Open to Disruption

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It is an honor for me to join with colleagues who are thinking about the value and facets of slow sociology. I agree that there is far too much pressure to push paper off one’s desk—to write grants and to produce ever more publications rather than slowing down “to reap the creative fruits of intellectual crop rotation,” as Judith Stacey (2007, 96) wrote in a book I coedited on public sociology. These pressures are mounting, as job markets constrict and universities become more dependent on external support (whether from federal institutes, private foundations, or wealthy donors). Such pressures intensify as more women enter the profession and bring with them responsibilities to the partners, children, relatives, and friends who so often can “disrupt” what Arlie Hochschild (1975) once called the clockwork of male careers. Because slowing down is becoming more difficult, it is all the more important to discuss it.

So many of the synonyms the thesaurus gives for “slow” indicate problems: “procrastinate,” “mire,” “retard,” “bog down,” “diminish,” “handicap,” and “loiter.” I recently sat on a university committee refereeing tenure cases. To be sure, most of the committee members spoke of quality, but many of them also talked in terms of numbers, emphasizing how much those faculty seeking tenure and/or promotion had (or had not) published as a mark of success in their career. I sit in a department where the current advice to graduate students is: “Don’t stay too long; just get something out.” And like Judith Stacey, I often hear good graduate students
say: “I want time to read, think, live and protest; I don’t want [your] life of publish or perish” or just “I want to have a life; can I slow down and still become a sociology professor?”

Although I certainly have been guilty of trying to write fast, to move material out of my head or off my desk (including, ironically, for this book), I find it useful to reconsider this strategy. As I reviewed my own experience for this piece, I realized that I am already a practitioner of slow sociology in several different senses. First, I have been working to understand and analyze families since I first began my now long career. This long gestation period led me to rethink the varied meanings and practices of families and especially the inequalities within and across them. Second, some of the particular studies I have done, especially the most recent one, returned to some of the earlier issues that concerned me. The multimethod research that resulted has taken much longer than I anticipated. Third, I have a family. The members of that family—especially my partner, daughter, mother, brother, and close friends—sometimes slow me down.

In this piece, I reflect on these three meanings and pathways. Moreover, I stand back from my own life and argue that these meanings and pathways are likely unequally distributed. Some aspects of slowness remain gendered. Slowing down, while maintaining a place in the academy, also often seems a luxury of those who are already advantaged, especially those who enjoy the privileges associated with class and academic position. The right to do slow sociology, I will argue, is all too often tied to cumulative advantage.

The First Sense of Slow Sociology: Rethinking Families

Many years ago, I began my life as a sociologist by studying families and gender (what were then called “sex roles”). I began by doing research on professional women—young aspirant that I was—who lived apart from their husbands so they could both have jobs. I had learned and rejected the common view that the nuclear family “fit” with the demands of modern societies—a “fit” based on a wife who could and would just pick up and follow her husband’s job anywhere it took the two of them. Coining the term “commuter marriage,” I studied and wrote about professional couples who lived apart, arguing that they were rejecting neither marriage nor jobs but were refashioning both as a way to produce greater gender equality in their marriages. I enjoyed the first of many collaborations, coauthoring Commuter...
Marriage (1984) with Harriet Gross; the book combined my research on dual-career professional couples with hers on couples in which one partner was a merchant marine.

Among the professionals, the families I studied each consisted primarily of husband and wife (they were like me: few had yet had children; few had much contact with relatives or friends). I chose this research site because I was interested in inequality within nuclear families, especially between husbands and wives, and I chose it because I had been shaped by the feminist movement and by Jessie Bernard’s now classic book arguing that every marriage contains two marriages—“his and hers” (1972, 8). This led me to want to interview both the wife and husband in each commuter marriage: it was occasionally difficult to get both to agree and it was often time-consuming to travel to their separate homes (I sometimes commuted long distances, just as they did, to do the interviews—whether from California to New York or from Boston to Washington, DC). I discovered Bernard was right in several senses. Not only did the very act of commuting entail an attempt to equalize the response to both “her” and “his” careers, but commuter marriage also looked very different to the women and men married to each other. I would sometimes go away from an interview and wonder about even the details, asking myself if I had made a mistake. Was this woman really married to the man I spoke to last week?¹ This showed up in their broad assessments of commuter marriage. Most women disliked it less than most men (though very few really liked it). It was the men who lost more than the women, at least relative to the marriages that surrounded them. This was reminiscent of the old experiments of asking two children—one poor, one wealthy—to estimate the size of a coin. It seemed much bigger to the poor. Commuter marriage, in an analogous sense, seemed much worse to the men who compared themselves to other men. Clearly, interviewing both spouses slowed me down, but taking the time to interview both spouses rather than only one of them provided a much richer understanding of marriage.

