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Journal of Contemporary Ethnography 2005; 34; 68

DOI: 10.1177/0891241604271341

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“
Ultimately, it
was my elusory
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correspondent
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recognize the
vulnerability I
shared with
participants in my
study.”

**RISK, REFLEXIVITY AND
AN ELUSORY BODY**

Transformations
in Studying Illness

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Journal of Contemporary Ethnography, Vol. 34 No. 1, February 2005 68-100

DOI: 10.1177/0891241604271341

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68

This article calls attention to how theory shapes ethnographic relations, practices, and “truths” about bodies and identities. While carrying out ethnographic research among men and women with heart disease, I experienced a “scare” for ovarian cancer, which interrupted the field-work, elicited revisions in research relations, and compelled me to see how my theoretic and purposive agendas had functioned as discourses that promoted and reinforced differences between research participants and me. My elusory body enabled me to recognize our fundamental similarities and alerted me to misguided turns I had taken in my research trajectory.

Keywords: *body; heart disease; illness experience; risk; reflexivity*

In a compelling critique of emergent trends in social studies of the body, Radley (1995) expresses concern about the predominance of constructionist theoretical agendas, which have gained favor in wide-ranging disciplinary contexts. Growing numbers of feminist, cultural, anthropological, and sociological analyses are informed by versions of social constructionism that limit the extent to which living, thinking body-subjects are at the center of analytic attention. Radley (1995, 7) laments the extent to which lived body-subjects are elusive within, and have been marginalized by, social theory:

The de-realization of the body-subject through representation leaves it (as flesh) marginalized. The consequence of this situation is that, within discourse, the lived body is rendered knowable only through the constructions that are its multiple realities, but its existence as a lived entity is effectively denied.

Radley goes on to develop his notion of the *elusory* body, contending that it is the nature of bodies to be elusory. First, bodies are elusive in constructionist accounts insofar as their existence as lived entities is “effectively denied.” Within semiotic versions of social constructionism, for instance, the body is construed as a symbol or signifier, while in Foucauldian accounts, the body is considered primarily as an effect of

AUTHOR'S NOTE: I presented different segments of this article at the International Qualitative Health Research Conference in Banff, Alberta, Canada, in 2000 and at the 4th Annual Colloquium, Centre for Sport Policy Studies: “Paradoxes of Illness and Fitness,” at the University of Toronto in 2003. I am grateful for constructive comments that anonymous reviewers provided in response to the initial draft of this article. I especially thank Scott Hunt, whose suggestions helped me to strengthen the work considerably.

discourse or an object of “controls, coercions or restraints” (Radley 1995, 3).

Bodies are elusory in another sense, according to Radley. He describes the elusory character of bodies through a discussion of dance, flirtation, and play—expressive activities performed by lived body-subjects. Meanings produced through bodily performances such as dance, flirtation, and play, he contends, are not reducible to, nor can they be contained by, discourse. Playful, dancing, and flirtatious bodies are elusory by virtue of their capacities to configure meanings and realms of experience in ways that exceed the limits of power and discourse.

Radley (1995, 20–21) articulates his strongest denunciation of social constructionism on the final pages of his essay:

Social constructionism results in a position where the affirmation of power and discourse in social life becomes translated into an endorsement of theory in social scientific practice. This turns what was a particular, though crucial movement in the development of ideas into an epistemological determinism, into a set of self-serving principles that must, in relation to the “body,” continually hide what they seek to find.

In this article, I take Radley’s laments as a point of departure for reflecting on ethnographic research I carried out on the social experience of heart disease. Indeed, although I did not intend it to be, my ethnographic research may have been guilty of the “endorsement of theory” that Radley criticizes in social constructionist accounts. Ironically, it was my own elusory body that alerted me to misguided features of my ethnographic research-in-progress.

This is a “confessional tale” (Van Maanen 1988) that expresses my own misgivings about the manner in which I became persuaded by a theoretical perspective and purposive agenda that had a threefold influence on my research-in-progress. First, the theory that gained ascendancy as I was carrying out the research influenced my subsequent way of seeing the field and its inhabitants by providing predetermined frames of reference and conceptual categories for interpreting what I observed. Among other things, these frames of reference encouraged me to emphasize and accentuate differences, rather than similarities, between participants and myself.

Second, my theoretic allegiance and the thematic agenda coinciding with it compromised my ability to achieve reflexivity during the course

of the fieldwork. While I worked hard to be reflexive initially, the crystallization of my perspective became an impediment to being reflexive in an ongoing manner.

Third, my theoretic and purposive agendas not only rendered the lived bodies of participants in my study elusive, in Radley's initial sense of the term, but made my own body elusive, by dulling my sensitivity to my own corporeality as a researcher. It was, ironically, my elusory, ambiguous, and unruly body—an alarmingly unanticipated ovarian tumor—that enabled me to recognize fundamental similarities between research participants and myself. The cancer scare alerted me to misguided assumptions that informed earlier stages of research. I conclude with some possibilities for rethinking relationships and objectives of ethnographic endeavors among people with illness.

INITIAL INTERESTS AND INTENTIONS

The field of medicine had always been of interest to me as I grew up in a family of medical professionals. My father was a physician, my mother was a nurse, and for many summers, I worked as a receptionist and unregistered nurse in my father's OB-GYN office in suburban Boston. My earliest training in sociology focused on the study of sport and fitness, activities in which I had a great deal of personal experience and enjoyment. Both are realms that typically assume and require unambiguously healthy bodies.

In graduate school, I began to concentrate on health, medicine, and illness as specialty fields within sociology. I suspect my motivation to study illness grew in the course of my father's seven-year struggle with multiple myeloma, a bone marrow cancer that ended in his death at precisely the time I was developing a dissertation topic. In the late stages of my father's illness, and after his death, I found myself turning to social analyses of illness, and especially to first-person accounts written by sociologists and anthropologists (e.g., Frank 1991; Murphy 1987). Drawn to these works, and in search of a disease, I pondered the possibilities for making illness the focus of my dissertation research.

Despite its status as America's number one killer, I was surprised to find that scant attention had been given to heart disease as a lived experience (Helman 1985). Social research had been done on heart disease (Cowie 1976; Radley 1988; Speedling 1982), but little ethnographic

work existed. Moreover, the bulk of attention focused primarily on men, as women in extant accounts existed principally as spouses of men with cardiac conditions. With a “gap-filling” rationale, I convinced my advisors that studying the experiences of women and men with heart disease was worthwhile. I negotiated my way into the cardiac units of two hospitals and accepted an opportunity to work as an intern and resident sociologist in two cardiac rehabilitation clinics.

I carried out ethnographic research during a two-year period, exploring the experiences of people with coronary heart disease (e.g., those who have had bypass surgery, heart attacks, or angina) through interviews, focus groups, and participant observation in hospitals, rehabilitation clinics, and people’s homes. I explored how people with heart disease manage and make sense of their conditions in daily life. I also explored how people understood their bodily conditions in relation to personal biographies and family histories; how they handled the social and bodily disruptions of illness; and how they (re)structured and (re)negotiated social obligations, relations, and identities in the wake of illness.

