I first got the idea for the study that became *Opting Out?* (Stone 2007) when I dropped off my younger son, Nick, for a playdate. He was in kindergarten, eagerly learning the ropes of school and making new friends. So, come to think of it, was I. His newfound friend’s mother (whom I’ll call Karen) became my newfound friend. And at this particular playdate, I wasn’t only dropping off my son, I was dropping off a present to Karen, a “stay-at-home” mom who’d just given birth to her third child. As I was leaving, she told me with obvious excitement that she had an announcement to make: she was going back to work. When I asked if she was returning to her former company, a prestigious investment banking firm, she looked at me as if I were, frankly, nuts. Waving that off as an impossibility, she told me she’d taken a position as a receptionist in a residential real estate office in our small suburban town, just to get out of the house and do something and because, with three children now, she’d lined up a live-in au pair anyway. I found elements of Karen’s story striking: the sudden timing of her going to work immediately after the birth of her third child, considering she had been out of the workforce for years; her complete dismissal of the idea of returning to her former job and instead taking a job for which she was wildly over-qualified; her enthusiasm about the prospect of returning to work; and the absence of any obvious economic reason for her to do so, given what I knew about her family’s circumstances. Parts of Karen’s story resonated with what I knew from my own scholarly expertise in women and work, but parts were novel, and confounding.

I can still recall the feeling of epiphany I had as I walked to my car after that conversation—that “click” moment of sociological imagination when
I realized I had found a subject that joined the personal and the sociological. The experience planted the seeds of my interest in other high-achieving women whose lives had taken turns that were to me, and perhaps to them, surprising and seemingly unpredicted. Nick was in kindergarten when I dropped him off for that playdate, and in college when *Opting Out?* was published—a long gestation period. Let me hasten to clarify (without seeming overly defensive, which I no doubt will anyway) that much of that period was devoted to other things. Professionally, being a department chair took up a lot of my time. On the home front, raising our two boys with my husband kept things busy. Along the way, a broken leg and herniated disc made for some unexpected downtime. Yet with all this on the work, family, and medical fronts, in hindsight, probably contributing equally to the number of years it took me to design, carry out, write up, and publish this study was the nature of the topic itself and its evolution from obscurity to highly charged visibility. Writing this leads me to reflect not only on why it took so long, but on what I was doing all that time. Looking back, I realize that many of the periods I regarded as frustrating and fallow—as wheel-spinning or unproductive—were, quite simply, not.

It is easier to say this now, when the story has a happy ending (a published book and promotion to full professor), than it was to live through, but it wasn’t only life that got in the way of research and slowed it: it was the subject itself. When I was talking with Karen, my immediate inspiration, it was the mid-1990s; it was not until the late 1990s that I began working on the subject in earnest. This was before the *New York Times Magazine* cover story by Lisa Belkin (2003) defined what I was studying as the “opt-out revolution.” That story, the most-commented on in the history of the magazine, unleashed a veritable torrent of discussion. It was the elephant in the room that I, even in the quieter confines of academia, could not ignore. While one might imagine that a desire to be responsive to the media controversy generated by this story would have speeded up my timetable, it had in fact the opposite effect, as I’ll describe. “Getting it right” (sidestepping ontological and epistemological debates, I believe empirical research yields some approximation of, and insights into, reality) was already a concern, but became even more important to me after the *Times* story. My desire to “get it right” was part of why *Opting Out?* had such a long gestation period, but there were other reasons as well, rooted in where I was in my career, decisions I made about the kind of work I wanted to do, and the kind of research I felt I had to do once I figured out what I really wanted to know.
Career Context

By the time of that playdate, I’d been department chair long enough to realize that the demands of chairing had taken their toll on my research. Most of what I was doing with regard to research at that point could be characterized, I suppose, as plotting my comeback, which took the form of wrestling with wrenching self-doubt about my ability to revive my research agenda, whether with old or with new projects. Despite knowing better, I’d agreed to serve as chair while an associate professor, and was still at that rank. When I finally decided to begin working on the project that became *Opting Out*? I had tenure, but was keenly aware of the need to make up for lost time. I wanted to be promoted—not just for a sense of personal accomplishment but as a demonstration that, as a woman, I was holding up the side. I also knew that I was facing an academic Catch-22: my publication slowdown and time-in-rank were going to make it hard for me to acquire the resources (i.e., grants) that I needed to support the research and publication necessary to move forward in my career.

