

Qualitative Inquiry in Psychology: A Radical Tradition

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Editors' note Psychology's claim to be a legitimate science led it to emulate the hard sciences' laboratory methods and statistical analyses. The "positivist" goal was to identify cause and effect relations among isolated variables that the experimenter could control. Success would enable psychology to "predict and control" behavior in people defined as "subjects" to manipulate. Seeking legitimacy, psychology discarded its traditions of qualitative, interpretive, "softer" methods that examined behavior in the less tidy real world. In this chapter, Louise H. Kidder and Michelle Fine remind us of the history and potential of qualitative methods.

As several chapters in this book point out, mainstream psychology's traditional research practices can sometimes advance critical psychology's goals, as by quantifying the existence of inequality. Yet narrowly focused hypothesis-driven research often misses sources and consequences of injustice that open-ended qualitative methods expose. Reintroduced to psychology by feminists and others over the past two decades, qualitative methods have regained some of their earlier appeal. Positivist approaches, however, remain dominant. In most areas of psychology, researchers seeking to use qualitative methods still must satisfy skeptical funding sources, hard-nosed dissertation and hiring committees, and editors who lack experience with alternative approaches.

Qualitative researchers seek not to manipulate "subjects" but to hear the voice of "participants" who join our search for knowledge and justice. They listen to what people say about their problems and, explicitly or implicitly, about the Good Life and the Good Society. By amplifying the voice of participants, qualitative researchers promote the values of self-determination and human diversity. Through dialogue with participants about the meanings of the data, they foster collaboration and democratic participation. In worrying about participants' well-being, they show caring and compassion. Thus, Kidder and Fine not only point to the merits of qualitative approaches in fully and accurately describing the human condition; they also emphasize the ethics of research, a defining feature of critical psychology.

Qualitative methods and psychology have had a long off and on relationship. It has been an intellectual flirtation, alternately secret and open, perilous and safe. Current debates about positivism and postmodernism give the impression that qualitative work is a new, quasi-scientific departure from tradition, a risky project for psychologists. Graduate students wonder whether their work will still be "psychology" if they use methods that look more like "anthropology." In this essay, we document the relationship between Qualitative methods and psychology and show how they have helped produce a critical psychology.

Qualitative methods and critical psychology are not co-terminous. Qualitative work does not automatically yield critical analyses and critical analyses do not require Qualitative methods. But they fit well together. Qualitative work helps psychologists see how class, race, and gender shape lived experience. By highlighting the subjectivities of researchers and participants, Qualitative methods bring to the fore the relations of knowledge, ethics and negotiated intimacy between researchers and the communities they study.

What Counts as "Qualitative"?

In 1987 we distinguished between two meanings of "qualitative" in ways that we still find useful:

Qualitative work with the *big Q* is field work, participant observation, or ethnography; it consists of a continually changing set of questions without a structured design. The big Q refers to unstructured research, inductive work, hypothesis generation and the development of "grounded theory" (cf. Glaser and Strauss, 1967). Qualitative work with the *small q* consists of open-ended questions embedded in a survey or experiment that has a structure or design. The hypothesis and questions do not change as research progresses. The same questions are asked of everyone. (Kidder and Fine, 1987: 59)

Our task now is to trace the connections between critical psychology and big Q Qualitative methods. We omit the "small q" variety from this chapter because the smaller qualitative moves are not part of a radical tradition; they do not present the same opportunities for critical work.

The work we call "critical" and "Qualitative" extends back more than sixty years. John Dollard in the 1930s conducted field-based Qualitative work on race and class relations in a Southern US town (Dollard, 1937). In the 1940s and 1950s Muzaffer Sherif and his collaborators immersed themselves in the rivalries of a summer boys camp to write about conflict and cooperation (Sherif and Sherif, 1953; Sherif et al., 1988). In the 1950s Leon Festinger and colleagues infiltrated a doomsday sect to observe what happens when prophecies fail (Festinger et al., 1956). In these classic studies social psychologists entered their subjects' lives without structured questionnaires, predetermined variables or research designs and no one doubted that they were doing psychological research. They were men who

became known for their laboratory and experimental findings but they began their careers with field-based Qualitative work. They produced essays and books in which they explored their biases, worried about their relationships and ethics in the field, and enjoyed the texture of field work.

When they wrote, however, about these aspects of Qualitative work, they were concerned that their biases, worries and relationships were *departures* from, indeed limits on, social science. Today these very same biases, worries and relationships are being resurrected as belonging to the heart and soul, the substance, of critical social research. Over the course of the past thirty to forty years, this history of Qualitative work has been recessed, buried in the background and footnotes of research psychology. In critical tradition, we take pleasure in foregrounding this Qualitative legacy within psychology, which appears to be coming full circle.

