

Qualitative Inquiry

<http://qix.sagepub.com/>

Talking and Thinking About Qualitative Research

Carolyn Ellis, Arthur Bochner, Norman Denzin, Yvonna Lincoln, Janice Morse,
Ronald Pelias and Laurel Richardson

Qualitative Inquiry 2008 14: 254
DOI: 10.1177/1077800407311959

The online version of this article can be found at:
<http://qix.sagepub.com/content/14/2/254>

Published by:



<http://www.sagepublications.com>

Additional services and information for *Qualitative Inquiry* can be found at:

Email Alerts: <http://qix.sagepub.com/cgi/alerts>

Subscriptions: <http://qix.sagepub.com/subscriptions>

Reprints: <http://www.sagepub.com/journalsReprints.nav>

Permissions: <http://www.sagepub.com/journalsPermissions.nav>

Citations: <http://qix.sagepub.com/content/14/2/254.refs.html>

Talking and Thinking About Qualitative Research

Carolyn Ellis

Arthur Bochner

University of South Florida

Norman Denzin

University of Illinois at Urbana-Champaign

Yvonna Lincoln

Texas A&M University

Janice Morse

University of Utah

Ronald Pelias

Southern Illinois University

Laurel Richardson

The Ohio State University

This script comes from an edited transcript of a session titled “Talking and Thinking About Qualitative Research,” which was part of the 2006 International Congress of Qualitative Inquiry, held at the University of Illinois at Urbana-Champaign on May 4-6, 2006. This special session featured scholars informally responding to questions about their personal history with qualitative methods, epiphanies that attracted them to qualitative work or changed their perspectives within the qualitative tradition, ethical crises, exemplary qualitative studies, the current state of qualitative methods, and challenges and goals for the next decade. Panelists included Arthur Bochner (communication), Norman Denzin (sociology/communication/critical studies), Yvonna Lincoln (education), Janice Morse (nursing/anthropology), Ronald Pelias (performance studies/communication), and Laurel Richardson (sociology/gender studies). Carolyn Ellis (communication/sociology) served as organizer and moderator.

Keywords: *qualitative methods; personal narratives; autoethnography; performance studies; epiphanies; ethics in research; narrative; storytelling; personal history*

Talking and Thinking About Qualitative Research¹

Nonchalantly Carolyn finds her way to Room 314A on the top floor of the Illini Union on the campus of the University of Illinois. This is the last

panel following three very full days of the Second International Congress of Qualitative Inquiry Conference. She has given a preconference workshop, attended and participated in many interesting panels, and met with old and new friends. The energy and passion at the conference has been palpable, but now she, like everyone else, is tired, ready to wind down.

“Here it is,” Carolyn says to Laurel and Art, who are walking behind her, kibitzing about a panel they’ve just experienced. The talk stops as they enter the room and are immediately caught up in the bustling activity surrounding them. People crowd around the several hundred, already occupied chairs lined behind rows of long narrow tables. Others seek out available space on the floor of this large room, the size of three regular meeting rooms joined together. The resourceful busily carry in chairs from other rooms and try to find openings for them.

Apparently, this panel has struck a cord. Attendees have come no doubt to be in the presence of well-known academics, Carolyn thinks. But more than that, she believes they’ve come to hear leaders in the field talk informally about themselves and share their personal views on qualitative research. That there are no paper titles in the program is no doubt an attraction. With all the methodological and creative innovations in the field, qualitative scholars, similar to other academics, still spend their time together at conferences listening to their colleagues read long papers that have been scrunched into 12- to 15-min time slots, making them nearly impossible to follow. It is clear from the attendance here and the buzz in the room that conference attendees are hungry for something different.

Carolyn pauses and feels a moment of stage fright as she thinks of meeting their expectations. Then, she springs into action, hurrying to the front of the room to make sure the panelists are ready and that things are in order. Laurel and Art join the other panelists at the front, who are seated behind a large rectangular table facing the audience. A large podium sets off to one side. The only mike in view is attached to the podium. “How are we going to have a discussion in this large room without mikes for everyone?” she wonders, wishing she had checked the room ahead of the session. She moves to handing out her IRB [Institutional Review Board] consent forms to the panelists. Chaos ensues as the panelists try to understand the need for the forms and figure out where to sign. Without further thought, Carolyn moves to the podium and places her tape player on the podium. Then, she checks the mike. She happily notices Cris McRae, one of her graduate students, holding a tape player. Crammed among other young people on the floor in front of her, he sits with his shoulders and legs drawn into himself, unable to even shift his weight without bumping another person. She hopes at least one of their tape recorders picks up the voices.

Carolyn Ellis: Welcome everyone; it's so nice to see all of you here. This is incredible. We didn't expect the room to be so large and full. I am sorry that we don't have chairs for everyone and that we don't have a mike to hand to the participants. I guess we'll have to talk loudly if we want to hold a discussion.

I'd like to start the session by thanking Norman Denzin for hosting the conference. Isn't this one of the best parties you've ever been to? It's been an incredible couple of days. The energy is fantastic. Now having all of you in the same room makes me think the whole building might take off.

Let me give you some background on how this session evolved. I am teaching a graduate class at the University of South Florida in advanced qualitative methods. You've probably seen the 12 students in the class, all running around with their IRB consent forms in one hand and their brand new digital tape recorders in the other. We've all fallen in love with our recorders. For our class, we decided that we would do an ethnography of this conference. So for the last few days, the students have been conducting formal and informal interviews and doing participant observation. It's been a lot of work but also a lot of fun. We hope to get current information about what's going on in qualitative methods and publish some of our papers.

Before arriving here, we went through the IRB process, which seemed appropriate given that this is a conference on "Ethics, Politics and Human Subject Research in the New Millennium." The main problem we had with the IRB was that they didn't want us to identify the people who responded to us. "No, you don't understand," I told them. "These are academics; they like to be cited and see their names in print. If I don't name them, I'll be in trouble!" It took many interactions back and forth and rewriting of the document before we actually got approval to name the people that we interviewed and quoted.

This session grew out of our project. I thought it would be interesting to ask in a group context some of the questions we're addressing in our individual interviews. That way you as the audience would have the advantage of hearing the replies and observing the interaction. My goal then is to ask these distinguished scholars six questions and give each of them 2 minutes to answer each one. Can you imagine a professor answering in 2 minutes? [The audience laughs loudly.] I gave each panelist the questions beforehand, but I don't believe anybody has prepared a 20-minute speech. At the end of each question, I had hoped to spend a few minutes with the panelists responding to each other. But now given the logistics, I don't know if that will take place or not. Good thing I'm into emergent research. Let's just go with the flow and see what happens.

[After introducing the panelists, Carolyn begins.]

Round I

Carolyn: First, I have asked panelists to summarize in 1 to 2 minutes their personal history with qualitative methods. I suggested topics for discussion, such as where and from whom they learned it, approaches and transitions in their approaches, and where they locate themselves now. Ron, would you start? [Ron begins talking from his seat, first facing the panelists, then the audience. Then he stands.]

Ronald Pelias: Can you hear me at all?

Audience: No.

Carolyn: [from the podium] Do you want to speak from here?

Ron: [nods and moves to the podium, while Carolyn sits down at the table] My background is in performance studies, and performance studies as many of you know is primarily a research or disciplinary area that's interested in using performance as a way of knowing. Throughout my academic career, I've been interested in staging performance text to discover the kinds of insights that staging might bring to bear. But when I turned to writing up performance, trying to translate what was discovered on the stage to the page, I found that my arguments often turned to citational proof. I found myself, as our tradition taught, making a rational case. Now there was something very unsatisfying about that for me. Just making a rational case over and over and over again dropped out so much. It dropped out the thing that attracted me to performance in the first place—a method that allows the affective to live. The rational case took away the experience. So I turned to autoethnography and I turned to performative writing as a way of trying to write what I felt was most meaningful in theatrical presentations. I turned to a strategy of writing that lets the heart be present.

Carolyn: Thanks Ron. Norm?

Norman Denzin: Well, I'm going to take 4 minutes, then I'll pass on the second time I'm asked to come up. I'll combine Question 1 and 2 in a narrative.