The ethnographic portion of my study involved participant observation in cardiac rehabilitation clinics and in the cardiac unit of a hospital. Once weekly, I carried out observations in the heart center of a hospital where I served as a volunteer during a four-hour shift for a period of one year. There, I met patients who were undergoing, or recovering from, cardiac surgery or a cardiac procedure following a heart attack or episodes of angina. I visited patients, set up educational videos, distributed written materials for patients and family members, transported patients from room to room, restocked supply cabinets, assisted with meals, and reassured patients who simply wanted somebody with whom to talk. I also ran errands for the manager of the heart center, assisted nurses with certain tasks, and facilitated communication between patients and nurses who could not immediately attend to patients in need.

In rehabilitation, I had the chance to develop an ongoing relationship with cardiac clients during their twelve-week period of rehabilitation. During an eighteen-month period, I observed a sequence of four one-hour rehabilitation sessions, which were scheduled three times per week. In a second clinic, I observed three days a week for the entire day (8 a.m. to 5 p.m.) during a six-month period. By observing exercise sessions, attending educational classes, and listening to participants’ stories on a routine basis, I learned how cardiac conditions influence daily

life and unfold over time. I developed an understanding of the chronic, ongoing character of cardiac conditions and how heart disease becomes a feature of one's daily life and identity. In the hospital, I had the opportunity to meet patients while they were hospitalized and being treated for an acute cardiac condition (i.e., heart attack or angina). After describing my project to prospective participants and receiving informed consent, I carried out initial interviews. I also carried out a sequence of six unstructured follow-up interviews with participants at their homes during the course of a year after their discharge from the hospital. My observations in the hospital provided snapshot exposures to a consequential moment in a person's life with heart disease. Follow-up interviews, as well as my observations in the rehabilitation clinic, provided moving pictures of how people proceed in efforts to recover from cardiac episodes and adjust to heart disease in daily life.

In addition to the ethnographic component of the research, I facilitated a series of focus group interviews among people with heart disease. I served as the facilitator of the focus groups, which took place in the education rooms of the cardiac rehabilitation programs of two cardiac rehabilitation clinics in Northern California. As collective interviews designed to stimulate discussion about the experience of heart disease, the focus groups functioned as ongoing cardiac support groups for participants. The discussions allowed me to learn about members' experiences with heart disease as they interacted with one another on a weekly basis. In all, thirty-seven people participated in the focus groups, which lasted for ninety minutes and took place once weekly during the course of two years. The size of the group ranged from three to twelve participants, and new members joined as the sessions proceeded. Twenty people participated in sequential interviews, from their initial period of acute symptoms and hospitalization through the year that followed.

I entered the field with a reluctance to endorse any particular theory or paradigm, preferring my aim of "theoretical eclecticism" and "epistemological pragmatism" (Turner 1992).¹ While much field-based research in sociology is predisposed toward inductivist strategies, I did not consider my efforts as adhering strictly, or exclusively, to inductivist logic. I considered existent social theory as essential to contextualizing and interpreting field-based observations and for producing unambiguously sociological accounts of social life and action. Entering the field, I considered my analytic task to involve, more aptly, "retroduction"—a

dialectical consideration of empirical observations and theory—resulting in a double-shaping of data with theory so as to establish a compatibility between the two (Katz 1983, 133).

INTERACTIONS AND REACTIONS: EMOTIONAL ENCOUNTERS WITH CARDIAC ILLNESS

Initiating the research, I committed my time and energy to developing detailed, self-reflective field notes. These notes not only document the interpersonal challenges of studying illness but also my performance of emotional labor (Hochschild 1983) to maintain my composure during hospital-based observations, one-on-one interviews, and during focus group interactions.

The initial focus group interview I held, in particular, was emotionally challenging. At that point in the research, I had been carrying out interviews and observations in hospitals, participants' homes, and the rehabilitation clinic. At the session in question, I met focus group participants for the first time. Seven people attended the initial session, which began with personal introductions. A fifty-three-year-old man named Jeff² was first to share his story:

I had quadruple bypass surgery. One of my valves was damaged during the surgery. They destroyed my mitral valve. Now my heart is operating at 20% capacity and I'm waiting for a transplant. . . . But since the passage of the [California motorcycle] helmet laws, the number of available organs has been cut in half. I've given up on my chances for a heart. . . . I'm living day to day, trying to be motivated for something. I can't work anymore. I don't get out much and talk to people.

Field notes I took directly after the focus group document the intensity of my emotions in absorbing Jeff's story:

Listening to Jeff's account, I could sense his despair and isolation. He's fatalistic, pessimistic, he's lost hope. I got a feeling in my gut right away as he spoke. His story was really getting to me. I took a few deep breaths. Tears began to well in my eyes. I opened them wider, hoping the air would force some of the moisture to evaporate. I hoped nobody would notice. As facilitator, I needed to convey some semblance of professionalism. I wondered how much longer I would be able to accomplish this

façade, if that's what it was. I began taking notes in an attempt to regain my composure.

A second participant's story was equally challenging, emotionally, and my field notes from that evening chronicle my response:

Next, Janie shared her story. She had her heart attack when she was 40, six years ago. She was married, pregnant, and a kindergarten teacher at the time. On the same day she suffered her heart attack, her *father*, also, had a heart attack. His was fatal. He died that day. I can't even imagine. . . . Janie had 17 episodes of heart attack or angina before the enzyme tests "came through"—before doctors believed her symptoms were cardiac. Prior to that, she said, a doctor kept insisting [her heart attack] was "all in her head." On the day of her heart attack, Janie lost her father and her baby in utero. In the months that followed, her marriage began to dissolve and she eventually got divorced. The physical demands of teaching became too much for her. She stopped working and began to live on disability.

Janie's sequence of losses was almost too much to bear. Too much to *hear*. She lost her *baby*, her *father*, her *husband*, and her job. She told us that she effectively lost *who she was*. She was intense, angry, telling her story. Sharing her story seemed to rekindle the rage she harbors against the doctors who missed the diagnosis. I think the doctors are still the targets of her rage.

Listening to Janie was even more difficult than listening to Jeff. Not just listening, but subduing the emotions that struck spontaneously. Her story brought fresh tears to my eyes. I was thankful they didn't roll down my cheeks. I had to take a deep breath to try to contain, or constrain, my emotions. Others in the room were also moved. I saw Fran wiping a tear, and Helmut was looking down, I think, so people wouldn't notice his own.

Not only was the focus group experience evocative of sympathy for listeners in the room, but it was also noteworthy for the extent to which I could relate to, and in some sense "see myself" in, participants themselves. Janie and Yusef, especially, were notable, given their youthful appearances and the relative invisibility of cardiac symptoms. In certain respects, I found it difficult to imagine that they had heart disease. Reflecting on Janie, my notes remark,

The thing is, I would never know, seeing her on the street, that Janie had *heart disease*, had experienced a heart attack. She is young, slim, mid-forties.

My notes express even more surprise about Yusef's cardiac condition. Elaborating on the similarities between us, my notes shift from a description of him to a recollection of my own past experience in triathlon training and competition. I was expressing disbelief about his (and by extension, my own) cardiac candidacy:

Yusef brought [heart disease] close to home. He's a biologist, recently graduated, with a Ph.D. from [a West Coast university]. He's highly energetic, athletic, a triathlete. He had the heart attack last July, in the middle of the bike phase of a triathlon in Saudi Arabia. [He] couldn't be further from my image of a person with heart disease. His only addiction is *exercise*. His only risk is "stress" . . . He could be *me*. He looks like the epitome of health. He's fit, forty-something, balances work with workouts. He reminds me of my own triathlon days. . . . Who would guess that *he'd* have a heart attack?

