

## 12 Boundaries and Selves in the Making of 'Science'

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*Margaret Eisenhart*

This chapter is about some practices of people actively engaged in what they call “science.” The groups and actors described here, including myself, mean different things by “science,” and we do different things with it. Yet, in one way or another, all of us are at work regulating a normative boundary—determining what counts as science and science education in contemporary U.S. society.

### **Introduction**

The main story line is mine. It is a story about trying to publish a book whose contents are in some ways marginal to what is normally considered science or science education. The book, *Women's Science: Learning and Succeeding from the Margins* (Eisenhart & Finkel, 1998), was published and is selling well but not without some hard feelings and loss of content. During the publishing process, what counts as science got tangled up with what counts as “credible” science and “marketable” literature. As a result, a book dedicated to a broad view of science and science education also became a contributor to the very same boundaries its authors intended to expand.

I offer this story not as “the truth” about what happened, but as a personally constructed narrative about my experience of boundary making in science and science education. I tell my “own” story, but in the tradition of critical autobiography (Behar, 1993; Gilmore, 1994), I attempt to reveal how the story is a product of both power relations (so-called “outside” or “larger” influences) and my subject position (my identities-in-context).

The story I tell and my way of understanding it also were informed by my recent attempt to apply practice theory (also referred to as activity theory or so-

ciohistorical constructivism in earlier works) to research on science and science education. In that work (Eisenhart, 1996; Eisenhart & Finkel, 1998; Eisenhart, Finkel, & Marion, 1996), I use practice theory as a way to consider together the insights of constructivists and sociologists of science. Generally speaking, constructivists view science as a socially and experientially produced set of useful ideas about how the natural world works. As such, science is not a fixed body of facts and theories but a set of ideas that changes over time as people produce new and more productive ways to observe and think about their experiences in the world. From this perspective, science pedagogy is an exciting process of introducing students to natural phenomena, to ways of empirically observing and testing them, and to theory building about them. Students are encouraged to “construct” their own ideas about the natural world based on their experiences and then to defend or modify the ideas in light of questions and challenges from others and their ideas. When science education reform is premised on constructivism, the preceding principles guide reform efforts. When science education research is based on constructivism, case studies of individuals, short biographies, and occasionally autobiographies have become popular methods of investigating what learners construct, how they use their constructions, and how they revise them over time.

In contrast, sociologists of science, along with some feminists and anthropologists of science, view science as a set of historically and politically *compelled* ideas about how the natural world works. As such, science is neither a fixed body of knowledge nor an empirically tested set of good ideas but a “technology” that tends to advance the interests of the historically powerful (Haraway, 1989; Harding, 1991; Keller, 1982; Lave, 1993; Minick, 1993). From this perspective, the crux of science education is to introduce students to the body of knowledge called “science” (cf. history) and to social critiques of it: what is included, what is not, why certain things are left out or ignored, what might be different ways of thinking about science and conducting it, and so forth. Science education reform premised on the sociology of science promotes this goal. Science research informed by this perspective tends to rely on methods of participant observation along with social, political, or literary critique.

Practice theory orients researchers to consider together the things that separately concern constructivists and critical sociologists, feminists, or anthropologists (Eisenhart, 1996; Eisenhart & Finkel, 1998; Holland, Lachicotte, Skinner & Cain, 1998; Lave, 1993; Levinson & Holland, 1996). Practice theory “locates persons in history and history in persons, focusing on the ways in which individuals and groups fashion and are fashioned by social, political, and cultural discourses and practices in historically specific times and places” (Skinner,

Pach, & Holland 1998, p. 3). This perspective reminds us that social constructions “should not be romantically portrayed as unleashed creativity and agency, but rather [they consist] of ambiguous activities still subject to domination and tied to identity struggles” (Skinner et al., 1998, p. 13). My application of practice theory to the study of science and science education has focused on “the ways in which individuals and groups fashion (the social constructivist part) and are fashioned by (the sociology of science part) social, political, and cultural discourses and practices.” In *Women’s Science*, we relied on methods of participant observation, interviewing, and case study analysis to show how small groups of women made sense of their work in science, and how they pushed against, as well as were constrained by, larger discourses and practices of science. In this paper, I use the same perspective to consider how I fashioned a story of one of my experiences during the *Women’s Science* study and how this story has itself been affected by larger discourses and practices in which I participate.