My tortured history with qualitative methods has gone through three or four phases. I'll sort of move through those and present that to you. I came into this field through sociology in the 1960s in a quantitative sociology department. The department also had a social psychology wing where they taught George Herbert Mead and Herbert Blumer and the Chicago School and symbolic interaction. But as students, our methodology requirements were all quantitative in terms of statistical courses and research design. There was no teaching of qualitative methodology whatsoever. In my doctoral program, we read Mead, Blumer, and Cooley, and I studied with Manford

Kuhn. We read at that time Goffman's [1959] *The Presentation of Self in Everyday Life*, a complete participant observation project, which had just come out. Nobody had any idea how he wrote it. *Boys in White* [Becker, Geer, Hughes, & Strauss, 1961] appeared at that time, and we didn't realize it then, but it was a very quantitative approach to doing qualitative research. It was filled with tables and quasi-statistics. Even as they wrote the book, Becker and Geer were publishing articles on problems of validity in participant observation. So we were funneled through a kind of validity framework into qualitative methods insofar as we used that as our model.

When I came to Illinois in 1966, I was asked to teach field methods, and I had never had a course in field methods. It was at that time that I wrote *The Research Act* [Denzin, 1988], to teach myself how to do field methods. I saw an interactionist methodology based on my reading of the Chicago School at that time. But I was also influenced by the concepts of internal and external validity emerging from Stanley and Campbell [Campbell & Stanley, 1966], Eugene Webb's notion of triangulation [Webb, Campbell, Schwartz, & Sechrest, 1966], and Blumer's [1969] notion that research methods aren't theoretically neutral. So *The Research Act* [Denzin, 1988] was convoluted: Methods are theoretically informed, but they have to be methodologically sensitive to issues of internal and external validity. Therefore, we have to combine multiple methods in what's called triangulation. That's the short story of *The Research Act*.

We were writing this in the middle of the Civil Rights Movement and the Vietnam War, and nobody's acting like that's going on. I'll stop here and come back to my epiphany as a basis of that war.

[Carolyn thanks Norman and nods in Yvonna's direction.]

Yvonna Lincoln: I'd like to give the honorable Senator Denzin some of my time. I'm a very simple person. "My Personal History With Qualitative Research" by Yvonna S. Lincoln or "What I Did in the Summer." I had two degrees in history—in medieval history actually—and so I came to graduate work in education very familiar with working with departments and records. But I also knew something about interviewing because I worked as both an undergraduate and a graduate student with a sociologist named Allen Beagle up at Michigan State University on the Ontonagon County. The project is the Out-Migration Project, which studied why young people were leaving the upper peninsula of Michigan. I did a variety of different kinds of research and data gathering and worked with a bunch of fun graduate students, each of whom had a chunk of this project. I learned a tremendous amount about field work from Allen Beagle and John Rieger, a visual sociologist who still studies rural settings and what's happening to rural land.

My second experience with qualitative research came when I worked on my doctorate, and the RITE project we were working on was in the process of being redesigned by the principle investigators who said, “Oh, we’re going about this all wrong.” That was back in the days when you could go to a reasonable project officer and say, “We mis-designed this study. We want to redesign it.” The project officer would respond, “What is it you want to do?” You’d tell them, and if it made sense, they would say, “Okay and we’ll give you another year’s work while you redesign it.” In the middle of that project, an extremely brilliant woman from the University of Minnesota found out about it and wanted to do some field work. Mary Corcoran came down to work with David Clark, who was the director of my dissertation. I traveled a lot with Mary Corcoran. She taught me so much about field work and how to get ready for it. She was my mentor, and she was and is a stunning woman, a very smart lady. I don’t think she had any training in field methods either. She just picked them up over the years. So that’s my introduction to qualitative research. I would locate myself as a postmodern constructivist . . . I think.

Janice Morse: I’m Jan Morse. I’ve got to be careful of what I say because I’ve made a lot of mistakes and, of course, I don’t want them to be published! I was at Penn State, which is a very quantitative school, where I completed a very fine master’s thesis—except it should have been qualitative. From there, I went to the University of Utah and enrolled in anthropology concurrently with nursing (both heavily qualitative schools) and did two dissertations in Fiji—both mixed-method design. One was quantitative with a little bit of qualitative (anthropology [Morse, 1984a, 1984b]), and one was qualitative with a little bit of quantitative (nursing [Morse, 1989]). My first academic appointment was at the University of Alberta, and I was assigned to teach qualitative research. Not only was it the first class I ever taught, but half of the faculty signed up. So I really had to get my act together.

Carolyn: Thank you. Laurel?

Laurel Richardson: Hi. I’m sitting here and deciding whether to start with my qualitative experience at my birth. I think the fact that I’ve been a marginal person with a foot in two different cultures from my birth on did construct me as a sociologist and later as a qualitative researcher. It’s a gift. I’ve been fortunate to be born into two different cultures. What moved me forward was the capstone course I took as an undergraduate at the University of Chicago. That course was called “The Organization, Methods, and Principles of the Sciences.” In that course, we dealt with intuitive work, deductive work, induction, reduction, and it wasn’t like there was only one way to do science. Rather, there were multiple ways by which you might come to know

something. It struck a responsive chord in me. I've always been engaged with how one "knows" and how one "tells" what one knows. So I'm going to talk about "knowing" now, and I'll talk about "telling" later.

The idea of knowing has always just intrigued me. What claims do I lay to knowledge? How do I know anything? How do I claim I know anything? And being born into two cultures, you never quite know what you do know because it's denied in the other one.

When I went to graduate school, I was fortunate to work with Edward Rose, who was one of the cofounders of ethnomethodology, and although he wouldn't call it qualitative, nevertheless my cohort was engaged in qualitative research. We did "natural" experiments about social life—breaching norms, breaking rules, creating new languages. At that time, I happened to be married to a mathematician, so they had me teaching statistics because clearly knowledge of such comes in the semen or something. So I was teaching the statistics and methods classes, although I had never had a statistics or methods class. At Chicago, we didn't take methods; we just read those nice theory books. I was teaching methods and teaching myself methods and teaching myself statistics at the same time. I was always a week ahead of the students, and I loved it. My dissertation was "Pure Mathematics, Studies in the Sociology of Knowledge." It was an historical analysis and a statistical analysis of math abstracts. (I had also done participant observation and interviews with mathematicians, but this material had no place in the dissertation.)

Very early in my career, I was into content analysis, and I presented my first paper at American Sociological Association using content analysis displayed in nice neat tables. The first paper I sent to *American Sociological Review* was an interpretative qualitative study of women in science. The name of the paper was "Women in Science, Why so Few?" The paper was rejected with a one-line sentence from the editor: "A women obviously wrote this because no one but a woman would be interested." I did quantitative work for a little while after that. I'll come back later for the rest of my qualitative history.

Carolyn: Okay. Art?

Arthur Bochner: Like Jan, I've made a lot of mistakes. Unlike Jan, I've made a career of publishing them! So I think the whole world knows about my history.

Carolyn asked, "From whom did you learn qualitative methods?" I didn't learn qualitative methods from anybody in particular—that is, from any teacher in particular. I just read a lot. I did have the good fortune, during my last year as a doctoral student, of being assigned as a research assistant to a

faculty member who really didn't know what to do with me. So he sent me to the library and told me to go to the current journals and look at every one that appeared to have any connection to communication research. "Take down the name of the editor, what sorts of articles they publish, and the submission instructions and put each one on a separate note card," he told me. So I took out a carrel in the library—in those days, you actually had to go to the library, you couldn't go to the library via the Internet—and I started at A and went to Z. Along the way, I discovered journals I'd never been introduced to in my graduate education, such as *Symbolic Interaction* and *Family Process*, and I would sit there and read the articles in these journals one after another.

I recall one in particular, written by a young sociologist named Norman Denzin—of course, I didn't know he was young at the time—on ethnomethodology. Ethnomethodology, to a large extent, was my first introduction to qualitative methods.

I later discovered in *Family Process* work by the Palo Alto group of systems theorists. Under the influence of Gregory Bateson [1972], they were examining video recordings of families with children who had been diagnosed as schizophrenic. They were doing nothing more than watching the films and taking notes, trying to interpret what was going on in these families. The whole history of the double-blind theory of schizophrenia evolved out of that work.

When I left with my PhD, I was hired to be the token quantitative researcher and methodology teacher in the Department of Speech at Temple University. I was the only empiricist in a department which was completely comprised of humanists doing rhetorical analysis and rhetorical theory. I had to subject my papers to my colleagues in the humanities really, who had a great influence over pushing me toward thinking more about history, culture, rhetoric, and language.

