Open to Disruption

Nelson, Margaret K., Hertz, Rosanna, Garey, Anita Ilta

Published by Vanderbilt University Press

Nelson, Margaret K., et al.
Open to Disruption: Time and Craft in the Practice of Slow Sociology.
Vanderbilt University Press, 2014.
Project MUSE. muse.jhu.edu/book/34408.

For additional information about this book
https://muse.jhu.edu/book/34408

For content related to this chapter
https://muse.jhu.edu/related_content?type=book&id=1316755
6

Disrupting Scholarship

Susan E. Bell

When Susan Bell first interviewed DES daughters in 1982, could she possibly have imagined the book she eventually wrote? It is not necessarily a recommendation for a book to be 30 years in the making, but DES Daughters is a remarkably seamless weaving of older and more recent approaches to studying health and illness.

Arthur Frank (2010, 817)

I admit to blushing when I read the first two sentences of Frank’s review of my book, not because of the praise he heaps on it but because of the date range he names in it: 1982–2009 is indeed a very long time for a book—not quite thirty years, but so long that it demands explanation. For many years I would not name the starting date, hoping to avoid the stigma of being so slow and preferring to pass as a “normal” scholar, one who managed to publish at least one article or book chapter per year. Even now I find myself start to sweat as I write these words and that is a signal that this chapter places the body at its center.

I was a postdoctoral fellow when I began my study of DES daughters. I then started a tenure-track job at Bowdoin College. I adopted a son and gave birth to a daughter within an eleven-month stretch of time before I was tenured, and completed the book when they were both in college. “Disrupting Scholarship” is about the ways in which my commitment to activism and raising my children interrupted my scholarship, the ways in which feminist and narrative scholars disrupted standard ways of doing sociology in the 1980s, and the ways my activist and family life disrupted and is interwoven into the thirty years it took me to complete DES Daughters: Embodied Knowledge and
the Transformation of Women’s Health Politics. I begin by briefly describing the book. I then trace tensions in my work between feminist scholarship and women’s health activism, describe changing epistemologies in various fields of social science, and explore how these changing epistemologies are reflected in the development of the book. I demonstrate my openness to learning about them and to working with them to expand my thinking, change my research strategy, and transform my writing. Following my discussions of these disruptions, I provide another context for understanding my version of slow sociology by focusing on how my personal struggle to balance my life with my work over the course of the project illustrates the need for long-term social structural and cultural changes.

My book tells a story about women who attained legendary status in the annals of medicine. They were exposed prenatally to a medication promoted as a wonder drug that would prevent miscarriage. In 1971, the *New England Journal of Medicine* published an article about DES (diethylstilbestrol) that announced what is now recognized as a watershed in medicine (Herbst, Ulfelder, and Poskanzer 1971). The authors of the study, physicians at Massachusetts General Hospital (MGH), reported an association between DES and vaginal cancer in women who were just fifteen to twenty-two years old. From the 1940s to the 1970s, between five and ten million women had taken DES during pregnancy. When their daughters became teenagers and some of them developed reproductive tract cancer, the MGH physicians identified DES as the first transplacentral carcinogen and the daughters took on the new identity of “DES daughters.” When DES daughters had trouble becoming pregnant and giving birth to healthy babies, DES was connected to miscarriage and other problems during pregnancy. These characteristics—crossing the placenta, disrupting the developing fetus, and affecting the bodies of DES daughters in multiple ways that often do not appear for many years—are those that identify DES as the first endocrine disruptor. After 1971, DES daughters and their mothers began to participate collectively as well as individually in DES regimes of practice, and in so doing they reconfigured the political landscape, with consequences for DES daughters, the doing of science, and the practice of medicine. Joining together to found the grassroots organizations DES Action (1975) and the DES Cancer Network (1982), activists challenged existing medical and scientific knowledge about DES. They also developed alliances with medical scientists to pursue funding and support for DES research, screening, and treatment programs.
My book argues that the DES story is about more than a tragedy that occurred to a population in the mid-twentieth century and more than a humbling experience for medicine. It shows that the DES story is also about a women’s health movement that questioned whether doctors always know best. These women were among the first to judge science based on their intimate, firsthand knowledge of their own bodies, and to join together in collective action for social change. DES daughters used what they had and created a different kind of social movement, one that actively contributed to the development of transformed relationships of power and knowledge, not only in the history of DES and care for DES daughters but also in the politics of women’s health more generally. *DES Daughters* connects three frameworks—feminist health scholarship, embodied health movements, and narrative—and weaves them together to tell a story about embodied knowledge and the transformation of women’s health politics.