#### **A Word about Autobiography as Method**

Autobiography is presumed to be an authentic way for someone to tell what happened to her, from her point of view. In its traditional sense, the genre of autobiography is based on a proprietary claim to a narrative and an identity (“this is *the* story of *my* life”) that is expected to conform to the requirements of realist narrative and non-fiction reporting (i.e., an autobiography is expected to be the truth about what happened with historical reference [Gilmore, 1994]). Put another way, the conventional understanding of an autobiographical text is that it “makes public that which has been private, typically claiming to avoid filtering mechanisms of objectivity and detachment in its pursuit of the truth of subjective experience” (Felski, 1989, pp. 87–88).

Many have suggested that the tenets of autobiography—to be authentic, true, and real—give it especially high status as a literary genre in the US. In colonial days, for example, “in accordance with the Puritan precept that literature is useful . . . authentic personal experience had greater prestige than poetry or any variety of fiction” (Sayre, 1988, p. 35). Today, autobiographies are especially popular because they give outsiders access to information (“private landscapes”) that only the authors know (Simon, 1999). Their popularity makes them an attractive genre for writers and makes them easy for publishers to sell. In fact, to gain attention and market share, some stories are advertised as “autobiographies,” when they may not be, at least in the conventional sense (cf. *I, Rigoberta Menchu*).

Although seemingly valuable and straightforward as a method for obtaining an authentic story, autobiographies also have been critiqued for their authors' naiveté in suggesting that their accounts are unfiltered or unbiased. Intended audiences, authors' purposes and vanities, historical context, and publishers' interests have all been implicated in the production of autobiographical accounts (Behar, 1993; Felski, 1989; Gilmore, 1994; Gray, 1997; Payne, 1992). For example, in discussing the autobiography of Elizabeth Keckley who was [First Lady](#) Mary Todd Lincoln's attendant and confidant in the [Lincoln](#) White House, Frances Foster (1992) reveals how Keckley tried to tell her story so as to portray Mrs. Lincoln positively, while publishers later edited Keckley's work to cast Mary Lincoln in a more negative light, consistent with prevailing public opinion at the time. Keith Byerman (1993) describes W.E.B. DuBois' autobiography as his attempt to rehabilitate his image in the eyes of the American public near the end of his life. Ruth Behar, in recounting the autobiography of Esperanza, a poor Mexican working woman, suggests that the story Esperanza told (of private suffering and struggle) would not be believed in her hometown because the townspeople considered her a combative woman who did not know her place. However, Behar knows that Esperanza's story will be believed across the border in the US, because it fits American readers' image of what life is like for poor women in Mexico.

These considerations mean that the story I tell next must be understood as "what happened" in a very qualified way. My story is not a neutral, transparent, or decontextualized account of what happened or what kind of person I am. It is positioned—presented from the perspective of someone with certain intentions at a specific moment in historical time. It also is situated—made to fit with my expectations about who will be in my audience and what they will expect from someone like me and the other "characters" in my story. It is a literary genre actively constructed, by a person in a social and cultural space, so as to be believed (as something that actually happened to me, the teller) and so as to be taken seriously (as something that is meaningful for those I expect to read this account).

### **My Story—Part 1**

The first part of my story concerns the "fashioning" of *Women's Science* by my co-authors and me. On one level, we wrote the book to report the findings of a research study we conducted about women successfully engaged in various forms of science. In telling this part of the story, I position myself as an academic writing mostly for other researchers and policy makers. Consistent with

this identity, I produce (below) a standard expository account of the research process and its results.

The study itself began in 1991, when the five of us—Eisenhart, anthropologist and educational researcher; Finkel, geologist and university science educator; Behm, middle school science educator; Lawrence, political activist and educational researcher; and Tonso, engineer and educational researcher—decided to do participant observations and ethnographic interviews in sites of science-related activities. Initially, we chose four sites where women were present and apparently successful in more than token numbers. The sites were a high school genetics classroom (40 percent girls), an internship for college students preparing to be engineers (26 percent women), an environmental action group (50 percent women), and a conservation corporation (46 percent of scientists were women). We were especially interested in these sites because they seemed to offer a striking contrast to the many reported examples of women's small numbers and special difficulties in science. We hoped to learn—through in-depth, first-hand data collection and analysis—why women's experiences in these sites appeared to be so much better than what has typically been reported for women actively pursuing science.