Then, in 1974, I picked up a little book by Jules Henry [1965] called *Pathways to Madness*, which still stands to me as a turning point in my life and career. Henry was a psychoanalyst and an anthropologist who took a room inside the homes of five different families who had children diagnosed with some mental illness and had been institutionalized. He did fieldwork in these families. In his book, he wrote theoretical novelettes about each family. They were narratives—stories that did the work of theory. I said to myself, "That's what I want to do."

Carolyn: [As Carolyn listens to these stories, she can't help but think how she'd tell her story, if she were one of the panelists.]

I think I've been a qualitative methodologist since early in my life. What else does one do in a small Southern town to keep interested and engaged? At the College of William and Mary, I got a BA in sociology and became mesmerized by the work of Erving Goffman. I was fortunate to get involved in an honor's thesis where I did fieldwork with the *Fisher Folk* [Ellis, 1986], an isolated community in the Chesapeake Bay. From that moment on, I was sold on qualitative methods and wanted to combine it with my interests in social psychology. In graduate school, I continued with my fieldwork in isolated fishing communities, working some with Jerry Suttles and Rose Coser. But it was with Gene Weinstein that I developed my social psychological and ethnographic eye.

I've continued being interested in qualitative methods and in the last two decades have turned more to autoethnographic methods, though I still enjoy teaching and doing qualitative work of all kinds. I consider myself a narrative ethnographer.

[After Round I, Carolyn tries to figure out how the panelists might engage in conversation and concludes that it won't work given the physical set up. Besides, given the number of panelists and the limited time, it would be difficult to have significant conversations anyway. Disappointed, she decides to move on to Round II. Maybe on another occasion, she muses.]

Round II

Carolyn: My second question is about epiphanies. Was there a moment of epiphany that got you into qualitative work or once you were there changed your perspective within the general arena of qualitative work? Some of you already started to address this so you can pick up wherever you left off. Ron?

Ron: As I was suggesting before, I had been doing all of this qualitative work in performance studies on the stage and as I was trying to translate that work to the page, it always felt like there was a certain kind of disjoint for me. The moment that allowed me to move into performative writing, autoethnography, and other alternative qualitative methods was when I read Buddy Goodall's [1989] *Casing the Promised Land*. For me, that was my moment of awakening, my epiphany. I had been staging literature and performing poetry and fiction in theatrical frames, but it had never occurred to me that I might be able to use what I was learning on the stage, learning from literature, as a scholarly writing strategy for the page. It was that book in particular that allowed me to see a way that could be done.

Norm: By the end of the 1970s, I had hit a brick wall and other walls as well. I had taken symbolic interaction about as far as I thought I could take it or as far as I thought it could take me. And I was profoundly dissatisfied with the wall the perspective had hit. That is, it had become closed off from all sorts of other discourses that I was being exposed to on this campus in criticism and interpretative theory, which was an interdisciplinary program in the humanities. So by the late 1970s, we were reading European social theory that was just being translated. Lacan, Heidegger, Foucault, the feminisms, and we were moving into semiotics. It was a 3-year project of being saturated with theory that sociology was excluding.

About this time, we formed a traveling minstrel show, and some of the members are on this panel: Carolyn, Laurel, myself, Patricia Clough. We would go to the symbolic interactionist annual meetings and do postmodern performances, and we would get booed and hissed. One of the more profound moments was when Laurel presented the life of Louisa May, the poetic representation of an interview transcript. She later published this as "The Skipped Line" [Richardson, 1993]. The room was like this, packed, and Laurel had distributed her transcript of this interview, which she then proceeded to poetically perform for us. I think it was Harvey Farberman, a symbolic interactionist, who raised his hand and said, "You skipped a line, and therefore the validity of what you are doing is at question. You are not being true to her life and to her words." That skipped line provoked a give and take in the journals and opened this space that we were in, a space of skipped lines, and it was okay to be there. Even if our colleagues didn't like it, that was the space we were going to be in. So then for several years, we did this kind of traveling road show and confronted a fair amount of hostility. But as we did, I think the momentum started to build behind us. I'll pick that up on the next time I come up here.

Yvonna: I'm not sure how to talk about epiphanies because I think I've had a lot of them. I want to talk about an epiphany and an ethical crisis at once. But before I do, you should know that I lived a sheltered childhood. I grew up in a very traditional family. My brothers ran wild. I was locked in the house for 32 years. And so I didn't have very much experience with a lot of stuff, and my epiphany came when I was out on an evaluation contract. I was very new. I had had my doctorate for about a year, and I was doing this evaluation contract and trying to be a good qualitative naturalistic evaluator. I began to suspect that in this project, the middle school coach was molesting some of the boys on one of his teams. And I didn't know what to do about that. That was before we knew very much about laws that said you had to report stuff. I went to the superintendent and said, "I don't have any hard

evidence, but there is a bunch of kids who are telling me things and I think you need to do something about it.” Once the young boys were questioned, it turned out to be true. I thought all qualitative researchers were all good and they encountered only other good people in the world.

My epiphany was finding out there really is evil in the world, there really are hideous things that happen to kids that never should. I have to tell you that that came as an epiphany to me because it was the first time I realized that. I know that really sounds stupid—see this is one of the things that doesn’t go in, right?—but this is the first time that I felt in my whole life that I had come face to face with what I geminately would describe as evil rather than bad or rude or discourteous or unchristian. It was evil, just evil. I was very young; I don’t think I was but 31 years old. It was quite frightening, and that was an epiphany for me.

Jan: I haven’t been lucky enough to have an epiphany. But what happened at the University of Alberta comes under the category of “what doesn’t kill you makes you stronger.” Every time I wanted to do research in the hospital, I had to go forward to the IRB and to a medical committee for approval and funding. They would ask me questions about qualitative inquiry and then my application would be tabled, and I would have to come back the next month for another round. But it was that that made me a good methodologist. It made me confident about what I was doing. It taught me the answers that were sometimes not in the book. And it gave me the skills to explain the nuances and assumptions of qualitative inquiry.

But they made things difficult for a green faculty member. For example, I wanted to do a project that took restraints off the elderly confused in the psychogeriatric ward. Now it’s not very nice to do research and to tie somebody up—this is the early to mid-’80s—but it’s okay to take restraints off. So my plan was to videotape four elderly restrained patients for a week, to remove the restraints, and to tape for another week to document any behavioral change. The difficult thing was to keep these people safe while they were unrestrained. I took the proposal to the psychic-geriatric unit. Of 24 beds, 22 of those people were restrained. And of course, my proposal was tabled, and I had to go and lobby each of the committee members and think of a good reason why they should give \$80,000 dollars to do this research. The following month they approved the proposal, and I danced over to the unit and said, “Hey guys I can do this research now!” And they said, “Jan, your proposal was such a good idea, we’ve taken all the restraints off.” But they had four patients who were incorrigible and still restrained, and I got to select two of those “difficult” patients [McHutchion & Morse, 1989; Morse & McHutchion, 1991]—which was interesting itself. But the bottom line is,

if you want to make change, you do not have to *actually do* research—just threaten to do it.

Laurel: I'm going to take some minutes off my Question 4 and use them here. I love the word *epiphany*. Every part of your mouth gets going: e-pi-pha-ny. Marvelous word. And I think Norm is the one who's introduced it into our living research vocabulary. I want to quickly talk about two of my epiphanies.

I've always been a qualitative/quantitative researcher—what is now called mixed methods. I find them kind of fun. Mixed-up methods. But I had a major car accident and was in a coma for some while, and I when I came out of that coma, I was not able to do my fourth-grade mathematics. That was a life changing epiphany. I lived through that, but my mind was pretty scrambled up. My first paper was a power analysis of *Paradise Lost* [Milton, 1667/2003]. I hadn't read *Paradise Lost* since college. I didn't even know I knew it, but I did. Things were scrambled up, but I wanted to continue being an academic. My department didn't want to tenure me because I might be brain damaged (little did they know what was yet to come). At that time, feminism was growing, but there was no structured way of teaching students about gender issues. I was a feminist, and so I decided to write a textbook, *The Dynamics of Sex and Gender* [Richardson, 1977]. Some of you may have read that book. Writing that book introduced a new field and helped establish it as one that did not require knowledge of statistics to make sense of the world. That is, to establish gender studies, women's studies, sociology of women, as fields of knowledge accessible to everybody. People without advanced mathematics. And through the writing, I retaught myself the bases of sociological reasoning. Writing for my life, writing so I would have a life. That was the first epiphany.