I simply couldn’t have written the book I eventually completed if I had finished it twenty years ago, or even ten years ago. It would have been much more narrowly focused on interview material, would have excluded the analysis of visual and performative evidence, would not have given attention to how science can be “undone,” and would not have shown how DES daughters created an embodied health movement. I would have been less visible in the text.

**Connecting Activism and Scholarship**

Before becoming a sociologist, I worked in feminist health centers on the East Coast and West Coast of the United States in the early 1970s to transform how women’s health care and knowledge about women’s health and health care are developed and taught. I chose a dissertation topic—a study of the science of DES from the time it was synthesized in 1938 to its release for sale in 1941—while I was working in feminist health centers, because I wanted to combine my activist and scholarly commitments (Bell 1987, 1995). I stopped working in women’s health centers before I became a full-time faculty member at Bowdoin College, and from the early 1980s to the late 1990s I combined feminist critique with women’s health information by writing chapters in what has been called the “bible” of feminist health, *Our Bodies, Ourselves (OBOS)*; I authored the birth control chapter in the 1984, 1992, and 1998 editions of *OBOS*. As an untenured professor, I was advised to clearly distinguish these activist and scholarly strands of work—to list them in two different categories.
on my CV—and to focus on the scholarly category of publications. That said, doing the research and writing for each version of “Birth Control” became my primary task for roughly nine months (the equivalent of a semester and summer—and the length of a pregnancy) in each instance, and thus privileged activist writing for large chunks of time.

In “Birth Control,” my work consisted of reviewing the scientific and medical literature about current and possible future birth control methods, reviewing national and international policies about the development and distribution of birth control, talking with scientists and clinicians about their research, distilling these materials, and translating them into a text that people without any specialized scientific knowledge could understand and use. In “Birth Control,” I connected the world inside the academy to the world outside of it. For me, the stakes were high: I wanted the text to be accurate and accessible. I wanted it to reflect a range of scientific views, and I wanted it to provide a critical perspective on this knowledge. I wanted to provide all of this for readers who were thinking about using birth control because they didn’t want to get pregnant.¹

Despite the periodic activist disruptions to my scholarly output when I wrote “Birth Control,” I successfully turned work on the 1992 and 1998 editions of Our Bodies, Ourselves into scholarly publications, and in each of them I reflected on the feminist and reproductive science of birth control. After I read and taught Emily Martin’s The Woman in the Body (1987), I incorporated her critique into the language of the 1992 edition of “Birth Control” (see Bell 1994). When I reviewed the literature about potential future methods of birth control for the 1998 version of “Birth Control,” I learned about a partnership between women’s health activists (Women’s Health Advocates on Microbicides) and reproductive scientists (at the Population Council) to produce vaginal microbicides to prevent transmission of HIV and other sexually transmitted diseases. I relegated the partnership to a long footnote in OBOS but later turned to a more sustained examination of this transformation in women’s health politics (Bell 2000, 2003).

To put it simply, for two decades I published two lines of work: one was activist and the other was scholarly, and although each drew inspiration and evidence from the other, I viewed them as distinct and I viewed myself as doing either one or the other. Eventually, my ongoing commitment to joining activism and scholarship found its way into DES Daughters, in my investigation of the emergence and effects of DES as an “embodied health
movement” based on a different conception of identity. I could see practices and identities differently in light of feminist and postmodern scholarship (e.g., Foucault 1978; Scott 1991; Haraway 1988, 1991). Haraway (1991, 181) questioned the possibility of an “identity” in which “we” can be clearly demarcated from “they” and argued that giving up the search for bounded individuals with distinct statuses or a universal totalizing theory could open up the possibility of heteroglossia and of taking seriously partiality and fluidity. Similarly, Scott (1991, 780) argued that “it is not individuals who have experience but subjects who are constituted through experience.” These ways of understanding selves and identities gave me a framework for seeing how my activism and scholarship could be intimately connected—and indeed that ignoring this connection could be problematic. As I discuss below, this scholarship also gave credence to empathy, connection, and responsibility as sound research strategies (Reinharz 1992). Many of these scholars included themselves in the analysis (e.g., Behar 1993), and thereby provided models for engaged research.

