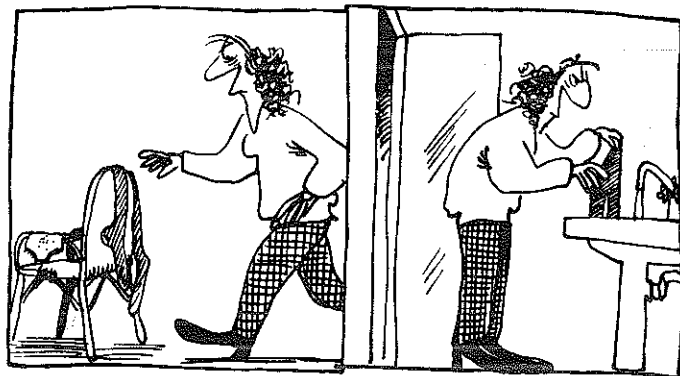


# Five



## Learning to Write as a Professional

**S**ociologists have begun to tell stories on themselves, recognizing that the impersonal reporting of ideas and research results that used to be thought scientific hides facts readers want to know (see the collections of autobiographical pieces edited by Hammond 1964 and Horowitz 1969). Most sociological autobiography has focused on how research is done, and writing deserves the same kind of attention.

I have already discussed how the institutions of scholarly life, especially schools, create the problems of scholarly writing. That discussion focused largely on the earliest phases of the scholarly career: school and just beyond. This chapter and the next look at writing problems as they arise at later stages of a career in sociology. In chapter 6, Pamela Richards discusses the crucial transition from the early post-student days to being a grown-up professional. This most immodest chapter in an immodest book tells some stories from my

thirty-plus years in the business and draws some analytic points from them.

The chief point is that no one learns to write all at once, that learning, on the contrary, goes on for a professional lifetime and comes from a variety of experiences academia makes available.

Sociologists don't think of writing as a serious problem until they have trouble getting their work written or published. They may dismiss it as blithely as an acquaintance who said, "Writing style? You mean when to underline and put in footnotes?" They may treat the skill of writing as a gift of God which they just happen not to have received, like the student who explained to his thesis committee (I was a member) that he knew his thesis was badly written but, you see, he wasn't verbal. They may realize that they have difficulty saying what they mean, but they think that they can farm the job out. The nonverbal student said it was OK because his wife was an English major and could take care of any problems. Others settle for hiring an editor they can ill-afford.

Not everyone develops the sensitivity that I did about writing clearly. I can pinpoint some of the events of academic life (largely lucky accidents I was, for whatever reason, ready to respond to) that sensitized me. English courses had something to do with it. As an undergraduate at the University of Chicago, I had a good practical course in writing, which concentrated on techniques of organization and rewriting. I probably learned there that the first draft was just a first draft that I should routinely expect to rewrite. On the other hand, a few years of graduate school, reading sociology books and journals, gave my style all the typical features I now edit out of my own students' work.

After I got my degree, several experiences with people who were now academic colleagues rather than teachers reminded me of that undergraduate wisdom. I got a Ph.D. in sociology from the University of Chicago in 1951, at the age of twenty-three. Not surprisingly, I

had trouble finding an academic job. Why should anyone hire such a child when they could have a full-grown adult for the same price (at that time, four thousand dollars a year)? I was lucky to get a research job, studying marijuana use, at seventy-five dollars a week. During the Christmas vacation, a Chicago street-car fell over onto an automobile driven by a member of the teaching staff of the Social Science II course at the University of Chicago. They needed a replacement in a hurry, and some friends already teaching the course knew me and vouched for me, so I got the job. That was how I met Mark Benney (since deceased), a British journalist who had begun adult life as a petty criminal and ended up teaching social science through the encouragement and help of David Riesman and Everett Hughes. He had published several books, and his experience as a professional writer showed in the grace and clarity of his prose, which I admired. Small, thin, and prematurely bald, Mark had a devious way that I attributed to his prison stretches. He was careful about what he said, so if he said something serious, you knew he meant it, and meant you to take it seriously.

I had already published an article or two in professional journals and must have thought I was pretty good, or at least competent. I drafted a paper based on my thesis, the study of Chicago public school teachers I've already mentioned. It raised some problems about education and social class that I thought would interest Mark, so I asked him to read it. When he gave it back, he said it was very interesting and then raised some points about the substance. Seemingly as an afterthought, he added, "Of course, I suppose you have to write it in that funny style to get it published in a sociological journal." I knew that he was a "real writer," so the remark stung, and I determined to go back and do it again, using some of the lessons about rewriting I had learned in college. I began to see that finishing a paper didn't mean you were done with it.