I had to balance a sense of urgency about what I wanted to do with pragmatism about what I could do. Objectively, the situation was pretty bleak. Trained in quantitative research, primarily secondary data analysis, my first instinct was to build on my prior track record. I applied for a grant from a special National Science Foundation (NSF) program designed to support women scientists whose careers had been interrupted—perfect, I thought. Intrigued by Karen, I’d been accumulating examples of women like her whom I’d come to know. With an eye to what their decisions portended for the larger issue of gender inequality, I proposed a project in which I would analyze career interruption (versus persistence) and career outcomes among college-educated women using National Longitudinal Surveys (NLS) data. My application was turned down.

This could have been pretty devastating, I suppose, but years of thinking about and sporadically working on this subject had yielded a few insights that kept me motivated and kept me going. First, my efforts at literature review had revealed that there was no research on this phenomenon of women of the feminist revolution and beyond who had pursued professional careers in formerly male-dominated fields and left them. Second, the continuing absence of women at the top of these fields begged to be explained, and there were beginning to be signs that some part of the explanation might lie with the fact that a not insignificant fraction of women were leaving the labor force in fields in which “being in it to win it” was a necessary
if not sufficient condition for success. Third, while there was little to no academic research on the topic, it had attracted some attention in the media, which seemed to me (and to feminist analysts such as Faludi [1991]) to be framing it in terms of “choice feminism” in ways that demanded further interrogation. A content analysis I did of these stories about what came to be called “opting out” (later updated in Kuperberg and Stone 2008) confirmed that the dominant narrative in the media reflected the reductionist rational choice model that I (and many other sociologists of gender inequality) had spent my career challenging. Women were “choosing” to go home, their decisions framed as proof of the rise of a neo-traditionalist gender ideology that reflected separate spheres and favored motherhood and domesticity over career. Fourth, and probably not least, I was putting faces to phenomena, perhaps for the first time in my career. Women like Karen and others I’d gotten to know through the “mom work” I was doing alongside my work as chair and professor personified issues I’d engaged with throughout my career. And they did so in ways that were complex, confusing, and counterintuitive.

During this period, when my CV would make it appear that I was not doing a whole lot of research, I came to discover that Karen was not an isolated exception. One example that sticks in my mind occurred while I was brainstorming with the PTA executive committee of which I was a member about the slate of officers for an upcoming election. Candidate after candidate was an at-home mother described in terms of her former profession—the ex-corporate CFO (chief financial officer), for example, who would make an ideal PTA treasurer. While not formally “in the field,” this experience and others like it gave me insights into the lives of former professionals turned at-home mothers that no doubt informed my future study of them. More importantly at this point, however, these women fascinated me and piqued my interest. I wanted to find out more about them. And I was propelled to do so more by sheer curiosity about the intellectual puzzle they represented than by anything else, since what I wanted to study was still a problem that had no name and, based on my NSF experience, not one other scholars found particularly important or compelling or researchable. In fact, one of the reviewers of my failed NSF proposal assumed that “opting out” was so rare a phenomenon among college-educated women that there wouldn’t be enough women (“a big enough N”) to study. While it’s true that “opting out” is a minority phenomenon, it is not so rare that small Ns posed a fatal problem.

Having lost time on the unsuccessful grant application, I had better luck when I applied for a fellowship at Radcliffe the year after I stepped down
as chair and was on sabbatical. Radcliffe, formerly the women’s college of Harvard, was at this point a research institute within the university. It had a special interest in the subject, an earlier study having shown that women graduates of Harvard’s professional schools had an unanticipated high rate of being out of the labor force ten years past graduation (Swiss and Walker 1993). My time there gave me the unencumbered ability to pursue my research and the intellectual companionship of an outstanding group of scholars. But now I had to face some stark realities about the contested nature of what I was studying and I was at a crossroads about how to go about studying it.