As I write in my book, young women who have become known as DES daughters have taken actions on their own behalf that have contributed to transformations in ways of knowing, investigating, and monitoring their hybrid bodies’ combination of natural and synthetic estrogens. Cultural discourses shaped their individual responses, and their collective responses created new pathways, transformed relations of power and knowledge, and contributed to making new spaces and conversations. Additionally, their organized actions, emanating from the groups they formed (DES Action and the DES Cancer Network), led to interdisciplinary, international conferences about DES in 1992 and 1999 where they worked to “undo science as usual” (Bell 2009) by introducing subjective knowledges into the conference discourses; their organized actions also led to funding and legislation favorable to research that continue to this day. These lines of research and practice have transformed the doing of science by incorporating activists in the conceptualization and conduct of DES science.

The DES daughters themselves (not the book) produced an embodied health movement. Embodied health movements frame their organizing efforts and critiques of the system through personal awareness and understanding of individuals’ experiences. They also challenge science by forming alliances with scientists to secure funding and legislation and by collaborating with scientists in the doing of scientific research itself. The politicized collective work of embodied health movements transforms illness
experiences, critiques medicine’s treatment of patients, and turns attention away from individual bodies as sites of risk to the risky environments in which they live.

The concept of “embodied health movement” was first proposed by Phil Brown and his colleagues (2004). It was given depth by Maren Klawiter (2008) in her study of breast cancer and Steve Epstein (2000) in his study of HIV/AIDS. Individually and collectively, DES daughters drew from cultural resources and put “together in novel ways bits and pieces of what they found at hand” (Layne 2003, 236–37) to create new knowledges, institutions, and practices. One of these new knowledges was rooted in their embodied experiences of worrying about having cancer, having cancer, becoming and not becoming mothers, and living with the lifelong effects of endocrine-disrupted, synthetic chemical–infused bodies. The DES embodied health movement embraces fluid collectivities. Its past reflects the ways in which illness, suffering, and uncertainty can become opportunities for making social change. Its lessons include the value of embodied knowledges as integral elements in doing meaningful science.

Disrupting Scholarship: From a Study of Stress and Coping to a Narrative Analysis

In a memo dated September 16, 1982, I wrote:

If you examine the assumptions (theoretical framework) underlying concepts of “coping,” “stress” [and] “social support,” you run into familiar terrain—a structural/functional understanding of the world. This has implications both for the formulation of these concepts and their study, as well as for analyses of them (e.g., Coelho, Hamburg & Adams [1974]; and Pearlin & Schooler [1978]). This perspective limits what can possibly be discovered as ways of coping, for example, or as relationships between individual and “social support.” . . . As Thoits [1982] and a few others point out, these relationships may not always be supportive—or I might add elements of them may not be supportive. The theories of stress, coping and social supports stem from (at least the sociological ones do) [Walter B.] Cannon’s and [René J.] Dubos’ notion of homeostasis. This notion has already been criticized by Marxists. Also, the methodology involved in studying ways in which people cope, whether or not they’re successful, etc., limits what can be discovered
about this. Using (or developing) scales predetermines what people are allowed to say, and how they’ll describe themselves. And experimental designs decontextualize them further.

If, on the other hand, you create another “open” theory, contextualize the study, and use a more flexible methodology, you can begin to understand both the concepts and the strategies people use and the outcome much more comprehensively. That’s what I propose to do.

My postdoctoral project was designed to enlarge and deepen our understanding of how people respond to and cope with exposure to synthetic hazardous substances—an exposure that increases the risk of serious illness. In my application for the postdoc, I wrote that I wanted to trace the emotional and physical impact of DES on a person’s life and that “a prerequisite to this is understanding the ways in which the psychological component of experience can be described and assessed.” Thus, I proposed to study the field of stress, coping, and social support. By the end of the postdoctoral fellowship, I was discouraged by the limits of this field, but nonetheless I designed and began the empirical portion of the research based on its epistemology.

The field of stress, coping, and social supports was a hugely influential field in medical sociology at the time. It offered a theoretical framework and empirical approach to understanding the health of individuals by attending to the effect of life events and ongoing circumstances on their health, as well as to their responses to these events, circumstances, and outcomes. Theories of stress and coping examine pressures in the larger social environment to which people react protectively. After a year of studying the literature and trying to understand what I heard in my pilot interviews with DES daughters, I became increasingly discouraged by its potential for explaining the emotional and physical impact of DES on a person’s life and for explaining how DES daughters lived with risk.

As I wrote in a series of memos from that period (which I unearthed for writing this chapter), I identified three major problems in this field of research. First, the concepts used in these theories and studies—stress, coping and adaptation, social supports—were loosely defined. Furthermore, while the analysts argued in theory that people interact with their environments, they treated people as passive individuals who must adapt to changes in their environments, seeking only to return to a state of equilibrium, or homeostasis, in the face of threats. Second, research in stress and coping did
not investigate how people’s responses help reproduce a stressful environment or, alternatively, help change it. The literature also decontextualized events and did not take historical circumstances into account. Third, most analyses of stress and coping at that time used quantitative measures and methods of aggregate statistical analyses.