Four features of Qualitative work facilitate critical analysis:

- 1 *Assuming an open-ended stance* Beginning with a hunch, researchers tentatively form and reform hypotheses. Revising hypotheses is not only permissible but necessary. Surprises abound.
- 2 *Reflecting on subjectivity and bias* Rather than treat their own subjectivity as an obstacle, Qualitative researchers take it as a datum. They examine their own subjectivities as well as their respondents' and note how both parties' biases and personal positions affect data collection and analysis.
- 3 *Worrying about relationships* Even if they follow ethics guidelines and receive approval from human subjects review committees, Qualitative researchers worry about their relationships with respondents and communities. Each day in the field is another test, another opportunity for having the door closed in one's face and being asked to leave the premises.
- 4 *Analyzing open-ended questions and writing kaleidoscopic interpretations* The stance begins with open-ended questions and persists in analyses that provide open-ended multiple, partial and kaleidoscopic interpretations. Like kaleidoscopic images that shift with each turn of the tube, the analysis of Qualitative work can yield multiple patterns from shifting viewpoints.

On Stance

In the early 1930s John Dollard went South from the Psychology Department of Yale University to learn how race operated within the social life of a town he called Southerntown. His stance was self-consciously Qualitative. He understood himself to be a Northern white psychologist from Yale, naive about Southern race relations. He recognized that his naivete necessitated that he be educated by his data:

The basic method used in the study was that of participation in the social life of Southerntown. This social sharing was of two degrees and involved two roles: there was first the casual participation possible as a "yankee down here studying Negroes" and second the more intensive participation and the more specific role of the life history taker . . . The primary research instrument would seem to be the observing human intelligence trying to make sense of the experience; and the experience was full of problems and uncertainty in fact. Perhaps it does not compare well with more objective-seeming instruments, such as a previously prepared set of questions, but as to this question the reader can judge for himself. It has the value of offering to perception the actual, natural human contact with all of the real feelings present and unguarded. (1937: 18)

At the heart of what we are calling a *Qualitative stance* is, as Dollard admits, a desire to make sense of the experience with "all of the real feelings present and unguarded." Unlike the hypothetico-deductive stance in which predetermined hypotheses frame a particular set of questions, a Qualitative stance enables researchers to carve open territory about which they have vague hunches rather than clear predictions. A Qualitative stance encourages broad-based inquiry into intellectual and social spaces which may be strange to the researcher, undocumented in other studies. Propelled by a desire to know what is unknown, to unravel surprises, to be alarmed and jostled in our own thinking, Qualitative researchers embark on a sometimes quite lonely, hard to predict, intellectual adventure.

You can hear in the Dollard example how a Qualitative stance enables, but does not guarantee, a critical analysis that surfs between macro structures and micro psychologies. Because the field of variables is not pre-drawn, the intellectual harvest can move generously across the terrain. The racial, political and economic hierarchies of the South in the early part of this century and individual "Negro" and "white" personalities could be connected, woven together in theory and research. That is, Qualitative researchers have the capacity to analyze across broad territory the nuanced strategies by which power operates through individual and collective psychologies. Not constrained by a predetermined menu of variables whose main effects and interactions we track, Qualitative work first permits a broad look and then forces a narrowing of focus once the map of the field becomes clear. This way of working requires being willing to give up control, going along for the ride, not always having hold of the steering wheel - and still taking good notes.

Letting Go of Control

Qualitative researchers may fantasize being a "fly on the wall," exerting no control at all over what happens next, but documenting it all. Fortunately, both everyday realities and current ethical guidelines make being a "fly," much less a "spy," virtually impossible. But that was not true in the past. When Leon Festinger and his colleagues became undercover researchers in a doomsday cult in 1956 they tried to achieve the status of a "fly" but they admit their failure:

We had to conduct the entire inquiry covertly, without revealing our research purpose, pretending to be merely interested individuals who had been persuaded of the correctness of the belief system and yet taking a passive and uninfluential role in the group . . . [but] our influence on the group [was] somewhat greater than we would like. (1956: 249)

By virtue of human subjects regulations, ethical guidelines and contemporary considerations of researchers' responsibilities, researchers today who want to study a group or a site are usually required to state who they are, make their intentions known, and obtain permission to be there. They might want to be invisible but usually aren't, so they are right to worry about how their presence might affect the people they observe.

Dancing with the Data

Qualitative researchers may lose control during parts of the data collection process, but they regain it as they "dance with the data" and in the dance resume the lead. In hypothetico-deductive work control is guaranteed when only a few variables are allowed onto the stage and satisfaction comes from saying "I knew it! My predictions were right, my hypotheses confirmed." In practice, of course, researchers rarely find all their hypotheses and predictions confirmed (technically, their hypotheses are not disconfirmed, and that is the most they can celebrate). In practice, experimenters and survey researchers are more likely to find one or two of their hypotheses are "safe" (not disconfirmed); they "fail to reject the null hypothesis." The remainder of the results section in a survey or experiment often has a bewildering array of significant results that were not predicted and non-significant findings that should have been significant. Authors then perform an intricate dance in the discussion section to make sense of an unexpected three-way interaction or explain why a predicted two-way interaction did not materialize. Part of a discussion section frequently contains guess work about what the subjects might have been thinking that made the results assume these unexpected patterns. Creating a new dance in the discussion can be satisfying when it works.