Several years later Jim Carper and I wrote an article

based on our study of the occupational identities of graduate students in several fields. We submitted it to the *American Journal of Sociology*, then edited by Everett Hughes, who had directed my thesis research and to whom I felt close and loyal. The manuscript came back, with a note from the managing editor, Helen McGill Hughes (Everett's wife and a sociologist as well as a journalist), saying that I was to understand that Everett really loved me, that he had written his editorial comments at four in the morning, and that I shouldn't take their violence literally. The comments certainly took me aback. Among other things, he said that whole sentences and paragraphs sounded like they had been translated from German, word for word. I didn't read German (or any other language, despite passing a university exam in French to qualify for the Ph. D.), but I knew that was bad. One memorable paragraph quoted one of our most ponderous sentences and added this commentary (given here in its entirety): "Stink! Stink! Stink!" Mark's casual joke had sensitized me. Everett's letter strengthened my desire to write clear, understandable prose that sounded like it had been English all along.

The final step in my addiction to serious rewriting came when Blanche Geer joined Hughes and me in a study of medical students. She took writing very seriously and taught me about it through serious discussions over single words in the drafts we were doing. We had wonderful and interminable discussions, for instance, about "perspective," a word and idea central to the theoretical apparatus of our study. The question was what verb we should use with it. Did people "hold" a perspective, or "have" one? Maybe they "used" a perspective. Each word's overtones were different, and distinguishable, once we focused on them. So the question was not which word was right, but what we wanted to say. We discovered problems through stylistic discussion, but we finally had to solve them theoretically.

Our conversations taught me that it really mattered how you said things and that you had a choice in the matter. They also taught me that rewriting was fun, a kind of word puzzle whose point was to find a really good economical way to say something clearly. My talks with Geer completed my conversion to taking writing seriously and were by far the most important of all these experiences, because they continued through our writing of a number of papers and books together.

The sociologists I had gone through graduate school with had habitually traded drafts of papers-in-progress with each other, and we had been pretty good about telling each other what needed doing next. I don't think I realized how this reading and commenting and being read and commented on among peers affected my professional development until I hired Lee Weiner as a research assistant a few years after I started teaching at Northwestern. I was away the summer he began work, and as a conscientious revolutionary, Lee (who later became one of the Chicago Seven) read all my correspondence, although it was not part of his duties. When I returned in the fall, he told me excitedly how much he had learned by looking through the folders I kept on papers I had written, seeing what my friends had written on, and about, succeeding drafts, and how I had taken those comments into account in my next version.

Several years out of graduate school then, I had built a pretty efficient writing routine around rewriting on the basis of friendly criticism of early drafts. I had learned to see rewriting as fun, something like doing crossword puzzles, not as an embarrassing task whose necessity revealed my shortcomings. I learned that thinking about writing, experimenting with my own style, and tinkering with other's work were fun too.

Maybe thinking of writing as an enjoyable game immunized me against the anxieties other people describe, but my relative lack of writing anxiety also had sociological roots. I had grown up in a strong theoretical tradition which also had a strong organizational

base. The Chicago school of sociology developed at the University of Chicago in the 1920s, under the leadership of Robert E. Park. (See, for further discussion of the Chicago school, Faris 1967, Carey 1975, and Bulmer 1984.) It had a coherent point of view, embodied in Park's writings and developed and carried on by a cohort of powerful thinkers and doers, most prominently Everett C. Hughes, Herbert Blumer, Louis Wirth, and Robert Redfield. It also had a long list of classic empirical monographs to its credit: *The Gold Coast and the Slum*, *The Taxi Dance Hall*, *The Gang* and, later, *French Canada in Transition* and others. I studied, along with a couple of hundred other post-World War II students, with the giants of the post-Park generation and grew up on that pile of monographs. We knew there were other ways of doing sociology, but few of us took them very seriously. Growing up in that tradition and setting gave me a theoretical arrogance, the comforting conviction that I had essentially learned all the general theory I would ever need to know from Hughes and Blumer, and that the theory was good enough to deal with any problem that came up. I knew, and know, better intellectually, but that hasn't affected the emotional result.

Knowing you are essentially right takes a lot of pressure off your writing, since you don't then try to solve sociological problems by finding the just-right way to formulate them. Some people solve theoretical problems by logical analysis. I learned to decide theoretical problems empirically. Either way is better than trying to do it by finding the right way to say it.

The growing number of sociologists and sociological specialties has produced a similar increase in sociological organizations and journals. Sociologists edit these journals, and editorial jobs are usually one of the honors that come to people who have been in the business for a while. Graduate training programs do not teach you how to edit a journal—how to copy edit papers, how to deal with the printer, or how to coax

authors to improve their work. Most journals cannot afford professional editors, so the sociologists who become editors do all that themselves. They learn the job by doing it, with the help of a few tips from their predecessors. My experiences as an editor, during which a hobby became a second profession, contributed a lot to my views on writing.

