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Textbooks on Qualitative Research and Method/Methodology: Toward a Praxis of Method

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Abstract: Methodology is a fetishism, an ideology, consisting of catalogs of precepts that make sense only when a researcher already understands what methodology is about. Although most scholars are aware of a theory–praxis gap in doing scientific research, they continue to produce methods and methodology textbooks that are useful only to the person who already knows how to do research. I provide a demonstration for proposing a way in which authors on "methods" concretely realize a particular form of articulating what they have done and how they have done it in exemplary studies. That is, I am arguing for narratives of praxis of method rather than for methods of practice.

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1. Method(ologie)s: Who Thinks Abstractly about Them?

As I am writing these words, I have just received my copy of *Doing Qualitative Method: Praxis of Method* (ROTH, 2005), a book in which I take readers through decision-making processes concerning all aspects of doing research in six large projects. Rather than pretending that one can write methodologies useful for beginners, I describe my own praxis of doing research, how I come to frame research questions, how I prepare, how I collect materials that become data sources, how I interview, how I use photo and videographic cameras, and so on. I took this itinerary, because the *métier* of a researcher consists of a modus operandi, a habitus, which is exhibited only in practical operation, where it "'reacts' in the face of practical choices—a type of sampling, a questionnaire, a coding dilemma, etc.—without necessarily [being explicated] in the form of formal precepts" (BOURDIEU, 1992, p.222). Over the past fifteen years, I have used many different textbooks in my courses and seminars on research and research design; each time, my students complained about the uselessness of the particular text. It is evident that most authors heeded very little the advice of BOURDIEU. As I was reading this summer through yet another book on method, my first reaction was that there should have been a sticker on the front of the book, warning readers aspiring to learn about doing research (Figure 1).

Figure 1: A sticker like this ought to feature on every book pretending to be a hands-on guide to research by presenting different methodologies and approaches. [1]

Whereas I have tremendous experience doing and writing research, the students in my seminars are only at the beginning of their research careers. Even if an introductory textbook on the topic of qualitative research may appear easy to read and informative to me, my students generally experience it as inaccessible. I have tried different books as texts. Each time and without exception, students complained about gap between what is written in the textbook—for example, about interviewing, doing ethnography, or analyzing data sources—and the assignments I had asked them to complete—doing an interview, ethnography, or analysis. Over time I have come to understand that the source of my students' problems lies in the gap between any form of plan—a recipe in a cookbook, an instruction to program a VCR, or the operating manual for computer software—and the corresponding situated actions that realize them. [2]

Any person who has tried to cook something from a new recipe has made the experience captured in the following "incomposible" (DERRIDA, 1998, p.7)

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statements, that is, statements that not only are contradictory *in themselves*, but also are contradictory *between themselves*:

1. To know that what you *are doing* is what you are supposed to do, you need to know that what you have *ended up with* is what you were supposed to have done;
2. To know that what you have *ended up with* is what you were supposed to have done, you need to know that what you *have been doing* is what you were supposed to be doing. [3]

Many cookbooks now provide you with images of intermediate and final products, which allow you to get out of the quandary because you can compare what you ended up with and what is in the book. But even with photographs provided you may not know where you have gone wrong in the event that your own end product does not look anything like the dish depicted. How can it be that we do not know whether what we are doing is what the instructions state that we ought to be doing? Those readers with ethnomethodological training know that the gap between plan and situated action is unavoidable and that it is better to think of *plans* as descriptions of actions the usefulness or accuracy of which can be established only a posteriori (SUCHMAN, 1987). [4]

The covers of books on method and methodology often promise a lot, addressing themselves to potential audiences much in the same way the many self-help books do that take so much space in (North American?) bookstores. This is a cultural phenomenon, there definitely appears to be a need for books that promise to be "down-to-earth," "hands-on," "real-life," "student-friendly," "accessible to and popular with students." Yet anyone familiar with hermeneutics knows that there are fundamental problems with the promises implied in such statements. Take, for example, these comments from methods books, which takes us right back to the warning label I recommended: "Through a detailed yet concise explanation, the reader is shown how these methods work" (MARVASTI, 2004), "Designed for readers without prior experience in data collection" (BERG, 2004), and "This represents a perfect brief introduction to research methods" (EMBER & EMBER, 2001). As I read this sentence, a sense of doubt emerges within me, possibly due to the word *explanation*. What does it take to understand an explanation? Both HEIDEGGER (1977) and RICŒUR (1986) would respond: *understanding*. That is, both hermeneutical philosophers attempted to show us that any articulation and explanation requires prior, existential (practical) understanding of that which is articulated and explained. Thus, "Die Auslegung von Etwas als Etwas wird wesenhaft durch Vorhabe, Vorsicht und Vorgriff fundiert" (HEIDEGGER, 1977, p.150)[1]; and "La compréhension est plutôt le

1 "The explication of something as something is fundamentally grounded in intent [fore-having], circumspection [fore-sight], and anticipation [fore-conception]." This is my translation, with the words in square brackets that Joan STAMBAUGH proposed in her 1996 translation. HEIDEGGER's German words have very different semantic fields than their English equivalents have, which has led different translators of his works to employ different words. The German prefix "Vor-" denotes "before," which led STAMBAUGH to use her constructions with "fore-." All these terms emphasize that explication (interpretation) presupposes practical understanding. Here again a problem with translation—STAMBAUGH translates "Auslegung" as "interpretation," which is not necessarily the best way. HEIDEGGER describes "Auslegung" to

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moment non méthodique qui, dans les sciences de l'interprétation, se compose avec le moment méthodique de l'explication. Ce moment précède, accompagne, clôture et ainsi *enveloppe* l'explication." (RICŒUR, 1986, p.201)[2] [5]

In the same vein, a classical study on classification showed that processes cannot be inductively derived from the traces (records) they left behind (GARFINKEL, 1967). The study took place in the context of another study, where sociology graduate students were asked to categorize hospital records so that its ways of processing outpatients could be inductively derived. Looking at how the graduate students worked, however, GARFINKEL found that they already used their understanding of hospitals and the events that actually and possibly occur there in the classification stage. That is, these graduate students used their prior understanding of hospitals in the classification (interpretation) of the records rather than implementing the coding rules necessarily described in context-independent ways. In this way, the project could only be circular in the sense that the inductively derived categories are those that are already at work in the situation. [6]

The upshot of these considerations is that we cannot understand explanations of research (methods, methodology) until we have already developed an understanding of research. It is this recognition that underlies my earlier described hunch and requirement for the warning label (Figure 1). What then can we do about this situation, whereby students of research understand instructions for doing research only after they understand what they are to do? In my own practice of research as a supervisor and professor of graduate courses, I have come to *understand* what initially seemed to me an outrageous statement:

> "[T]here is no manner of mastering the fundamental principles of a practice—the practice of scientific research is no exception here—than by practicing it alongside a kind of guide or coach who provides assurance and reassurance, who sets an example and who corrects you by putting forth, *in situation*, precepts applied directly to the *particular case* at hand." (BOURDIEU, 1992, p.221) [7]

Today, my own graduate students learn to interview by interviewing alongside me, to do ethnography by doing ethnography with me, to analyze data by analyzing data with me, and to write research articles by writing research articles with me. Even in a graduate class, I have done all aspects of research with the students resulting in a published study (ROTH, RIECKEN et al., 2004). Even when a class is too large, there are solutions that are better than having students read methodologies, for example, asking them to do research in pairs or triplets

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be "die Ausarbeitung der im Verstehen entworfenen Möglichkeiten" (p.148). RICŒUR's (1991) English translation of "Auslegung" in fact is "explication." This definition seems to be closer to that of explication as "the process of developing or bring out what is implicitly contained in a notion, proposition, principle etc.; the result of this process" (OXFORD ENGLISH DICTIONARY, 1989 [online]).

2 "Understanding is above all the nonmethodical moment that—in the sciences of interpretation—combines with the methodical moment of explanation (explication). That moment precedes, accompanies, closes and in this envelops explanation (explication)." (This is my translation. The French *expliquer* implies both explication and explanation.)