In Qualitative research the dance begins early. If you are paying attention, your loss of control will be apparent sooner than you wish. The data jump at you quite unpredictably, forcing the researcher to dance – abandoning or transforming hunches, hypotheses, favorite theories. To illustrate: in a participant observation study conducted in the early 1980s, I (*Michelle*) spent time in a hospital emergency room with a young woman I call Altamese. Altamese had just been gang raped. I was a "rape crisis volunteer." She was the "survivor." We spent hours talking, with Altamese tolerating my awkward and anxious attempts at humor. In embarrassed retrospect, I was trying to "talk her into" ways of coping. Toward this end, I doused her with all that *I knew* would be "good" for her, as a feminist counselor, social psychologist, white woman academic . . . report them, tell your social worker, let your family know, don't keep it in . . .

At some point Altamese had enough of my maternal, if not colonial, advice and politely let me know that she had her own strategies. She would not press charges, or let her family know. From her community (an African American neighborhood in North Philadelphia), rarely was a woman believed by the police. If she told her brothers about the rape, they might go out and kill the accused. If she told a therapist it might help for a moment but the pain would still be within. Once I stopped talking and listened to her story, I could hear her way of making sense, that is, surviving, in a world where neither the justice system nor the streets were very trustworthy; where protecting her mother, brother and children was more important than abstract notions of justice.

I could have kept talking, or surveyed her to determine that she was "resistant" or "helpless" (Fine, 1984). But by listening I was able to unravel, partially and through my own eyes, how she saw and experienced the world and then how poorly feminist programs, drawn and designed from "our" middle-class experience, fit the needs of Altamese. The strategies and institutions I presumed could "save" me (courts, counseling, other women, family) might only prosecute or further endanger Altamese. To follow her steps I had to give up mine and recognize the extent to which race and racism, poverty and classism, personal and cultural circumstances made a difference in how we did/would respond to a gang rape. In retrospect this seems dumb, racist not to have known, embarrassing. But at the time I thought I was equipped to frame Altamese's story with the feminist and psychological categories I had available. Altamese taught me a different dance, and only then did the data make sense. Without a Qualitative stance I never would have heard her music. I would have assumed that *she* missed the beat.

Caught Off Balance

For those of us who cherish intellectual, political and emotional control, a Qualitative stance can be a curve ball, knocking a researcher into an uncomfortable sense of being off balance, ill equipped, without resources to "cope." When I (*Louise*) joined as a participant observer in a personal defense class for women I planned to discover whether the classes made women feel fearful or capable of resisting an assault – as though this were a clear dichotomous choice (I hear a 2 × 2 research design looming!). The simple answer to that simple question was that it depended on what covert messages their instructors gave. Students who took lessons from a woman and a man who unequivocally stated that no one had a right to "tamper" with their bodies felt empowered to resist. Students who learned from a man who was more ambivalent about women fighting back ended the course believing they had less right to resist. But my participant observation in the classes raised a more disturbing question which I had not anticipated: how do women learn to define "danger" when ordinary heterosexual courtship involves "aggressive initiatory activity on the part of

males" (Goffman, 1977: 329)? Had I approached the study only with questionnaires I would have failed to see the confusion the women experienced. The Qualitative stance, being there, off balance, with all pores open, revealed patterns that made my original hypotheses look naively clean and simple (Kidder, 1994).

We find a paradoxical contrast between the apparent simplicity of the Qualitative stance and the power it provides to uncover meanings and reveal complex patterns. Simply being in the field, open to what happens next, following leads and hunches, revising hypotheses, following one's nose are so unlike the methods of experimental and survey design and analysis. They seem like unsophisticated and undisciplined research maneuvers, but their simplicity is illusory. Working without a structured questionnaire, without a sampling procedure or formal research design, requires daily decisions about what to observe, whom to interview, what to say next. The discipline is rigorous, severe and sometimes lonely.

On "Bias"

Built into a Qualitative stance is the explicit recognition that we, as researchers, import our lens, our "bias," to our studies. Our data are filtered through that lens, as are our interpretations, which are, therefore, always partial. We note, however, that while all research is shaped through researchers' subjectivities or biases, it is typically only in Qualitative work that these subjectivities are acknowledged, studied, interrogated and written about.

Indeed, with pride and/or embarrassment, Qualitative researchers frequently admit that they bring to the research project a package of personal identities, interests and investments (e.g. Kidder in Fine et al., 1996; Liebow, 1993). Consider an interview with Harriet McAdoo about how she became interested in her program of research:

John [McAdoo] and I came to Ann Arbor in the Fall of 1966 and our youngest, Anna, was pre-school age. Whenever it seemed natural and playful, we always made it a point to reinforce a positive attitude about our children's racial image and it was no different with Anna. Sometimes when giving her a bath, I would let the rinse water slowly wring from the washcloth so we could trace a stream from her shoulders to her toes. Our eyes followed the stream, and we would laugh and play and I would sometimes say something like, "Look at Mommy's pretty brown girl." Shortly after arriving in Ann Arbor, Anna entered a pre-school program. One evening John and I were sitting on our bed watching television while Anna was taking a bath. Soon she rushed into the room and climbed into bed between us while saying, "Mommy and Daddy I know I'm a pretty brown girl but sometimes I wish I could be a White girl." There was no alarm, sadness or tension in her voice, but it was clear to John and me that our little one, who by every indication was a strong and healthy and happy child, was taking on racial attitudes that were far removed from how she saw herself personally - you know, her sense of self worth. Shortly thereafter I completed my first year paper

and spelled out what was to become my major focus at Michigan - the study of identity development in children, especially Black children and more particularly, the study of self esteem and racial attitudes. (reported in Cross, 1991: 78)