The more I thought about family and gender—and the more I talked with Harriet Gross about the differences between the professionals and the merchant marines—the more I understood that focusing on a gender divide not only revealed but also concealed much, especially about differences among women and among men. Understanding inequality required movement from the study of inequality between spouses within families to
inequality across different families. How did a comparison between working-class and professional women shape our understanding of family? Did class trump race, as some were arguing, or did middle-class Blacks and Whites practice family in different ways? Unpacking and reconnecting gender, class, and race is an arduous process. But these questions consumed me (and many others) for several decades.

I increasingly took a particular perspective, joining with a small number of others (e.g., Stack 1974; Hansen 2004) who were suggesting that too few of those who study work and family have looked beyond relations between spouse and partners or parents and children to examine kin connections. With a number of graduate student collaborators, I came to understand that this emphasis on marriage and the nuclear family—with its exclusion of the extended family—is narrow, even deleterious, and misses much of family life, especially outside the middle class. With a generous grant from the Rockefeller Foundation, we did interviews in Massachusetts that allowed us to examine the ways in which kin connections are not only gendered but tied to education, employment, marriage, and parenting—all of which, we argued, act as greedy institutions that interfere with broader kin connections (see, for example, Gallagher and Gerstel 1993; Gerstel and Gallagher 1995).

We were still focused primarily on gender and class, with little attention to race. Most of our respondents, all living in Massachusetts, were White. Earlier work (especially that of Carol Stack) led us to think that the focus on the nuclear family might be particularly likely to overlook the family experiences of women and men of color, and that an emphasis on the nuclear family promulgated a vision of family life that dismissed the social resources and community ties especially critical to the survival strategies of those in need.

To go further with the comparisons we wanted to make, we needed larger samples—those available in national data sets. I put together a workshop with a number of graduate students interested in the topic to talk about the issues and data sets we would need. A key member of this group was Natalia Sarkisian, a graduate student who was already sophisticated in the use of quantitative methods. We decided to collaborate: she wrote a dissertation, and we jointly authored some articles. We located a national data set with some appropriate items (the National Survey of Households and Families) and used it to show that many Americans rely on extended family members for all kinds of support and help. But we also found—as
we had hypothesized—that such reliance is not spread equitably across the population; instead, women, people of color, and those with fewer material resources are more likely to rely on and give to extended kin. (For summaries of work that came out of these analyses, see Sarkisian and Gerstel 2004; Gerstel and Sarkisian 2008; Gerstel 2011; Sarkisian and Gerstel 2012).2

We expected these differences, which were shaped by policy and politics; the myopic focus on the nuclear family was not a characteristic confined to academic research and media reports. Amy Armenia (another participant in the workshop), Kate McGonagle, and I again sought national data, this time to address policy that promotes particular kinds of families. A fortuitous invitation by the Congressional Commission on the Family and Medical Leave Act (FMLA) to examine employee use of the FMLA provided an entry point—a policy site, an audience, and some relevant data.3