After years of editing the works of friends and colleagues informally, I took on two serious editorial jobs. In 1961 I became editor of *Social Problems*, the official journal of the Society for the Study of Social Problems, an organization that had been started in opposition to the monolith the American Sociological Association was turning into. I understood my job to be (and I think it was so understood by those members of the SSSP who had an opinion) to put out a journal that was somehow different from the "establishment" *American Sociological Review* and *American Journal of Sociology*. I wasn't sure what that entailed, but I thought I ought to try to find a home for articles that were not welcome, for one or another reason, in the larger journals.

What would make an article unwelcome? Most SSSP members thought that the establishment favored heavily quantitative work, work based on structural-functional theory, and work that was apolitical (and therefore in a real sense conservative). The SSSP thus favored work that was nonconservative, not biased toward the quantitative, and used either "Chicago" or, in later years, Marxist theories. In any event, it wanted to be open to whatever wasn't Eastern establishment. I must have accepted all that as reasonable, even though establishment journals had published my own nonquantitative, non-structural-functional work often enough.

So I took over as editor with the notion that my responsibilities consisted of publishing antiestablishment materials. I had also decided (though no one made this part of my official or unofficial responsibili-

ties) that I was going to do something about what I thought was the sad state of sociological writing by rewriting what appeared in the journal as much as necessary. With that in mind, I recruited people for my editorial board who wrote well and knew what good writing was and who I could therefore count on to help me.

I learned a lot from my first few issues. Once I assembled my first issue (and I'll speak about those problems shortly), I rewrote every paper in it extensively. That was a more intensive and more educational experience of editing than I had ever had. Doing so many papers by so many people in so many styles in such a short time made me feel like a newspaper copy editor. I learned to go through a paper rapidly and to spot the things I knew I would without doubt change immediately. (I never understood how I did some of what I learned to do: for instance, to spot a typographical error in a page of galley proofs from across the room when I couldn't even read the type.) But I also learned that I was not going to rewrite all the papers that way, much as they might need it. It took too long, and I had other things to do. I might do a few pages of a piece, to show authors what I had in mind, but after that they would have to do it themselves or it wouldn't get done. In the last few years, some larger journals have begun to employ copy editors, but even they cannot afford what it would cost to edit journal articles the way, say, a textbook is rewritten.

I learned another lesson when I assembled the articles for my first issue. A journal is supposed to come out regularly, every second month, like the *AJS* or *ASR*, or quarterly, like *Social Problems*. If you missed your deadline, you lost your turn in the printer's queue, people complained about their magazine being late, and the officers of the sponsoring organization wanted to know what was wrong. Better to come out on time. That did not mean that you published work you didn't think was good, but that you published work that was

good, no matter what its breed: quantitative or qualitative, Chicago-style or structural-functional. Every journal editor I have ever talked to has agreed that, whatever prejudices they secretly expected to implement on assuming office, they soon found that the main thing was to get enough decent articles to fill the journal and get it out on time. Authors who think editorial prejudice accounts for their work being turned down or sent back to "revise and resubmit" are, for that reason, almost always wrong.

Of course, a lot of prejudice can be hidden in the definition of a "decent article." But here I am convinced by Stinchcombe (1978), who argues that when sociological analysts are doing good work they are all doing the same thing. Their work often looks more different than it is because they try to inflate its significance by using "portentous names," derived from "epochal theories" to describe what they do. (Many fields in the social sciences and humanities foster this practice, not just sociology.) Because good work is basically the same whatever its theoretical label, "good" is a professional and catholic judgment, like the judgments of musicians or dancers, who usually recognize when others are performing well, even if the judge doesn't care much for what they are doing. When sociologists show me work they think has been turned down because of prejudice, it is almost always badly organized and badly written. (I know that that is the voice of the establishment talking and don't know how to convince skeptics I am right, other than to point to the contents of the journals, which are always more various than critics think.) The prejudices that do exist operate more subtly, as when the editor decides that one badly written, poorly organized piece is worth putting some special effort into, but not another. The lesson for people who do unpopular work is not that they can't get published but that they shouldn't expect editors to do their work for them. No one should, but some have a better chance of that happening.

I had a different editorial experience when I undertook to edit a series of books for the Aldine Publishing Company in 1962. Alexander Morin, then president and himself a social scientist, thought it would be worthwhile to put together a series that represented the Chicago tradition, broadly conceived. This led me to deal with book-length manuscripts and with authors who had the anxiety that goes with the commitment to a book. I also learned the necessity of thinking about how much a book could be expected to sell, not because Morin was a crass businessman but because if too many books lost money there wouldn't be any series. I learned the importance of subject matter and having something to say about it. People who did not care about your fabulous contribution to social theory might nevertheless read your book because they cared about the problems of death in hospital settings or the way mental illness was defined by family members, professionals, and the courts. We eventually published some fifteen books, and the series was reasonably successful, the sellers making up for the bad guesses.