The second epiphany was having a book contract on "unwed mothers" and finding myself unable to write. I was frozen. The crisis of representation had truly hit me. I didn't know how to write. For whom do I write? Whose life can I write? What do I say? At the same time, I was experiencing the tension between two sides of myself: the scientist and the poet. I wanted to feel more integrated. How was I going to put myself together?

I ended up writing up a life history interview of the unwed mother, "Louisa May," as a narrative poem, and presenting it at a sociology conference. This experience—along with talking with others who were also involved with writing themselves out of the crisis of representation—created the space in the discipline and in our world where we could be a community. People who were interested in altering qualitative methods, who recognized poststructural thinking, post-postmodernist critique, feminism, queer theory,

and so on could now have a space in which to create community. The experience of performing Louisa May at an ASA convention, where people swore at me and accused me of fabricating my research led to my involvement with others who had their epiphanies in the same space.

I position myself now as a feminist poststructuralist who is very happy to be alive and very happy to be here.

Art: Before I read Norm Denzin's [1989] book, which focused on epiphanies, I didn't know what an epiphany was. Now I see them everywhere. In 1988, my father died suddenly of a heart attack. I was at an academic conference, a National Communication Association convention in New Orleans. And my world was shaken by that experience. I wrote about that in a story I call "Narrative and the Divided Self" [Bochner, 1997]. My father's death exposed to me the cleavage of my experience as a human being and as an academic. I had always struggled with this distance between the personal and the academic. I realized after my father's death that my days were numbered, and it was time to stand up and do what my heart said was important.

At the time, I was in the somewhat enviable position of being the chair of the USF Communication Department, and I felt it my calling, I guess, to develop a new PhD program that very much embodied what Laurel just said about a sociology without quantities. I never believed that communication was the stuff of quantities to begin with. At that time, in 1990, we had the opportunity of developing this program which everyone in my discipline and even many in my home university said would never work. But I was firm in my conviction that there were people out there, especially women, third-world people, indigenous populations, who were yearning for such an opportunity.

I also had a serendipitous meeting with Carolyn Ellis in 1990. I attended a lecture she gave in of all places, the business school, and as I heard her give a short narrative taken from her book *Final Negotiations* [Ellis, 1995], I said to myself, "She's giving my talk." There's someone else out there in another discipline who believes all the things I believe, and that, as they say, was the beginning of a beautiful friendship and the start of our project on ethnographic alternatives.

Carolyn: [With the introduction of her relationship with Art, Carolyn muses about how she would tell her story of epiphanies, all closely tied to relationships she has had.]

My first epiphany occurred when my brother Rex died in 1982 in an airplane crash on his way to visit me. My world was turned upside down, and I think this was the only time in my life I would define myself as depressed. Not only had my brother died, but my partner, Gene Weinstein, was entering

the final stages of a chronic disease. The survey study on jealousy I was doing seemed insignificant, and I craved to explore and try to understand what I was feeling—to get myself out of the depths of despair. That was the beginning of my turn to autoethnography, to exploring and writing about myself and my situation to learn about human behavior. Finally, I was able to connect my love for social psychology with my love for engaged qualitative methods.

My second epiphany came with the death of my partner Gene Weinstein and the responses I got to my writing *Final Negotiations* about our relationship and his dying. I felt the narrative story I was writing was the best sociology I had ever done and to get the varied responses I received was mind opening and mind boggling: “This isn’t sociology or research,” “this threatens the whole sociological enterprise,” and so on. All of it made me more determined to make my case that this was sociology. Norman’s response to a paper I did on introspection—that I was being schizophrenic—helped me move from trying to fit into a mainstream sociological model to finding my own place on the margins, one that connected humanities and social science and advocated for an emotional sociology that cared about people.

My third epiphany occurred when I met Art Bochner and found a like-minded colleague and partner. Together, we created a synergistic relationship and ethnographic project, and we were able to do more together to advance an interpretive and humanistic social science than either of us could have done alone. I count that as one of my luckiest days!

Round III

Carolyn: My third question is about ethics. Have you ever experienced an ethical crisis in your qualitative work? If so, describe and tell what you did about it. If you’ve already answered this question, you might want to pass or you may talk about anything you want.

Ron: I’ve found myself in ethical trouble most often when I’ve written about others without first doing a participant check. So first, I would recommend a participant check. But I should also say that doesn’t always solve the problem. I did a little piece once about my father having cancer, and I checked with him, talked with him, showed him what I had written before it went to publication. When it finally came out and he read it again, he said, “I can’t believe you told everyone I had cancer. I never had cancer.” “Dad,” I said, “remember, I had you look at this awhile back.” “No, I didn’t,” he answered. “I would have never said I had cancer.” He did have cancer and

was successfully treated but had chosen to deny that part of his life. Seeing this description in print, he had difficulty accepting he was being written in that way publicly. Sometimes, people don't realize the materiality of an article or book until it lands on their lap.

I'll share another little anecdote with you. In another piece, I wrote a composite character. I was trying to juxtapose the New Orleans' frats with the New Orleans' "yats." The "yats" speak in that rich New Orleans accent that turns "Where are you at?" into "Where y'at?"—a greeting that translates into "How are you?" I was describing a female "yat," a composite. My sister, who more closely aligned herself with the frats, read this particular passage, thought I was describing her, and was deeply upset. It took her 2 years before she approached me with her concern. And I had to say, "No, it isn't you." All this is simply to say, even after participant checks, whenever we are evoking others or simply creating characters, we have to be ready to deal with the interpersonal consequence of those evocations.

Norm: I reset the challenge . . . to wrestle with this crisis of representation concept that Laurel talked about. I think we may not understand what it is today, what that means to us. And what it meant to me in the early '80s was a profound interruption in the whole project. Here, we had taught ourselves how to do, in my case, Chicago School sociology. I had written a book on my daughter's learning how to speak. *Childhood Socialization* [Denzin, 1977] was a series of essays in which I studied my daughters as they played games and used language. It was in the tradition of Cooley and Mead. I was proud of it.

Then, we get this notion that there is no clear relationship between reality and these representations. That as a writer you are deeply implicated in what you are writing and it is an unstable position, and your methodology is not neutral, but rather, it is constitutive of the very thing you are writing about. So this raises the issue of how do we rearticulate ourselves to the empirical world? And where do we go?

My first attempt to resolve that was really not very satisfactory. I did a long ethnography of *The Alcoholic Self* [Denzin, 1987a] and *The Recovering Alcoholic* [Denzin, 1987b], which was published in two volumes. These were essentially autoethnographic works disguised as traditional ethnography, and I was on almost every page in one way or another. But I wasn't acting like I was writing about myself; I was still struggling with how to write about myself in this space.

I couldn't do that project today. The IRBs had not come forward in the early '80s in the way they are today. Because then, we were still hovering under the whole framework of what anthropologists had been doing for

decades. Which was, you go out in the field and you study people and you lie and you disguise your intentions. And I wasn't lying or disguising my intentions, but I was taking notes in my mind about something I was going to write, which I later wrote. When that was all done, I experienced another crisis, which was that I had no right to do that. What gave me the license to write about those people? Because I had taken the license of my discipline as permission to write what I had written.

So I came to where I stopped doing fieldwork, and I did a series of studies of what I call video ethnography. I turned to cinema and wrote critical books on cinema, on film, on alcoholism, on disruptions in America in the late 1980s and early 1990s. I felt that I had no license to talk about another person any longer.

Now I'll just probably use up the rest of my time. It was when I discovered the performance framework. When I discovered, unbeknownst to me, that there was this group—many of them on this panel—in performance studies, who are creating poetic performative texts based on their own autoethnographic experiences, I began to feel some kind of liberation. So then what I was struggling with was what am I going to write about now if I want to go to this performative space? And that's what led me to my current project, which has been a series of interventions in what I call "Searching for Yellowstone" [see, for example, Denzin, 2001], a reconstitution of history and nature and our rereading of Native Americans in popular culture. I locate myself at the intersection where those events cross. These texts I'm writing now are meant to be interventions into the history, the popular, popular memory. I'm using myself as a focus for moving the critique into the popular, into the current historical moment. And that's how I moved then from the second epiphany to where I am now. Thank you.

Yvonna: [from her seat] I'm passing.

Janice: [from her seat] I'm passing.

Laurel: [as she walks to the podium] What are we talking about?