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and by providing opportunities for collective analysis of their (videotaped) research processes. [8]

Books on methods and methodologies necessarily present precepts for research independent of particular research projects, because they aspire to describing method and methodology *in general*. Abstract—from Latin *abstractus*, drawn away from *abs*, off, away + *tractus*, past participle of *trahere*, to draw (OED, 1989)—precepts for research action strive to pertain to all possible situations in which the actions apply, and therefore leave out, by necessity, the very contextual factors that mediate our choice of one action, theory, or interpretation over another. "Examples" are introduced after the fact and as an afterthought to make a step toward the novice reader. What do we learn by thinking abstractly about method and methodology? G.W.F. HEGEL somewhere said something like "We learn to think abstractly by thinking abstractly." Thinking abstractly has its problem. "Wer denkt abstrakt?" [Who thinks abstractly?], HEGEL (1970) responds his rhetorical question by saying, "Der ungebildete Mensch, nicht der gebildete" [The uneducated person, not the educated]. He elaborates, thinking abstractly does not do justice to the complexity of the object of the subject matter at hand. One possible implication of this is that reading books on methods and methodology, because they deal with the subject matter in an abstract manner, leads to inferior thinking about research. [9]

But let me come to sketch the structure and content of textbooks on qualitative method and methodology, before deepening my analysis and elaborating possible avenues for writing "methods" books. [10]

2. About Textbooks on Qualitative Research Method/Methodology

On the back cover of a methods textbook that I recently read, we can find the promises for it to be "an invaluable resource for students and researchers alike, helping them to undertake effective qualitative research in both sociology and courses in social research across the social sciences." Having read the rather short volume, I am reminded of the ambiguity that advertising agencies use to misguide buyers so that they expect more from a product than it can hold. In seven chapters the author covers interviews, ethnography, visual sociology, data analysis, research writing, and ethics in qualitative research. The author and publisher may defend against misgivings such as mine by saying that this is a book for a survey course—which leads me right back to HEGEL and his question, "Who thinks abstractly?," that is, in a cursory and superficial (survey) way.[3] [11]

Generally I find that the most introductory textbooks fail to do justice to the complexity of doing research. The frequently used chapter summaries relate to doing research as last night's news feature about Einstein's research relates to

3 Incidentally, to survey means to take a broad, general, or comprehensive view of something, as in surveying the surface of the earth (OED, 1989 [online]). The adjective *superficial* literally denotes the quality of being at or on the surface.

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the work the physicist has actually done—in the news, physics was so distorted to the point of being unrecognizable.[4] [12]

The point that bugs me most about methods textbooks is that the authors do not appear to speak from experience. In many instances, methods books are in fact iterations of texts that others have written, compendia of iterated and reiterated instructions that students then as now find incomprehensible—because to comprehend an explanation of what to do, one already needs to have the practical understanding of the doing. Some authors ground themselves so much in the work of others—using their examples, extensively quoting them, retelling the contents of their works, almost ventriloquizing them—that I wondered why a particular book was written in the first place given that its entire content already existed in other published works. This raises the question, *who* writes textbooks on method, *why*, and *what-for*? Have these authors not made the same experiences as I have with graduate students, who do not understand the methods books. In fact, as I am writing I remember my experiences while taking methods courses: Although I achieved at or near the top in my statistics classes (I never took courses in qualitative methods but learned them in and through praxis), I did not understand why because I felt that I did not know and understand; it was only when I did my first statistical analyses. [13]

A give-away of an author's speaking for others rather than from his or her own experience is the deferral of agency in constructions such as "Her [another author's] specific instructions include …," "[authors] (2002) write that …," and "According to [author], there are at least fifty-seven different ways of doing DA …" This ventriloquizing of other writers on method goes so far that in some sections of his or her book, an author provide the materials and interpretations of other authors rather their own. There are also many places where the author cites another author, but has not read the original text, giving rise to citations such as "[author1] 1992, as cited in [author2] 2001: 172)." [14]

All of these features distance us from the way in which research is done. Rather than moving closer to research, methods texts actually distance us from research, as it is about what others write about research rather than a description of the research as conducted by the textbook authors themselves. We come to know very little about why a researcher or analyst does what he or she does, why or how a person makes this interpretation rather than another, how some research decision or interpretation is historically situated in the biography of the person who is doing or has done the research. Last night's news presented Einstein's theories in such an abstract way that G.W.F. HEGEL would have attributed it to food for the uneducated. I sense that he would have said something similar about the relation of textbooks on qualitative methods or methodology to qualitative research. Good generalization cannot be achieved "through the extraneous and artificial application of formal and empty conceptual constructions" (BOURDIEU, 1992, p.233). [15]

4 In English, the popularization is sometimes described as "watering down." But in my experience, more is at work than a watering down, which would mean getting a less-concentrated dose of something. In popularization, the "dose" cannot be recognized any longer.

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The simplifying and simplistic tendencies in textbooks are perhaps most poignantly embodied in the dichotomous juxtaposition of positivism and constructionism that one can find in many books on method (e.g., LINCOLN & GUBA, 1985; ALVESSON & SKÖLDBERG, 2000), apparently pursuing the equally dichotomous agendas of quantitative and qualitative research. Rather than dwelling on the issues, I want to make just a couple of points. [16]

First, the use of "isms" to portray the rather complex landscape of human intellectual endeavors may well be a form of lazy thinking—of the kind that HEGEL and BOURDIEU wrote about. Even if I accepted thinking in terms of isms, neither positivism nor constructionism takes account of or covers the ground that is covered by the dialectical approaches in sociology (for a good discussion of such approaches and their strengths and weaknesses see SEWELL, 1992). The advantage of dialectical approaches is that they neither abandon the existence of the natural (material) world nor claim that this world can be known as it is. [17]

Related to the first point—because authors often make dualist statements—they confound ontology and epistemology, rejecting the claims of "positivists" that there *is* a material and knowable world and adopting the claims of "constructionists" that *everything* is (socially) constructed. In dialectical approaches, the structures of the material world, resources for actions (all communicative acts are materially embodied, and therefore the social world), are accepted as much as their counterparts in individual|collective consciousness (schema); and both forms of structures only exist in their dialectical relation with human agency. This leads us to an agency|structure dialectic, where the structures themselves emerge as a resource|schema dialectic.[5] A dialectical approach, because it admits contradictions in the subject matter to which thought is applied, has a greater potential than dualist approaches to deal with the complexity of human endeavors, including social science research. [18]

Second, the qualitative–quantitative distinction does not lead our field much further for a number of reasons. At the base of it, all perception is both quantitative and qualitative (ERCIKAN & ROTH, 2005): Whereas continuous light rays fall onto a two-dimensional array of photoreceptors, from and to which continuous electrical signals travel making connections to other parts of the brain, we experience the world in categories. It has been shown that the mathematics of fibrations yields exactly the kinds of transformations required linking the quantitative and qualitative aspects that lead to cognition and experience as described, for example, in phenomenological terms by Edmund HUSSERL (PETITOT, 1999). [19]

Focusing the discussion of *how to do research* on a dichotomous distinction around the quantitative–qualitative divide makes many novices think that one

5 Here I make use of the Sheffer sign "|" to create dialectical concepts, which are only true and useful if they contain a contradiction (e.g., ROTH, HWANG, LEE, & GOULART, 2005). We can think of the concepts created in this manner in terms of "both/and." Thus, a *quantitative/qualitative reduction* should be taken as denoting a reduction that is always *both* quantitative *and* qualitative.