As we told the commission, the FMLA allowed many people to take family leaves who would have been previously unable to do so. But we also had reason to remain skeptical about the actual reach of the FMLA, especially given our understanding that gender, class, and race jointly shape family patterns. We found that although gender neutrality in family leave taking was a primary goal of the FMLA, actual leave taking since the passage of the act has been far from gender neutral. We found that it is overwhelmingly women (especially those married) who say they need leaves, take leaves, and take longer leaves, especially for other people. These gender inequalities interact with race: it is White men who are particularly unlikely to take a family leave (Armenia and Gerstel 2006). Class position also affects the ability to take family leaves and the length of those leaves: while less affluent women are more likely to perceive a need for a family leave (Gerstel and McGonagle 1999), they take less time off than more affluent women. Worse, these data led us to expect that many organizations were not abiding by the act’s mandate. But how to pursue this conjecture? Locating yet another data set (this time the National Study of Employers collected by the Work and Family Institute), that is precisely what we found. This data set did not provide direct data on actual noncompliance, but Amy Armenia and Coady Wing, my two younger colleagues, invented a way to use missing data to assess noncompliance. Our analyses showed that between 25 percent and 43 percent of corporations legally required to follow the FMLA still do not comply with it two decades after its passage (Armenia, Gerstel, and Wing Forthcoming). We came to argue that nonresponse with survey questions
may represent a particular form of regulatory avoidance or defiance. I wondered if more direct data from observation at the workplace might be a way to explore these work-family issues in greater depth.

**A Second Sense of Slow Sociology: Researching Jobs and Families**

About seven years ago, Dan Clawson (a colleague with whom I had collaborated) and I were talking about our shared interests in inequality, work, and family. Along came a university offer for us both to take a semester off from teaching to write a grant proposal about families. We didn’t object; we took the offer and eventually spent a year creating a long and consuming research agenda about the inequality of hours and schedules (another ironic twist). We probably would never have written a grant or done the resulting study if we had not been prodded by university largesse in the form of “seed” money. The money was part of the new institutional agenda being developed because of the growing pressure on the school to raise money and get support from somewhere outside the university.

We were turned down the first time we submitted our proposal to the National Science Foundation (NSF). The second time around, we got lucky; put another way, advantages were accumulating. Writing proposals is a time-consuming process that can be more “costly” than writing for publication. Although one can sometimes resubmit to the same funder, there is rarely a different place to resubmit the proposal when it is turned down, as most are. But in our case, the NSF was now calling for multimethod research. In other words, they were calling for slow sociology.

Following our joint interest and understanding of the underpinnings of inequality, we studied four occupations whose members vary by class and gender—physicians, registered nurses, emergency medical technicians (EMTs), and nursing assistants—all in health care, because that allowed us to look at the “webs of time” across occupations and organizations in a single system while ensuring variation in the families (the better to assess how they affect work hours and schedules). We collected five types of data: We mailed eight hundred surveys to those in each of the occupations and then observed at eight sites (including hospitals, nursing homes, doctors’ offices, and EMT sites). We conducted over two hundred intensive face-to-face interviews, with respondents in the four occupations and with others who shape the hours and schedules of these occupations. Finally, we collected a variety of
documents (including union contracts, official work schedules, and actual work schedules). Working on this multilevel, multimethod project, I learned still another meaning of “slow sociology,” because our study of time took about a decade, absorbing the intense efforts of both of us (along with a number of graduate and undergraduate students who helped us out).

We designed the study around these four occupations because that allowed us to assess the ways in which the number and allocation of work hours and other scheduling decisions are the result of collective experiences and struggles, as well as products of relations between coworkers in the same occupation, among different occupations at a single workplace, among different workplaces, and among family members. The way to methodologically approach these issues, however, only gradually unfolded as we did the research. We initially thought (and proposed to the NSF) that we would do brief site visits at a couple of organizations. And that is what the NSF funded us for. As we did the interviews and conducted those abbreviated site visits, however, we realized that to understand both occupation and organization, we needed to do more (real) fieldwork. That way we could observe the same workers over a number of days. We could watch workers interact with other workers and with the managers who made decisions about their hours and schedules. And, we could see the differences and relationships among organizations (as they handed off patients and workers to one another). Consequently, what were initially conceived of as brief site visits grew into more extensive observations as we hung out in the organizations we studied.