McAdoo's vivid story tells us about the genesis, and also the design, of her research. While some researchers "come clean" about their investments, and others note their "blind" spots or their "dis-ease," most stay silent. Qualitative researchers, in contrast, unpack their biographic baggage at the research site because they are as likely to be asked questions about themselves as they are to ask questions of their research participants. Indeed, some Qualitative researchers feel so obliged, even eager, to come clean that they devote as much time to reflecting on themselves as on their subjects (e.g., Kamala Visweswaran, 1994; Esther Newton, 1993; Jill Morawski, 1994). This self-reflection and admission of subjectivities is becoming quite fashionable in the 1990s. But it is important to remember that it was taken up by John Dollard some sixty years ago, and by Muzafer Sherif, who wrote in the early 1950s about researchers' group identifications, biases and subjectivities:

The research man [*sic*] has his own group identifications. We have noted that every group represents a point of view as it stands in relation to other groups. Every group has its own explicit or implicit premises as to the nature of human relations, as to the directions that the values and goals of group relations should take. *From the outset, research and generalizations are doomed to be deflections or mere justifications of the point of view and premises of the group or groups with which one identifies himself, if one does not start his work by clear, deliberate recognition and neutralizing of his personal involvements in these issues.* If this painful process of deliberate recognition and neutralizing of one's own personal involvements is not achieved, his autism will greatly influence his design of the study and his collection and treatment of data. (Sherif and Sherif, 1953: 11)

What Sherif called "autisms" can also form part of the data for a participant observer. This self-conscious acknowledgment of "personal involvements" and the need for researchers to reflect critically on their positions was shed from much psychological research during the 1970s and 1980s when there was a belief that researchers should be disinterested. Social science was supposed to be written, as Donna Haraway would say, from a "God's eye view" (Haraway, 1988). Such beliefs are actually quite dangerous in so far as they obscure or neutralize the typically privileged bases from which researchers write. While these biases may be unacknowledged, they are far from absent. They are simply denied and distorting.

Again, when we draw from the history of social psychology, we find those cubby holes, those methodological corners into which admissions of researchers' "biases" were stuffed, hidden, apologized for. David Rosenhan, a social psychologist who entered a psychiatric hospital under the guise of a patient, wrote in *Science* magazine about how stunned he was to experience the depths of depersonalization provoked by his short stay in a mental hospital. After he and a number of graduate assistants faked their admissions into psychiatric hospitals, Rosenhan wrote:

Neither anecdotal nor "hard" data can convey the overwhelming sense of powerlessness which invades the individual as he is continually exposed to the depersonalization of the psychiatric hospital. It hardly matters which . . . hospital. (1973: 265)

I and the other pseudopatients in the psychiatric setting had distinctively negative reactions. We do not pretend to describe the subjective experiences of true patients. Theirs may be different from ours, particularly with the passage of time and the necessary process of adaptation to one's environment. But we can and do speak to the relatively more objective indicators of treatment within the hospital. It would be a mistake and a very unfortunate one to consider that what happened to us derived from malice or stupidity on the part of the staff. Quite the contrary, our overwhelming impression of them was of people who really cared, who were committed and who were uncommonly intelligent. Where they failed, as they sometimes did painfully, it would be more accurate to attribute those failures to the environment in which they, too, found themselves than to personal callousness. (1973: 268)

Rosenhan's confession is written as a departure from, rather than constitutive of, his research. In this text he reveals his "overwhelming impression" that belies the impossibility of extracting ourselves from our research. His personal experience dramatizes the power of institutional arrangements on the otherwise "good judgement" of staff and "sanity" of residents. Without his experience inside the institution, Rosenhan's work would have lacked the passion and much of the evidence that makes his study, "Being Sane in Insane Places," compelling.

Rosenhan, Sherif, and Dollard saw their emotional responsiveness to their research as a temporary, fleeting loss of control – a threat to social science rather than part of the data of social science. They presented their experiences apologetically, as confessions of feelings out of control, which might have contaminated the data. We suggest, in contrast, that confession helps to shape, understand and interpret data. Indeed, as we will see, one woman's confessions may turn out to be another wo/man's data.

To illustrate: twenty years after writing about field work on expatriate Westerners in India I (*Louise*) can now see even more clearly my place in a former colony. The field notes that I wrote contained stories from dinner parties, shopping trips and conversations after the structured interview was formally "finished." Going back to those notes I can now write more clearly and honestly than before about the postcolonial remnants of whiteness by placing myself in the context. When I write not as a disembodied narrator but as a person who was present in "the master's house," those confessions become data:

As a first-timer I had thought I could assume a neutral social class or caste. I quickly lost that naiveté and innocence when for \$100 a month my husband and I could rent a comfortable home and employ the help of a man and woman to shop, cook, carry water, launder, clean and sweep. They and we were age-mates, in our twenties, but Bob and I were "master" and "ma'm" to them and they were "Jodi" and "Sylvi" to us. They were newly married and Sylvi was pregnant. One day neither Jodi nor Sylvi came to work so I cleaned our outdoor toilet despite advice from Indian friends that I should not do jobs that were dirty, low caste,

for servants. When Jodi came the next day he might have sensed my irritation and explained why he had missed work. He said simply "Ma'm, my baby died."

