next fieldwork experience, the content of the
interview questions, the way that events and actions are
viewed. This shaping takes place because, in the act of

brining order to them, I have decided that certain events
and stories are data—and in the process I have decided that
others are not.

I keep doing this—the data collection and housekeep-
ing—until I’ve done enough, until the data feel complete,
another analytical turning point. The term for this in qual-
itative research jargon is “saturation,” that point when addi-
tional data collection yields no new information. It isn’t that
simple, of course. There is always new information to be
had, but the quality drops off and the novelty is less evident,
and there is “enough” of the story waiting to be told in the
files back in the office. The decision to cease data collection
comes out of writing and reading the notes and knowing
enough about what is there to know that it is enough. Thus
suspending data collection (at least for the moment)
depends upon simultaneous data analysis. After data collec-
tion is set aside, analysis becomes the primary task and the
fun begins.

So many authors make it sound so easy. Their themes
“emerge” from the data. They do this kind of coding and
that kind of coding, sometimes using analytic software.
And then, like magic, they present their results. It isn’t like
that for me, although I do believe, at least a little bit, in the
magic. For me it is a stop and go, erratic, exciting, and frus-
trating struggle to transform my data into a coherent and
interesting story that enlightens me, as I hope it does oth-
ers. Neither the process nor my description of it is neat or
pretty.

First I perform the order rituals. I go to the office sup-
ply store and buy the requisite yellow pads—narrow-lined,
3-hole punched, rigid back. I buy my favorite extra fine
point, always black, writing pens. I buy some boldly colored
pens and highlighters, and perhaps a new box of file folders
and some disks. (Yes, I do my writing at the computer—the
pens and yellow pads are mostly about ritual.) Then I clean
my desk. I establish piles and make decisions about what
goes into each pile and what can be discarded. I might even
dust. Because all of these actions involve getting ready to
analyze data, I feel like I’m accomplishing something.
Bringing order to my surroundings helps bring order to my
thinking.

I can begin to go through the data now, highlighting
key points, writing labels in the margins, making notes to
myself. Sometimes I do this with qualitative analysis soft-
ware, but I confess that I take aesthetic pleasure in reading
and marking text on paper. And I like the time that it
takes—time when I am thinking, organizing, and learning
my data. The notes I make begin to give words to my think-
ing.

I continue reading and marking until it all blurs into a
repetitive, seemingly meaningless task. Everything stops. I
go for a walk—usually from my desk to the refrigerator. I find my colleagues or family. “Is that the most recent issue of a journal? I guess I need to read it now.” Writing this, I recognize a pattern in my work—being in the field and then distancing myself, being in the data and then leaving them. And that is what I think analysis of qualitative research is all about—for me. What I am looking for is insight—seeing and understanding the ordinary in a new way and discovering and expanding knowledge of the particular in a way that makes me see other particulars in a different light. This requires stepping in and stepping out, seeing things from multiple perspectives.

I get up close, reading the words, thinking about what I saw and heard and how it felt. I make copious notes and identify key ideas and use red pens for some things and purple pens for others. At some point I stop—my notes have become another pile of data, diffuse and disorganized and often puzzling. Now the walks need to be longer than the distance to the refrigerator. This is a good time to garden or vacuum. It doesn’t hurt to have someone to talk at, someone to listen as I try to organize my thoughts into coherent phrases. The listener is allowed to contribute ideas, but it isn’t necessary that they do so. This is also a good time to manage data in graphic ways—to map or classify events, for example, or play around with grids or flow diagrams. Sometimes I do the “quantitative” part of the analysis here: demographics, descriptive statistics, listings of historic information.

When I return from this “vacation” (which can last for quite some time), it is to my analytic notes, not to the data (even if I have gone through only a portion of the total). Overlaps and similarities are suddenly evident. Ideas clump together and misfits stand out. I am ready now to hone in on some key areas—dimensions, themes, codes, categories—whatever I want to call them. I can see that these are the 3 (or 5, or 10) areas this study is about. Some of them are what I thought the study would be about before I began, but others are new. I give each of these a name.

Then I go back to the data. This time I mark all of my textual data in some way—I usually mark-up the text into segments that represent a complete idea. A segment can be a phrase, a paragraph, or several paragraphs—what defines it is that it is all about the same thing, one of those areas I defined from my notes. Agar (1986) reduces data in this manner to chunks that seem to be “major breaks in content” (p. 178). Nygård and Borell (1998) write of dividing all data “into smaller parts according to shifts in meaning” (p. 113). I also use a catchall term (e.g., “other,” or “stuff”) for content I currently think is irrelevant to the analysis. (An example of this might be a section of an interview transcript that contains idle conversation about the weather.) Every segment is labeled, and all of the textual data are segmented. A segment can have more than one label. This process is relatively fast (because it deals with bigger units than the initial coding), and when it is completed, the chunks of text can be sorted and aggregated by label, a task where technology proves helpful.