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cannot count or provide summary statistics while doing "qualitative" research or that one almost has to be obsessive about operational definitions to ascertain a socially sanctioned appearances and the appearance of scientificity (BOURDIEU, 1992). It leads to *monomaniacs* of method who hail the combination of two methods as a major breakthrough. Not surprisingly, many graduate students attempt to begin their research with the statement, "I want to do a qualitative (quantitative) study." This is very simplistic way of thinking about research—a simplicity that is associated with *thinking abstractly* about research typical for the superficially informed and uneducated. Although many seasoned researchers know that it is much better to begin with an interesting question and then looking for ways of finding answers in a disciplined way, they do not stop writing books and teaching "survey" courses that demand a form of abstract thinking that leads these novices to think abstractly.[6] Pace Georg Wilhelm Friedrich HEGEL! [20]

Instead of distinguishing research into a qualitative–quantitative category system used by many authors of qualitative research methods, I propose to locate studies on a continuum between the extremes of lived-in world, which we experience through our testimony, and the ideal worlds of theory, characterized by pure, idealized structure (Figure 2). Various quantitative|qualitative reductions, take us from lived biography and experience to their re-presentation in narrative, grounded theory, numerical scales, to blips in multivariate statistics and linear structural relation models. The further we move to the left, the more we are affected and confronted with concrete human experience, especially suffering and care; the more we move to the structure end of the scale, the more distance we put between ourselves and the situation we investigate, that is, the more we abstract—*draw* knowledge *away*—and distance ourselves from biographical experience, care, and suffering to end, in the limit, in idealistic, PLATONic models of what the world is about. To show how we might orient ourselves to write about our praxis of method, creating narratives *about* research far to the left in Figure 2, I provide two demonstrations in the next section.

6 My own graduate students begin to do research as soon as they begin their program of studies. That is, I do not require them to have taken certain courses prior to begin getting involved in a research study. Within a year of being graduate studies, both at the MA and Ph.D. levels, they present their first papers, submit their first manuscripts, and so on. They do not do so on their own, but in my company. Over time, they require my presence less and less until, at the end of their program, they can "fly on their own."

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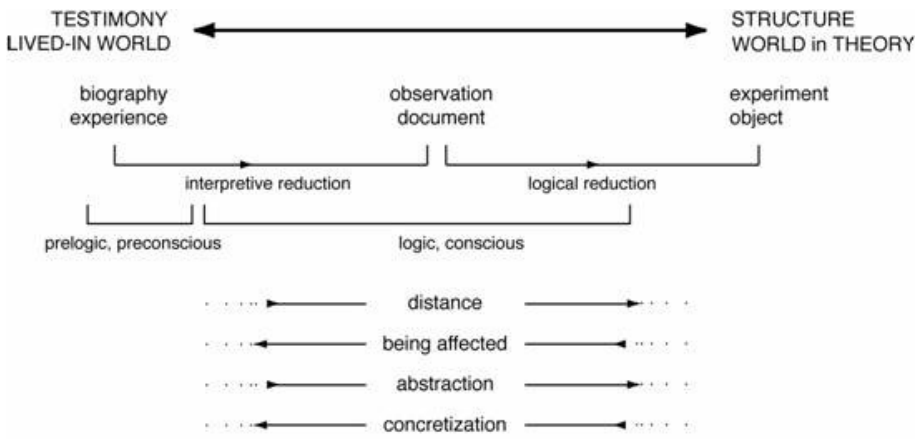

Figure 2: Research results can be distinguished in terms of how near or distant the conceptualizations are to the way in which the people under study experience their everyday world. [21]

3. In Search of Alternatives: Two Demonstrations

In this section, I attempt two brief demonstrations of how researchers can write books about what they have done and how they have done it, and which subsequently has come to be accepted in the field as valid research. Much as expert cooks do not follow *a* or *the* recipe, expert researchers do not conducted their investigations following a blue print—the method, methodology—but rather, in a "protracted and exacting task that is accomplished little by little, through a whole series of small rectifications and amendments inspired by what is called *le métier*, the "know-how," that is, by the set of practical principles that orients choices at once minute and decisive" (BOURDIEU, 1992, p.228). My two examples come from the same study, which I conducted together with my graduate student Michael BOWEN over a three-year period among ecologists working at several universities in Western Canada and, in some cases, working for other research institutions. Although we had started our research as educators, interested in finding out about how science is conducted for the purpose of creating learning environments for middle and high school students that showed some likeness with the way in which science really is done—*authentic school science*—our focus and interest had turned to science studies (e.g., ROTH & BOWEN, 1999b), sociology (e.g., ROTH & BOWEN, 2001), and anthropology (e.g., ROTH, 2004a). [22]

Both Michael BOWEN and I had been trained natural scientists—biology and physics, respectively—and statisticians; but at the time we started our ethnographic work, we have had eight years of experience doing large-scale interpretive research studies in schools. Doing the ethnography together with Michael BOWEN provided a set of opportunities to deal with issues that potentially weaken the strengths of findings from interpretive (observation) studies. [23]

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3.1 Ethnography

Although this narrative account of what we did is necessarily and inherently linear —this is the nature of language, which, written or spoken, always unfolds in time, one word at a time—research is by no means linear. The research questions already imply audiences of potential articles, our competencies in the *doing* of research, and potential participants in this research project. [24]

3.1.1 Research questions

We conducted our ethnographic study of knowing and learning among ecologists because we wanted to design formal learning environments that provided younger students with the opportunity to do learn doing research with some family resemblance to the real thing by doing research—in the spirit of HEGEL's precept already cited above. Research questions do not emerge from a vacuum but usually have their cultural-historical and biographical origins. In our case, we had specific interests knowing more about scientific communities, especially knowing more than we have come to know through our own scientific training. Unlike practiced in some universities, where graduate students are encouraged to research just anything they are interested in, and this independently of the fact whether such research has already been conducted—research that aspires to contribute over and above what a community of practice currently knows orients itself toward and transgresses that which is known. (Graduate students usually do not know what the cutting edge of knowledge is, which is why I involve them in *my* research that in turn provides them with funding.) This requires deep familiarity with the current literature. From our readings, we knew that previous ethnographies had studied laboratories and that field ecology, because it has some resemblance to anthropology, often involves a lone researcher, probably would be different. We also were interested in ecology, because our initial work in schools focused on eighth-grade students doing ecological research and because ecology is much more accessible for students than physics (my own discipline) and chemistry. [25]

3.1.2 Recruitment and negotiating access

Recruitment and access are mediated by what we want to research, on the one hand, and by the contingencies of the field, on the other. As ethnographers, we cannot just decide to work with one or the other community, involving this or that person, but our choice emerges from a dialectic relation of the two. In our situation, Michael BOWEN already had a M.Sc. in biology, he knew and was "hanging out" with biology graduate students while doing his Ph.D. in education, and he knew biologists in the area from a research stint a decade prior to his Ph.D. research. It is as part of his acquaintance with the culture that he came to meet Samantha, a doctoral student doing research on various lizard species. Michael did not just ask her to be a subject but declared his interest in terms of our research question. To allay any negative emotions that scientists often experience when they think of themselves as *guinea pigs*—in my experience, scientists more often than other research participants make this association—he

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offered his services as a research assistant. I had previously read a book (COY, 1989), in which there were several chapters promoting apprenticeship as an ethnographic research method. Michael and I already were using the metaphor of apprenticeship for thinking about learning science in middle and high schools, so extending it to learning about the culture of ecology through an apprenticeship lived while serving as research assistant made a lot of sense. Samantha agreed, and Michael was to spend about seven weeks with her in the same field camp. During the following summer, I spent a week with Samantha. [26]

3.1.3 Participants and context

Michael had many informal conversations with Samantha prior to going into the field with her. As part of these conversations and conversations with other students at the university, he found out that among her peers (graduate students and professors), Samantha stood out in her ability to understand mathematical representations and to do statistics. Her undergraduate background was in mathematical biology, and she repeatedly taught a fourth-year undergraduate course in statistics. She extensively used multivariate statistics and was known in the department as a "statistical wizard." [27]