As Behm, Tonso, Finkel and I began our work in these four sites, Nancy Lawrence was investigating two abortion activist groups—one pro-life group (“PL”) and one pro-choice group (“PC”)—for her dissertation on the meanings of “choice” in contemporary U.S. society. In my capacity as her dissertation advisor, I read over her fieldnotes of the meetings she attended. I was surprised to find that discussions in the meetings of both groups focused on the biology and technology of reproduction. I also was surprised to learn that when members of the two groups were asked in interviews why they joined, many mentioned the opportunity to learn more about reproductive biology. Given that women comprised nearly ninety percent of the members in both groups, given that a large number of the women members had advanced degrees, including some in science or engineering, and given my interest in women engaged in science broadly construed, I was fascinated with this discovery. I thought of it as something special—something that marks good ethnographic studies and makes them exciting to do. I saw the opportunity to learn something new and unexpected by watching and listening to how women used science in these groups. In my mind, Nancy had found something wonderful—an unexpected interest in science among groups comprised almost entirely of women. We decided to add the abortion groups to the science study.

The first complete version of the resulting book manuscript had ten chapters. Five of them—the book's heart—consisted of case studies of each group.

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One chapter was devoted to each of the first four sites in turn; then the two abortion groups were discussed together in the final case study chapter. As each case was told, their distinctive features, as well as patterns across them, were revealed. We found that science certainly was different in each site, but we also found clear patterns in the way women thought about science and in their experiences relative to men's.

The story we tell in the book is based on well-established social science methods of data collection and analysis. The story also is perverse, or counter to popular expectations, in two important ways. First, the book challenges the commonly held idea that "to work in science" means producing facts or theories about natural or physical phenomena. For example, we call political action work "science" when it involves the use of scientific information to promote environmental causes, just as we call schoolwork "science" when it focuses on the genetics of fruit flies. We do *not* argue that these activities are identical, but rather that together they constitute an arena of practices that are thought of as "science" by their participants and that depend on each other for their meaning and status in contemporary U.S. society. We knew that such a broad view of science would chafe at conventional expectations—both academic and popular—about what constitutes science.

Second, because the book tells about women who are successful and enjoy their work in science-related activities, it contrasts with the three most widely accepted "truths" about women and science: (a) that women are not inclined (by nature or culture) toward science (the conservative position); (b) that science does not appeal to women because of its assumptions, content and procedures (the feminist position); and (c) that success for women in science is blocked by discrimination (the liberal position). None of these positions sufficed to explain the experiences of the women we were studying.

At the time, this perversity meant two different things to me: The book might become well-known because it was unconventional in a provocative sense; or, it might be disregarded because its unconventionality placed it outside the boundaries of what most people would expect (cf. Esperanza in her hometown). No one who writes a book wants it to be ignored, so this ambiguous situation made me nervous.

In the preceding account, I portrayed myself as an academic justifying her actions primarily in terms of social science research standards (the credentials, the methods, the sample, the systematic comparisons, and so forth). But when the results turned out to be unconventional, I worried that my careful scholarship might be ignored or worse: that my identity as a credible academic might be

challenged. As it turned out, something like this did happen, although I am hard-pressed to admit it!

### My Story—Part 2

As the book took shape, Finkel and I began talking to the funding sponsors about the results and to book editors about publishing them. In the process, we experienced how social pressures, historical precedent, and economic power can work to constrain authors' liberty (or agency) to publish their findings. This is the second part of my story.

On September 10, 1996, I finished a complete draft of the manuscript that would eventually become *Women's Science*. On that September day, I sent the manuscript off to the University of Chicago Press with a great sense of accomplishment and relief. To have it finished, off my desk, and out of my mind (almost) was wonderful! I was particularly satisfied with the way the chapter on the abortion groups was integrated with the rest. From early on, some important people questioned their inclusion or complained about it. But now I was impressed with how well they fit, and I thought they were crucial to underscore some important points. Nonetheless, earlier complaints came back to haunt me, and inclusion of the abortion groups later came to threaten the book's publication.

Lawrence first wrote about the abortion groups in "The Language of Science and the Meaning of Abortion," a paper she presented at a national conference in 1994. Here are a few of the examples she included to illustrate the use of science in the abortion groups.

From literature discussed in the groups:

"When a human life begins is not a religious, moral, or philosophical issue; it is a scientific and biological one.

The Nobel Committee noted that life begins with the activation of ion channels as the sperm merges with the egg in fertilization. All cells have electrical charges within and outside the cell and the difference is known as the membrane potential. Fertilization changes the potential to prevent other sperm from joining the fertilized egg."