I recall this story to place myself within the frame of what I say about other expatriates. In some of the stories expatriates tell about themselves they sound guilty of arrogance and abuse of privilege while they tacitly claim innocence. The terms "guilt" and "innocence" are not used in people's stories or explanations of their own actions, but they lie close beneath the surface of expatriate experience and the surface is fairly transparent. (Kidder, in Fine et al., 1996)

Researchers who mine their own experience can find interpretations they might have missed otherwise. Clinicians who use their own experience cautiously and honestly, as happens with counter-transference, can also arrive at interpretations they might otherwise have overlooked. But there are also risks for researchers who examine their experiences in the field or in their written texts.

Elliot Liebow's book *Tell Them Who I Am* (1993) describes how he takes a chance and benefited from being vulnerable. He entered the world of homeless women and became the object of their gazes even as he wrote about them. As he served them meals and accompanied them to courts and government offices, the women learned that his survival too was precarious because he had cancer. His telling was not an experimental manipulation but a truthful explanation for his actions and absences.

Some of the women would perhaps characterize me as a friend, but I am not certain how deep or steadfast this sense of friendship might be. One day, Regina and I were talking about her upcoming trial about two months away. I had already agreed to accompany her to the courtroom and serve as an adviser, but Regina wanted further reassurance.

"You will be there, won't you?" she said.

As a way of noting the profundity that nothing in life is certain, I said, jokingly, "It's not up to me, it's up to The Man Upstairs."

"Well," she said, "If you die before the trial, you will ask one of your friends to help me, won't you?" I looked hard at her to see if she was joking, too. She wasn't. She was simply putting first things first. (1993: xi)

Qualitative researchers may put themselves into the picture and pay attention to both their feelings and the feelings of the people with whom they are working. There are no enumerated rules for how much to focus on self versus other, how much to reveal of self and other, how to respect boundaries and "do no harm." These are ongoing considerations rather than fixed rules for Qualitative researchers. These questions surface endlessly and sit stubbornly in the foreground of Qualitative work. They can no longer be buried in footnotes.

In field work, the researcher is self-consciously part of the context. Participant observers are visible and vulnerable. Actors who cannot follow a uniform script, we act and speak spontaneously, never quite sure we made the right move. We improvise when we encounter persons or situations for whom we were unprepared. No two days or encounters are identical. Research participants are not all alike. Their differences are what

interest us, and the researcher must decide in each instance how to ask questions, what to reply when asked questions and what to do next. Participant observers are vulnerable to being asked to leave or stay; being asked personal questions or ignored; being the subject of another's gaze or marginalized. We are not just actors in the field. We are acted upon and expected to be responsible. Joyce Ladner, a sociologist by training, made such a plea when she wrote:

The relationship between the researcher and his subjects, by definition, resembles that of the oppressor and the oppressed, because it is the oppressor who defines the problem, the nature of the research, and to some extent, the quality of interaction between him and his subjects. This inability to understand and research the fundamental problem – neo-colonialism – prevents most social researchers from being able accurately to observe and analyze Black life and culture and the impact racism and oppression have upon Blacks. (1971/1987: 77)

Feminist psychologists, echoing the arguments asserted by Joyce Ladner almost thirty years ago, are today resurrecting the call for reflexivity and responsibility in psychological research. These scholars are writing about *how*, not if, our subjectivities sculpt the stories that we tell and the ones that we don't.

For psychologists trained in the positivist tradition this may be a discordant call. We have been taught that subjectivities are out of place, should be contained, monitored, whited out. Being scientific has meant being objective, detached and without passions (see Jill Morawski, 1994, for a wonderful analysis of this position). Paradoxically, however, it is argued by some, ourselves included, that researchers who reflect on rather than ignore our personal investments, thoughts, emotions and relations to those under study may find clearer visions.

On Ethics

Qualitative researchers' open-ended, question-seeking, nose-following stance and immersion in the life of a community or group engenders ethical problems that differ from those of experiments or survey work. Neither informants nor research participants in participant observation are anonymous. Their being known to the researcher (and vice versa) makes both parties personally vulnerable in a way that guarantees of confidentiality do not entirely erase (cf. Liebow, 1993). People whose communities and social relations are documented have a stake in how data are represented and they worry about the misuse of data by hostile audiences. Even if the researcher intends to help, the impact of the research is hard to anticipate and its publication can affect community residents in unpredictable ways.