The purpose of Samantha's research was to (a) describe the natural history of a particular lizard species (e.g. body size, habitat preferences, movement patterns); (b) determine basic life history traits (e.g. life span, survivorship, and litter size); and (c) identify the fecundity and survival costs of reproduction. Samantha conducted her research at the northern-most boundary of the area where the particular species was believed to occur. Although southern relatives of the species had been researched by others on occasion before, very little was known about this species. Samantha drew on research on other reptilians for ideas about how to capture life history information, but also thought that there were particular adaptations that her subspecies must have undergone to be able to live so far north. Finding out how to represent the lizard and its environment was central to her work. Her task, therefore, was one of bringing order to this lizard species and the lizards' lifeworld without knowing beforehand what that order might be. This, as I show here, involved becoming intimately familiar with the phenomenal world and structuring it in common (e.g., temperature) or new and not so common ways (distance of capture site to a rock pile, bush, forest edge); these structural aspects of the setting then became starting point for her statistical analyses. [28]

3.1.4 Data sources

Rooted in our earlier work that was largely directed toward educational psychologists and science educators, Michael and I were oriented to establishing massive amounts of materials, our data sources, from which we constructed our data that we used in support of our narratives. Here I make a distinction between *data* and *data source* that not many research yet make (ERCIKAN & ROTH, in press). In my practice, *data* are used in support of claims, they show and are used to

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teach about patterns we observe. *Data sources*, on the other hand, are the materials we collect and which constitute the resources for constructing data. [29]

Initially, we collected anything and everything that we can think of as having to do with the situation we study. The idea is that the more material we have, the more our subsequent research narratives are constrained—much like forensic scientists and anthropologists try to find as much material evidence as possible to constrain their conclusions about "*who's done it*" or what the causes of some death were. (The popular novels by the University of North Carolina anthropologist Kathy REICHS or the repeatedly copied television series CSI-Crime Scene Investigation [with the rock group THE WHO's song *Who Are You?* as trailer song] are full of examples of evidence and disconfirmation of hypotheses.) Thus, my resource base with respect to this ethnographic study is extensive, consisting of observations recorded in fieldnotes, photographs, audiotaped conversations in and about fieldwork, videotapes of data collection in the field and field laboratory work, and formal interviews conducted during the winter months, which Samantha spent on her home campus. The database further includes a complete set of Samantha's laboratory notes from 1996–97, her dissertation, and the articles and reports published to date based on this work. There are also videotapes of poster sessions at local and national conferences, videotaped talks about her work in university seminars, and all slides and notes used for these diverse presentations. [30]

In addition to collecting anything we come across, we also conducted specific interviews concerning, for example, research practices in general and mathematical practices specifically. This practice, too, has its cultural-historical and biographical origin. As part of my interest in scientific practices, I have come to read *Science in Action* (LATOUR, 1987), in which the author recommends following the representations scientists and engineers construct, transform, and use as part of their work in order to find out about their culture. In my school-based research, I had begun to become interested in the use and interpretation of mathematical representations in general and graphing in particular (ROTH, 1996). To have some references for evaluating how students interpret graphs, I had established some tasks. I was also in the process of asking scientists to interpret the same graphs. In fact, faced with the failure of many scientists to interpret the graphs from undergraduate textbooks we initially struggled with questions about what to do. We then thought about doing ethnographic research without knowing how to do it; this is what started us in the first place. Now, Samantha agreed to do a think-aloud session concerning that set of graphs in addition to participating in a series of more open-ended interviews, for which I invited her to talk about various aspects of her work. I also asked her to explicate or explain some of the ways of doing what she was doing in the field. [31]

To exemplify the kind of data sources produced in our ethnographic work, I provide a field note as an example. At the time, I was staying in a bed and breakfast place near the field camp, which Samantha and some other graduate students and postdoctoral fellows occupied. The field camp was associated with some laboratory space where the lizards that we had captured during the day

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were housed—the gravid females until they had given birth the others until after they had been measured and marked. I might have tried to stay in the camp had I not been accompanied by my wife (Sylvie), who, at the time, was my research assistant and also participated in the collection of data sources (e.g., taking photographs, videotaping). [32]

3.1.5 Field notes

My field notes contain entries of very different types, some are observational, some are methodological, and some are theoretical. Often, these different types of field notes are inseparably intertwined. Let us take a look at the notes that I recorded at the end of one day in the field with Samantha. This was already during the second year of our research; I had conducted five interviews with Samantha during the winter while she resided on main campus, and had read all the notes, interviews, and interpretations Michael BOWEN and I had produced during that time. I had seen all photographs and video that he had shot, and had seen all videotapes from conferences we attended with Samantha and videotapes of presentations she had given at the university. In a way, therefore, my regard was not innocent, but already cued to some particular things all the while attempting to remain open to new and unfamiliar aspects in her work. [33]

The following is my "field note entry" made on the day indicated.

Jul 22, 1998

Research at Dewdney Trail

Hunting

Another scorching day on the Dewdney Trail. When we left, I saw the sign that warned hikers that part of the trail was closed by a private owner who would call the police for trespassing. We begin the day at "First" around 11:30am and measure the directions between a number of the capture sites done with Michael B the year before. Again, there is one site we can't find. Furthermore, we measure a few new distances by using intermediate points—one distance was around 110 meters and the tape measure only 30 m long. In this way, she connected her sites.

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Michael [Roth] flipping a rock in the bushes

After, we move to the last hill and begin searching it. Sam released on the way in the boa and the lizards caught the day before—other than the females. We search and are moving quite quickly. All four at different heights of the hill. By 12:30 we break. During break, Marc measures the temperature on the top of some rocks next to the pine. The temperature maxes out at 60 degrees C. In the shade it is not that hot, but as we are flipping the rocks, some of them are barely touchable.

Stories

During the break, there are again lots of stories. Sam is counting the socks and asks Marc whether he had enough. Marc says yes, and then Sam told about a time that she had lots of them and had to stuff several into the same sock. I asked whether she could tell them apart, and she said, that she noted "the little one" and "the big one" which she then coordinated with the capture site. Then she got of to telling that that, putting them together worked for lizards and boas, but not for salamanders, and then told a story when she had several in the same sock only to realize when she was back home that one had eaten all the others. "Bad idea," she commented. Marc then also had a salamander story. It was a big tiger salamander in the same cache or box with other salamanders, which was eating all the others in the box.

Pam and Marc discussing strategies during a break from flipping rocks

There are also the confirming stories, when Sam suggested that the ponderosa pine made for good habitat. Actually, it was Marc who had started to talk about how much he liked that species. Then both chimed in to talk about how well these pines made for habitats. Marc then told a story about a tree that he had somehow picked out. And eventually he noted that there were three owls living in it.—It started with one of them noting some tree ahead of us and wondered whether it would be a good place for nesting. And large enough. After that, the stories about habitat and nesting.

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We talked about the possibilities of bears coming out. Marc then told another *story* about having seen a mother with her cups, and then he provided a description that they were cinnamon color. Then there was another male bear—also on the road to the ferry—which also was, and this seemed to deserve special mention, cinnamon color.

Both Marc and Sam used the binoculars again to scan the marsh. Usually they begin simply scanning with their bare eyes, then when there seems to be something, they use the binoculars. Today, there were pelicans and the white birds from yesterday which were declassified as pelicans. Now they were much too small, compared to the other birds. They were both speaking admirably of these birds, and Sam announced that on Sunday she might take her bicycle to ride on the dikes to the place where the pelicans seemed to stay.

Another *story* was about a humming bird nest which Marc had found and where the young ones were almost fledging.

I note that they talk a lot about *projects* they or people they know are working on. At one point, Marc talks about a project at Kimberly and there was also another person whom Sam knew who worked on a project. Sam asked, "the same one?" and Marc answered, "not the same but a similar one." Earlier, when we were just out of the car, I asked Sam if she had heard from Belle and whether Belle had finished with her program. She said she didn't know about finishing, but had met her earlier in June and knew that Belle was heading to Central America (Costa Rica) for a *project* which didn't pay, but she got her fare paid for.