Forbidden Subjects

I knew when I first became interested in the subject of professional women at home that it was potentially controversial. It was probably no accident that there was so little research on the subject, because academic researchers, especially then, which was just before the explosion of interest in unpaid or caregiving work, tended to focus on women’s paid work experiences. As I wrote in my study of women with superb professional pedigrees who are at-home mothers, even “acknowledging their existence does seem a dirty little secret” (Stone 2007, 8). There was about these women, as Ann Crittenden (2001) pointed out earlier in The Price of Motherhood, a hint of taboo. To study them, I had to wade into the battlefield of the mommy wars, which pitted stay-at-home moms against working moms. I had to shed light on a group of women who were often viewed as naive (at best) or as hapless victims of false consciousness or even as traitors to the feminist cause—a group that many observers would rather just ignore, hoping the women would learn the error of their ways. These sentiments were already evident in the popular literature of the 1990s, but would reemerge even more pointedly and vehemently after “opting out” became a high-profile phenomenon, as exemplified by books such as Hirshman’s Get to Work (2006) and Bennett’s’s (2007) The Feminine Mistake.

As I talked more about my then very nascent work in progress, I sensed interest—tempered with wariness—among my colleagues, many of whom reported, sometimes sotto voce, knowing women similar to those I had encountered. This was a wariness I shared. I had to be open to the possibility that prevailing media explanations might be correct—that women were changing their preferences about work and family, that so-called neo-traditionalism was real. At that moment, in the late 1990s, for example, we
were beginning to see evidence of a now well-documented stall in the gender revolution (Cotter, Hermsen, and Vanneman 2007). Going where the data led me also meant that I had to be attuned to the implications of my findings. This idea of “getting it right,” which hovers over most researchers, was hovering that much closer in light of what my research might have to say about high-achieving women, about the gender wars, and about our understanding of gender inequality and reasons for the stall in closing the gender gap—about issues that had shaped my own coming of age and my life’s work. Was I opening a Pandora’s box?

To address these concerns, I felt I needed to get a better sense of national trends in the behavior I was observing firsthand in my relatively affluent suburban community. My quantitative training compels me to ask questions like how much, how many, and how representative? At this point, which was still years before the “opt-out revolution” article, there was relatively little attention being paid to the now well-documented leveling off of women’s (especially mothers’) labor force participation in the 1990s. From my review of popular literature, I had identified college-educated married moms as being the target demographic for further study; I knew both from existing research and my own observations that they were in a position to exercise the option to quit working. When I analyzed Current Population Survey data for this particular group, I found that there was indeed evidence of a decline (this analysis covered the years 1977 to 1998) in this group’s labor force participation and that a relatively sizable share (on the order of one in five) were at home taking care of family. This analysis, which I subsequently updated (Reimers and Stone 2008), gave me a basis for going forward—it appeared that something was happening and it wasn’t only in my backyard.

Having satisfied myself that the subject was worth studying based on recent trends, I then had to confront the question of how to study it. The fellowship gave me a year during which I ultimately decided to jettison a quantitative approach in favor of a qualitative one. This was, admittedly, a risky decision for a variety of reasons, the foremost being that I had no training whatsoever in qualitative methods (and respected the methods enough to know there was a lot I needed to learn). Furthermore, feeling out of sync with my career already, I knew that qualitative work was typically slower going than quantitative (though I think the amount of time needed to do good secondary analysis is often underestimated too). Finally, my area of inquiry, gender, attracts some of the best qualitative researchers in sociology, so I was entering a new arena that was competitive and exacting.
Offsetting these considerations, and finally winning the day, were others, some tied to question and method, some tied to personal predilection. One of the frustrations of working on gender inequality using a quantitative orientation is that one has to wrestle with the hegemony of economists’ rational choice models (which offer essentially the same explanation for women’s behavior and gendered outcomes as voiced by the media). In these models, outcomes are revealed preferences. Thus, attitudes, tastes, and preferences that underlie women’s choices don’t need to be measured, and usually aren’t, typically consigned to the so-called black box. The direct effects of tastes and preferences, along with other effects (like that of discrimination), are captured empirically in residuals or unexplained variation. For me, as for most sociologists, tastes and preferences are important and distinct from actual behavior. Behavior is seen as reflecting both individual-level influences—human capital endowments and attitudes, for instance—and supra-individual opportunities and constraints, typically framed as structure. Given the centrality of “choice” in the media narratives and in the prevailing academic paradigm, it seemed critical to interrogate choice—to attend to aspects of both individual decision-making and structure. Working quantitatively, this is hard to do, even if one is so inclined. The reality of secondary data analysis is that attitudinal indicators or others that tap into motivations are typically pretty sparse in large nationally representative datasets. If I wanted to unpack “choice” to understand what women really want—their preferences for work, family, or a combination thereof—and the context within which they were forming their preferences and making (and framing) their decisions, I needed to use qualitative methods.