Listen to the words of Kenneth Clark, reflecting on his own research findings with respect to Black children's self-images:

"We were really disturbed by our findings," Kenneth Clark recalls, "and we sat on them for a number of years. What was surprising was the degree to which the children suffered from self-rejection, with its truncating effect on their personalities, and the earliness of the corrosive awareness of color. I don't think we had quite realized the extent of the cruelty of racism and how hard it hit." The interviewing and testing proved a moving and shaping experience for Clark. "Some of these children, particularly in the North, were reduced to crying when presented with the [black] dolls and asked to identify with them. They looked at me as if I were the devil for putting them in this predicament. Let me tell you, it was a traumatic experience for me as well." (Kluger, 1975: 400, quoted in Cross, 1991: 29)

Clark worried about the impact of his research on the children and on the community in general. While his methods were not Qualitative with respect to kinds of data, his ethical concerns – printed in an interview, *not* as part of the research text – reflect the kinds of ethical concerns that are raised by thoughtful Qualitative work.

Indeed, many such questions arise in response to and in the midst of Qualitative work. "Who owns the data?" becomes an ethical question that laboratory-based participants might not think to ask. Who has veto power? What will happen to the relationships that were formed in the field? What are the researcher's post-data-collection obligations? How can these data be misused against informants? Whose interpretation counts? These ethical dilemmas cannot be resolved simply by following guidelines that promise anonymity or confidentiality.

These concerns, voiced recently and eloquently by Brinton Lykes, Jill Morawski and Gail Hornstein are, however, not new to psychological research. They were raised in the decades that preceded us and even then the technical practices of masking participants' identities, obtaining informed consent and debriefing after deception were recognized as insufficient answers.

In the 1970s Philip Zimbardo wrote about his concerns for research participants in his controversial and important study of "The Psychology of Imprisonment" (Zimbardo et al., 1975). Zimbardo and colleagues asked undergraduate Stanford men to role play prisoners and prison guards for two weeks. After six days Zimbardo found the "inmates" had internalized their parts and that the role play was adversely affecting the mental and physical health of men on both sides of the bars. Zimbardo et al. wrote:

When a former prison chaplain was invited to talk with the prisoners . . . he puzzled everyone by disparaging the inmates for not taking any constructive action in order to get released . . . Several of them accepted his pastoral invitation to contact their parents in order to secure the services of an attorney . . .

We were no longer dealing with an intellectual exercise in which a hypothesis was being evaluated in the dispassionate manner dictated by the canons of the scientific method. We were caught up in the passion of the present, the suffering, the need to control people not variables, the escalation of power and all of the unexpected things which were erupting around and within us. So our planned two week simulation was aborted after only six (was it only six?) days and nights. (1975: 279)

Zimbardo and colleagues reveal quite a bit in these two paragraphs. With the hindsight of twenty years, it seems incredible that these researchers were even allowed to contemplate such a study, much less run it and publish the results. But at the time, Zimbardo's confession, like Rosenhan's, was astonishing. This admission, about the rapidity with which roles were internalized uncritically, constitute data, not an interruption of an "otherwise" scholarly text.

Qualitative work leads researchers into deep and complicated confrontations with ethical practices. Momentarily, perhaps, we have been lulled into comfort by APA guidelines, institutional review board reviews and the procedures of informed consent. But the contemporary pot of ethical troubles boils over. Qualitative researchers are at the front of these debates, although these debates should concern all researchers. Listen, at length, to the discussion that Brinton Lykes puts before us as she rethinks her informed consent form, amidst her work with Guatemalan refugee activist women:

Concretely, the form has been a major obstacle at the beginning of each interview. Intended to "protect the subjects of the research," the women with whom I spoke experienced it as a barrier or hurdle. My conversation with one of the women is illustrative. When I presented the form she suggested that she had already agreed to talk to me (otherwise she would not be there) and that by agreeing to tell her story she had indicated her consent. Her consent meant that whatever she would tell me was a part of our public record to be used in support of the Guatemalan struggle as I understood that task. She found my choices concerning future use of the material as described on the form confusing and suggested that her signature was unnecessary, that we had already settled the question of the use of the material. When we had finally seemed to agree that she had *de facto* chosen Form A or unrestricted use and she checked this line she again refused to sign her name, indicating that she did not see why it was necessary.

By the third interview I began to discern that I was in fact misinterpreting the participants' reality *and* our relationship. I had come to this project with a clear analysis of my power as Other, with my role as a university professor with a Ph.D. and as a white North American. Yet I was also a concerned researcher who was acutely aware of the ways in which researchers have taken advantage of subjects, misinterpreted their reality, and given them inadequate access to their own labor. I recognized the many ways in which the participant both makes her/himself vulnerable in sharing his/her story and has no real control over how the researcher reconstructs that story. I had therefore designed a form that I thought would address this imbalance of power, providing a base from which the participants could assert their agenda. I would, thereby, "empower" the participants. (1989: 176-177)

While the debate has advanced, Qualitative work continues to bump into ethically messy territory. Ethics, in fact, constitutes a site in which Qualitative researchers are forging new ground that the full discipline of psychology needs to grapple with. The basic ethical guidelines or procedures which we all blithely follow do not serve research participants (Fine, 1995).