I concluded that key aspects of DES daughters’ experiences fell beyond the theoretical and methodological framework of the stress and coping literature. For example, the range of strategies DES daughters adopted in response to threats they faced could not be characterized as solely passive and defensive. They broadened the potential range of choices for young adult women in their roles as patients, consumers, and political actors.

In the analysis of pilot interviews, I saw stories emerging in response to the question with which I began: “How did you find out you were a DES daughter and what was it like?” (Bell 1988). Although I did not ask the women to tell stories, they often answered my opening question and subsequent questions by telling stories. Seeing stories in the interviews tapped into and reflected what is now referred to as the narrative turn in the social sciences. At the time, a number of scholars, including my mentor at Harvard, Elliot Mishler (1986), argued that because interviewers and respondents jointly construct interviews, respondents will recall and report some material in the form of stories if interviewers allow them to. This depends on how they negotiate their dual and shifting roles as interviewer/listener-to-a-story and respondent/narrator during the course of the interview. During interviews with DES daughters, I listened carefully without interrupting, allowed the women to direct the flow of the interview (pace, topics, topic shifts), and remained noncommittal. Although I had not intended it to, my interview approach created space for DES daughters to tell stories to tie together significant events and important relationships in their lives, and to make sense of their experiences.

Social scientists had begun to draw inspiration from literary scholars (e.g., Mitchell 1981) and had begun to consider how the form and content of stories told during interviews could shed light on the experiences of individuals (e.g., Labov 1972; Paget 1982, 1983; Mishler 1986), including illness experiences (Williams 1984; Kleinman 1988). Today, “narrative” is a term used ubiquitously in social science discourse, and there are different—competing—strands of narrative research (Riessman 2008; Andrews, Squire, and Tamboukou 2008; Polletta et al. 2011). At the time I made my turn, there were few guideposts for what “narrative” means or what would constitute “narrative analysis.”
Building on the work of Labov and Waletzky (1967), I defined narrative as a sequence of ordered events that are constructed in a meaningful way for a particular audience in order to make sense of the world or people’s experiences in it (Hinchman and Hinchman 1997). More than a list or chronicle, a narrative adds up to “something.” A narrative has a point, or a moral lesson (the answer to the “so what” that one might ask after it ends). How it is put together—the images and explanations woven into it, and the position of the narrator in relation to the events and audience—all convey something important about a narrative’s meanings. In addition, a narrative is produced collaboratively; the particular context in which a narrative is produced shapes its production and interpretation. When a person’s life is interrupted by an illness, narrative offers “an opportunity to knit together the split ends of time, to construct a new context,” and to fit the disruption caused by illness “into a temporal framework” (Hydén 1997, 53). Narrative analysis takes the story itself as its “object of investigation” (Riessman 2008).

In its reflexivity, the narrative turn is one response to disenchantment with the “dominant ‘Cartesian’ paradigm of rationality” at the core of modern social science (Hinchman and Hinchman 1997, xiv). Narrative approaches counter traditional social science models of knowledge by stressing that there are multiple truths, constructed by knowers who are socially and historically located, about a world that is neither fixed nor independent of knowers. At any point in time, there is a plurality of truthful narratives that differently positioned members of a culture can reasonably claim. Because narratives are constructed at particular moments in time and directed to particular audiences, they are about pasts of the moments in which they are told; a truthful narrative might be substantially different if told in other moments or to other audiences (Williams 1984, 198).

After turning to narrative, my perspective widened and expanded and over the years has incorporated and contributed to the development of narrative studies (e.g. Bell 2002, 2004, 2006). The body of work in narrative now reaches across many fields of research, such as the study of images (Harper 2000; Rich and Patashnick 2002), politics (Polletta 2006), letters (Jolly and Stanley 2005), medicine (Charon 2006), law (Ewick and Sibley 2003), and public discourse (Cameron 2006; Baxter 2006). The core evidence in DES Daughters is personal narratives from in-depth interviews. The stories—personal narratives—that emerged during these interviews include rich and complex clues about the ways individual DES daughters have understood and engaged with medicine since the 1960s. But by tracing
details of narratives in different contexts and genres, I am able to make sense of DES daughters’ accumulated knowledge and experiences of their bodies and reveal their participation in old and new regimes of DES. The book analyzes narrative discourse in the autobiographical documentary film by DES daughter Judith Helfand, *A Healthy Baby Girl* (1996), letters to the editor of the *DES Action Voice*, panel presentations and discussion at the National Institutes of Health–sponsored 1992 DES Workshop, and the program of the thirtieth anniversary celebration of DES Action in 2008.