As we came to the end of data collection, Dan and I were worried, feeling overwhelmed. We were swimming in data and asking, “Now what are we going to do with all of this?” It took many readings of transcripts and fieldnotes, along with analyses of the survey data, to make sense of what we had. We knew we needed time off and applied to the Russell Sage Foundation (RSF) for a year to get away from course and committee obligations so we could find time to draft the book, Unequal Time. Again, we got lucky—more “luck” for the already advantaged.

The RSF makes resources available only to a small number of scholars who then experience what Robert K. Merton long ago called “cumulative advantage.” Updating the idea, DiPrete and Eirich argue that cumulative advantage produces an inequality among scholars that grows over time, for it is a “general mechanism for inequality across any temporal process (e.g., life course, family generations) in which a favorable relative position becomes a
resource that produces further relative gains” (2005, 273). The other side of this: slowing down at an earlier stage may produce cumulative disadvantage; slowing down in the later stages (to collect richer data and analyze it) may depend on—as it produces—cumulative advantage.

Now, a little on the substance that came out of this long research process—a substance that is particularly appropriate for this anthology on disruption: As Dan and I culled the data, read and reread our transcripts and field observations, and cut and pasted from draft to draft, we came to see that a central theme was the pervasiveness of “normal unpredictability” and the inequality in strategies to deal with or control that unpredictability. We observed that all employers and employees, as well as their family members, deal with the pervasive unpredictability created by disruptions. All workers sometimes get sick; need time to care for a sick kid, mother, or friend; want a vacation or a mental health day; get stuck in a snowstorm; must stay an extra hour on the job because a day’s work is unfinished or a coworker had to leave early; miss a ride; need to run unanticipated errands before businesses close; hit a traffic jam; wait on a delayed bus; or need to take a relative to a dental appointment. As one of our more articulate colleagues summarized it, “Shit happens.”

Both class and gender shape how employees and their families can and do respond to these unpredictable events. We could, for example, observe what I had begun to tap in my earlier quantitative analysis of the FMLA: On the one hand, organizations often deny time off to low-wage women workers (who are often single mothers) and punish them for unpredictable events (a late bus, a sick child, or even a snowstorm). On the other hand, employers are much more likely to be lenient about such events when professional women encounter them (even though these women often also have more resources to cope with such disruptions). Professional women insist on and often get time off to take care of a sick child or are excused when a fire at home makes them late. Professional men use other strategies still: they turn to their others—like wives and nannies—to take care of what one called “life’s little inconveniences.” In short, we observed the ways gender and class interact when disruptions occur.

Understanding this unpredictability and the collective responses to it required a sociological vision that looks both vertically (say, looking at relations among employees) and horizontally (looking at hierarchies at the workplace and in families; examining linked organizations, linked occupations, coworkers, regular coworkers, and “irregular” workers).
Understanding those networks required data that is multilevel (different occupations, different organizations) and multimethod (fieldwork, intensive interviews, a survey, and schedules). Dan and I came to believe that if we had studied only one occupational group (say, just doctors or just nursing assistants) or if we had studied only one organization (say, just a hospital), or if we had collected only one kind of data (say, just a survey or just face-to-face interviews), we would not have been able to understand the collective nature of time, the pervasiveness of normal unpredictability, or the inequality that sustains different degrees of control. However, both getting and analyzing all the data required sometimes had us feeling like we were moving at a snail’s pace.

The Third Sense of Slow Sociology: Rethinking My Own Family

Much research shows that women are still responsible for more of the unpaid work of the home—whether it is housework, the care of children, or the care of extended kin. Women must juggle or “weave” (to use Anita Garey’s [1999] evocative term) family and work. The “clockwork of male careers” still serves to deter and constrain women more than men. In this sense, doing slow sociology—because of family demands—is gendered.