On Data and Interpretive Authority

The data involved in Qualitative inquiry are, after all, open ended. They may be observations, narratives, stories, interviews and/or focus group transcripts. They may be written by informants, written by researchers, taped or accumulated through archives. The nature of Qualitative data, however, is *not* simply that they are "not numbers," but that they are analyzed with an ear for what informants are saying rather than an eye on predetermined categories and hypotheses. Listening to Qualitative data requires that researchers be willing to change hunches or hypotheses. We may be surprised, perhaps embarrassed. We will look naive. As John Dollard admitted sixty years ago:

The only possible conclusion from this experience was that I had the typical sectional bias to be expected of a Northerner and I thereupon set out to isolate and discount it. For one thing, I began to pay serious attention to what Southern white people told me about the interracial situation and although I did not always agree with them, I always learned from them. The discovery of sectional bias has another advantage, namely that I realized I was irrevocably a Northerner and ended my attempts to pass for anything else. (1937: 35)

Dollard was willing to listen hard to what his data were saying, and hear Southern voices. Only then could he understand that his own perspective was simply one among many.

Leon Festinger and colleagues began their social psychological research on dissonance reduction in the field. Their data were richly Qualitative. These researchers confessed to working like detectives, following leads, probing, and "nosing" about in an unstructured open-ended manner, for they did not know what would happen next in the doomsday sect they were studying. They even confessed to doing their work covertly, but that was not considered a serious breach of ethics in 1956.

In the first place, it is clear that we were unable to rely on the standard array of technical tools of social psychology. Our material is largely qualitative rather than quantitative and even simple tabulations of what we observed would be difficult.

We had to listen, probe, and query constantly to find out in the beginning who the members of the group were, how sincerely they believed the ideology, what actions they were taking that were consonant with their beliefs, and to what extent they were propagandizing or attempting to convince others. Later, we had to continue to accumulate this sort of data while further inquiring about what was going to happen next in the movement: when there would be another meeting, who was being invited, where the group (or individuals) were going to wait for the flood, and like questions. Our data, in places, are less complete than we would like, our influence on the group somewhat greater than we would like. We were able, however, to collect enough information to tell a coherent story and, fortunately, the effects of disconfirmation were striking enough to provide for firm conclusions. (1956: 249)

If Qualitative work entails a particular stance, subjectivities, ethics, data and interpretation, then we must come to understand Qualitative inquiry as

not merely a "deviation" from a methodological norm in psychology, but a set of rich commitments, with a long history, a way of conducting research with and on, not despite, social relations and community life. It is at this point, however, that the dilemmas of interpretation emerge.

We do not believe, as others have argued (see Fine, 1995), that Qualitative work entails simply the transcription of "others' voices." Nor do we believe that narratives, observations or collected stories speak for themselves. With the arrogance of researchers (Qualitative and quantitative), we contend that psychologists have a responsibility (for some it is a desire, for others it is an anxiety, for us it is both) to assert interpretive authority over the data. Quoting again from Joyce Ladner:

There must be a strong concern with *redefining* the problem. Instead of future studies being conducted on problems of the Black community as represented by the deviant perspective, there must be a redefinition of the problem as being that of institutional racism. If the social system is viewed as the source of the deviant perspective, then future research must begin to analyze the nature of oppression and the mechanisms by which institutionalized forms of subjugation are initiated and act to maintain the system intact. Thus, studies which have as their focal point the alleged deviant attitudes and behavior of Blacks are grounded within the racist assumptions and principles that only render Blacks open to further exploitation. (1971/1987: 77)

Ladner understood the role of the researcher as both critic and transformer of prevailing frames around social science data. For Ladner, the very point of conducting social research is to interrupt the "common sense" frames, ideologically driven by social arrangements or what she calls "the system," and to provide alternative lens for viewing social behavior.

To take this position means recognizing that Qualitative researchers are not merely tape recorders, ventriloquists or photographers (all of whom do a fair amount of editing, interpreting and splicing themselves). We are engaged analysts of social relations. By accepting such a professional stance with and upon communities and individuals, researchers select and design theoretical frames for the data, offering ever partial, temporary and kaleidoscopic interpretation(s) to readers, inviting them to generate their own.

As Qualitative workers we are intimately aware that we are the writers of record. We negotiate the data, wander through the margins of local meanings, listen to and puzzle over the words offered by informants and ultimately have the final word. While quantitative researchers are the final arbiters of which questions to ask and what the data mean, Qualitative workers enter neighborhoods, homes, nightmares, and dreams. When Qualitative workers return home with hours of field notes, voices on tapes, intimate knowledge of communities under siege, they grow uncomfortably aware of many incompatible responsibilities. There is a responsibility to *hear what informants are saying about their lives and the meaning of their experiences* and a responsibility to construct interpretations that may or may not conform to what informants have told us. These are the responsibilities

of theorists and researchers across disciplines, across methods. But for Qualitative researchers, this often feels like a betrayal by those of us who try to research *with* people rather than *on* them.

We take the position that in our writings, research psychologists need to advance a theoretical framework around the "voices" of informants, to help analyze these voices in their historic and current circumstances. That is, we cannot merely reproduce narratives or present them as if the interpretations were self-evident. Whether we agree with the words of informants or not, whether we even like them or not, we have an obligation to surround their words with analyses for which we are the authors. Easier said than done.