Carolyn: Ethics

Laurel: When I stopped talking before—the thing about the community—it's good to have the community, but you need to have something beyond that. And I want to make sure we give Norm credit not just for this Congress but in giving us places to publish. Without those publications, none of us gets promoted, tenured, and so on. So not only did the space open where we could think about things differently and meet differently, but Norm really, and Yvonna, they really did provide places for us to publish. Norm, how many journals do you run? And how many handbooks do you edit? So I think that's an important part. The publication absolutely matters. I wanted to

make sure that was clear. I also wanted to thank these two people [Art and Carolyn] and Mitch Allen for AltaMira Press [now publisher of Left Coast Press] and the series of qualitative books so that people can write books and have them published. It's not just enough to love each other. There is a real world out there we have to deal with.

Ethics is always an important question for me, and I've had some minor experiences with them. I wrote a book called *New Other Woman: Contemporary Single Women in Affairs With Married Men* [Richardson, 1987] and one of the questions that become more interesting was men thinking they were in my book when they weren't. Some men thinking their woman came and talked to me when they hadn't. Now those women are at risk, even though I never interviewed them. Whenever you do anything, you don't quite know what the consequences might be. In a later book of mine, *Fields of Play* [Richardson, 1997], I obliquely intimated that my cousin possibly had sex with her father and the baby was born very not okay and died shortly thereafter. The same day her father died . . . my cousins haven't read my book, but my sister and my brother-in-law are just horrified that I used that nonsecret.

In *Travels With Ernest* [Richardson & Lockridge, 2004], a recent book co-authored with my husband, I chose to accept the ethical decision he had made to tell the story of his father having been sexually abused and its role in his subsequent suicide, despite the grief we would both receive from his siblings for having revealed this family secret.

And I'll tell you about an ethical thing going on right now. I won't call it a crisis; I'll call it a challenge. A dear friend of mine is dying, and I'm keeping a journal, I'm writing a book. The working title is *Death of a Friend*. I talk to her on the phone, and I keep my journal. Have I told her I'm studying her? Should I tell her I'm studying her? No, I've not told her. And no, I'm not going to tell her. What if I told her? She has said to me, "I've lost everything and now I am losing my stories." And I have said back, "Would you like me to tell your stories?" "Would you?" she has said. That to me is a greater and more ethically grounded permission than me saying, "Hey, I'm writing about our phone conversations." There are other ethical challenges in this book, one being the telling/not telling of her secrets. So I am thinking and writing now about these ethical challenges. But my ethics always have to be situated ethics. I'm telling her stories, which is what she wants to happen.

Thank you.

Art: The question "What gives us the right to tell their stories?" raises a second question: "Are there stories that come to our attention that need to be told but have not been inquired into or requested?"

I have one example I want to give you briefly. It revolves around this project I did about 5 years ago on geriatric care managers, which was inspired by my experiences during the last 4 years of my mother's life when she was declining into dementia and Alzheimer's disease. I had received consent from a geriatric care manager I had interviewed three or four times previously to interview her in the nursing home. I was sitting and doing the interview, and I had my tape recorder going, and she had signed all the consent forms, and we had done all the IRB stuff. Along came a nurse, my mother's nurse practitioner, and the tape recorder was running when she came over and greeted me at the table. She knew I had been looking for her that morning. We began to engage in a very spontaneous, unplanned conversation in which a number of stories were revealed about her and her industry's orientation toward death and dying. I've since written a story about this but have not published it, though I think it's one of the best stories I've ever written. It's called "Paperwork," a title I chose to reflect how bureaucracy and paperwork are given priority over the impulse toward caring and empathy at the end of life.

I have about 90 minutes of tape of this conversation. Should I publish it? I never asked her consent. The story puts her in a very bad light. The people who read the story tell me how much they dislike her, though that really was not my intention. How do you answer a question like this? On the one hand, we need some guidelines about what rights we do have to publish stories that we hear. On the other hand, there's another argument that goes: if they hadn't behaved so badly, we wouldn't have the stories to tell.

Carolyn: I can't help myself. Though I am the moderator, I have to tell an ethical story. I hadn't planned to, but I would really like to join in the conversation, because I did experience a real ethical dilemma.

I was educated as a qualitative methodologist, and I went out into the field and studied isolated fishing communities for my first project. In one community that I studied, most of the people were illiterate. At that time, to get IRB permission meant you just filled out and signed a short form saying "I have permission to study them, and I use pseudonyms" or in some way disguise who they are. My training then led me to think that you go out and interview people, and you participate with them, and you try to get all the information you can, hoping they forget you are a researcher. The more information you get, the better fieldworker you are, especially if you come back with information about their hidden lives.

So I wrote *Fisher Folk* [Ellis, 1986], and to be honest with you, I didn't think that much about how the people would respond, though I thought, "They're illiterate; they'll never read this work." So the work was published.

And unfortunately—well, right now I think that it was fortunate—a professor of mine when I was an undergraduate, who had wanted to publish a book on this community and never had, copied parts of my book, highlighted sections, took them into the community and read them to the fisher folk.

The fisher folk had known I was doing research. But I had been there for 9 years, and they forgot. At this point, to them I was pretty much just Carolyn coming to the community to visit. They were extremely hurt by what they heard. I had described them as smelling like fish and other things equally devastating. These people had become really good friends of mine. I loved them and cared for them, and what I said was very painful for them and also for me. I went back to the community and talked with them. Some people forgave me. Some people never did forgive me.

This event led to a paper I wrote about this experience called “Returning to the Field” [Ellis, 1995]. This experience did not lead me to do autoethnography—which some think—because I was already doing autoethnography when all this happened. But it did lead me to start thinking about the ethics of research in a whole different way. That there are all kinds of ethical issues that the IRB doesn’t concern itself with. These are issues that I talked about in another panel here, issues that occur in particular when you become friends with the people you study. IRBs are concerned only with procedural ethics; they assume that you go into a setting, that you don’t know the people who are there, that you study them, and get out. That’s the end of the relationship. When you develop friendships, as all of you know you do, the ethical issues are deeper and more complex. This experience has led me to be really concerned with doing ethical research from a relational standpoint and to think deeply about what we owe the people whose lives we want to put in our studies. Thank you for indulging me.

Round IV

Carolyn: The next question is: “Do you have a favorite qualitative study and why?” Some of you have addressed that already too, so pass if you don’t want to say more about it.

Ron: I think Carolyn’s question is an impossible question to narrow down to one. So I’m going to name several really quickly. I have to start with Carolyn’s *Final Negotiations* [Ellis, 1995] for the absolute power of that tale. I also think of Carolyn’s *The Ethnographic I* [Ellis, 2004] for its wonderful ambition. I think of Lisa Tillman-Healy’s [2001] *Between Gay and Straight* because of the compelling stance of the researcher. I think of Dwight

Conquergood's article "Rethinking Ethnography" [Conquergood, 1991] that powerfully situates performance in ethnographic work. I think of Lesa Lockford's *Performing Femininity* [Lockford, 2004] for the wealth of her insights and the eloquence of her style. And of course, I think of Norman Denzin's [1996] *Interpretative Ethnography*, which for me is one of the richest summaries of the contributing forces to a qualitative sensibility.

Norm: I just think that everything everybody at this panel has written is my favorite.

Yvonna: Leading an impoverished life as I do, I have two favorite books. I actually have a lot of favorite books, and I've been recommending them to students. I'll mention two. One is Margaret Wolf's [1992] *A Thrice Told Tale*. I love that because she tells the story three different ways. It seems to me to be truly the first experimental text we ever had that experimented with different discourses and rhetorical forms. My other favorite book is something probably serious sociologists think is totally trivial, but I think John Van Maanen's [1988] *Tales of the Field* is the funniest doggone introduction to different genres and to some of the illusions that fieldworkers hand themselves. It's just a wonderful book, and students come back when I assign it and they say, "You need more of these kinds of books."

Jan: I read the question to the letter, so I have articles, not books. The first one that I give all my students is from archeology and is called "The Golden Marshalltown: A Parable for the Archeology of the 1980s," by Kent Flannery [1982], and this is the funniest—but wisest—thing you can ever read! My favorite article that's ethnographic is "Some New Dying Trick: African American Youths 'Choosing' HIV/AIDS" by Silvy Tourigny [1998]. It's about youth in Detroit who choose to become HIV-positive so they can be eligible for the services provided for those with HIV. It's very powerful.