These labeled segments I have developed are “categories.” Frank (1997) reminds us that developing these is “an essential step in qualitative research” but that “categories are not an end in themselves” (p. 85). The different aggregations of portions of data that I’ve identified as being about a particular “something” are the next challenge. Up until this point I have been labeling and sorting, processes that are preliminary to interpretation (although interpretation is certainly happening as I do them). Now I read the data again (and often again and again) within each labeled area or category. “What is the same, what doesn’t fit, what is this or that all about?” I also ask the critical (to me) question: “What is not here that I thought would be?” Frank calls this stage the “process by which data are recontextualized in the world of social thought,” a process “based on the theoretical concerns of the researcher” (1997, p. 85).

It is time-consuming to do these analytical acts. But the acts of doing are only a small part of what takes time. Thinking time is vital. Thinking time might be focused at my desk, but it includes time doing such things as walking, watching the woods outside my window, talking, commuting, taking showers—in other words, activities that allow my mind to wander and puzzle over what I am finding. Sometimes I have a profound sense of “so what?” at this point. For example, in a recent analysis of data from an ethnography of people who make crafts at home and sell them at craft fairs I wanted to understand how these people were able to establish worker identities without the presence of usual social markers of such an identity. When I got to the point of answering this question in my analysis, I was overwhelmed by the sense that I had just successfully recreated the steps described in any how-to book concerned with establishing a home-based business. Finally, I realized that worker identity wasn’t an issue for most of the people I had interviewed. Going back to the data I recognized that many identity-related things were happening, but the “identity thing” wasn’t about what I had thought it was about.

Before going on with data analysis and interpretation, I go back to those data I have previously dumped into the category of apparent irrelevance to the study. Labeling them in this way does not mean they should be excluded from analysis. Sometimes they contain wonderful surprises, as I found in my study of crafters. In the stack of textual units describing events that occurred during interviews, I discovered frequent mentions of parents calling or arriving for the
morning, dogs being “sat” for vacationing family members, or others’ children being cared for that day in the crafter’s otherwise empty “nest.” It was these little bits of description, quite incidental to the interview, that helped me make sense out of the economics of what people were doing, leading me to the area of household economy theory (Dickie, 1998). This area of theory was very productive.

When I get beyond the “so what?” point, and when I have reviewed all of my data again, I begin to have some understanding (sometimes informed by theory) of what is going on in my categories. Then I can start to write.

Writing is the final stage of analysis for me. The ideas that can’t be expressed clearly, the insights that seem insignificant in print, the keys that don’t fit any of the locks—all become irritants, provocateurs, preoccupations. This is the point when I must finally face the pet ideas that have seemed so “perfect” and discover that they don’t work—the data don’t support them or they don’t really describe what I have seen. Ultimately they are left behind as I reflect on what is “me” and what is the phenomenon I am studying. This is also the time when the story has to hang together. It must make sense, and it must describe what has gone on. (This is a lesson learned in graduate school—if an idea felt awkward or was difficult for me to write clearly, my dissertation advisor inevitably identified and rejected that idea.) The description is not just about what happened in terms of what was being studied, but how I came to the particular understanding of it. The writing must convince. Most of all, it must convince me. Have I been true to my data? Would others familiar with the situation agree with my analysis, or at least agree that my view is one legitimate way of understanding what I saw and heard? Am I comfortable with my assertions? I think this “argument” with myself is based on my “grounded commitments” (Frank, 1997) to participants, colleagues, and knowledge. When I feel satisfied that I have met these commitments then I can finish the paper and send it off for review.

I cannot imagine that anyone would ever write such an account as I just have, even with more specific detail about a particular study, in a research paper. But surely we can have a little more of the researcher and his or her thought processes, even angst, shared in the account of a study. The angst is reassuring, not just because it shows that others experience it as we (or at least, I) do, but more to the point, because it invokes the experience of the analysis and interpretation and gives credence to the conclusions of the investigator. To the extent that it is part of the “natural history of the project” (Rowles, 1991), it supports the verisimilitude of the research account. Banning (1997) describes the dialectic struggle of qualitative data analysis, which he says is “the struggle between (the proper role of) the ideas being brought to the data and the ideas emerging from the data” (p. 130). This struggle is absolutely essential and should not take place off stage.

If I try to label the analysis I do, I can find some degree of fit with various well-defined methods, but not a perfect fit. I do analysis in the manner I learned to do it, which was not according to some orthodoxy. The process has certainly been informed by the many explanations of data analysis that are available, but it still feels like it is my own in some way. And so I cast my vote for fewer labels, less jargon, and more “real life” in the account of data analysis in qualitative research papers. To this I add a plea not to set aside the process in a special section labeled “data analysis,” but rather to integrate it with the account in a naturalistic manner that enhances the descriptions and findings of the author. Can we accept this format, as reviewers of manuscripts and readers of papers? In 1991, Hasselkus warned that we seemed to be “gradually circumscribing qualitative research with an orthodoxy of rules to which it must conform” (p. 3). She went on to say, “this bend toward a dogma of qualitative inquiry can potentially smother the creative elegance of such research” (p. 3). The manner in which data analysis has been described since then often confirms her fears. We need to continue to have open discussions about these issues and encourage movement beyond any real or perceived “orthodoxies.”