In concluding the field notes related to the focus group session from that evening, I commented on the emotional challenges the research posed at this early stage:

This research is emotionally harder than any teaching I've ever done. . . . I tried to control my emotions, and took notes respectfully, as one of the listeners in the room, trying to understand the others. . . . I was playing my part, just listening to their stories and hearing details of their suffering. This is going to be harder than I ever imagined.

In hospital visits, I also encountered people whose stories were moving and who impacted me considerably. I felt various degrees of surprise, incredulity, frustration, and sorrow, along with the spontaneous workings of my emotions. I often spoke about the most moving cases with my research advisor. At this early stage of research, I was simply listening to and taking notes on the complexity and diversity of participants' stories. My field notes documented the substance of participants' stories as well as my responses to them but made no attempt to analyze or theorize the accounts I heard.

DEVELOPING ETHNOGRAPHIC ASSUMPTIONS: BODIES WITH ILLNESS, BODIES AT RISK

Despite my early identification with and sympathy for participants in my study, my field notes gave way to constructions of difference as I spent more time in the hospital and rehabilitation clinics. Indeed, the fact that the study was conceptualized as a study of *illness experience* may have prompted me to focus on what I construed as signs of illness and forms of physical disablement coinciding with having heart disease. In documenting illness, then, I looked for and recorded evidence of troublesome, problematic bodies. My presumption that my participants were ill, and that I was in good health, set the parameters for my ethnographer/participant relationship. Moreover, my experiences in the field confirmed further my self-image as healthy.

BODIES WITH ILLNESS: CONSTITUTING DIFFERENCE

The primary difference I imagined between my informants and me was the fact that they had heart disease and I did not. I also emphasized differences between us in relation to bodily capacities and appearances and degrees of vulnerability. Distinctions I observed in carrying out research and that I chronicled through fieldnotes can be characterized according to the following oppositions: young/old, healthy/sick, unlimited capacity/limited capacity, invulnerable/vulnerable, and in control/out of control. Indeed, in my ethnographic imagination, the former terms characterized my own status, while the latter terms described the status of research participants, marking fundamental differences between my body and theirs.

The physical status of my body differed fundamentally from those of research participants insofar as I was disease free and they were diagnosed with heart disease. Compared to participants, I was relatively young—in my early thirties—while with a few exceptions, almost all participants were in their fifties, sixties, seventies, or eighties.

During participants' acute episodes with cardiac symptoms, when I was performing fieldwork in the hospital, I especially noted forms of physical incapacitation and physical limitations stemming from having heart disease. Moreover, I focused and commented on the vulnerability of participants' bodies, made evident by their appearance of being sick.

Consider, for example, a field note narrative that I titled, "Bloodied Battered and Bruised," based on an interaction I had in the cardiac unit with an elderly woman who had just undergone bypass surgery. In it, I describe Mrs. Sims, whose difference from me I constructed along a number of dimensions:

Mrs. Sims was the frailest elderly woman I'd yet seen in the Heart Center. I guessed her to be in her mid-eighties and it astounded me to think she'd just been through bypass surgery. . . . Mrs. Sims was connected to a steady stream of oxygen, as greenish tubing entered each of her nostrils. She had dark bruises all over her arms and fresh, red blood appeared on the surface of her skin in places where the skin had torn. Her skin must be paper thin, I thought—it looked as though it would tear open and bleed with the slightest touch. Mrs. Sims could see that I was looking intently at her skin. "I've been taking steroids for so long my skin just tears. I bleed so easily."

[Preparing to attend the Going Home class], Mrs. Sims pulled the bed covers out of her way, and began to pivot, swinging her legs down over the side of the bed. I worried she'd fall off the bed. She was heavily medicated and would be dizzy following surgery. I dashed over to steady her, afraid to cling too tightly for fear of tearing her skin. "Wait, I'll get a wheelchair!" . . . I placed the canister of oxygen into its holder on the back of the wheelchair. Mrs. Sims clutched her red, heart shaped pillow. "I can't go [anywhere] without my pillow. It hurts like *crazy* when I cough. Without my pillow, I think I'd die of the pain." Mrs. Sims seemed on the verge of tears. I tried to imagine what it would be like to have surgeons grind their way through my sternum, slice and sew my coronary arteries, and then staple me up again. . . . As I took my parting glance at Mrs. Sims before leaving the room, the thoughts crossed my mind: I don't ever want to go through what Mrs. Sims has been through with open heart surgery—at any age.

My comments construct Mrs. Sims as frail, old, in pain, discolored and disfigured, and physically vulnerable. Weak, dizzy, and in need of supplemental oxygen, according to my field notes, hers was an incapacitated body. As the oldest patient I had yet seen on the floor, I identified Mrs. Sims as remarkable among my participants and remarkably different from me. While I initiated my study to understand features of the experience of having heart disease, my comments make it clear that

heart disease and its medical interventions are firsthand experiences that I absolutely hope to avoid.

PROFESSIONAL OBLIGATIONS AND THEORETICAL ALLEGIANCE

Several months into my fieldwork and interviewing routines, a conference commitment compelled me to say something meaningful about the research I was carrying out—almost before I was prepared to do so. To that point, I had not put much time, explicitly, into the work of theorizing. To say something sociologically meaningful, I resumed my reading routines, gravitating toward Foucault's (1977) writings, based on work I had read by Lupton (1994, 1995). Rereading sections of *Discipline and Punish*, I became optimistic about the relevance of Foucault's theory as a framework for interpreting social and interactional features of the educational sessions and exercise regimens of cardiac rehabilitation. Indeed, Foucault's concepts of *power and knowledge*, *discipline*, *governmentality*, *biopower*, *panopticism*, and the *medical gaze* could be applied fruitfully to contextualize and interpret the activities and routines that I was observing in the clinic. The paper drew on Foucault's frameworks in describing confessional, disciplinary, and surveillance technologies of risk reduction in the rehabilitation clinic that normalize deviant (i.e., ill, cardiac) bodies. There are, of course, degrees of truth to this characterization, but by casting my gaze in this direction, a good deal of ethnographic reality was being eclipsed before my eyes.

The conference paper described the rehabilitation clinic as a place in which "fitness" is defined by and in relation to cardiac risks (e.g., percentage of bodily fat, blood pressure, LDL/HDL cholesterol ratio):

Each risk is treated as a warrant for medical intervention. Each risk factor is the basis for defining individual bodies, or practices of bodies, as either "at risk" or "normal" with respect to a specific measure. Rehabilitation technicians apply epidemiological knowledge, based on the aggregate incidence of disease across categories in a population, to individual clients by calculating "Risk Profiles," which stratify cardiac cases according to relative degrees of risk. The more risk factors a client has (e.g., high cholesterol levels/ratio, hypertension, smoking, family his-

tory, age, diabetes, obesity, being sedentary, Type A behavior, high stress level) the higher their total score on the risk profile. Each client receives an indication of risk specific to each risk factor, as well as a total risk score, which stands as an indication of their likelihood of having a future heart attack. The practice of risk profiling personalizes risk, and hence, allows for customizing exercise prescriptions for specific clients. Clients' risk profiles are accompanied by a set of prescriptions and proscriptions to guide individual behavior change.