Laurel: I also like everything. There are so many things to read and the things I like are not so much by genre as the impact on me—when I'm surprised, when I'm delighted, when I didn't know it, when it moves me. And what I like best is whatever I last read that moved me.

Art: I echo all the choices of my colleagues on the panel. I'd like to mention a few other works that have had a huge impact on me. Laurel Richardson's [1997] *Fields of Play* and Jane Tompkins' [1997] *My Life in School* are two I'd choose because they tell very important stories about the life histories of teachers and the impact of how the institutional culture under which we labor influences our lives. I strongly believe that we should not only be focusing our critical lens on the sociopolitical culture of the countries in which we live but also turning a critical eye to the institutions that have had so much influence over us.

I also mourn the death of my good friend Dwight Conquergood, and virtually anything that you can find that Conquergood has written is worth the read and will change your life.

One other book that I think is very much, at least in my view, cutting edge is by Ross Gray and Christine Sindling [2002], which we published in our *Ethnographic Alternative* series with AltaMira. The authors studied breast cancer and prostate cancer survivors and based their book on some very traditional psychological research, including interviews with doctors, survivors, and friends of survivors, which was funded by a grant. They did a rather conventional orthodox empirical study and then turned it into a text and a performance script that was directed by a woman theatrical director in Canada. The survivors themselves took part in the performances, which were taken out into the community, including in hospitals throughout Canada during grand rounds. I think this has all the elements of basic research, performance, performative writing, and community activism all in one, and I highly recommend it.

Carolyn: Carolyn muses about her favorite books and is glad she doesn't have to answer the question she has asked.

What would I say? *Presentation of Self*, by Goffman [1959]—a classic. *Tally's Corner* [Liebow, 1967]—one of the first ethnographies that engaged me with its style. Norman's *Interpretive Ethnography* [Denzin, 1996]—extremely useful. Krieger's [1983] *The Mirror Dance*—that showed how writing could be different. Any and just about everything Laurel and Ron write—always evocative and provocative as they take the next step that needs to be taken. Plus, whatever Art writes, especially the theories and stories piece [Bochner, 1994]—yes, I'm biased! Like the work of Norm and Yvonna, Art's work lays the groundwork and framework for all we do. And of course, the books in our *Ethnographic Alternative* series, co-edited with Art. Ruth Behar's [1996] *A Vulnerable Observer*, Carol Rambo's [1995] articles, especially the one on being abused by her father . . . ah, yes, I'm glad I don't have to answer this question.

Round V and VI

Carolyn: [Carolyn looks at her watch and notes that only a little time remains. First, no discussion among the participants; now she wonders if there will be time for audience participation. She knows audience members would like to ask their own questions. How often does one get to ask questions of the leaders in the field? Still, the session is going well. The audience is

quiet and attentive; no one leaves the packed room, in spite of how uncomfortable it must be to be jammed together in their seats and sitting on the hard floor. The room is so hot, she notes, glancing at the open windows along the side of the room and wiping the beads of perspiration off her face. She stands and says:]

I'd like to combine Questions 5 and 6. Then, hopefully we will have time for the audience to ask a few questions at the end. Question 5 concerns the current state of qualitative methods. How would you characterize the current state of qualitative research? For example, what are its strengths and tensions? How do you see the connection of qualitative work to humanities and to arts and sciences? To politics, culture, and social justice? And then, I'm going to add challenges and goals. What are the major challenges qualitative researchers face in the next decade? What would you most like to happen in qualitative research politically, practically, and/or academically and intellectually? What goals do you have for yourself in your work? And yes, you have only 2 minutes to answer. [The audience laughs.]

Ron: One of the things that stops me with Carolyn's question is that it is really hard to know what exactly we're saying when we say qualitative research. There's a journal in the speech field, *Qualitative Research Reports in Communication*. Not too long ago, I sent them a little essay. It was a simple phenomenology, boringly traditional in its approach. Probably the article was boring in and of itself, but that's another matter. What is interesting to me is that piece came back to me, without review, with the editor's note, saying "I'm sorry, this isn't what we consider to be qualitative work." The question of what constitutes qualitative work and what we are pointing to when we say that becomes pretty tricky.

The other issue Carolyn's question evokes is the relationship among the sciences, the humanities, and the arts. I would like to suggest that all of these fields have been in the business of legislating how the "I" should appear. If we think about literary studies in the last century, we can begin with I. A. Richards [1926] and the distinction he wanted to make between science and pseudoscience. His distinction gave rise to the New Critics and the formalism that is still being taught in many of our undergraduate English departments today. This particular work takes as its primary task to discover, in part by removing the biographical and psychological dimensions of the reader, what is in the text. The idea is to get it right, to nail it down. This is a desire for objective truth rather than for subjective truth.

An alternative logic situates the "I" quite differently; it invites the articulation of feeling and the enunciation of the possible. So this privileging of the affective, the uncertain, and the possible seems to me a different way

of thinking about qualitative work. There is a considerable amount of qualitative work that in many ways still mimics what's going in the sciences and certain branches of the humanities and certain branches of the arts. The work I like best—and I believe this is where the strength of qualitative methods resides—is when it articulates how claims matter on the level of the individual, when it shows how the material consequences of discursive systems, legislative policies, and interpersonal interactions happen on individual bodies.

Norm: When Yvonna and I put forth the notion of moments of inquiry, originally five moments I think, or seven moments, what we were attempting to capture is that in the last two decades there has been a profound revolution in the thinking about and the doing of qualitative inquiry. It's as if the field, in my mind, had sort of reached a certain steady state for almost a half century, and it just kind of stopped developing. Suddenly it went into fast forward and went through so many iterations over the last two decades. Many of those iterations have been talked about in the editions of the *Handbook of Qualitative Research* [Denzin & Lincoln, 1994, 2000, 2005]. So there is so much to stay on top of and to understand in terms of these developments that weren't there when those of us on this panel came into the field and taught ourselves to do qualitative research. At the same time, to reflexively reflect on all of these transformations and to locate ourselves in the present in those spaces is a profound challenge because we really haven't had time to digest all of these changes and transformations.

As they've happened so quickly, I think there are centrifugal forces or contrary forces that are trying to move us back to what it was like before these transformations happened. As if we could go back to *Boys in White* [Becker et al., 1961] and stop time and say there's this group who don't count, they don't do numbers, but they contribute to the study of the world through their mixed methodologies and their interviews and their narratives and their life stories and their case studies. And we like them, and we have one or two of them on our faculty, and they publish and they do good work and they're going to get tenure and kind of leave it at that. But for this cohort that has come to this conference the last two years, the call is radically different. It is to transform and change the spaces in which we exist in the academy. And to take hold of the terms that define our existence in relationship to the other disciplines and the journals and the apparatuses and the departments and tenure and recruitment and teaching and instruction and funding and publishing and journals. To take hold of our own existence, our own history, and make it into a dream that was there from the beginning when we were called into this space. To do something different than what we had been doing up until we got here.

[The audience applauds loudly.]

Laurel: [to Yvonna] “How will you follow that ‘I Have a Dream’ speech?”

[The panelists roar with laughter.]

Yvonna: Oh man. . . . I actually have two sets of questions from Carolyn and so I’m going to work off both of them. I had this long conversation with Lisa Shaw today. And she said, “What do you think about where qualitative research is?” I outlined a theory very different from what she’d been hearing. I think one of the strengths is the growing number of practitioners. And I think unless you’re going to take those people out and shoot them, qualitative researchers are not going away. I’m sorry Grover Whitehurst [Assistant Secretary of Education with the U.S. Department of Education]. I think that we have a compelling story to tell, and I think there are more and more people interested in being a part of creating that narrative. So I think one of the strengths is that we’re onto something with qualitative research, and it’s not going to go away. And Bush can hope as much as he wants, and we’re not going anywhere.

I have a very hopeful look about the future even though I don’t have a very hopeful look about funding for the future. I think that this is a field ripe and rife with tensions. Sometimes they get annoying so I just go back to my room in the hotel. But sometimes the tensions are points where there is highly fruitful discussions, debate, and argumentation going on. One of those tensions currently going around is the influence of the right-wing methodological fundamentalists on qualitative research. I think one of the tensions has to do with the regulatory aspects of IRBs. One of the tensions has to do with how we describe ourselves, and this gets back to the story Ron was telling. Wait a minute, phenomenology is pretty fundamental, what do you mean you aren’t interested in this kind of qualitative research? What exactly are you interested in if you aren’t interested in this?