The multifaceted nature of embodied health movements like the DES daughters’ movement opens them up to be studied in a nonlinear way. In fact, their fluidity lends itself to a narrative understanding of them. Narrative analysis emphasizes multiplicity, fluidity, and reflexivity in its approaches to understanding social life. The construction of DES daughters’ stories in interviews, published accounts, workshops, and films—just like the events that the stories portrayed—took place in specific historical contexts and shifting relations of power.

**Disrupting Scholarship: Feminist Transformations**

In a memo dated May 3, 1987, I wrote:

Stella had received my letter 5 years ago, recruiting subjects for my study. She had saved (and not returned) the postcard. She was calling me now to find out whether I had results, because she’s just beginning to think about filing a law suit . . . .

It would be safe to think of subjects as cases, as individuals I interviewed at one point in time to produce data that is no longer connected to a person. It would be safe to distance myself from them during the interview, to present the same self to each of them, to remain unconnected to the particular words, emotions, and conditions of each interview. Allowing myself to empathize, to be drawn into their lives, to let their words (and world) pour over me, is much riskier. I feel responsible to them; I feel that the core of the narratives, the points, are crucial not only because I’m trying to seek a “truth” about living with risk, but because I’m trying to preserve the integrity of the truths each woman—each individual person I interview—tells. My goal then, is to speak not only to the discipline to the scholarly world, but to these women, to my subjects. In a
sense, I become, in Tracy Paget’s words, a “vehicle” for them to tell their stories; I recast them in the language of science to make them acceptable, to give them scientific respectability, to explain their universality, or at least their relevance, to others. At the same time, I must inevitably distort in order to draw out. This is a problem all scholars face—the necessity of cutting and slicing and splicing—but when I remember whose life I must cut and slice and splice it makes the task not only difficult but painful.

The theoretical framework of my research, and the introduction of narrative analysis, changed before I finished recruiting subjects and conducting interviews. Until the early 1990s, I employed a conventional model of qualitative interviews (Lofland 1971). I designed the study to include a purposive sample of thirty adult women—ten DES daughters who had had DES cancer, ten who had not had DES cancer, and ten women not exposed to DES—and to conduct two semi-structured, in-depth face-to-face tape-recorded interviews with each of them.4 To identify DES daughters with a wide range of experience, I used intermediaries: clinics treating DES daughters, physicians specializing in the treatment of DES cancer, the attorneys for a class-action lawsuit, and DES Action. Each of these sources sent a letter to a random sample of potential subjects on my behalf, describing the study and asking them to contact me (by returning a postcard or telephoning me) if they were interested in learning more about my work or participating in the study. I excluded women from the study if I already knew them from my participation in women’s health or DES networks. Using this strategy of recruitment, I was able to identify and interview seventeen DES daughters between 1982 and 1986. During interviews with these women, my approach was consistent with conventional understandings of qualitative interviewing. I did my best to avoid influencing the interviews and introducing bias into the collection of data (see Mishler 1986). This entailed assembling a sample of respondents and eliciting answers from them in an objective, value-free, and (ironically) disembodied way. I tried to conceive of the DES daughters as cases for producing data and of my proper role as a distanced interviewer. I tried to present the same self to each of them and to remain unconnected to the particular words, emotions, and conditions of each woman as I interviewed her.

My approach to the interviews changed over the course of the study, as my conceptions of my position as an interviewer and knower changed and
as I became more enmeshed in the world of DES daughters. My changing understanding of interviewing, recruiting subjects, and the design of the project reflected feminist studies of sociological practice (Roberts 1981; Stacey 1988; Personal Narratives Group 1989; Reinharz 1992). I allowed myself to empathize with the women I interviewed, to be connected with them, to feel responsible for them as individuals, and to contribute to the embodied health politics of DES. I wanted to acknowledge and represent multiple truths and to speak not only to the discipline and to the scholarly world, but to my subjects.