The paper was well received, and the panel organizer asked whether I would consider contributing my paper to an anthology she was developing on the body. Indeed, the possibility to publish my paper made me delve more earnestly into Foucault's work, and hence, I spent more time explicating frameworks and less time being reflexive about my field experiences. I became especially attracted to the social and political analysis of risk, which provided further support for my way of seeing in the field. Beck's (1992) thesis about reflexive modernization, for instance, provided a framework for comprehending the larger historical forces shaping the social production of risks and the social and political significance of risk in late modernity.

Indeed, although my ethnographic research was still underway, my publication-in-progress constructed the bodies in question as sites of risk and as objects of disciplinary routines to reduce risk. My retroductive analysis further established the goodness of fit between Foucault's ideas and the bodies in question. *Risk* emerged as the concept of central significance in my mapping of the field. Inspiration from and a commitment to these theories sharpened, but simultaneously narrowed, my visions of the field, leading me to focus with precision on particular things while blocking out others. I was developing a blind spot for alternative ways of seeing the bodies of those who participated in my study. I may also have grown hard of hearing, in certain respects, with regard to the depth and complexity of their stories of suffering.

I began to view participants in my study, themselves, as "cardiac risk managers." Living with heart disease, I surmised, was largely a matter of managing and making sense of risk. The discourse and rhetoric of risk became the lens through which I came to see the field, my participants, and eventually, myself. Although I was not aware of it at the time, my vision of "the field" had been colonized by the discourse on risk. In Radley's terms, the bodies of my informants had been rendered elusive.

This version of social constructionism was hiding, in particular ways, the bodies that I sought to understand, including my own.³

BODIES AT RISK

A primary agenda that I pursued at this point in the study was to understand how people with heart disease account for their conditions and respond to being at risk. In the following excerpt from my field notes, I explore Mr. Phillips's account of his condition in an attempt to elicit his strategy for managing risks:

Mr. Phillips offered his explanation for having heart disease, pointing out the fact that his father and all his brothers had died of heart disease. "I knew it would hit me eventually. I was kind of waiting for it, wondering when it would hit." I asked what Mr. Phillips was doing to manage his condition and whether he was making any changes in his life. "Lifestyle's not gonna change anything. It's in my genes. I eat what suits me and I smoke. So what? Like I said, everyone in my family dies of heart disease."

I proceeded with my benevolently intentioned questions: "Even with your family history, you still smoke?" Mr. Phillips was furious: "People have been smoking since the Stone Ages! Heart disease is in my genes! Smoking has nothing to do with it!" I was stunned by his response to what I thought was a well-intentioned question. I might have probed further, to see how Mr. Phillips accounted for his smoking but opted to wait for another time.

Mr. Phillips denied, adamantly, that smoking had any influence on his having heart disease. I interpreted Mr. Phillips's refusal to acknowledge tobacco as a risk, and denial that smoking was implicated in his disease process, as evidence that his reasoning was somehow faulty or misguided and that his strategy to manage risks was foolhardy.

In contrast to Mr. Phillips's refusal to initiate changes in light of his disease, during this period of fieldwork, I began to implement changes in my own practices in relation to cardiac conditions despite the fact that I was disease free. For example, I became self-conscious about my dietary choices, pondering the foods I ate and their relevance to cardiac risks. An excerpt from my field notes shows how I deliberated over food choices in a manner that was explicitly informed by cardiac risks:

[Taking] a short break from visits with heart patients, I went to the coffee shop to swallow two steaming cups of black coffee and a piece of apple pie. I turned down the scoop of vanilla ice cream that the woman behind the counter tried to tempt me with: "I don't think so. I'm [visiting] patients in the Heart Center. I don't ever want to have to go through what those folks are going through." I realized it was bad enough that I was eating pie, whose crust is filled with artery-clogging lard. But, I figured, apples are filled with fiber, and pectin, which lowers the bad cholesterol. Besides, I was starved. The pie wouldn't kill me—not today at least. There was little else in the coffee shop that appealed to me and I didn't feel like going over to the main cafeteria of the hospital, where I might have purchased a salad.

As the excerpt suggests, I was producing a rationalized reading of my diet in relation to cardiac risks and, hence, was experiencing and interpreting food choices as relevant to the cardiac conditions of research participants. My motive was precautionary—I sought to reduce risks and avoid what participants were experiencing by virtue of having heart disease. Not only did I see myself and body as different from theirs, but I also sought to sustain and increase those differences, if possible, through strategies of risk reduction. My actions were informed by the assumption that I could influence health outcomes through disciplining my diet.

In the cardiac rehabilitation clinic, cardiac risk profiles became a way to characterize clients, and I began to describe participants according to their risk profiles. That is, I understood their challenges as reflections of the specific risks highlighted by their profiles. In my field notes, I describe Feliciano's predicament in relation to risks specified by his profile:

Feliciano told me about his family risk factors: his dad had suffered a stroke, his mom had high blood pressure, he was 90 pounds overweight, physically inactive, and smokes cigarettes. . . . [He's] going uphill, improving. With exercise, caution, and motivation with stress reduction, a good diet and stopping smoking, I think he will be able to reverse the clogging in his arteries. He told me he's not smoking much: one cigarette in two weeks. This seems astonishing to me. How can somebody curb the cravings so as to have just one in two weeks? And what brings on the need for that one? . . . From what I gather, [his spouse] continues to smoke in his presence and does not go to another room to smoke. Even second hand smoke can be construed as a risk. Not to mention the temp-

tations that must surface when he constantly has to face [his spouse] smoking away like a chimney in the wind. God he has the odds stacked against him. . . . I think that unconsciously, [his spouse] wants him to keep on smoking. Her own denial creates a smokescreen that she can't quite see through. Cough, hack, choke, wheeze.

Curious, in my fieldnotes above, is the degree of optimism I express for Feliciano's ability to reverse the clogging in his arteries, if only he takes measures to reduce risks relevant to his profile. My moralistic evaluation of the fact that his spouse continued to smoke in his presence highlights the extent to which my thinking, like much of disease prevention discourse more generally, construes risk reduction as a moral imperative and evaluates unfavorably those who fail to do so properly.

Not only did I understand participants in terms provided by risk profiles, but I also began to view my own body in relation to them, eagerly submitting to measures used in rehabilitation to assess my cardiac risk. By doing so, I was achieving a putatively objective measure of my degree of difference from research participants and, at the same time, evidence of my good health and low-risk status. On the basis of measures taken (e.g., blood pressure, body composition, blood lipid panels), I interpreted my risk as negligible and considered the probability of getting heart disease to be remote.