Making the case for a qualitative approach even more compelling was the absence of research on the subject, classically a situation calling for exploratory, hypothesis-generating qualitative techniques. These women and their motives were opaque to me and there were a number of plausible competing hypotheses—the push of hostile or unfriendly workplaces, the pull of children and family, changing attitudes and preferences, and, in an era of heightening income inequality, the resurgent power of a previously declining income effect. Finally, given that I was treading in waters known to be contested (“mommy war” territory), the framing of specific questions posed special challenges. Designing my own survey was premature. Given my interests, and given my prior research (my dissertation, for example, had used a two-wave longitudinal survey to examine the persistent effect of adolescent aspirations on women’s adult labor force participation), and given the
existing research (Gerson’s [1985] *Hard Choices* was a particular influence),
the semi-structured life-history interview seemed the perfect fit.

**Retooling**

During my sabbatical, I was fortunate to be surrounded by colleagues who
were premier scholars of gender, many of whom were also expert qualita-
tive researchers. I did a lot of reading based on their suggestions, but as I’ve
found is true for learning any method, talking to knowledgeable practition-
ers about their experiences could get me only so far: the best way to learn
a method is finally just to do it. I also admit to finding the recommended
reading somewhat frustrating. While I knew that qualitative approaches did
not lend themselves to the more standardized process of quantitative analy-
sis, I was nonetheless a little surprised to find how “gestaltish” it all seemed to
be. Descriptions of coding, for example, left me completely baffled, and led
me to work out coding schemes on my own. Subsequently, I’ve been heart-
ened to learn that qualitative coding appears to be challenging for everyone,
but at the time I thought it was just me. I was also somewhat amazed to find
out how some of the people whose work I most respected analyzed their data
(typically from interviews). A common technique appeared to consist of put-
ting quotes on pieces of paper or index cards and arranging and rearranging
them (often on the floor) to see patterns. While this clearly worked for my
colleagues, this is where my quantitative side kicked in—I needed a com-
puter. Thankfully, there were a number of text analysis software packages
available and a growing interest in their use. A workshop in ATLAS.ti saved
the day and gave me the security blanket of computerization.

I also was fortunate to have hired an outstanding research assistant, Meg
Lovejoy (then a doctoral candidate at Brandeis, now a PhD), who was not only
well trained in qualitative methodology but gifted at it. In doing my study, I
arrived at a kind of hybridized qualitative-quantitative approach. Whenever
I was leaning too much toward the latter, Meg would pull me back with a
gentle reminder that I was being quantitative. Nonetheless, from the outset, it
was important to me to try to address the inherent subjectivity of qualitative
research. I did this in a number of ways. First, and most importantly, I double-
coded all my interviews (with Meg as the second, independent coder). Second,
I made use of ATLAS’s capacity to break out and count codes by categories.
While frequency per se wasn’t the final yardstick, simple counts to understand
the prevalence and patterning of emerging themes reassured my quantitative
side and confirmed (or sometimes didn’t) that I was hearing what I thought I was. Third, I compared attitudes, tastes, and preferences to behavior and circumstance, looking for points of congruence (or lack thereof).