From fieldnotes taken at a pro-life meeting:

We were advised to "begin with research" and "know absolutely all about fetal development, abortion procedures, and local abortionists." We were encouraged to "be conservative with facts and statistics" so as not to be falsely accused of magnifying favorable numbers. "Know your history," we were instructed.

From interviews with group members commenting on what the group does:

“We describe the nature of the unborn child, development in the womb, the scientific facts of reproduction, when conception has occurred, when reproduction has occurred.

My knowledge of reproductive health care has increased considerably. I’ve learned things about my body in the past two years [while in the group] that I didn’t know and I’m twenty-five, and it’s really pathetic. . . . I don’t understand why I didn’t actively seek out this information before, and I also don’t understand why it wasn’t more readily available to me.”

I first wrote about the abortion groups in an interim report to one of our funding sponsors in 1994. Here is an excerpt from that report:

This [section] focuses on the way in which the language of science has been appropriated by [the pro-life (PL) and pro-choice (PC) groups] and used as a vehicle of “empowerment” for members of the groups, most of whom are women. We find that both groups draw on the . . . discourse of the hard sciences. In so doing, they accomplish two things. First, they turn woman-centered and locally *shared* concerns into two opposing positions, each supported by different scientific “facts.” Second, by repeated reference to the scientific facts supporting their positions, they suggest that their causes are unbiased, apolitical, and “serious.” Both groups purposefully simplify, distort and manipulate science, but they also succeed in leading members to take an interest in science, to believe that they can understand the science behind the issues, and to take personal pride in that understanding. By means similar to the processes . . . in our other outside-of-school sites, the social arrangements in PL and PC make scientific knowledge worth having, especially for its use in public discussion and debate. As in the other sites, women are motivated to learn and use science. However, in these two most politically active, woman-centered, and female-dominated groups, the science available was the worst, i.e., it was weak, unsophisticated, distorted, and divisive.

My first clue that inclusion of the abortion groups could be problematic came when a reader from the funding sponsor called to talk to me about the report and our progress. She told me that she did not see the connection between the abortion groups and the other four. She wondered whether we really needed to include the abortion groups. I argued that they were relevant because many of the patterns we found in PL and PC supported findings from the other groups, yet the situation in PL and PC made clear that we would have to face the distinction between sites where science was credible from those where it was not. At the time, I was not sure the reader agreed with my assessment of the group’s relevance, but I never thought seriously about omitting them (and she did not



ask me to). What I did think about was the need to make a stronger case for their inclusion.

A year or so later (in 1995), when I reported our findings at a conference (using similar language to that in the interim report excerpt), the question about the need to include the abortion groups came up a second time. This time, it was raised by several women in the audience who felt that the groups' inclusion "distracted" from the importance of our findings about the other four sites. Again, I attributed the question to my inability to articulate the special importance of the abortion groups.

The next piece of this story did not occur until many months later after drafts of most of the book chapters were completed, and I wrote a long book prospectus to show to publishers. (Often authors will write a book prospectus and hope to get a publishing contract before anything of the book is actually written. In this case, I was concerned that the contents were unconventional, even without the abortion groups. I had decided that I would have a better chance getting a contract with a publisher who would take the book seriously if my points and arguments were well-thought out and clearly written in advance. Thus, I did not "shop the book around" until I was sure what would be in it.)<sup>1</sup>

After brief face-to-face meetings with me at a conference, two publishers expressed interest in seeing the prospectus. (Getting publishers interested in your book manuscript among the many they receive and hear about is itself a difficult task. In my experience, personal contacts—friends, colleagues, or mentors who know a publisher and can recommend your work—are the best means of introducing yourself to publishers. In this particular case, I had some advantages over the first-time book author because I had already published two books, already knew some publishers, and had many close colleagues who had published books.)

Within twenty-four hours of enthusiastically receiving the prospectus, one book publisher told me in no uncertain terms that she could not publish the book with the abortion groups included.

At first, I thought I could persuade her to reconsider, but I was wrong. She was adamant. The second publisher was still interested but noncommittal. He wanted to see the complete manuscript. When I met with him to discuss a time line, I told him about my experience with the other publisher and asked whether he was comfortable including the abortion groups. Unfortunately, I do not remember his immediate response, but I am sure he did not say that I should omit them. In his later written comments on my prospectus, he suggested that I add more discussion to clarify the points about the abortion groups.