Of particular significance in convincing me of my low-risk status was the score I received on a blood test used to measure cholesterol levels. As a central component of cardiac risk profiles, my score on the test was so low (i.e., favorable) that rehabilitation technicians were astonished. An excerpt from notes I took during my sixth month in the field chronicles my observations at the time:

I arrived [at the rehabilitation clinic] a little bit early today. [The technicians] were particularly excited when I entered. Apparently, they had my cholesterol results in their hands. Neither had seen a lipid panel like mine before. Ever. My HDL exceeded my LDL, apparently this is unheard of. My overall reading was astoundingly low, and my ratio of total cholesterol/HDL came out at 1.9. Apparently this is phenomenal. The lower the better. Anything under 3.5 is "great" according to [one technician]. She had never seen a ratio of 1.9. They began to quiz me on my daily practices: Do I exercise? "Yes I run a 7.7 mile loop each day." I also told them that I eat salmon or tuna twice a week. I informed them of my habit of consuming a tablespoon of flaxseed oil every evening. I admitted that,

for all practical purposes, I'm a vegetarian. I eat a lot of oat bran and that, with the exception of sprinklings of Parmesan on my pasta, I gave up cheese to avoid saturated fat. I admitted that I *do* eat butter, rather than margarine. I've read that margarine is even worse than butter, as it has trans fatty acids that wreak havoc on lipid metabolism and promote the production of series 1 and 3 prostaglandins, which are deleterious to blood panels and contribute to atherosclerotic conditions. I told her I eat lots of garlic and onions, take antioxidant vitamins A, C, and E, along with ginger, ginseng, turmeric, cayenne, and chromium picolinate, all reputed to lower serum cholesterol levels.

Of particular significance in the excerpt above is the extent and specificity of my knowledge about practices reputed to influence cholesterol levels. Moreover, by educating myself about lipid metabolism, I employed technical language not even used in the rehabilitation clinic (e.g., "prostaglandins"). The degree of discipline I had incorporated into my own dietary, supplement-taking, and exercise routines went above and beyond the relatively modest prescriptions and proscriptions advocated by rehabilitation staff to clients. As a student of risk reduction, I was becoming a high achiever, with an immaculate, cardiac-conscious lifestyle.

Also of interest in the excerpt above is the degree of confidence I expressed in communicating my risk reduction strategies. My presumption that the cholesterol test I took was determined by acts of my own volition stands in sharp contrast to the skepticism expressed by one participant, Walt. Whereas I considered my lipid profile to be a direct result of my dietary choices and exercise routines, Walt insisted that disciplined eating had no effect, whatsoever, on his cholesterol levels. A field note excerpt chronicles what Walt considered the fruitlessness of dietary restrictions for influencing cholesterol levels:

Walt mentioned how every member of his family had died an early death stemming from heart disease. All had astronomically high cholesterol. His is in the mid 300 range. He insists efforts to reduce cholesterol through diet have been futile. One time he got so fed up with dieting, which didn't work, and the cholesterol-lowering medication, which was a nuisance, that he went on an "angry binge." He ate whatever he wanted, drank heavily, and enjoyed the good life for several weeks running. When he had his blood work done, his cholesterol levels had improved dramatically. Now, he's decided not to bother with dieting or medication as his best results came after he suspended the medications and binged

his way to lower cholesterol. He is fifty-something and has already outlived all predecessors in his family.

As did Mr. Phillips in the cardiac unit of the hospital, Walt considered his cardiac risks as outcomes of genetic inheritance. By way of contrast, I viewed my low-risk status as the outcome of a heart-smart lifestyle. Indeed, my account conveys my assumption that I willfully controlled my risks. Their accounts express skepticism about the ability to control risks. Through field note commentaries, I was remarking on lifestyle choices, which constituted us as different and accentuated those differences by noting our relative ability or willingness to manage risks. According to my field notes, mine was a well-managed body in control of risks, while theirs were, by comparison, poorly managed and out of control.

The confidence and certainty I expressed about the management of my own body contrasts sharply with field note commentaries describing the degree of uncertainty that several research participants faced. In many instances, my field notes depict participants in relation to the unpredictability of their bodies and the precariousness of their lives.

Consider, for example, Ken, a cardiac transplant recipient who was waiting for a second heart. My notes comment on the extreme uncertainty in his life. His extensive atherosclerosis resulted in angina that limited him physically and caused him pain. He was on a wait list for a second heart while he participated in cardiac rehabilitation. An excerpt from my field notes documents the uncertainty of his bodily condition, along with my own concern about his future:

Wow, Ken had really bad angina today! He took nitro[lycerin] constantly. He kept indicating his pain was a seven on a scale of 1 to 10. Ten representing the pain of a full-blown heart attack. He told me he had to go to the hospital over the weekend as his pain was so bad he thought he was having a heart attack. I hope he gets a new heart soon. He's worrying he'll die of a heart attack any day now.

He's on the list for a transplant but probably won't be receiving one for quite some time—up to 2 years. I'm not sure that will be soon enough for him. He has BAD angina. He pops the nitros constantly. Sometimes he has angina when he comes in, stemming from the stress of driving over. His coronary arteries are badly clogged and they can't do bypass or angioplasty for him. I'm not sure why but transplanted people can't do these. He needs a new heart and fast.

Not only was Ken's body problematic on a day to day basis, but his future was highly uncertain, and my notes elaborate on these uncertainties.

I constituted my differences from participants through my actions and interactions in the field. Yet I imagined the differences between us in more dramatic terms after leaving the field. After producing detailed field notes chronicling conversations, observations, and insights about what I had observed, I donned a T-shirt, running shoes, and shorts, strapped a walkman around my waist, gulped a pint or so of water, and trotted out the door for a hilly 7.7-mile run. In addition to running, I enjoyed thrice weekly trips to Gold's Gym for rigorous sessions among fitness fanatics of all body types. There, I lifted weights in ambitious workouts, preparing for an upcoming bench-press contest. My exercise routines confirmed my endurance, strength, and vitality. And the consistency with which I adhered to them, along with my immaculate diet, convinced me that I was the master of my bodily destiny.

Through ethnographic practices and daily life, in and out of the field, I placed myself in the discursive field of cardiac risks. In this capacity, I constituted research participants as fundamentally different from me along dimensions derived from cardiac risk discourse. Moreover, in highlighting the physical appearances, debilities, and vulnerabilities of those I studied, I created an account that reinforced and sustained assumptions about the uncertainty and precariousness of participants' bodies relative to the certainty and stability of my own.

EMBODYING ETHNOGRAPHY: BODILY AND RELATIONAL DISRUPTIONS

Analysts of illness often evoke from those they study explanatory accounts that express how people make sense of and interpret the cause of their condition (Cowie 1976; Dingwall 2001; Frank 1995; Kleinman 1988; Williams 1984). In sharing stories of their heart disease, many participants in my study reflected on how they had denied, trivialized, or normalized bodily experiences that they later interpreted as cardiac conditions. In rereading bodily signs after being diagnosed with heart disease, they accounted for their previous failures to comprehend that what they were suffering from were cardiac conditions (Cowie 1976).

In light of what I now know about my body, I engaged in a similar process, in which I normalized and trivialized bodily symptoms while carrying out the research. At the time, I sensed that my lower abdomen

was larger than it had been in my youth. My clothes still fit, and my weight had not changed, but my pants were a bit tighter around my midsection. I reasoned that growth in my girth was related to a long, protracted aging process, involving the enlargement and downward shifting of my abdomen. Rather than worry about or medicalize changes such as these, I viewed them as natural and predictable bodily changes. I reasoned further that the countless hours a day I spent seated in front of a computer while transcribing interviews or producing field notes accounted for changes that I perceived as an increase in the size of my abdomen.