I think there are a lot of varieties, brands, subspecies, whatever, drifting around. Many of those different models have different criteria for judging them. But we knew that long before Norman and I got cognitively hooked up. Mary Lee Smith told us that in the late ‘70s. I think the tensions are very good. My notion is a metaphoric Gordian knot, and I’m always trying to keep the knot very loose. I rather resist having people wanting to tie down the knot and focus into one model, one paradigm, one set of this, one outline for that.

I like the fact that right now we’re in ferment. That’s where I see us. And I think that being in ferment is not one of the tensions but one of the very real strengths. I think when we come out of the ferment, it has to be not in relation to the National Research Council. It has to be in relation to this

community. That we need to decide who we are. We don't need to decide who we are in light of who Grover Whitehurst and Dick Shavelson [School of Education, Stanford University] say that we are or are not. Those are decisions that we need to engage as a community. We are the scientific community in qualitative research. It's up to us to decide who we will be and when we will be that.

Jan: I want to talk about where we are now and where we are going. At the moment, I think we are in a good position, but we are not in such a fortunate position everywhere. We have pockets where qualitative research is striving to be and pockets where qualitative inquiry is really struggling to stay alive. In his closing address of the QHR [qualitative health research] conference in Edmonton a few months ago, Carl Mitcham, who is a philosopher of science and technology, made a very interesting argument that the sciences in quantitative inquiry have almost outdone themselves—quantitative inquiry is becoming increasingly expensive for smaller and smaller gains in knowledge and discovery. And that the time for qualitative inquiry is coming, for there is still much to discover qualitatively. I kept thinking of the \$28 million project funded [S.U.P.P.O.R.T.] by Robert Wood Johnson on end-of-life care, which had no statistically significant findings. And at the end, they called in qualitative folk to try and save this project.

On one hand, I'm very hopeful for qualitative inquiry. On the other hand, I believe the warnings that Julianne Cheek [Cheek, Garnham, & Quan, 2006] is giving us that we have to watch our back. There is game playing, and as a journal editor, I'm quite aware of the politics of impact factors and the perceived worthiness of information that is now regulating our universities, regulating what we do, and regulating who gets funded. So one part of me is very hopeful and excited but my other eye wary. We have to support each other. We have to cite each other, we have to cite qualitative journals, we have to fight or we are going to die.

Laurel: I started by telling you I was interested in knowing and telling. And I was talking about knowing and now it's time to talk about telling. Simmel! Georg Simmel has been essential to my intellectual life. Simmel is interested in how form shapes content. How what you can say depends on the form you choose. You can't say everything in a particular kind of form. What you might say in a science article, what you might say in a poem, is different. The form is different, and therefore, the content is shaped differently. I know different things through different forms. What I'm so excited about for contemporary qualitative research is how the forms have burgeoned. How there are so many different ways in which people can tell their stories. Not just on the written page. On the Internet. Artistically. I'm doing altered

books, as some of you know, which is another form. People do drama, they do performance, and they do music. The formats are just expanding. Which means there are multiple ways, more and more different ways by which stories can be told and through which qualitative research can be reported and things can be known.

In most universities, you don't have to do that five-chapter discipline thing. You can alter that. You can really expand and do different things. I'm really excited about the burgeoning of different kinds of formats because the content can just be amazing things you might know. What you might know, what I might know from you.

Along with that I've been in Australia for almost 2 months, and I had a fantastic experience doing qualitative research with communities I never thought even knew about qualitative research. The Sisters of Mercy are doing projects around the world with people with AIDS, and I got to work with them. I got to work with circus people; they wanted to know all about qualitative research methods. Three of them have e-mailed me subsequently that they want to get PhDs in qualitative research—because they didn't realize that academia could be so much fun. So we should take a broader view of the spaces where people do research. It's not just large research universities. It's all kinds of places in the world and sites in the world that we just didn't think . . . who thinks that circus performers want to do PhDs in qualitative methods. That's fantastic. And how they might perform them. It's an emergent thing. And with Internet, my university is going to forgo hard copy dissertations. It's going to all be on the net. So once it's on the net, what difference does it make if someone is dancing on the net or writing a paper on the net? So there are all these new formats and therewith new content. I tend to be an optimist. I'm just always glad to be alive, and I'm glad we have a world.

I like this last question. What goals do you have for you and your work? I have for me the same goals that I have for you. To do what you want to do. Enjoy it, love it, publish it, share it, and stay in my community, please.

Art: I'm just curious, how many of you out there hold tenured positions in universities? [About 15% of hands are raised.] Okay. That's quite a few of you. Some of what I have to say is especially addressed to you because you are in a less vulnerable position than others in the room. It's interesting that we've called these last two meetings a "Congress in Qualitative Inquiry." I think this project is about our own institutional activism and its political qualities. I think the question "What will qualitative inquiry become?" is a political question. And it's in your hands. Most of the panelists here have a lot of gray hair. We've been around the block. We've done what little we can to initiate and advance this cause. The future is in your hands.

As I said in my opening remarks, I can remember a time in my own field, communication, when quantitative empiricists were just trying to get their foot in the door. There was just one per department. And that was only 30 years ago. Now, by and large, they are the gate keepers; they control the field. Thirty years might sound like a long time to some of you, but it's really not. Fifteen years ago, we didn't have *Qualitative Inquiry*. We didn't have the *Handbooks*. We didn't have the corpus of projects, or not many of them, that have been discussed or referenced at this congress.

It is up to you, the next generation of qualitative researchers, to go back to your universities, or wherever you are working, and do what you can, in an activist way, to inspire some of the changes we've been talking about at this Congress. I think this is a tremendously important part of our whole experience here. At this conference, I had conversations in the halls and in meetings with nurses, psychologists, social workers, educators, sociologists, performance artists, communication theorists, ethicists, political scientists, health and family practitioners, and counselors. All of them at this meeting. All of them coming because they feel a calling and a cause. Now we have to engage in some sort of political activism wherever we work. If you're called upon or if you have any opportunity to serve on editorial boards, to serve in positions of any influence or power in whatever discipline you belong to or in whatever institution you are employed, you need to do this. And to nurture and mentor young students—I can't think of anything that is any more important than that. Giving them hope, encouragement, support, assistance, and the wisdom of your own experience. We all have a responsibility, an ethical and moral responsibility, if we believe in the work that is being done here, to do whatever we can individually and collectively to shape the future. Meet your colleagues in other departments who are doing similar work. Talk to each other. Have conversations. Form institutes. Form work groups. Get involved with each other. Don't isolate yourselves. The future belongs to you.

[The audience claps loudly.]

Carolyn: Carolyn feels she has been at a rally. "I'm ready to answer the call," she muses. "Take me," she wants to stand up and say. "Take me. I'll go." She feels the positive energy of the current state of qualitative methods right here circulating through this room. The excitement is with the interpretive/participatory/activist qualitative researchers who are called to follow their hearts, not model themselves after quantitative scientists. This panel puts an exclamation mark on the conference, reflecting the positive feelings that have been present among more than 900 delegates from 55 nations who have gathered to attend 16 preconference workshops and present more than 800 papers and performances in more than 180 sessions. What energy! It's

as though people have been let out of a locked closet or better they have unlocked the doors themselves. This is the future. We have moved from standing on the margins of mainstream orthodox research looking for acceptance and instead have formed our own centers, publication outlets, and conferences. Although there still is work to be done and barriers to move past, as these speakers note, the excitement of possibilities fills the air.

The hot room suddenly feels breezy as cold chills run up and down her arms, no doubt the effect of listening to the passion of her colleagues. The rest of the audience seems to feel the moment as well. They sit quietly but lean forward, eyes wide open, taking it all in, wanting more, more speeches like these where panelists have fire in their bellies and emotion in their hearts, panels where people talk about what they feel and the mistakes they made as well as their thoughts and accomplishments, panels where people want their work to matter, panels where audience members feel talked to or with rather than talked at.

[Carolyn stands and speaks.]

Carolyn: I've learned in my time in this profession that professors always fill up whatever time is available. We have managed to fill up the entire 90 minutes. We didn't get to have a discussion, but we at least got to talk informally and personally and hear some things about what scholars think and feel that you don't normally get to hear at formal conference presentations. My thanks to the panelists and to the audience for coming.