There were two major challenges to the analysis. First was parsing out what I called “choice rhetoric”—the extent to which women framed their decisions in the language of choice and preference—and distinguishing this from the reality of their options, or structure; the second was trying to understand the relative weighting of workplace versus family influences on women’s decision-making. Not only were these the central questions of my study, they were central questions in the larger media accounts of women’s lives and options with regard to work and family. Being confident in my conclusions required performing numerous iterations, when I would painstakingly comb through interviews. Meg and I worked out narrative summaries for each woman in addition to coding (and in hindsight, probably overcoding) their interviews in order to try to distill the key influences on and circumstances surrounding their decisions. But to someone who was new to the methodology, and increasingly conscious of the importance of what I was discovering (about which, more below), this stage—which occurred over years, not weeks or months—was critical to feeling I could move forward to present these results.

While at Radcliffe, I made some other design decisions. My first intention had been to study professional women who were mothers, both currently working and at home. The question then was broader: Why did some continue with their careers and others not? What were the factors differentiating continuity from interruption? What was the mix of work and family influences leading to one decision or the other? I began to do some preliminary interviews, some with working mothers, some with at-home mothers. What I heard from the working mothers confirmed the research we already had on hand, pointing to the importance of (truly) supportive bosses and husbands and to the size and importance of women’s relative contribution to household income. What I was hearing from the at-home mothers, however, was surprising and countered the prevailing narrative: they were telling me very little about motherhood, but a lot about unyielding workplaces. I could have continued with the kind of balanced design I’d originally envisioned—that is, to include both groups of women—but as I took stock of the time and money required to arrange, conduct, transcribe, and analyze each interview (in addition to the considerable legwork necessary to identify women for the study in the first place), I decided after these early interviews to focus only on the at-home mothers, who were underresearched and little understood.
From this point on, my course was set. For details of my methodology, see Stone 2007. Exactly as described in the texts on qualitative methodology I’d been reading, I hit the “saturation point” fairly quickly, at around fifteen interviews. Still nagged by my quantitative mindset, no doubt, I continued interviewing to boost the final $N$ to 54. I also wanted to see if what I was finding held up across regions, age cohorts, and occupation types (it did), and was guided in my additional interviews by a rough quota sampling design.

The time at Radcliffe, then, turned out to be pivotal and critical. The work I did there solidified and encouraged my interest in the subject. It helped me frame my methodological orientation and gave me the initial opportunity to retrain myself and retool my skills, which was in some ways a never-ending process that lasted the duration of the project. It also gave me the chance to conduct some preliminary interviews that resulted in a major change to my research design (to focus only on the group who had left the labor force)—a decision informed by pragmatic resource considerations as well as by emerging findings. In the years after my sabbatical, I continued my interviews, which took me across the country, and I presented some preliminary results, but otherwise mostly engaged in the continual analysis and meaning-making that is part of the reflexive process of qualitative research.

I was to encounter yet another interruption, which occurred just about when I thought I was finishing my interviews. In the weeks—and months—after 9/11, work and family, especially the importance of family at a time of tragedy, were major themes in the national narrative of comprehending and healing. Work-family narratives were also a crucial part of what I was trying to pull apart and distinguish between. Not to trivialize the event, but from my perspective, 9/11 posed a threat to the internal validity of my study, specifically in the form of a history effect (Shadish, Cook, and Campbell 2002). I was concerned that 9/11 and its aftermath would raise the salience of family in women’s retrospective accounts of their decision-making. To avoid this and the possible bias it introduced, I suspended interviewing for a year—another painful delay, but one I thought justified and necessary from the point of view of research design.

**From the Problem That Had No Name to “Opting Out”**

What I found as I did those initial interviews was how much I enjoyed them. I was astonished, and grateful, that women were so willing to talk with me. Having guaranteed them anonymity, I nonetheless marveled at their candor
and openness. Because my research until that point consisted almost entirely of secondary data analysis, I had never actually talked with my subjects, much less interviewed them in their own living rooms. This firsthand experience further heightened my sense of wanting to get it right—to fully and accurately convey the lives these women were sharing with me—and became another reason why analysis of their stories was so time-consuming. I also knew that the women I was interviewing would likely read my book, or certainly have easy access to it. Not only did I want to get it right, I wanted to write it up in a style that was accessible to them and women like them.