I look at my vita. Pre-tenure, there is a speedup (as I commuted between a job and my partner). Post-tenure, there is a big gap. That gap occurred when we were raising my (adorable) daughter. My daughter is out of college now, so she interrupts me less, but my mother stepped in to take her place. A few years ago, my now ninety-eight-year-old mother left her home in North Carolina to move to an assisted living residence near me in Massachusetts. She is no longer depressed, at least most of the time. We talk at least once a day and I visit her at least two or three times a week. But I never feel I do enough. While the pervasive norms about intensive mothering are oppressive, the absence of such norms for the care of elderly relatives is also oppressive. Both produce guilt, at least for women. From California, my brother calls about once a week and flies in three or four times every year. He spends much less time with our mother, and I doubt he feels guilty, at least most of the time. He reports that his friends are impressed by his commitment and willingness to come so often. My brother and I (with encouragement from our mother and friends) are doing gender in conventional ways. When my mother first moved up to be near me, I said to my brother, “Oh well, I’ll get less done; one less
publication—so what?” He agreed. Sometimes, though, I not only feel guilty, I also get upset that I can't focus more on one piece or another.

I was surprised by the gaps in my vita, especially when my daughter was young, even though I had taught and written about the constraints that caregiving places on paid work lives. I was also surprised by how I felt about those demands. Much literature (including some of my own) on caring for children and elderly parents focuses on the burdens such work entails. And to be sure, caregiving is burdensome. But there are also great pleasures to be had from these intense relationships, including the sense of meaning and connection they provide, the sense of virtue that can come from being able to give care to the people you love, and the sense of fulfillment that can come from giving way to this part of gender norms after having resisted them for all these years.

To be sure, family slows you (and me) down; that is, it slows professionals down. Given my interest in inequality, I keep reminding myself that even this is unevenly distributed. Slowing down imposes relatively little penalty on professionals compared to low-wage workers. As Budig and Hodges (2010) show, earnings penalties for motherhood are significantly worse for women in low-wage jobs than for those in high-wage positions. The same is true for time and the control over it. That is, there is little penalty for doing slow sociology if one is an affluent professional, advanced in one’s career, who has the luxury of responding to and controlling the normal unpredictability that intense family attachments routinely demand.

Conclusion

Let me confess: I am telling a linear story here. One study built on the previous one. One grant was made possible by an earlier one. One collaboration led to another. One advantage led to another. But that linearity is only a partial truth told in the interest of showing my pathways to slow sociology. There was far more happenstance—indeed, far more disruption—than this narrative would seem to imply. Moreover, thinking about slow sociology has alerted me not only to the connected pathways I have described here but also raised for me a number of additional questions I cannot answer. Let me conclude this piece with those questions.

First, what is the effect of technology? We hear arguments on both sides, and as far as I can tell, the data to answer this question are not very
good. In my professional life, moving from one project to another, I have witnessed and used what seems like an avalanche of new technologies: I have shifted from cutting and pasting transcripts on the living-room floor to using NVivo to code, search, and organize data; from writing and revising on a typewriter to drafting and editing on a computer screen; from visits to the library to online searches; from tape recorder to digital recorder; from standing in long lines to enter piles of computer cards on a mainframe to running Stata software on my own computer. Astonishing. But it is not obvious how these technologies—some for quantitative analysis, some for qualitative research—affect the quantity and quality of writing and publication. Although typing up fieldnotes, transcribing, and coding may be faster with these technologies, collecting field observations remains just as slow. Although finding existing national data sets with relevant questions—typically available online—might be faster, combing through them and creating usable data remains slow. Although finding appropriate literature is surely faster, it is easy to get distracted by yet another find, and reading is just as slow. Of course, I experience the learning, using, updating, and fixing of this technology all too often as disruptions that slow me down. Overall, I suspect that none of these changes did much to speed up my thinking (and rethinking) and none erased the disruptions that interfere with the slow process of generating and assessing ideas.