I catch a rubber boa. It was more lively than the ones Sam had shown in the nature center and the ones we had seen in the lab. I grab a hold of it and call out. Pam comes and also notes the big belly. She decides that she would empty its stomach later in the lab rather than doing as yesterday. I note that I have some white stuff in my hands, it smells a little which sticks to the hand until later the afternoon. Sam inspects the boa and notes that it is full of biting marks. She suggests it is from a fight. She puts the boa into the sock, hands me a tag which I shove into the ground next to the rock. She hands me the thermometer and I stick the end of the wire under the rock. The temperature drops and drops until it reads 34.1 degrees. Sam then measures the distance and absolute angle together with Sylvie and then asks me what the reading was. I tell her 34.1, and then she asks me to measure outside. I pull the wire outside, but then Sam says that I should switch the button and put the wire back. So I do that, and note that there is a button that can be switched back and forth. What I had actually measured was the outside temperature and the inside was 38.0. Later during the break, she pulled the boa to show Marc the bite marks, when she noted that the boa had coughed up everything and that it smelled putrid. Actually, at this point she remembered that there was an odd smell she had noted earlier but to which she hadn't paid attention. When I haul in the tape, it comes without the metal attached. Sylvie holds it in her hand, and Sam notes that she had only fixed it with some masking tape because it had broken before.

Processing the boa

Marc weighed a plastic tray empty, then with the food item which the boa had coughed up in the sock earlier in the day. The food item came to 3.8 g. Neither Marc nor Sam can come to a decision whether its a mouse (less likely) or some other rodent. Pam decides to put it into a formalin solution with some other critters including two lizards she had accidentally killed in the traps. The boa comes to about 18 g. Sam decides to feed it

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> tomorrow together with Marc, for she wants the boa to have as close as possible the amount it had given off.
>
> Sam measures the length of the boa using a wooden meter, one of the ancient ones, then the snout vent length. Marc notes the length, Sam says something about the right row, but Marc can assure her that it was in the correct row, but he didn't note that it was in the incorrect column. When it came to enter the snout vent length, he was perplexed and I told him that he had entered the data one cell to far to the right. He reenters the number into the correct one, and writes the snout vent length over the previously noted number in the cell to the right.
>
> The boa craps again after measuring it when Sam asks Marc to hold it. Marc then tells the story of one of his friends who had handled a python. They stretched it, and when it was completely stiff, it crapped and it went straight into his friends face. [34]

Reading these notes, readers may find them both unstructured, noting what the day had been, and structured, where I was writing about particular issues highlighted in bold face. For example, Michael BOWEN and I had come to notice the *stories* that ecologists tell each other. Much of what they know is told in the form of stories, not only what happened to them but also conceptual knowledge and field work method. My entry concerning the use of stories both testifies as much to the presence of stories between Marc and Samantha as to my sensitivity to them. *Stories* therefore were something that immediately alerted me to pay attention, as it was a category in our research—we eventually wrote about it as a major vehicle for enculturating ecologists into the discipline (ROTH & BOWEN, 2001). This is but a case of the resource|schema dialectic, where being alert to and having the schema of story allows me to see/hear them, but I would not hear or see them if there was not some physical sounds out there that I could have heard as stories. The notion of *projects* as an organizing feature also is highlighted as the "processing" of an animal. "Processing" is a technical, *emic* term that ecologists used to gloss a series of practices, which I attempted to capture in the field notes, on video, and in photographic form. [35]

Other things I noted were not marked as something that was of particular theoretical interest. That is, at the time I was doing the research I did not know that what I was writing would become a salient and important issue in my later thinking. I had simply made the entry without thinking about *why* this was standing out for me—here, for example, the fact that it was very hot. But in the very marking the hotness I structured the field of resources, the particulars of which became salient to me only later. Also, in these notes I make a comment about the putrid smell and about the possible dangers of bears. Sometime later, I think I was thinking about the *discipline* of ecology and then had an association to physical *discipline*, and most vivid images about the exam rooms where students sit aligned, on hard chairs. Somehow the word *hardship* came to my mind, and then I made an association: becoming a member in the ecology *discipline* (as practice, as mental discipline) involves submitting to the *physical* discipline in the field. Michael BOWEN had heard numerous stories of people who dropped out of field ecology because of the exacting toll fieldwork was taking—the idea for an article was born. Now Michael and my own fieldnotes and memory traces became

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sources for constructing data for a piece that we thought would be of interest to the readers of *Qualitative Sociology* (ROTH & BOWEN, 2001). The subject matter of the article had emerged, was dawning on me, not because I tried to apply this or that method (e.g., grounded theory) to the data, but because of a happen-stance connection I made between two different senses of the word *discipline*. [36]

3.1.6 Use of cameras

The field note also exemplifies my use of photographic cameras. At the time, it was one of the first affordable digital cameras, which I tried out because I could import the photographs into my laptop computer and into documents while in the field. This was their advantage: at the end of the day, I not only wrote the field note but also entered the photographs at the appropriate place. After that I abandoned the use of digital cameras, because of the low quality of reproduction, particularly for the purpose of publication and because the amount of memory available did not allow me to take many pictures directly in the field, forcing me to monitor how many pictures I had left. For the following study, I took several rolls of film and used a SLR camera. Now, however, I am back to using a digital camera. Mine is very small but has sufficient memory for capturing over 180 ultra-high quality images of about 4Mb. This translates into images for publication (300 pixels per inch) of about 7 inch by 5-½ inch. Now I am even more independent than I was with the photographic SLR. [37]

In the field, I use the camera most frequently in a *note-taking* mode, attempting to capture salient moments rather than attempting to get the right artistic shot. I picture anything and everything and do not mind having multiple images of "the same thing." Here, for example, both photographs in the field notes also communicate aspects of the duress of fieldwork, the first in terms of the thickness of the brush I was wading through, the second in terms of the steepness of the slopes on which we were hunting the lizards. I also use the camera to photograph documents, which, using an optical character recognition program, can be used to produce searchable images. This makes me independent of photocopiers and of collecting a lot of sometimes bulky paper and artifacts. This is particularly relevant when the research site is hours away from home or requires a temporary stay in the field. In this research project, it took me a two-hour wait and ferry ride and nearly ten hours of driving to get to the ecological field station. Thinking about how much to take was therefore an important consideration. [38]

3.1.7 Constructing and structuring data sources

Filing data sources is an important aspect of fieldwork, particularly for people who, like I, attempt to collect a lot and a lot of different materials. In the present study, I created an html-based system, which allowed me to keep my notes, import any photographs, or link to other documents that could be reached via a simple click. Thus, the field notes reproduced above actually presented themselves as a page in a browser interface subdivided into two windows (Figure 3). On the left, I had a list of the days in the field. By means of a mouse click, the

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page for the particular day opened, displaying the notes, photographs, and other links. Organizing the materials in this way took a little work, but it allowed me to make linkages and to access a lot of related materials. Even now, years after the actual fieldwork, I can easily find anything I collected or recorded.