Other Concerns (and More About Labels)

There are other ways than those I have mentioned in which content in the data analysis sections of papers seems to conform to a received dogma about what we should do in the process. Earlier I wrote of the “grounded commitments” I had to the people and situations I studied and the knowledge I discovered. These are ethical concerns that authors frequently address with “member checks” and “triangulation” methods, saying that they have conducted these acts to support rigor. But are such processes necessary to qualitative research? Must every study include them? And what do the terms mean? Often they are used without discussion. But what exactly did the researcher do? What happens when different sources of data disagree? What happens when research participants don’t agree with the researcher’s version of what took place? Does that mean the researcher is wrong? With respect to member checks, Schwandt (1997) points out that “it is not entirely clear how the procedure actually helps establish the truth of findings” (p. 88). He describes the consensus of many researchers that “member checking is not profitably viewed as an act of either validation or refutation but is simply another way of generating data and insight” (p. 89).
Recently I asked a colleague who does quantitative research to review a qualitative paper I had written. She gave many helpful comments about both the writing and the content, and in the midst of this commentary raised the question of whether I had done member checks. Here was a quantitative researcher applying what she presumably understood to be a gold standard for all qualitative studies. Has the member check become a requirement? Member checks are most appropriate, but not necessary, for research in a constructivist paradigm (Guba and Lincoln, 1994). They are not essential or even relevant in all qualitative research. If an author claims that his or her findings represent the thinking of those who were participants in the study, then the process of how these understandings were discovered and negotiated needs to be part of the account. But we should be clear that presenting the ideas of participants is only one form of discovering knowledge, and while faithfulness to the data is essential, there is also a responsibility to interpret those data that ultimately rests with the researcher.

We are rightfully concerned with the rigor of our own research as well as that we read, but listing activities associated with rigor, and saying that these were done does not constitute establishing rigor. If I write that I engaged in “peer debriefing,” “triangulated” different data sources, and established an “audit trail,” I tell the reader that I followed some accepted research practices, but I do not convey the essence of those actions. With respect to one aspect of rigor—validity—Wolcott (1994) suggests its corollary in qualitative research might be “not getting it all wrong” (p. 347). He lists nine things that he does to try to meet this challenge:

- Talk little, listen a lot
- Record accurately
- Begin writing early [he uses the texts for something akin to “member checking”]
- Let readers “see” for themselves
- Report fully
- Be candid
- Seek feedback
- Try to achieve balance
- Write accurately (pp. 348–354)

Amplifying his points, Wolcott dismisses the notion that any methods “safeguard against error” (p. 351), asserts that informants can get it wrong too, argues for providing primary data along with interpretation, and insists that subjectivity is a strength of qualitative research. There is much to recommend in his list, not just in support of “not getting it wrong,” but in establishing the rigor of the research process in its entirety.

Many authors succeed in doing just that. I do not wish to use too broad a brush in painting my discussion of my concerns about data analysis. Many fine descriptions and exemplary discussions of data analysis processes do exist in the occupational therapy literature. These studies teach me new ways of thinking about phenomena or cause me to recognize situations worth thinking about that I have taken for granted. To a large extent, this paper is about the other studies, those that annoy or fail outright. But it is also about my own struggles with the process.

There Isn’t an Easy Way

Qualitative research data analysis should not be easy. Ultimately it takes an enormous amount of intellectual “sweat” by the humans who are trying to make sense out of a situation, a setting, or a culture they thought was interesting enough to study. Analysis and interpretation are the heart of that sense-making process. Different research traditions and methodologies require different sorts of data analysis activities and call them by different names even when they are essentially the same. Coding of some sort lays the foundation for most analytical processes (although the explicit intent to use coding to develop theory is peculiar to Grounded Theory [Strauss & Corbin, 1990]), but the nature of the coding process is specific to the researcher(s). The development and explication of the categories that are defined through reading and coding the data can become the “secret world” of the researcher, where magic does indeed take place but is never shared. But it shouldn’t be that way. We need to learn what the labels and jargon mean, then throw out the terms and describe what we do. We need to tell the research story, including the challenges of data analysis and how we resolved them, to support our interpretations. Researchers must be present in the stories they tell.

Our challenge is to accept and encourage qualitative research that is what it is. The form it takes in the literature should fit the nature of the study. If we can let this happen and demand excellence of those who do qualitative research in occupational therapy, then we will have achieved maturity as a discipline that uses qualitative methods effectively.

References


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