The Initial Scare

One day as I was lying face down on the grass, pulling my foot into a strategic position to stretch a tight left quadricep prior to an afternoon run, I felt something that was not quite right underneath my body. Perplexed about what it might be, I announced, to nobody in particular, "That's not me." It was a warm sunny day on the playing fields beside the university athletic complex. The splashing water in the Olympic-sized outdoor pool and the shrieks and shouts of ultimate Frisbee players drowned out my voice, which nobody else could hear. I remained prostrate, moving my trunk from side to side, as if to confirm my suspicions about what I felt beneath my abdomen. I felt no pain, but it seemed as though I were balanced atop a cantaloupe-sized water balloon. But the water balloon was inside me.

Curious, but not overly concerned, I went about my business with an uneventful run. As I showered in the locker room afterward, I began to compare the contours of my body with those of the women around me. There were four or five others present in the large collective shower, including one whose features approximated my own: she was fit and trim, about my height, and had a downward sloping, protruding abdomen, much like mine. I felt relieved. I returned to my locker, dried myself off, and began to dress. This woman and her friends were in the same aisle of the locker room where I was stationed.

Moments later, one of the friends asked her, "When's the baby due?" I could not help but overhear their conversation. "July," the woman responded. It was late April. I did the math, calculating that my own anatomical features approximated those of a woman who was six months pregnant.

I knew I was not pregnant but felt compelled to call the health center. The diagnosis of a pelvic mass during my gynecologic exam produced an instant disruption in research routines. I missed several sessions at the rehabilitation clinic to attend medical appointments with specialists. When I returned to the clinic, clients quizzed me about my absence. I had nothing to hide and told them about my mysterious mass. My field notes chronicle this turn of events:

Today was an odd day in rehab. There I was, among a collection of people with cardiac conditions but all I could think about was the status of this mass. Clients expressed more concern about my condition than their own. Yusef gave me his home phone number and suggested I talk to his wife. She went through the same surgery for an ovarian growth. The depressing thing is that she went to a gynecologic oncologist in [the city], rather than a gynecologist. The surgeon ended up taking everything out, even though her tumor was benign!!! Talk about lawsuit material. I know he's trying to reassure me by having me talk to his wife, but her story isn't exactly stress reducing.

Out of the field, my protruding abdomen and plausible gynecological pathologies became primary preoccupations. I continued with my exercise routines, but with each step I took along the route of my 7.7-mile run, I experienced the weight of the mass inside me. I continued pumping iron in preparation for the bench-press contest but wondered whether the soy shakes I consumed as sources of protein to build muscle had somehow stimulated the growth of the mass I harbored. Meanwhile, my mother phoned me almost daily, in a futile attempt to get me out of the gym for fear that the stresses and strains of lifting would force a rupture in the mass.

In the weeks prior to surgery, I read a series of books, surfed Web sites, and cultivated an impressive knowledge about gynecologic pathologies. Through this process, I performed my own differential diagnosis, reading and reasoning about my body in relation to the possibilities and probabilities described in chapters on cysts, tumors, and other pathologies detailed in gynecologic textbooks.

Despite the fact that I had been experiencing my body in relation to cardiac risks, my pelvic mass forced me to assess and experience my body in relation to risks for gynecologic conditions. It was an especially emotional experience for me to do so, as my father, a gynecologist for

more than thirty years, had passed away and was no longer available as a source of wisdom, comfort, or reassurance.

Finally, in a most masochistic manner, while contemplating the worst possible scenario to this unfolding drama, I sought information about the experience of ovarian cancer by reading Gilda Radner's autobiographical account of having ovarian cancer (Radner 1989). Radner's story prompted me to ponder how much time would remain if I woke up to a malignancy. Where would I live? Who would take care of me? Would I ever finish this work? Would I want to spend what little time remained writing about heart disease?

I began to fret over the inadequacies of my support systems, lamenting the isolating effects of research and writing. No longer taking classes, I was not meeting new students entering my graduate program. Living on a loan and not earning a salary, I had no job or colleagues with whom to interact on a daily basis. My best friends had either left to accept tenure-track appointments, fellowships in far-off lands, or dropped out of school. Indeed, research and writing routines had blinded me to isolation that I began to experience for the first time.

As grim thoughts such as these entered my consciousness, I underwent a series of tests in efforts to diagnose the mass with greater accuracy. Abdominal and transvaginal ultrasounds, intravenous pyelograms, cystoscopies, and CA-125 tests replaced the cardiac stress tests, body composition measures, blood pressures, and lipid profiles, which had captivated my attention earlier and which define and diagnose cardiac bodies.

Diagnosis Benign

Surrendering to major surgery, I was completely incapacitated by general anesthesia. I feared going in, What if they consider the mass benign but it's really malignant? What if they mistakenly assess it as malignant and remove all my reproductive organs? What will be missing when I wake up? What if I do not wake up? Questions of this sort became far more pressing than the questions informing the research.

I did wake up and was instantly informed that the mass was benign. Upon getting my staples removed, my surgeon showed me the pathology report and described the 3.3-pound tumor, serous cystadenoma, as a "precancerous" condition. Noting that "cancer comes from some-

where, and ovarian cancer comes from what you had,” he did not waste his words with technical jargon to describe pathophysiological details of carcinogenesis. The malignant form of the tumor, serous cystadenocarcinoma, is a rapidly progressing and almost always life-threatening form of ovarian cancer.

Having a precancerous condition for a life-threatening form of cancer was a deeply humbling experience. My extreme confidence about my health and bodily capacities, which I had taken for granted throughout my life, had been shaken. I was embarrassed that I had ever had the audacity to believe that I could determine my own health destiny. I also wondered whether the tumor would have become malignant had it not been removed in time.

I became preoccupied with “what if” questions while reassessing the structure and organization of my life, my vision of the future, and my relationships with family members, friends, and research participants. In effect, I was taking account of my mortality for the first time. Despite the fact that I had heard countless stories from participants whose acknowledgements of mortality were central to the heart disease experience, I did not fully appreciate what it means to face one’s mortality until *mine* was the one in question. Embodying uncertainty and experiencing fear were acutely distinct from writing about them in others’ lives. My experience enabled me to appreciate the tenuous nature of the body and the ambiguity of the boundary between health and illness.

REVISIONS IN RECIPROCITY

Upon being diagnosed with a pelvic mass, in many respects, mine became the body in question. During conversations with clients in rehabilitation, I was no longer the observer but was the object of observation, as they consoled and reassured me that everything would turn out well.

When I returned home following surgery, I was unable to drive for about a week because of my surgical incision. Lilly, one of the participants in my study, came to visit me. I had called her to cancel a scheduled interview, but she insisted we could carry it out at my place. I was surprised when she arrived with a bag of groceries—something I often did for her when I went to her house for an interview. At my apartment, we spent our time talking about my circumstances rather than hers. After an hour in which I shared my story, I identified the irony of the

situation: our statuses had been reversed—Lilly was interviewing me about my bodily trouble. Not only was it reassuring to share my story and commiserate with her about my ordeal, but it made me consider our similarities for the first time. Put simply, both of us had problematic, uncertain bodies. Lilly had endured a series of bodily troubles in her life, and I was living through my first.