Figure 3: In the evenings, I constructed my database including the field notes I was writing and the copies of images and other sources that are linked into the text. The HTML interface organizes the data sources by date and makes links to all the data sources collected on that day. [39]

I use this format to create multiple files, for example, files in which I record initial interpretations, codes, and so on. By means of links I can backtrack from those other files to the original data source. In this way, I establish a close linkage between the ultimate publication and the raw stuff initially collected. Because this linkage is reproducible by others, I thereby establish both an *audit trail* and a documentation of *progressive subjectivity*, both important elements in the process of establishing the credibility of interpretive research (GUBA & LINCOLN, 1989). That is, keeping records in this way achieves two features that really matter to me in my research. On the one hand, my data sources become organized an manageable, although they are substantive in many instances—already as a high school teacher, I had produced 3,500 pages of interview and classroom transcripts in a simple study! On the other hand, I establish structures that allow others to make judgments about the quality of my research. [40]

3.1.8 Reflexivity

The ethnographic research project Michael BOWEN and I conducted had several levels of reflexivity. On the one hand, we both were trained natural scientists,

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each having obtained a master's degree. This was advantageous, because it provided us with easy entry into the scientific culture. It also provided a disadvantage in the sense that there was the danger that comes with *going* or *being native*. Because we *were* scientists, we would have come with veils that mediated our perception. But then, because we were scientist in different fields, we had an advantage again, for it turned out that Michael BOWEN and I saw many situations in very different ways—which turned out to be mediated by our different disciplinary origins. As an ecologist, he was oriented more towards holistic and descriptive understandings (despite his statistician training!), whereas my own ways of seeing were characterized by the mathematical approaches of physicists. Thus, evaluating the analogies and examples Samantha and other ecologists gave while interpreting graphs, Michael was looking at their ecological content, whereas I evaluated at the comparability of the analogies in terms of the mathematical structures of source and target situation (e.g., ROTH & BOWEN, 1999a). [41]

Another reflexive approach was embedded in the way we actually did the fieldwork, particularly during the first year while Michael BOWEN was in the field. Every night he would send photographs, transcripts, and field notes electronically to me. I read them and, being distant from the field, asked him to elaborate, explain, and justify what he had written. I also asked him to seek further information on particular issues. That is, whereas he was "on the ground," coping with the everyday life of collecting materials and assisting Samantha, I had a more distant relationship. This came with two advantages. On the one hand, it turned out exactly those things that Michael forgot to write down but which were necessary for others to understand what he was talking/writing about. On the other hand, I already was involved in conceptualizing, seeing similarities across the different documents he sent. The following excerpt from one of my email notes exemplifies this reflexive relation.

> To: mike2
> From: mroth@uvic.ca (Wolff-Michael Roth)
> Subject: NOtes Jul 14, 15
>
> Hi Mike:
>
> Nice description of field work you guys conducted. A few comments:
>
> 1. As I read this, I was thinking about the relationship of the work you do and knowledge construction and what we ask students to do. Here you guys spend days in the field, flipping rocks and catching a few lizards. You take 'em home and feed them... In schools, we (not you and I) ask students to do a lab in 40 minutes and make sense of it, often in the discovery mode, that is, students are required not just to make sense but to get the right answer too. There is certainly an imbalance that scientists are allowed and paid for doing things slowly, cogit, and come up with a fact (relationship, ...) or two over a period of months. Kids in schools are supposed to be knowledge producing machines... Makes no sense.
>
> 2. Can you describe the lizard catching technique, i.e., what happens after you flip the rock and you see one? You dart for it? Two hands?

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> 3. How are speed trials done? What are they supposed to measure?
>
> 4. Lizard marking technique. I am not sure how the number of toes missing, there is encoded? A700.0, I understood the position of the 7 because of the particular foot that the 100s code. But why 7? If 3 toes are to be coded, the following would make sense to me
>
> 000 = 0 3 toes missing
> 001 = 1 2 toes missing, 0s indicate left 2
> 010 = 2 2 toes missing, left most and right most … [42]

So far, I have described some of the features of ethnographic research, the decisions I have made, the contingencies that mediated my research in various ways, and the emergent character of my understanding of what was happening in the situation I tried to understand. In the following, I provide an example of how I used some of the data sources established during this project in a research article about communication in the scientific workplace (ROTH, 2004b). [43]

3.2 Conversation analysis

I made use of the materials collected as part of the ethnographic study among ecologists in a variety of circumstances. In the article exemplified here, I grounded important aspects of my argument in some of the videotaped materials from the study. That is, in this section, readers encounter more of the kind of data sources I established—but I focus on analyzing and interpreting data sources and the construction of the data. Here, I articulate the background of the paper, provide an excerpt from the manuscript featuring data and analysis, issues in and of transcribing. [44]

3.2.1 Background

I had become interested in studying language and communication both in schools and workplaces. Much of the research especially in schools focuses on communication in terms of words. However, I was aware of a small number of researchers—including Christian HEATH and Charles GOODWIN—who had begun to look at the role of gestures and body position to communicative action. While I was doing another ethnographic study in a biology laboratory, I was struck by how the researchers appeared to know what others were thinking even without a single word being exchanged. Nothing was being said and, in fact, if someone had talked about what was currently going on—e.g., on the monitor—it would have been considered inappropriate for someone who knows. That is, I had a sense that there were levels of communication that went beyond simple words, gestures, and body positions. But what was it and how should it be framed? The call for papers for conference on workplace communication in Glasgow, Scotland, became the occasion to track the issue. The conference was interesting because of its topic *and* because it was in Scotland, a country I had not visited before. That is, the paper had its origin in part in my desire to see Scotland, which, if I wanted my travel to be paid out of research grants, required me to present a

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paper. (This may seem a lowly reason, but is in fact an integral aspect of our life as academic, as I know from the conference narratives of my colleagues.) [45]

Whereas HEATH and GOODWIN had begun to include body orientation and gestural deixis in their analysis of verbal communication, the world in which human agents are embodied is merely taken as ground over, against, and about which the communication takes place. In my study, based on, and in extension of, my previous research in school science classrooms, I proposed to include perceptual modalities for analyzing communication, which was therefore, consistent with recent work in situated cognition, understood as distributed across verbal, gestural and perceptual modalities. I wanted to use detailed analyses of workplace situations to support the argument that the unit of analysis for pragmatic studies of communicative action at the workplace should account for all three rather than only one or two of these modalities. [46]

3.2.2 Data and analysis: The results

Among the data sources to be analyzed, I included the materials from the new ethnographic study. But I remembered that there were many variables in Samantha's work that emerged in the course of her work. I also remembered a particular incidence while the variable "rock pile" emerged in and through the interactions with a research assistant. I went back to the videotape and original transcript, and, because I wanted to show how the variable emerged in real time, made a refined transcript that followed the conventions of conversation analysis (CA). I did this because I had become familiar with CA as a means of uncovering the method used by participants in providing each other with the sense of what is going on; I knew two studies that looked at how research and the interpretation of data was done in real time, and both were grounded in ethnomethodology (GARFINKEL, LYNCH, & LIVINGSTON, 1981; WOOLGAR, 1990). Thus, I chose CA because not so much on explicit grounds but based on a sense that what I was going to do would be recognizable as CA. The following excerpt from my manuscript exhibits a small piece of the data and the text that followed.

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Episode 5: CRE 23/07/97 28:53

01	S:	I just wonder what would classify- it would be, we need to come up with a cri- (0.3) terion for what's a rock (.) pile.	
02	B:	Yeah.	
03		[(16.9)	
+		[((Both stare at a rock assembly	
		next to them))	
04	S:	Is that, so [that wouldn't	
+		[((*Points to ground,*	
+		classify as a rock pile?	
		circular gesture above rocks))	
05	B:	Yeah, and then there is, like if you said it was like ten rocks or something within a half-meter radius or something.	
06		[(9.1)	
+		[((Sam, as Belle, closely looks	
		at the rocks to be classified))	
07	B:	Sorry.	
08	S:	Yeah.	
09	B:	I am causing you problems?	
10	S:	No, it's just, I am just trying to think of how to classify (.) and then there is the additional problem that I've already-	
11		(1.4)	
12	B:	Counted without [doing it so-	
13	S:	[Measured all of these sites without doing it.	
14	B:	But it doesn't matter.	
15	S:	The intention was that this nearest rock distance would esti- (0.2) would be a measure of that (.) but I am not sure if it necessarily is (.) because there is cases when there is (0.9) zero distance but not a rock pile.	
16	B:	Yeah.	
17		(2.5)	
18	S:	Well let's try it. (1.3) Maybe develop something or other.	