Both in recruiting DES daughters and in the interviews, I became less concerned about maintaining distance and neutrality. As I became accustomed to talking with DES daughters about my ongoing work, I felt more comfortable about being candid about my study when I met them and becoming more flexible and open with them during the interviews. I met informally with DES daughters at conferences and workshops and talked about my study with them. Often, DES daughters would volunteer for interviews. In 1992, to complete the sample of DES daughters, I followed up on offers by three of the DES cancer daughters and invited them to participate in the study. When one of them asked to see what I had written about DES before the interview, I sent her a copy of one of my published articles. She began the interview by responding to the article and I incorporated her response into my analysis of her interview (Bell 1999a, 369–370; 2009, 55–56). My shift in strategy was both a pragmatic response to the difficulty of locating DES daughters who had developed this very rare cancer and a reflection of transformations in the study of lives by feminist scholars (Behar 1991, 1993, 1996; Behar and Gordon 1995).

At two of the conferences I gave formal talks based on my study. In 1992, I presented an early draft of “Looking at Bodies” with my coauthor Roberta Apfel (Bell and Apfel 1995). The article focuses on the symbolic and material meanings about gender and sexuality surrounding women’s bodies by interpreting three accounts about the surgical removal of a woman’s vagina and its reconstruction after DES-related cancer: a presentation given by a feminist health activist at a DES Action conference, a presentation given by a gynecologic oncologist at the 1992 NIH-sponsored DES Workshop, and an interview from my study. Several DES daughters spoke to me after the talk and told me they were ambivalent about making their vaginas “public.” In the conclusion to the article, Apfel and I obliquely responded to this
concern: “The three vignettes represent the sense in which our focus on vaginas risks and resists the reduction of women to an organ-specific care and an essentialist theory of gender. . . [DES cancer daughters’] intentional reaffirmation of gender norms and sexual boundaries, combined with the uncertainty they feel after reconstruction, suggests both the importance of having a vagina and the limitations of using the vagina to define women’s identities” (Bell and Apfel 1995, 16, 19).

By 1987, when I heard from Stella, I had already begun to incorporate feminist, postmodern, and narrative scholarship into my research. The memo I wrote after we talked on the phone is rich with clues about how these changing epistemologies were influencing the research process, how I was working to resolve tensions between scholarship and activism, and the risks I felt about doing the work differently. Although I did not know it then, I was still in the initial stages of the project. I had not yet even completed the interviews. Three months after writing the memo, I adopted my son, and two months later I became pregnant with my daughter.

**Disrupting Scholarship: Life’s Vicissitudes**

E-mail to Rosanna Hertz, September 1994:

> When I realized that I was pregnant (7 years ago now!) I realized that it would mean a real slowing down of my scholarly work, but that it was worth the price. Seven years later I can think back to my rather cavalier attitude (I was SURE it would be a few years, never seven!) to changing my work rhythms along with my life course. But I must say, it remains more important to me to be the mother of two children than it would have been to be the author of a book and god knows how many refereed articles. . . . But seriously, I look around me to colleagues whose children are now in high school, see them working just as systematically and wholeheartedly as I did before having children; think about how comparatively young I am and that I too have a number of years ahead of me without young children (and with tenure); and figure that it’s basically ok that I haven’t done as much by now professionally because I will do it over the next 5 or 10 years instead. When I can think this way, I don’t feel so crazy as I do when I think about all the projects I haven’t completed at the same time my friends have completed [so] many.
E-mail to Matilda White Riley, September 1995:

As I mentioned when you were here, I’d really appreciate an opportunity to talk with you sometime in the next few months. I’d like your advice about how to manage to fulfill my obligations at Bowdoin without being completely overextended here. I’ve found in the last year or two, but especially since becoming chair, that the requests from Bowdoin for administrative positions have increased. Simultaneously with the growth in the department and the multiple changes in it, the demands within the department have increased too. . . .

I haven’t been able to carve out extended periods of time for writing. Instead, I’ve had to figure out ways to write that can be done in brief snatches. I’ve never been very good at this, but at this stage of my research, I’m eager to produce a book manuscript, and this demands ongoing and extended attention. . . .

What all of this means is that I’d like some advice from you about how to be a good citizen at Bowdoin, especially to work to strengthen the department, and yet to remain fully engaged in a professional life beyond Bowdoin.

E-mail from Matilda White Riley, September 1995:

This is a hasty but highly sympathetic reply to your e-mail about the characteristic “woman’s dilemma.” . . . My main advice concerns “phases” of life. Right now you are chair—surely this assignment will be handed around? AND, more importantly, you have little children—they do not stay little for long. And after that the strains can be lessened. So I fear you must think of long-range scheduling of: (1) research and writing and (2) outside professional activities. In general, why not postpone (2)—you can always catch up there? And consider how to FOCUS writing on your most readily publishable material? Publications do count (outside as well as inside), but new research may be too time-consuming to be an immediate possibility.