By 2003, five years after my initial year at Radcliffe, I had finally reached the point, after endless analysis, of being fully confident in my results, having checked them in numerous ways and finding them robust. I was ready to go public and to begin writing the book that I had always anticipated would be the major end result. Propitiously, I was also invited to give a paper at a conference at the University of Pennsylvania. Sponsored by the Sloan Foundation, its focus was on assessing the progress of women in the professions. Jerry Jacobs, the organizer, knew of my work and asked me to talk about it. That conference, as I recall, was on a Friday; the weekend before, the *New York Times Magazine* featured a cover story titled “The Opt-Out Revolution” by its work-life reporter Lisa Belkin (2003), with the teaser “Why Don’t More Women Get to the Top? They Choose Not To.” When I first saw it, and understood what it was about, I couldn’t read it, certain that the years of research I’d been doing had just been scooped, and sure that (as is often the case) the cover story was a prelude to a forthcoming book. When I finally read it, I realized that Belkin and I had heard different things and come to different conclusions. More significantly, perhaps, by the end of the week it was clear that the topic I’d been working on for years—the problem that hadn’t been recognized as a problem and that had no name—had morphed, literally overnight, into a subject of considerable interest and controversy.

Meanwhile, I presented my paper and received excellent comments from Heidi Hartmann, a scholar whose opinion I valued. While generally laudatory, she wondered whether my sample was not skewed a little old and whether what I was picking up reflected experiences of an older cohort of women (i.e., women in their forties). While I had tried to sample from a range of ages, the fact was that the majority of my sample at that time was indeed women in their forties, which was not surprising given that these were professional women who’d deferred marriage and childbearing while
pursuing advanced education and training in their twenties. Nonetheless, this critique resonated with me. When I’d thought no one would be listening—when my problem still had no name—I’d been okay with this weakness, reasoning that no study is perfect. Now that it was possible, thanks to the *Times* story, that my work might actually find an audience, and because it was clear from my results that I would be challenging a high-profile interpretation, I felt an added pressure to respond to the comment of an eminent scholar and, again, to get it right. In this instance, this translated into going back into the field to interview an additional eleven women in their thirties so that the final sample was evenly split between thirty- and forty-year olds, which added at least another year to the study.

At this point, time was of particular essence, not only because of the length of time I’d already invested, but because of the controversy the article had ignited and my desire to see my research enlighten the ensuing discussion. What I haven’t mentioned yet is funding. Part of the relatively slow pace of my work was due to the need to raise money to support it. Because I hadn’t been publishing regularly, I did not feel I was competitive for major external funding. Instead, I turned to relatively small (but critical) internal grants from my institution, including invaluable support from an NSF ADVANCE grant to Hunter College (Virginia Valian and Vita Rabinowitz, PIs) to promote the careers of women in science. I’d exhausted those options and needed more money faster than internal funding could provide. I applied to the Sloan Foundation and was funded by Kathleen Christensen’s program in Work and Family, in part I think because I was the only person out there who had actually conducted research on “opting out,” for which I will take some credit for prescience. With Sloan’s support, I was able at last to complete the remaining interviews, the results of which confirmed what I had previously found. I was also able to get some release time to write. Having spent years with these results paid off in that I completed the manuscript relatively quickly, aided by Naomi Schneider, my wonderful editor at the University of California Press, who facilitated its path to publication in 2007. Remarkably, four years after the “opting out” article’s publication, interest in the subject remained high, giving me the opportunity to provide the counternarrative—to tell the stories of women who were being talked about, and talked at, but rarely given voice.

Decisions about research design were not just technical decisions made to meet academic standards, but to enhance the credibility of what I was finding. Was I overly cautious? Probably, but chalk that up not only to the
challenges I’ve described, but to the burden of being a rookie at qualitative methods and, no doubt, trying harder. The study was better for the decisions I made, even though each one incurred a delay. In the end, it was the methodology itself—the fact that I so closely engaged with the women I was studying—that really motivated me, because getting it right wasn’t an intellectual exercise, it was an obligation to them. In hindsight, the fact that I probably identified with women’s stories of careers interrupted, having seen my own research career interrupted, also gave me an empathy and insight into their lives that I would otherwise not have had. If so, perhaps it was worth the interruption.

References