Second, what kind of research is most slow sociology? I have heard arguments between those who do qualitative research and those who do quantitative work about which takes longer. Of course, almost everyone thinks that what they do is the more labor intensive and time-consuming. My experience doing both suggests it is a draw. Relying on national data saved us the time and money we would have had to spend on collecting our own data, but using these existing data sets meant we had to rethink how to empirically define family, class, and race, and to identify what items might help us tap, if only imperfectly, the issues we wanted to address. Relying on existing national data means having to rely on the conceptual choices that other researchers made about which items to include or exclude. With such massive amounts of data designed by someone else, it is easy to get distracted and go astray—to start “number crunching” (“I’ll just run this and this and this . . .”). Doing both qualitative and quantitative analyses in a focused way demands constant revision and rethinking. This is, to be sure, a slow process—especially when the family one wants to study is not the
family in vogue among the researchers who design and direct the collection of national data sets.

Third, what of collaborations? Noting my multiple collaborations, the editors of this book asked me if I could reflect on the ways in which collaborations might both speed up the process and slow it down. I collaborate because collaborators help me develop ideas. I collaborate because doing so allows me to use methods, especially the quantitative methods that Gallagher, Armenia, and Sarkisian know, that would otherwise be beyond my reach. I collaborate because I like the structure that collaboration provides. I collaborate because I like the company (most of the time). But does that speed me up or slow me down? I don’t know. Another empirical question I can’t answer.

Finally, when students say “I want to have a life; can I slow down and still become a sociology professor?” what do and should we say? A colleague of mine at an elite institution routinely gathers together her women students and tells them not to have kids until after tenure so they can work fast and furious before then. She is not alone. Narratives of success today still tend to emphasize the straight and narrow path—careers where there are speedups and little room for disruptions (harking back to Hochschild’s clockwork of the male career).

This volume is important because it tells another story—one of the wisdom of allowing disruptions to do good research or disruptions to provide good care. Part of this message to slow down is gaining ground in the mainstream media. A recent *New York Times* article (Schwartz 2013), with the headline “Relax! You’ll Be More Productive,” pointed out that too many of us eat at our desks, leave work late, and work during vacations to maintain “a seemingly unsustainable pace.” The article’s punch line: those long, arduous hours not only make us sick but diminish the amount and quality of work we do. On reflection, I support that story. But as I have also tried to argue here, speedups are associated with gender and class, as is the right to slow down. It is often women, especially those who are advantaged, who are likely both to face the pressure and have the privilege to practice slow sociology, even if there remain some penalties they encounter when they do so. To return to where I began: as Judith Stacey reminded us, we must “revamp our institutional structures and the intellectual culture in our discipline and in the academy more broadly” (2007, 92). Advocating a slow sociology, then, requires revamping the institutional structures that we live and work in.
Notes

1. Such separation is still a dilemma that many professional couples today must face, although many universities and some corporations have become more sensitive to the plight of dual-career couples and are replacing outdated antinepotism rules with programs for partner hires.

2. This argument is based on a number of research projects, all collaborative, that I have developed with a number of graduate and undergraduate students, only some of whom I cite here (especially Amy Armenia, Shelley Erikson, Sally Gallagher, Mariana Gerena, Rachel Munoz, and Natalia Sarkisian).

3. Moving beyond prior legislation, which had allowed only maternity leave, the FMLA mandates job leaves for employees’ own serious personal illnesses, including maternity “disability.” It also guarantees twelve-week unpaid leaves to employees providing care for newborns or newly adopted children, as well as those caring for seriously ill dependent children, spouses, or parents. This act was an important step forward.

4. I want to thank Rosanna Hertz for this formulation of “vertical” and “horizontal.”

5. I am not using the royal “we” here; my partner was and is an “equal sharer” in parenting, as Francine Deutsch (1999, 2007) refers to those relatively rare women and men who divide the labor of child care.

6. Some years ago, one of my older colleagues casually said to me, “Now that you have a kid, you will understand families.” I thought he was being dismissive and sexist. And he might have been, given that he was invoking the old “insider” versus “outsider” debate that has now reappeared in both online and face-to-face sociological discussions of, for example, race (can White people write about Black history or Black culture?). Rarely, however, is it explicitly raised with regard to the study and talk of child or elder care issues. But now I think my colleague was in some sense correct (even if some of the best work on these issues comes from people without children). My caregiving responsibilities shape the way I think about and teach family. It also shapes how I think about slow sociology.

References