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With this security, I went back to work on the manuscript. In the meantime, the publisher sent my prospectus out to two anonymous reviewers. Ten months later, I received the reviewers' comments. They were generally positive and made a number of important suggestions that I later incorporated. However, one raised the question of whether the discussion of PL (the pro-life group) and PC (the pro-choice group) contributed anything different than was already evident in the earlier chapters about the conservation corporation and the environmental action group. When discussing this question with the publisher, I argued for a chance to address it and make the case for the abortion groups in the complete manuscript. He agreed to my request. Several months later (September, 1996), I sent him the complete draft, which he in turn sent back to the same two reviewers for comments. These reviews also were generally positive and constructive, but this time, both suggested possibly omitting the chapter on the abortion groups because it added too little (new) to the story and too much to the book's length. The publisher concurred, adding that a shorter book could be more attractively priced.

For several months after that, I tried to figure out new ways to argue for the groups' inclusion. For example, I talked to a copy editor and showed him the manuscript. He thought that fifty pages (the length of the chapter on the abortion groups) could quite easily be cut from the overall text, thus leaving space for the abortion chapter. I suggested this to the publisher, and finally, he made clear that he did not think he could make a strong case for publishing the book to his editorial board unless the chapter about the abortion groups was removed. At this point, I had completed the manuscript, had already undertaken two revisions, and now felt threatened with a rejection. Reluctantly, I agreed. The decision of the editorial board was positive, the necessary revisions were made, the chapter on the abortion groups was removed, and the book was scheduled for publication.

Thinking back now about these events, I know I wondered from the beginning: What did it really mean when the reviewers said that the inclusion of the abortion groups did not *add anything* to the overall argument? One possibility is that there were no new data about science or women in the chapter about the abortion groups once the environmental action group and the conservation corporation had been presented.

I did not think this was the case. For example, the science available to the women in PL and PC was more superficial and selective than in any of the other groups. Nonetheless, the women—many of whom were very highly educated—were eager to learn more science in the group and felt politically emboldened by the scientific information they got there. For anyone seriously interested in op-

opportunities for “teachable moments” in science, or for promoting scientific literacy in locally relevant contexts, or for increasing the involvement of women in science, PL and PC looked like good candidates. They were places where these opportunities seemed to exist but were being missed.

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In addition, women in the pro-life and pro-choice groups were considerably more active in educating themselves about science than women in the other groups. The pro-life and pro-choice group members voluntarily joined the groups in part to learn more science and be able to use it in public debates about abortion. Like members of the two environmental groups, pro-life and pro-choice group members drew on science in an effort to improve their arguments with the public, but unlike in the other two groups, pro-life and pro-choice women did this for themselves, their families, and friends, and not as employees. They received no salary, no raises, no titles, and no promotions for their effort. Like many other groups that have been started by one or two women sitting around a kitchen table and talking about immediate family or local problems (e.g., hazardous wastes, undiagnosed illnesses, drunk drivers), the pro-life and pro-choice groups consisted of people who came together primarily so they could learn what they needed to know to be more effective citizens in a public debate they cared deeply about. In my mind, their participation in trying to learn more about science—however selective or unsophisticated—as a means of strengthening citizen activism is a legitimate and important activity for scholars of contemporary science practice to consider, and it is a type of activity not as clearly illustrated in the other sites of our study.

Finally, *even if* the abortion group chapter made the exact same points as the chapters on the conservation corporation and the environmental action group, why was the suggestion made to omit the abortion groups rather than one of the other two? Why were there no suggestions to incorporate the findings from the abortion groups into one of the other chapters that made similar points? If the abortion group chapter did not make the same points, why should it have been omitted?

I can only speculate about answers, but something was going on. Was the *possibility* of any important “science content” in abortion groups just too extreme for the likely audience—mainly academics—of our book? Why was the possibility of “real” science in a conservation agency and even an environmental action group (and of course in the high school class and the college internship) plausible in a way that it was not in the context of the US abortion debate, *even though* we could document the presence of some science-related activities and women’s interest in science there? Was one chapter about science in abortion

groups so controversial that it could compromise the credibility of the whole book? Why would these possibilities be so hard to accept?