Once I could drive, I returned to the focus group. I had been forced to cancel a session that was scheduled to meet the day after my surgery. Two participants who learned about my condition in the rehabilitation clinic shared with the others my reason for canceling the session.

Upon my return, participants were primarily concerned about me and wanted to hear about my hospitalization and surgery. As with any participant in the group, a person whose condition requires surgical intervention becomes the center of attention upon returning. In this instance, I had undergone the medical treatment and hospitalization. In telling my story to the group, it occurred to me that our relations were reversed, even if temporarily. I was not the researcher, chronicling and tape recording others' stories of problematic embodiment. I was, instead, one of them—a storyteller, with my own ordeal to communicate and share. It was, through the act of sharing my story with participants, that I recognized more clearly the similarities between us that were muddled by risk discourses, research practices, and theoretical agendas that had taken hold. It dawned on me that I was one of them—a vulnerable, precarious, body at risk.

DISCUSSION

The differences I imagined between participants and myself were as much a product of my theoretic captivation and field-note constructions as they were the medical categories I had relied on to develop and design the research. Far more effectively than Foucault had managed, the scare made me critical of the categories and designations that were the basis for articulating my research problem and topic in the first place.

As a biographical disruption (Bury 1982), my scare prompted me to appreciate, personally, what my sociological analysis only suggested theoretically: awareness of my mortality and the ambiguity of the boundary that separates health from illness. The scare was my wake-up

call, just as research participants had described their heart attacks. It was my elusory body, rather than intellectual acuity, that prompted me to appreciate and recognize similarities between research participants and me. We shared similarities deriving from our common status as inhabitants of uncertain bodies; from our experiences of bodily discomforts, diagnostic procedures, and invasive surgical interventions; and from confrontations with our mortality.

The scare encouraged me to scrutinize, more critically, calculative orientations to risk that comprehend bodily destinies through the logic of probability. Indeed, risk discourses, articulated through the idioms of biomedicine and positivist science, do nothing to prepare people for the consequences of fate, bad luck, or “outlier” status. Risk discourses informed my sampling procedures, while my research practices looked for and chronicled differences that existed between research participants and myself. In pondering my own at-risk body, I could see more clearly how disease designations and risk profiles elicit social categorizations and identities that do as much to separate as unify people.

COMPASSIONATE SOCIOLOGY: BEARING WITNESS TO HUMAN FRAILTY

Theory, writing, and ethnography are inseparable practices (Denzin 2002). Endorsing particular theories, like donning specific glasses, can clarify and sharpen, just as it can blur and distort our vision of the field. Theories, like glasses, can illuminate and expose what was invisible or unrecognizable prior to their use, just as they can render unfamiliar what we had previously taken for granted before seeing through them. Theories, like glasses, may enable us to see or focus in on particular features of a field better than others, while making it difficult to see the entire picture all at once. But resisting or suspending theory is no more innocent, or even possible, than is shutting our lids to avoid being biased by what lies before our eyes. However wide or squinted our eyes, however thick, thin, tinted or absent our glasses, our descriptions are *interpretations* of the field (Atkinson 1990; Atkinson Coffey, and Delamont 1999; Clifford 1986, 1990; Denzin 1997), which offer “renderings of reality” (Charmaz 2000, 533; 1995). Our interpretations are necessarily partial and incomplete. As tentative possibilities among many ways of telling the story (Mitchell and Charmaz 1996), our

interpretations involve inevitable blind spots that preclude our conveying the whole truth and occlude our seeing all of what is in the field.

At the start of this article, I noted that Radley (1995, 21) is troubled by the fact that constructionist theories effectively deny the lived, body-subject. By privileging “constructions that are its multiple realities” social constructionism serves as a kind of “epistemological determinism” whose “self-serving principles” “continually hide [the bodies] they seek to find.”

The “self-serving” principles that Radley (1995) alludes to can be observed not only within accounts that endorse constructionist theories but also through a range of analytic and organizational practices that social researchers rely on. Frank (2001, 360) criticizes the process by which social scientists assimilate the experiences of ill people by reducing the specificity of suffering to “extralocal categories that organize responses,” contending that researchers, first and foremost, should be committed to encountering and bearing witness to suffering:

Whatever aim the research seeks, the researcher cannot evade the responsibility of first encountering suffering and then keeping the specificity of that encounter at the center of the project, refusing to assimilate that encounter to extralocal organization. The aim of research is to oppose the censoring of all the things that do not fit.

Charmaz (2002, 303), also, warns that researchers’ imposition of a “narrative frame” can mask, rather than illuminate, participants’ experience of illness and especially the meaning of their suffering. She is especially wary of “fleeting relations” in qualitative research involving “one-shot” interviewing (1995, 58), which preclude the development of sustained relations between researchers and respondents and increase the likelihood of misuses of power and authority.

Through my research practices, I assimilated participants’ experiences of suffering to “extralocal categories,” in Frank’s terms. During early stages of research, I applied a theoretical frame that limited my capacity to represent, or comprehend fully, the diversity and meaning of their suffering. Participants’ experience of heart disease became colonized by and subsumed within the rhetoric and discourse of risk. My preoccupation with risk turned my ethnographic attention away from unique and complex stories of suffering and toward a narrower, medicentric interest in cardiac risk management. Moreover, my fervent

following of Foucault and his attendant discourses of power and knowledge diminished my capacity for being reflexive about my own body, while suppressing my access to its truths.

Frank (1997, 106) considers illness as a moral occasion and the telling of illness stories as acts of witness (1995, 1996). From his view, studying illness ought to be less about *explanation* and more about *bearing witness*:

Both one's actions as an ill person, and one's actions toward the ill, begin with a capacity to recognize suffering and to communicate that recognition. This recognition ought to be self-evident, but sadly it is not ... the issue is not explanation. Rather it is what you and I, as embodied human beings, make of each other, what we each make of ourselves, and how we move from the hermeneutic circle of those configurations into actions toward each other: caring for each other or pushing each other away. The issue is the lived ethics of bodies.

For Frank, the sharing of stories of illness is a moral activity that enables us to recognize our shared bodily vulnerability.

Charmaz (1999) also underscores the moral character of illness, contending that there is an implicit hierarchy of moral status in suffering. She notes how stories of suffering can reflect, redefine, or resist moral status. When I departed from Mr. Phillips's room, I refused him the moral status that he sought to achieve through his assertions. My questions, unwittingly, imposed a lesser moral status on him—one he angrily opposed through his declarations. Had I been disposed simply to listen to him and accept, rather than question, his claims, I might have cultivated a deeper appreciation of his suffering and a greater understanding of the meaning of his illness. My own discomfort resulted in my dodging out of the room and, hence, denying him the opportunity to speak. In this capacity, I did not hear the depth and complexity of his story and failed to bear witness to the specificity of his suffering. Ultimately, it was my elusory body and its correspondent suffering that enabled me to recognize the vulnerability I shared with participants in my study and to bear witness to their suffering in the way Frank advocates.