"In the first instance, Sam points to the ground, orienting Belle to the area at their feet, and asked Belle whether the unspecified 'it' would count as a rock pile. Though as a research assistant, Belle's status is not that of an equal, Sam takes her input (Belle nods) as a valid case of categorization. Belle then suggests a possible operationalization (line 05). If 'it' has 'ten or more rocks within half a meter radius', it is a rock pile. Her utterance in fact offers a structure for going about doing the operationalization rather than an operationalization: in the same utterance, she uses twice the vague quantifier 'like' and the twice the placeholder 'or something'. That is, the vague language serves interactive purpose (e.g., Jucker et al., 2003), allowing her to leave the content of the operationalization open all the while proposing the structure of arriving at one through subsequent interactions. At the end of the episode (line 18), Samantha takes up and

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> thereby confirms the openness toward settlement of the content of the operationalization by using vague category identifiers ‘something or other'.
>
> Sam does not respond at first; she then articulates her concern with the fact that she has already done a number of sites (line 10). That is, she has already collected the measures she wanted at a range of sites without having collected the information as to whether a capture site is in a rock pile or not. She raises an additional concern: there are zero distances to the next rock, but the capture site is not a rock pile (for example, when a lizard was caught on top of a rock). In the end, she decides that they should try collecting an additional measurement on each capture site that involves classifying ‘it', and if ‘it' is a rock pile, measuring its distance to the capture site.
>
> Here, Sam already has a perceptual sense that the yet-to-be-specified ‘it' does not classify as a rock pile (line 04). That is, Sam has a sense that the perceptual gestalt does not lend itself to be articulated as ‘rock pile' but she does not have a way of telling or gesturing it from its alternative. An alignment with Belle does not yet exist. However, within their participant framework, she can imply distinctions by pointing to one or the other entity (‘it') even though they do not yet have a verbal description to communicate a distinction. Which ‘its' are rock piles and which ‘its' are not rock piles is a matter of an emergent scheme of categorization that draws on the embodied, perceptual sense of the nature of a rock pile. There already exists an initial, perception-based sense for telling which ‘its' are rock piles and which are not. But each instant requires a renewed interaction as to the particulars of the specific classification. That is, by looking at a number of rock piles, their embodied understanding further develops as they attempt to operationalize just what makes an ‘it' a (non) member of the group ‘rock pile'.
>
> As the situation unfolds, Sam and Belle briefly talked about whether it would make sense to bring in junior naturalists to resample all previous sites to get the information missing because of the introduction of a new variable. The next episode relevant to the nature of rock piles arises when they attempt to make their first classification (line 19). Sam questions whether a particular lizard capture site lies in a rock pile or not. Rather than beginning with the criterion stated earlier by Belle (line 05), she draws on her embodied sense to make two contrasting classifications. She points at the ‘it' in front of her, which she articulates as not being a member of the class, and contrasts this with another ‘it', which she does regard as a member (line 20). By using an indicator of propositional attitude (Jucker et al., 2003) ‘I would say', she sustains the unspecified and open nature of the ‘it'." [47]

This text was shaped by my intent to write an analysis that will arrive at an empirical account that ideally contains three elements: (a) an account of the action or actions that are accomplished including compelling evidence from the material sources; (b) a grounding of the formulated action in the lifeworld of the members; and (c) an explication of what it is about the observable evidence (manipulation, gesture, talk) that makes the evidence a proper, witnessable account of what is really being done (SCHEGLOFF, 1996). [48]

Here, the text that follows the data has two complementary functions. First, it articulates what is there in the data to be seen; it is a way of describing the material reality of the transcript and photographic materials to shape what becomes salient to the reader. Second and related, it is a way of teaching

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readers *how* to look at these materials to see what the author (I) suggests to be seen. In my situation, the paper is about the role of visual aspects in communication. I therefore provide a transcript that exhibits the visual aspects *and* I provide a pedagogy that teaches readers how to see the visual aspects. [49]

3.2.3 The work of transcribing

Although I strive to transcribe all of my tapes as quickly as they are recorded, I do not transcribe them with the accuracy depicted in the example provided. Simple considerations for the amount of time that there is in a single day show that it is prohibitive spending the time required for doing CA-quality transcripts. Thus, the transcript in the article as featured is the end product of a long process rather than the beginning point in my analysis. Normally, I make quick rough word for word transcriptions with measuring such things as time between words and other sounds, overlaps, emphases, and so on. I do, however, include video offprints (see example) wherever I think this to be salient. Sometimes I include the images directly in the rough transcript, especially when there are only a few; at other times I save the images and make hyperlinks in the transcripts that allow me to bring up the image while I am reading. The choice is driven by the memory requirements of images: many images make WORD files so large that the computer becomes too slow and even crashes. [50]

Once I am working on a particular article, such as the one on the visual modes in communication, I transcribe selected episodes in full detail. In the case of the *Journal of Pragmatics* paper, I had made previous field notes about the fact that the Samantha operationalized variables *after* she already knew how to categorize certain situations. Thus, in the context of the rock piles, she already knew what was and what was not a rock pile prior to her definition of a rock pile. In the paper I wanted to show how the operationalization eventually emerged in and through the interactive work with her research assistant. I revisited all the videotapes, identified the episode in which the event occurred, and then transcribed the entire session at the end of which the rock-pile category was established. [51]

3.2.4 The work of analyzing, coding

The way I do this form of analysis is as follows. Once I identified an event as salient and make it the object of analysis, I digitize it so that I can view it over and over again moving rapidly to the places I want to get. I then move image-by-image, second-by-second through the tape, preparing a transcript and writing anything about it that comes to my mind. Sometimes I simply hit the return key, then begin a new line with "COMMENT:" and then write whatever I want to note. Sometimes there are so many things that emerge into my consciousness that I use a pencil to jot down individual words that serve me as external memory in tracking down the different images and ideas that seem to be flooding me from nowhere. I then work the points into my typewritten notes and scratch them off as I am done with them:

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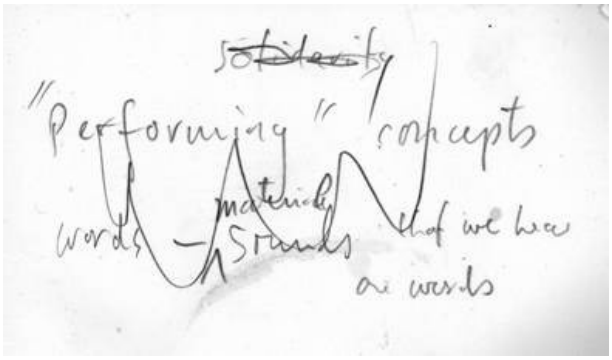

Figure 4: A scratch pad serves me for jotting words and fleeting ideas much faster than I can work them off or enter into my typewritten notes. I then work them off and integrate them into my ongoing writing. [52]

Sometimes students ask me about this statement about "ideas that are flooding me from nowhere." I cannot articulate where they are coming from, otherwise I would have articulated where they had come from. At the same time, one of the conditions for this flooding to happen my be the extended amount of time I spend with particular data sources—I sometimes spend weeks transcribing and analyzing without a specific paper in mind. This extended time attunes me to the data, makes me see events in a new light, and makes me see things that I had not seen before. The following is a typical empirical note (transcript, commentary) made at the time.

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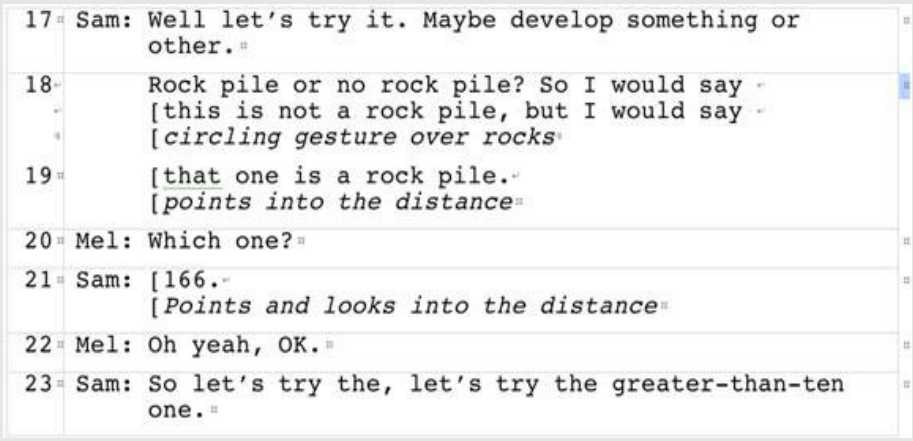
Empirical note:

They turn to classifying a plant and measuring some distances before returning to the classification of the rock pile before them.