In the chapter on the abortion groups, we made very clear that, although the women were there in part to learn science and more about it, the “science” they received was highly selective, incomplete, and inaccurate. We were not celebrating the science there, only the context that motivated women to be interested in learning more about science—a context often said to be missing in school and workplace science. Why was it not important that our readers consider a motivating context that, much more than any of our other sites, constructed pretty poor science and relied on it as a lever for political activism? If poor science is being produced in sites that are of special interest to women, why is it not important to know what these sites are and what their characteristics are?

Finally, if length were a real issue for marketing purposes, why was there no interest in finding other ways to shorten the manuscript? Why, when all the chapters were roughly the same length, was the abortion chapter singled out as [the](#) way to cut down on the book’s size and to broaden its market?

Could it be that the idea of science in the abortion groups was just too inconsistent with the whole category of taken-for-granted ideas about what science is, who does it, and how? Could it be that groups of women who care deeply about an issue that affects them directly are *never* considered scientists or legitimately engaged in some kind of science? If so, in Donna Haraway’s terms (1989), the possibility of science in abortion groups would be an especially perverse (counterstereotypic) reading of what science is: It would be a reading of science that makes its boundary much wider (more inclusive) than normally accepted, much wider than either the conservation corporation or the environmental group would require. Like Behar’s ~~“Esperanza”~~ whose autobiography would not be believed in her hometown because it was too inconsistent with how townspeople viewed her, was there something about the “story of science” in pro-life and pro-choice groups that was simply too inconsistent to be taken seriously? Although I can’t definitively answer these questions, it seems to me that ideas and pressures around what constitutes “real science” and attractive marketing were used to block the publication of the material about the abortion groups.

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### Discussion and Critique

Authors of critical autobiographies are supposed to analyze (or deconstruct) their accounts in at least four ways: in terms of their intended audiences, their purposes for telling the story, the identities motivating their portrayal of self, and some form of social critique. As I think now about the audiences I hope to reach

with this article, three are salient: science education researchers, science education policy makers, and academic book publishers. With these groups as audiences, my story—both parts of what I constructed above—was affected in some specific ways. First, because I think most people in my audience consider themselves “academics,” I wrote my story so it would be believed and taken seriously by academics. Because I consider myself an academic too, I thought I knew the best way to tell them a believable story: I should tell what happened in the most “objective,” “true-to-life” way I could. That is, the story should be non-fiction, it should be about things that really happened to me, and these things should have happened in the way I said they did. My audiences would not expect imaginative details, selective omissions, or revised endings here.

I trade on my academic credentials to enhance the chances that my audiences will believe the story (that it happened to me) and will take it seriously (consider it meaningful to them). I already have an identity as an established educational researcher—someone whose stories *should* be believed and taken seriously by others who consider themselves academics. I already have a book published, by a very reputable press, on the work my story is about. I do not expect to have trouble convincing my audiences that the story is both authentic and worth listening to.

As I think about it now, I am struck by the selfish reasons I have for writing this story. One is to bolster my identity as a good educational researcher and scholar: The academic contents of my book, over which I expected to have full control, were abridged, even in the face of my arguments. How could such a thing happen to me? I am a full professor. I have been doing well-regarded educational research for more than twenty years. I have a track record in book publishing. Why couldn't I publish the book I wanted to publish?

One interpretation, suggested (at least) by both reviewers and editors, is that I failed to make a good enough case for including the abortion groups. This answer fills me with fear: Maybe I do not have the high academic identity I thought I had. Maybe it was just my ethnographic imagination or my eagerness to bust the boundaries of “science” that led me to see science in the abortion groups. This is neither the interpretation I want for myself nor the one that I want for my audiences. Interestingly, my telling of the story now attributes the suggestion that I was not smart or persuasive enough to *me* (not the reviewers) and to my naiveté about what was going on *early* in the chronology of events.

In my story, I overcome the suspicion of inadequacy by implicitly placing the blame for what happened somewhere else—on the reviewers and publishers who, constrained by larger, more powerful external forces, restricted my academic freedom. This is a favorite interpretation among confessing academics

(cf. Atkinson, 1990): We pride ourselves in being careful investigators, accurate recorders, systematic analysts, and sure of our results, but sometimes we just are not powerful enough to beat larger, more compromising forces. This interpretation not only preserves the status of an academic but can enhance it: When powerful forces exert influences beyond one's control, one can still come across as doing good research in a difficult situation, smart enough to recognize external influences, and concerned enough to write about them so others can fight back in the future. As it turned out, this was the way I constructed my story of "what happened."