As women, we have extraordinarily high self-expectations. BUT, as I have learned, we also have predictably long lives and there should be time in the future for activities not feasible today.
The everyday world of becoming a mother and having a family punctuated the work life I spelled out in the previous sections of this chapter: studying the literature on narrative and feminist social science; changing my approach to recruiting, conducting, transcribing, and interpreting interviews; becoming close to rather than remaining distanced from DES daughters; writing “Birth Control”; finding ways to combine my activist and scholarly identities; and so on. Rosanna Hertz and I exchanged e-mails regularly for five years when she was editor and I was on the editorial board of *Qualitative Sociology*. Our e-mails were about journal business, life at liberal arts colleges, scholarship, and taking care of our children. We gave each other advice about child care and making lists, carving out time during the week for writing, chairing departments, and balancing our commitments. For both of us, this dialogue, and the life worlds it represented, was constant.

Matilda White Riley was a mentor to me at Bowdoin. She had been president of the Eastern Sociological Society (1976) and the American Sociological Association (1985–1986), and a molder and shaper of the sociology of aging and of the interdisciplinary study of age. Her contributions to scholarship, even into her nineties, were inspirations for a vision of positive aging. She also was the first woman hired by Bowdoin as a full professor. With her women students, she shared (as she put it) a brash intellectual self-confidence and awareness of the power of co-education to infuse liberal education with excitement and challenge.

My personal ability to balance my family life with my work was uneven. While I recognized, was open to, and participated in disruptions brought about by my commitment to activism and to feminist and narrative scholarship, I often hesitated to be open about my struggle to balance my work rhythms with my life course. In my e-mail to Matilda, I left out my children, formulating the struggle as one between different obligations in my job. Matilda brought them back in and included my obligations as a mother alongside of those to my work. Rereading Matilda’s response to me in preparing this chapter reminded me of my pleasure in mothering—of my deeply held belief, as I had written so emphatically to Rosanna, that it was “more important to me to be the mother of two children than it would have been to be the author of a book and god knows how many refereed articles.”

The excerpt from my e-mail to Rosanna and the exchange between Matilda and me capture my ongoing struggle at a particular moment in my professional and personal life. My children had just entered elementary school and
I had just been promoted to full professor. I had begun volunteering at the school when my son entered kindergarten, and continued to do so until both children went to middle school (and parents were no longer welcome in the classroom). I wanted to be engaged in the everyday school life of my children and their classmates, and my flexible work schedule enabled me to do this. A few years later, I turned a “crisis” about my son’s entry into fourth grade at the start of the school year into an article about the reproduction of social class and explored how parents and teachers and home and school are linked together through parents’ unpaid labor (Bell 1999b; Smith 1987).

Strains of “this phase of life”—as Matilda put it—lasted through the time my children entered college, and not because there were any major crises. There was just the dailyness of it, and the peculiarities of my particular circumstances and choices, and the simultaneous pleasure and impossibility of balancing all of these commitments without long-term social structural and cultural changes.

**Scholarship Disrupted and Transformed**

One of the themes of this collection is openness. To this point, I have demonstrated my openness to learning about changing epistemologies and incorporating them into my ways of thinking, doing research, and writing. But what made me open to these changes and to slowing down my work? I do not have one simple answer. A partial answer is that I felt responsible to DES daughters and I wanted to find ways to preserve the integrity of their words and worlds. I kept trying out frameworks for understanding and following my sense of what I needed to know about DES in order to understand DES daughters’ experiences and to write a book that connected individual life experiences with social structure and social change. In my quest, I kept following the DES daughters’ leads. They wanted me to look at pictures. I was not initially interested, but much later on I took another look at their pictures and other visual work. They organized and advocated for DES science and I went to conferences with them. These and other times I followed DES daughters’ leads just felt right to me.

Given what I believe about identity and experience and feelings and what I have written in this chapter, “feeling right” is a completely inadequate response, one that needs unpacking. Another partial answer to the question is historical time and starts with a reference to Mills’s (1959, 5) concept of “sociological imagination,” which “enables its possessor to understand the
larger historical scene in terms of its meaning for the inner life and the external career of a variety of individuals.” I came of age in the late 1960s and I went to graduate school in the mid- to late 1970s, at a time when there was much ferment in the academy: feminist and postmodern scholarship had begun to take off, and a lot of it was in Boston. Like so many in my generation, I was particularly drawn to a model of collaboration, community, and open exchange that emerged during that historical period—a model that fostered continual dialogue and exploration more than closure.