Episode



17	Sam:	Well let's try it. Maybe develop something or other.
18		Rock pile or no rock pile? So I would say [this is not a rock pile, but I would say [*circling gesture over rocks*
19		[that one is a rock pile. [*points into the distance*
20	Mel:	Which one?
21	Sam:	[166. [*Points and looks into the distance*
22	Mel:	Oh yeah, OK.
23	Sam:	So let's try the, let's try the greater-than-ten one.

The two have walked to the next capture site just a few meters away.

Theoretical note:

The interactional organisation of classification has built in the production of order that can be defended in other spaces. That is, because it is achieved interactionally, oriented toward the production of intelligible, accountable action, participants not only achieve classification in a collective sense, but also produce the accountability of the phenomenon. Interactional achievement is oriented both internally to the achievement of the classification task and externally, to the accountability within a community of practice. The work of Sam and Mel is not just producing an operational definition that works within the group, but more importantly, producing a definition that works outside—readers of the Ph.D. thesis or scientific articles. [53]

At this stage, I also read articles and books related to making sense in the work-place, for example, a variety of ethnomethodologically oriented studies and, in the process, made theoretical notes that linked what I was reading to my particular data. The theoretical note in the previous paragraph is the outcome of a particular moment of my praxis of working in this manner. As I am writing this essay, a copy of *The Paris Workshop* (BOURDIEU, 1992) lies on my desk, family room table, or night table together with a pencil—which I use to write comments into the book—and a highlighter. When I write, such as at this minute, I return to the highlighted passages and notes and work them into my theoretical account. [54]

3.2.5 The work of writing research

My work of writing research begins with taking field notes and transcribing, continuously elaborating, commenting, summarizing, collecting, and highlighting on previously written materials. Perhaps mediated by my hearing problems, oral discourse is highly ephemeral to me and inaccessible to my analysis. My writing, however, constitutes a form of thinking that I can analyze. Its very materiality

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lends it to objectification, to my distancing from it, to my critical stance, and therefore to my further advances I make. That is, through my writing, my thinking continuously turns upon itself—thinking materially exists in my writing, which becomes the objectified object of further writing/thinking. [55]

At some point, I have the sense that I want to write about a particular issue; at other points, someone invites me to write; and at yet other moments, I am tempted to go to a particular conference because of its topics, the people that attend it, or its location. I then sketch a one-paragraph, very condensed claim that I subsequently attempt to support or undermine through my analysis while working on a full-length paper. In the process, I am not just thinking about the conference but also about a particular journal where I ultimately want to publish the piece. I will immediately use its citation and referencing formats, ascertain that I locate myself in the discipline's intellectual history, read as many articles from the journal as I can to get a sense for it stylistic conventions, in-jokes, turns of phrasings, and so on. That is, I am not telling my story and then try to find an audience for it. Rather, the story I tell—the particular content I explicate and the genre I use—is oriented towards the intended audience. This both constrains as it enables what I am writing, much like the tools of a tradesperson provide both constraints and affordances to doing what he or she does. [56]

3.3 Commentary

In an essay, there is insufficient space to enter into all aspects of conducting and writing research. Nevertheless, I attempt in these lines to demonstrate that there are other ways to write "method" than the currently dominant genres, which, as I argue in the beginning, fail to provide thick descriptions of the very context that mediates what we do and how we do it, our decision making processes, and the emergence of ideas seemingly from nowhere. There is no other way—and here I concur with BOURDIEU—to acquire the social scientific habitus than seeing and experiencing it in the face of the practical choices. This is what I do with my graduate students, of which I can only have a few—taking many, I couldn't be doing what I promise to be doing by accepting graduate students (BOURDIEU, 1992). If our students cannot be there in our presence, the next best thing, as I attempt to show here, are stories that articulate what we are doing and why we are doing it that are as close as possible to our biographical experience of doing research. It turns out that in field ecology, where graduate students learn to do research while spending time on their own in the field, stories are the main medium for communicating aspects of method as lived practice (ROTH & BOWEN, 2001). [57]

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4. Coda

4.1 From methodology to praxis of method

The possibility to teach methods and methodology through books and lectures is part of an ideology more common to North American—and of other regions, where researchers aspire to adopt North American ideals—than to traditional European scholarship. The books on method I am familiar with, some of which I reviewed for this journal, all reproduce this ideology. What we, the more experienced researchers, ought to do instead is provide ways and means for others to learn doing research by researching and to avoid methodology and descriptions of methods (in general) prior to the students' (and other newcomers') understanding of what research is and is about. In this, we ought to heed the advice that

> "Methodology is like spelling of which we say in French: *c'est la science des ânes*, "it is the science of the jackasses." It consists of a compendium of errors of which one can say that you mist be dumb to commit most of them." (BOURDIEU, 1992, p.244) [58]

Learning to do reflexive research means to break with one's ordinary presuppositions and the principles ordinarily at work when we cope with the world. This epistemological rupture involves breaking with (rather than simply celebrating) ordinary experience, common sense, and normal and normalized modes of thought. This requires more than perusing a little text, more than reading a survey text of general methods of doing research; it requires a critical reflexivity that cannot be learned other than with others, who mediate the processes of objectivity the object and the subject of research. In teaching *doing research*, our most vital task is therefore "establish as a fundamental norm of scientific practice the conversion of thought, the revolution of the gaze, the rupture with the preconstructed and with everything that buttresses it in the social order—and in the scientific order …" (BOURDIEU, 1992, pp.251–252). [59]

4.2 A reflexive turn

Interestingly—and perhaps ironically—the authors of textbooks on qualitative research present their materials in a distanced and distancing way, attempting to present precepts that are valid independent of the particulars of specific research contexts. In this, such authors—though they disavow of "objectivism" or "positivism"—make a very similar mistake as have done those that they accuse: they have attempt to move much farther to the right in Figure 2 than is useful in a context of teaching how to do research. [60]

BOURDIEU's (1992) recommendation that there is no better way to acquire the *modus operandi* of research than by experiencing and observing it in practical operation locates teaching and learning in the biographical experience—to the very left in my Figure 2. Idealistic instructions such as "1. Define the research problem. 2. Select a source for the visual material to be used in the study …" fall

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somewhere much farther to the right. The use of idealistic, constructed narratives to exemplify situations fall somewhere in the middle of the continuum. My own narratives about doing research are relatively far on the left without—because of their nature as re-presentation (making present again) of my research they constitute observation documents achieved by interpretive reduction—being able to locate themselves in praxis itself. This, then, leads me to make *another reflexive turn*. [61]

4.3 Another reflexive turn

Here at the end, a reflexive comment. "How," you may ask, "did I come to use *these* examples rather than any other one that I could have selected from the past fifteen years in my research?" I do not exactly know the reasons, but I can describe some of the context within which the use of the data sources emerged. For one, I was more than displeased with the way in which textbook authors present discourse analysis and conversation analysis. Also, I do not think that ethnography or any other "method" is dealt with in a way that is really accessible to novices. I was thinking about presenting something about how I was establishing material evidence, and how I constructed my records. There was a sense that I wanted to have photographs and video, use some screen prints of the way in which data sources are organized, and simply tell some stories of being in the field and doing research rather than talking about doing research or writing about what other people wrote about research. [62]

In this context, I began opening a few files and, in the process, the idea about the ethnography with Samantha emerged into my consciousness. I then searched for the files, clicked through the different sources I had. I then remembered that I had done a paper in which I used CA on some episodes involving Samantha. Then, it occurred to me that rather than presenting materials from different studies to provide a more unified approach by describing what I had done and why in the context of just one study. Then I began to write and follow some of the associations I had to my reading of BOURDIEU, HEIDEGGER, and RICŒUR until I arrived at this point, which constitutes the final sentence of my manuscript. [63]

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