My identity as a good advisor and friend to graduate students also was put at risk in this episode. I felt guilty about the outcome, particularly its effects on Nancy Lawrence, who studied the abortion groups. Although Nancy remained a co-author of the book (and rightly so), her case study chapter was no longer included in the book. In other words, unlike me and the other co-authors, she was not able to show off her particular contribution to our study and our thinking.

From the beginning, Nancy had trusted my intuition that finding evidence of science in the abortion groups was significant (recall that her dissertation study of the groups focused on another topic). From the start, she was more skeptical than I; but I pushed her to explore the issue and she did. Many long hours of her time were devoted to analyzing her data and to writing and revising her case study chapter. Despite the various objections, I assured her that her chapter would remain a part of the book. This was a promise I did not keep. I knew there was a sense in which I had sold out in order to get the book published, and she had been the price. It was a high price to pay. Writing the story as if powerful forces prevented me from doing what was right by Nancy pushed back my sorrow at disappointing her and made me feel better about myself, at least momentarily.

In standard research practice, a critique of my subjectivity (if it were included at all) could be used to strengthen the validity of my account (Eisenhart & Howe, 1992; Howe & Eisenhart, 1990; Peshkin, 1988). This might occur in one of two ways. First, I might use my announced biases to account for (in part) the things I paid attention to or cared about in my story (see especially Peshkin, 1988). Second, by providing information about myself as a person and about my motives, I could give readers information to judge for themselves the biases assumed in my account (Howe & Eisenhart, 1990).

In what I am calling "critical autobiography" though, this critique has a different purpose—to show the author's vulnerability to the same kinds of motives, concerns, and partiality that influence the so-called "subjects" or "participants" in social science research. Just as I depicted the reviewers and editors as worried

about credibility and marketability, I was too. Just like I worried about making a strong case for the content and argument of the book, they did too. It is not as if they and I live in different worlds—I, the innocent, dispassionate raconteur; they, pawns of some larger pressures. I am not immune from their concerns, issues, or pressures, nor are they from mine. We are all in this story together—negotiating identities, testing boundaries, considering options, and trying to make a mark somehow on this product that I thought of as “my” book.

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### Conclusion

In the end, it seems that we could push the boundary of science only so far. In *Women’s Science*, we were able to demonstrate multiple and alternative ways of practicing “science.” We were able to present stories of women who were interested in science and in learning more about it, and who were successful in a variety of different capacities. Yet, we could argue for science in an environmental action group and have the argument accepted, but not in the abortion action groups. Science-related work done by women championing environmental causes was plausible; science-related work done by women arguing the pros and cons of abortion was not. In our case, funding agencies, reviewers, and publishers served as guardians of the boundary. Using our data, our findings, and my credentials, I negotiated with these guardians for more space inside what is considered legitimate science for discussion and debate in schools and other public settings

I think we did gain (or reinforce the importance of) some unconventional space for science. As a result of reading the published version of *Women’s Science*, perhaps more science educators will consider using the forms of science–ecology in the service of state legislation, cartography in the service of raising public awareness, mechanical engineering in the service of disabled access—that appeared in our sites as models for some of their science projects or curriculum units. Perhaps more people will consider the possibilities for scientific literacy that exist in community-oriented science activities (activities defined by participants as “science-related” such as PL and PC) and after-school programs.

But we did not gain all that we might have. In the face of pressure to conform, I colluded with the other boundary guards to omit the abortion groups. Whether this was due more to my intellectual limitations; to social history (expectations) about science, women or abortion; or to economics (marketing strategies) is not as important to me as the fact that the outcome limited the purview of “science” more than I think it should—more than is necessary to think broadly about the kinds of science that are relevant, interesting, and important in

contemporary public life and thus important for schools and young people to consider. That I might have pushed the boundaries farther (but could or did not) is a chance I hope will come again.

### Notes

Nancy Lawrence and John Tryneski, participants in the events I describe here, read a draft of this article and commented on it. They gave me their own views of what happened and why. I changed a few things to better represent their positions, but they will not agree with everything I have written here. Nonetheless, they generously offered me their comments and support, for which I am very grateful. I also want to thank Joe Harding and Leslie Edwards for their comments on the earlier draft of this paper.

<sup>1</sup> I included these details because they may be unfamiliar to readers who have never published a book; however, I placed them in parentheses because they are not part of the main story line of this article.

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