Although this might explain broadly why I was particularly open to the changing epistemologies and to slowing down, the details of my life over the thirty years of writing the book are also important; that is, the connections to and networks of scholars and activists—some of them overlapping—that I continued and expanded throughout the period. These connections flowed from, tapped into, and continually renewed my openness. For example, I met regularly with a narrative studies group in Cambridge beginning in the late 1980s and continuing to the present. The group fostered the development of individuals’ narrative imaginations by responding to works in progress and in its early years organizing a series of interdisciplinary conferences. Individuals in the group contributed to the development of the field by founding a journal and publishing key texts. I was also part of a network of feminist scholarship. This consisted of ongoing exchanges of e-mails, papers in progress, face-to-face conversations, and phone calls with other feminists. They, too, were (and are) asking questions about how empathizing, being drawn into the lives of our “subjects,” and attending to emotions could become acceptable social science. In addition to the network of DES activists I have described already, the “Birth Control” project also enmeshed me recurrently in close connections with women’s health activists and a broader network of people committed to peace and justice. They were resources for information, critique, and reviews of birth control chapter drafts as well as sounding boards for valuing and writing about different forms of knowledge.

I could not have written a book conclusion almost thirty years ago with sentences like these:

To study events in embodied health movements requires attention to detail, tracing multifaceted threads over time, across space, and in the talk that unfolds—such as in interviews and letters, autobiographical film and performances, and interdisciplinary scientific workshops. These social movements cannot be studied narrowly. . . . In the DES
embodied health movement, the talk that unfolds includes encounters between patients, doctors, nurses, scientists, legislators, mothers, daughters, and families. Their encounters take place in different times and locations, before and after surgery for reproductive tract cancer, surrounding infertility treatment, and in doing and undoing science as usual. . . . “They” comprise a changing constellation of people, along with individuals’ perceived interests, health needs, and situations. The complexity of embodied health movements requires following the tape-recorded and transcribed talk surrounding slides and computer-generated images of internal organs and tumors in teaching hospitals and conference panels and discussions, as well as still and moving images beyond text in film and theater. The narrative analysis [in this book] integrates this wide array of practices and exemplifies their transformative possibilities. (Bell 2009, 174–75).

There were not ways of knowing in scholarship that could make these practices known. Even as ways of knowing emerged, they demanded attention to detail, patience, and a willingness to slow down. And I did. I raised two children and embraced intellectual and political challenges that I took up and integrated into my book. The projects I am currently working on build on and work within the frameworks I participated in making over the course of almost thirty years. They assume complexity, reflecting the tenets of activist, narrative, and feminist practices. I would not have chosen to slow down quite as much as I did—and I do not plan to repeat it—but in the end, I am happy I did not write the book I set out to produce almost thirty years ago.

Notes
I would like to thank Vicky Steinitz for her astute reflections about this chapter.
1. Thanks to their successful exercise of power, in 1996 Linda Gordon and Barrie Thorne persuaded the then editor of Contemporary Sociology to include Our Bodies, Ourselves in a list of the ten books since 1971 that had had the most “influence on both academic disciplines and the world” (Gordon and Thorne 1996). In her reflections on the “missing revolution in sociology” a decade later, Thorne (2006, 475) wrote that Our Bodies, Ourselves, “and the women’s health movement of which it was a part, challenged dominant definitions of what counts as theory and as science, and highlighted the political construction of bodies and sexuality, an angle of vision only later sketched out by Foucault.
Our Bodies Ourselves . . . reflects the broad and questioning outlook, oriented to
the lives and welfare of women rather than to professionalized knowledge, that
continues to animate the best of feminist scholarship.”

2. For recent reviews of the field, see Thoits (2010) and Avison et al. (2010).
3. “Stella” is a pseudonym.
4. Changes in the project’s epistemology called into question the logic of a
“control” or “comparison” group. Although I began the study by recruiting and
interviewing women not exposed to DES, I decided to limit the study to twenty
DES daughters, half who had had cancer and half who had not.
5. Quoted with the permission of Lucy Sallick (Riley’s daughter).

References


New York: Springer-Verlag.

Macmillan.

Arbor: University of Michigan Press.
———. 1993. Translated Woman: Crossing the Border with Esperanza’s Story. Boston:
Beacon.
Boston: Beacon.

California Press.

———. 1988. “Becoming a Political Woman: The Reconstruction and
Interpretation of Experience through Stories.” In Gender and Discourse: The
Ourselves.” Women’s Studies International Forum 17: 9–18.