The panel ends with Norman inviting everyone to the town hall meeting that follows where a constitution for the organization and a policy on IRBs will be presented for ratification. He invites them to attend that evening an old fashioned cookout with a Cajun band. [For Carolyn and others, the music and the dancing that evening—the embodied engagement—will prove to be another highlight of the conference.]

Carolyn thinks about how to design and implement panels where participants have a chance to engage more with each other in conversations, building on what each other has to say, and where audience members have a chance not just to listen but to speak back and say what's on their minds and in their hearts as well.

But for now, this is fine, real fine. She's ready to dance.

Note

1. Postscript on method: Thanks to Christopher McRae for recording and transcribing this session. After Cris transcribed the original tape, each participant then had the opportunity to read and edit her or his section of the transcript, though I [Carolyn] asked them to preserve the

flow and informality of their speech. I then read the transcript with the minor changes the participants suggested and listened to the tape to get a sense of the emotion and tone of the speeches and the occasion. I edited for consistency, omitting some of the redundancies and unnecessary words but attempting always to keep the meaning of the oral texts. In the rewriting, I added my thoughts and feelings along with descriptions of the panel. Panelists then had an opportunity to respond to this version. This text is a coproduction by all the panelists.

References

- Bateson, G. (1972). *Steps to an ecology of mind: Collected essays in anthropology, psychiatry, evolution, and epistemology*. Chicago: University Of Chicago Press.
- Becker, H., Geer, B., Hughes, E., & Strauss, A. (1961). *Boys in white: Student culture in medical school*. Chicago: University of Chicago Press.
- Behar, R. (1996). *The vulnerable observer: Anthropology that breaks your heart*. Boston: Beacon.
- Blumer, H. (1969). *Symbolic interactionism: Perspective and method*. Englewood Cliffs, NJ: Prentice Hall.
- Bochner, A. (1994). Perspectives on inquiry II: Theories and stories. In M. Knapp & G. R. Miller (Eds.), *Handbook of interpersonal communication* (2nd ed., pp. 21-41). Thousand Oaks, CA: Sage.
- Bochner, A. (1997). It's about time: Narrative and the divided self. *Qualitative Inquiry*, 3, 418-438.
- Campbell, D. T., & Stanley, J. C. (1966). *Experimental and quasi-experimental designs for research*. Chicago: Rand McNally.
- Cheek, J., Garnham, B., & Quan, J. (2006). What's in a number? Issues in providing evidence of impact and quality of researcher(ers). *Qualitative Health Research*, 16(3), 423-435.
- Conquergood, D. (1991). Rethinking ethnography: Towards a critical cultural politics. *Communication Monographs*, 58, 179-194.
- Denzin, N. K. (1977). *Childhood socialization: Studies in the development of language, social behavior, and identity*. San Francisco: Jossey-Bass.
- Denzin, N. (1987a). *The alcoholic self*. Newbury Park, CA: Sage.
- Denzin, N. (1987b). *The recovering alcoholic*. Newbury Park, CA: Sage.
- Denzin, N. K. (1988). *The research act: A theoretical introduction to sociological methods*. New York: Prentice-Hall.
- Denzin, N. K. (1989). *Interpretive interactionism*. Newbury Park, CA: Sage.
- Denzin, N. (1996). *Interpretive ethnography*. Thousand Oaks, CA: Sage.
- Denzin, N. K. (2001). Cowboys and Indians. *Symbolic Interaction*, 25(2), 251-261.
- Denzin, N., & Lincoln, Y. (1994). *The handbook of qualitative research* (1st ed.). Thousand Oaks, CA: Sage.
- Denzin, N., & Lincoln, Y. (2000). *The handbook of qualitative research* (2nd ed.). Thousand Oaks, CA: Sage.
- Denzin, N., & Lincoln, Y. (2005). *The handbook of qualitative research* (3rd ed.). Thousand Oaks, CA: Sage.
- Ellis, C. (1986). *Fisher folk: Two communities on Chesapeake Bay*. Lexington: University Press of Kentucky
- Ellis, C. (1995). Emotional and ethical quagmires in returning to the field. *Journal of Contemporary Ethnography*, 24, 711-713.

- Ellis, C. (1995). *Final negotiations: A story of love, loss, and chronic illness*. Philadelphia: Temple University Press.
- Ellis, C. (2004). *The ethnographic I: A methodological novel about autoethnography*. Walnut Creek, CA: AltaMira.
- Flannery, K. (1982). The golden Marshalltown: A parable for the archeology of the 1980s. *American Anthropologist*, 84(2), 265-278.
- Goffman, E. (1959). *The presentation of self in everyday life*. Garden City, NY: Doubleday Anchor.
- Goodall, H. L. (1989). *Casing a promised land: The autobiography of an organizational detective as cultural ethnographer*. Carbondale: Southern Illinois University Press.
- Gray, R., & Sindling, C. (2002). *Standing ovation: Performing social science research about cancer*. Walnut Creek, CA: AltaMira.
- Henry, J. (1965). *Pathways to madness*. New York: Vintage Books.
- Krieger, S. (1983). *The mirror dance: Identity in a women's community*. Philadelphia: Temple University Press.
- Liebow, E. (1967). *Tally's corner*. Boston: Little, Brown and Co.
- Lockford, L. (2004). *Performing femininity*. Walnut Creek, CA: AltaMira.
- McHutchion, E., & Morse, J. M. (1989). Releasing restraints: A nursing dilemma? *Journal of Gerontological Nursing*, 15(2), 16-21.
- Milton, J. (2003). *Paradise lost*. New York: Penguin Group. (Originally published 1667)
- Morse, J. M. (1984a). Breast- and bottle-feeding: The effect on infant weight gain in the Fiji-Indian neonate. *Ecology of Food and Nutrition*, 15, 109-114.
- Morse, J. M. (1984b). The cultural context of infant feeding in Fiji. *Ecology of Food and Nutrition*, 14, 287-296.
- Morse, J. M. (1989). Cultural responses to parturition: Childbirth in Fiji. *Medical Anthropology*, 12(1), 5-44.
- Morse, J. M., & McHutchion, E. (1991). The behavioral effects of releasing restraints. *Research in Nursing and Health*, 14, 187-196.
- Rambo (Ronai), C. (1995). Multiple reflections of child sex abuse: An argument for a layered account. *Journal of Contemporary Ethnography*, 23, 395-426.
- Richards, I. A. (1926). *Science and poetry*. New York: Norton.
- Richardson, L. (1977). *The dynamics of sex and gender: A sociological perspective*. Chicago: Rand McNally College.
- Richardson, L. (1987). *The new other woman: Single women in relations with married men*. New York: The Free Press.
- Richardson, L. (1993). Poetic representation, ethnographic presentation and transgressive validity: The case of the skipped line. *The Sociological Quarterly*, 34, 695-710.
- Richardson, L. (1997). *Fields of play: Constructing an academic life*. New Brunswick, NJ: Rutgers University Press.
- Richardson, L., & Lockridge, E. (2004). *Travels with Ernest: Crossing the literary/sociological divide*. Walnut Creek, CA: AltaMira.
- Tillmann-Healy, L. (2001). *Between gay and straight: Understanding friendship across sexual orientation*. Walnut Creek, CA: AltaMira.
- Tompkins, J. (1997). *My life in school*. Boston: Addison-Wesley.
- Tourigny, S. (1998). Some new dying trick: African American youths "choosing" HIV/AIDS. *Qualitative Health Research*, 8(2), 149-167.
- Van Maanen, J. (1988). *Tales of the field: On writing ethnography*. Chicago: University of Chicago Press.

Webb, E. J., Campbell, D. T., Schwartz, R. C., & Sechrest, L. (1966). *Unobtrusive measures: Nonreactive research in the social sciences*. Chicago: University of Chicago Press.

Wolf, M. (1992). *A thrice-told tale: Feminism, postmodernism, and ethnographic responsibility*. Stanford, CA: Stanford University Press.

Carolyn Ellis is a professor of communication and sociology and a codirector of the Institute for Human Interpretive Studies at the University of South Florida. Her books include *Fisher Folk: Two Communities on Chesapeake Bay*; *Final Negotiations: A Story of Love, Loss, and Chronic Illness*; and *The Ethnographic I: A Methodological Novel About Autoethnography*. She is currently working on a book titled *Revision: Autoethnographic Reflections on Life and Work*. Her interests are in interpretive and artistic representations of qualitative research, in particular autoethnographic narratives.