The lived ethics of bodies that Frank identifies in encounters with illness is not unique to the study of illness. Bochner and Ellis (1999, 493) consider ethics as the definitive feature of all ethnography and pose a

series of questions that address what they consider as the ultimate objectives of research:

We view ethics as the core of ethnography. What are our obligations to other human beings? What are our obligations to ourselves? What sort of human beings do we want to become? How can we become more caring, more involved, and more helpful? How should we live our lives? These questions are ethical, but they are also political and spiritual as well. They are not questions that make you worry about whether you are being scientific or not. They are more about the consequences of human differences, about the potentials and limitations of human communication and relationship, and about how to keep conversation going.

Recent debates about the future of ethnography demonstrate decidedly different visions about its purpose. The vision of ethnography that Bochner and Ellis propose represents a shift away from realist objectives and positivist protocols in qualitative research to endorse the interpretive turn, which highlights the constructed character of ethnographic accounts and embraces alternative writing strategies.

Charmaz (1995) suggests that the interpretive turn in ethnography compels researchers to clarify the position they occupy between positivism and postmodernism. She submits that many researchers working within the interpretive tradition of empirical ethnographic research stand and move between positivism and postmodernism. I agree with Charmaz and recognize the need for empirically grounded social inquiry that addresses the material conditions of the social world while acknowledging the constructed and partial character of ethnographic representations of field realities. Elsewhere (Wheatley 1994, 423), I use the metaphor of dance to endorse a “flexibility of mind” that allows for movement across the boundary that separates positivist from postmodernist positions:

Dancing allows for, in fact, requires ongoing movements, displacement and improvisation. . . . There are, indeed, dances we prefer, and others that we might not or cannot perform. Must we rank or privilege a particular style or set of steps over others? It may be more strategic to learn to dance many dances—to venture on either, or both sides of the fence that [divides postmodernist from positivist positions]. . . . We can constitute new ways of marking territories, articulating politics, inventing “sides,” and doing dances. Productive possibilities will be enacted in the dances

that we do—our styles and steps make [ethnography] the lively and inventive endeavors of political praxis. Static positions, fixed borders, and implicit hierarchies inhibit our movements as well as their transformative potential.

I view the interpretive turn in ethnography as enhancing our individual and collective efforts to explore, interpret, and represent the social worlds that we encounter and inhabit. Our heightened sensitivity about the moral and ethical implications of research and writing should inspire, rather than impede, our movements.

The criticism that Frank communicates above describes precisely the analytic and writing strategies I adopted in organizing and presenting my ethnography of heart disease (Wheatley, forthcoming). Post-modernist perspectives inform features of the work, yet my writing relies on realist conventions in assembling and presenting evidence to promote an argument. While I did bear witness to suffering in the manner Frank advocates, my writing has yet to achieve the ideal he endorses.

Regardless of our epistemological persuasions and theoretical allegiances, the interpretive turn in ethnography compels us to grapple with ethical and moral issues from start to finish. Denzin (2001, 24) highlights the ethical and moral dimensions of ethnographic research and considers reflexive interviewing as an activity and relationship that belongs to a moral community:

As researchers we belong to a moral community. Doing interviews is a privilege granted us, not a right that we have. Interviews are things that belong to us. Interviews are part of the dialogic conversation that connects all of us to this larger moral community. Interviews arise out of performance events. They transform information into shared experience.

From this perspective, ethnography itself is an embodied performance (Ellingson 1998; Monaghan 2003; McCorkel and Myers 2003; Reich 2003; Spry 2001; Turner 2000; Warren and Fassett 2002), which, like dance, flirtation, and play, mentioned by Radley (1995), configures meanings in complex and paradoxical ways. Meanings emerging from my ethnographic practices early on were enabled and constrained by the power and discourse of Foucauldian thought. Yet the meanings produced by my body were not reducible to, nor could they be contained by, the discourse that had captivated my consciousness, directed my

gaze, and informed my ethnographic endeavors. My body configured meanings in ways that exceeded the limits of even my own ethnographic imagination, disrupting the retroductive analysis in progress and transfiguring the work in tangible ways.⁴

Most importantly, the cancer scare transformed the relationship between research participants and myself, forcing me to recognize our shared vulnerability, precariousness, and frailty as human beings (Turner 1993; Turner and Rojek 2001). The theoretical eclecticism that directed my steps at the start, and when I turned to Foucault, provided the latitude necessary to accommodate my shift in perspective along the way. Mine was a consequential shift in vantage points, one that gave me access to a shared moral community of speakers, listeners, and bodies.

My previous theorizing—and even moralizing—had threatened the dignity of the research, as well as my relationship with research participants. My cancer scare was a humbling experience that shattered the confidence I had about my body and forced me to question the adequacy of my interpretive account of the field.⁵ Moreover, it promoted a form of reconciliation (Frank 1996) along with a reflexive rediscovery of my place within a moral community of sufferers. It is a place that privileges empathy over explanation, compassion over judgment, and the lived bodies of participants over a way of seeing that obscured what I sought to find.

NOTES

1. Turner (1992) laments the fact that so many sociologists use a specific theory or paradigm and proceed to criticize the inadequacies or incompatibilities of alternative perspectives for addressing particular problems. He considers the practice of promoting a specific theory, while denouncing others, as a “strategy of annihilation” that sustains what he sees as the endless sectarianism in social theory. As Turner explains, most work on the body can be understood in terms of the distinction between foundationalist and antifoundationalist approaches. Foundationalist approaches construe the body as an objective reality that exists independently of its social construction. Antifoundationalist approaches do not take the body as a unitary or universal phenomenon but as constructed through practices and discourses that vary across time and place. Turner suggests that there is no need to treat the body, categorically, as either a constructed or a fixed material entity. Instead, some theoretical integration or rapprochement between these perspectives allows for new sociological possibilities. The particular problems we address influence where we position ourselves on the continuum spanning these positions.

2. All names used are pseudonyms.

3. Not only was this version of constructionism rendering bodies elusive, as Radley suggests, but my study was becoming decidedly medicocentric (Frank 1997), not unlike the slew of illness behavior studies inspired by Parsons's sick role formulation (1951, 1975). Just as sick role obligations informed countless studies about patient compliance in the 1950s and beyond, my preoccupation with risk led me to view participants' conduct, in key ways, as either sensible or misguided with respect to their pursuit or neglect of risk reduction practices.

4. Radley (1995) is skeptical about the turn to narrative as the privileged source of meaning about the body since it gives primacy to experience that is spoken. He insists that meanings are configured and originate in the materiality of the body-subject and that denotations of bodily experience through storytelling are inadequate as conveyors of those meanings. I am less skeptical than Radley is about narrative. Evocations of experience through storytelling are constitutive elements in the configuration and transformation of meanings, identities, and relationships. It was through the act of storytelling that I accessed certain truths about similarities I shared with participants in my study. I cannot ignore the transformative power of stories, along with the inadequacy of rational explanations for comprehending fully the truths and mysteries of that power.

5. The loss of confidence I allude to can be viewed favorably. Behar (1999, 484) insists that ethnographers ought to go "everywhere" with "heads bowed" and "never with too much confidence." By way of contrast, Hallett and Fine (2000, 612) applaud the "audacious confidence" that ethnographers from a century past had in their work and express dismay about what they consider as the decrease in clarity, confidence, and authority in contemporary ethnographic research. I share Behar's sentiments and now believe that *humility* is the starting point for ethnographic enhancement.

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