This last criterion for Qualitative research is perhaps the most difficult for young scholars, especially graduate students. For a researcher who is immersed in a site, engaged with the voices of people, trusted by those who trust few, it is hard to turn the corner and impose one's own interpretation of why people are saying what they say, doing what they do, resisting as they resist, capitulating when they capitulate. But that is our task. Partial, temporary and tentative, we have a responsibility to position ourselves in relation to our data, and our position will not necessarily be the same as our informants' (have no illusions - they will not agree with each other either).

A Taste for the Field

Uncertainty about what might happen next is what some people find appealing and others find abhorrent about Qualitative work. This is part of the stance that we described earlier and what we failed to say then is that this is as much a matter of taste as it is a matter of science. We have written this chapter not only to examine the science and art (cf. Wolcott, 1995) of Qualitative work but also to give readers a taste of the pleasures and problems. We might not persuade anyone to like Qualitative work; at most we can describe how we've tried it and liked it.

We each could have conducted our work strictly as survey research projects. We did, in fact, include structured interview schedules and Likert scales, semantic differentials, and short open-ended questions (qualitative with the small q). The structured scales and interview schedule were comforting because they provided data we could "count on." But the daily encounters, the stories participants told to us, their problems, their privileges, and ours were riches we would not ignore. We wrote field notes for pleasure (and work). We have acquired the taste.

The radical possibilities that emerge within Qualitative psychology are only beginning to flourish. Some Qualitative psychologists are moving into participatory work, in which once-informants are now peers, collaborating with us as researchers (see Lykes and Mallona, cited in Fine et al., 1996; Phillips, 1995). In two urban middle schools, Michelle is conducting

participatory research with young adolescent ethnographers, faculty, parents and doctoral candidates. Together, in these schools, with many different lines of vision and within a bouillabaisse of Qualitative data, we are creating a culture of inquiry. In the field of biography, Abbey Stewart and Gail Hornstein are inventing methods for feminist dialogic collaboration with living, and in the case of Hornstein (1994) with deceased informants, co-constructing lives, dialogue and biography. In cultural studies Louise has pried open the study of whiteness as a "secondary analysis" of Qualitative work done originally in India on expatriates, and British social psychologist Michael Billig (cited in Fine et al., 1996) is interviewing working-class whites about race and the royal family. In media studies, Corinne Squire (cited in Fine et al., 1996) is analyzing the representations of race, class and gender through a textual analysis of daytime television talk shows, connecting national policy, media representations and social attitudes of the white and African American viewers of these shows. In organizational and group dynamics, Linda Powell (cited in Fine et al., 1996) and Nancie Zane (cited in Fine et al., 1996) tape and transcribe, collecting Qualitative information to excavate the workings of race, gender and authority in schools and work settings. In sexuality studies, Leonore Tiefer (1994) draws from deep interviews and case studies to extract the ideological and political scaffolding upon which "good sex" is currently being constructed. In each of these instances of critical work, it is Qualitative inquiry that has enabled radical theorizing, the embroidering of structural issues with everyday psychological life, stretching the intellectual and political terrain within which psychologists do our work.

When the purposes of research are to learn what could be, to unravel what has been and to imagine the unimaginable rather than to test predetermined hypotheses, the methods available and questions to be asked are endless; the "variables" that once so securely contained our thoughts come undone like a poorly knit sweater; the ethics grow dense and need sensitive conversation; the interpretations are multiple. The methods are Qualitative. The discipline is psychology. And to those students interested in Qualitative work, welcome.

4

Ethics in Psychology: Cui Bono?

Laura S. Brown

Editors' note To the naive observer, professional ethics should be about a discipline's moral implications, about the harmful and beneficial effects of theories and practices on individuals and societies. But this is a naive view indeed. Organizations of professionals such as doctors and lawyers devise codes of ethics that protect the professional at least as much as they protect the public. As pointed out in Chapters 1 and 2, detrimental outcomes in psychology do not necessarily reflect malevolent intent by psychologists, but they are injurious nonetheless. In this chapter, Laura S. Brown clarifies how psychology's professional ethics codes primarily serve the interests of professional psychologists.

Brown demonstrates how power and control in professional interactions are held primarily by the psychologist. Similarly, decision making processes concerning ethics are governed by professional bodies with little input from the public. Significantly, the moral dimension of ethics codes is based largely on individual ethics: the codes regulate the micro-ethics of the therapeutic relationship but neglect social ethical issues such as oppression, discrimination, and inequality. Since the codes generally assume that harm derives from the aberrant behavior of a few unscrupulous psychologists, they conveniently exclude more subtle violations such as the perpetuation of power inequalities and the deleterious effects of labeling people. This chapter challenges the notion of science as the ultimate good, denounces legitimized power inequalities, and decries ethics codes that adopt lowest common denominator values.

Disillusioned with existing codes in psychology, feminists affiliated with the Feminist Therapy Institute began to create their own code in the 1980s to explicitly and proactively address issues of oppression, exploitation, discrimination, accountability, and social change. Brown points out the benefits, as well as the difficulties, of trying to create an ethics code that takes ethics seriously.

Organized psychology in North America passed its century mark in 1992. Yet it was not until midway through this first one hundred years that the American Psychological Association (APA, 1953), still the largest body of psychologists in the world, saw fit to create a code of ethics. The energizing impetus for the creation of the code likely rests in the collective soul-