Working as a book editor showed me a larger dimension of editing. I found that I could see an inner logic struggling to express itself in others' work more easily than I could see it in my own, just as I could see redundancy, fancy talk, and all the other faults in their prose more easily than in my mine. Since I wanted to criticize manuscripts in a way that would induce authors to fix them rather than just get mad (otherwise there would be no books for the series), I had to learn to be precise about what bothered me. I also had to tell them the facts of life about commercial publishing. I explained to first authors who had taken their contract to a lawyer that, yes, the contract did favor the publisher but not to worry about it since few publishers took advantage of those clauses. (With more and more publishers becoming subsidiaries of conglomerates, that advice may not be as true as it used to be.)

My own experience with editorial prejudices has

been minimal. The one area where I suffered a little had to do with a major change in the practice of sociology journal editors. My first articles, drawn from my master's thesis, were about jazz musicians. Following the practice of the exemplars I had used (e.g., Oswald Hall's articles on medical careers and Whyte's *Street Corner Society*), I quoted extensively from my field notes and interviews. But musicians didn't talk as politely as doctors (or as Hall reported they talked). They said "shit" and "fuck" a lot and, in the interest of scientific accuracy, and with a little mischief in my heart, I quoted them verbatim. That was acceptable in my thesis but editors in the fifties routinely replaced these words with dashes: "f——" and "s——." (This practice reached a height of foolishness in a postwar issue of the *AJS* devoted to the U.S. Army, in which Fred Elkin's article "The Soldier's Language" ended up largely dashes.) I forget which of my articles was finally allowed to contain bad words written out; it might only have been when they were published in *Outsiders* in 1963. Of course, dirty talk now appears routinely in published sociology.

When I described my writing seminar in chapter 1, I said that I had told the class about my own writing rituals, but I didn't say what they were. Since I began giving the class, I have started writing on a computer, so that I no longer do what I described there. But here is what I told the class then; it's the way I wrote most of what I have written, and I am not sufficiently aware of my new computerized routine to give a fair account. (What I can say of it is to be found in chapter 9.) The entire procedure is tailored to the rhythms of the academic year.

I am lazy, don't like working, and minimize the time I spend at it. So, although I have written a fair amount, I have spent relatively little time at the typewriter. I would begin what eventually became a paper by talking, to anyone who would listen, about the topic I was going to write about. When I began teaching, that meant

that I talked to my classes about it. (*Art Worlds* started out as the transcribed recordings of the lectures I gave the first time I taught the sociology of art, eight or nine years before the book was finished.) If I was invited to give a talk somewhere, I tried to persuade people that they wanted to hear about my "new research interest," that is, the paper I was beginning to work on. Those talks did some of the work of a rough draft. I learned what points I could get to follow one another logically, which ways of making a point people understood, and which ways caused confusion, what arguments were dead ends that were better not entered at all.

I had not, when I began relying on talking as a way of getting something started, read David Antin's explanation of why he writes by talking, but I recognized my own feelings in his description:

*because ive never liked the idea of going into a closet to address myself over a typewriter . . . what kind of talking is that? . . . ive gotten into the habit of going to some particular place . . . with something on my mind but no particular words in my mouth . . . looking for a particular occasion to talk to particular people in a way i hope is valuable for all of us*

(Antin, 1976, i)

After talking about something for a while (usually several months or longer) I would get restless. I seldom recognized the feeling for what it was. It ordinarily did not strike me during the school year or even during most of the summer vacation. We have for many years spent our summers, and any other time off from teaching, in San Francisco, returning to Chicago just in time for the beginning of the fall quarter. About three weeks before the day we departed, I would suddenly, with no premonitory symptoms I could notice other than this vague restlessness, sit down and start typing all day and half the night. I typed double-spaced on legal-size yellow ruled pads. I tore each sheet off the pad carefully. If it didn't tear neatly at the perforations, I didn't

use it. I didn't rewrite—not then, anyway—just kept typing. If I had trouble making a point or couldn't see how to end an argument, I made brackets by combining the slash and the underline (I love the computer's ability to produce several varieties of brackets) and said something like "I can't get anywhere with this now." Then I went on to some other point I could write about.

I added up my production frequently and announced to anyone who would listen that I had done six pages or, counting lines and estimating words to a line, 2500 words. I tried to avoid crossing anything out, but was not rigid about it. If I saw a better way to say something, I replaced the old phrasing with something better. I also, quite neatly, inserted new passages where I thought them necessary, either by cutting and pasting or marking in the text on page 7 where the inserted material on my new page 7A would go. (It pleased me when secretaries complimented my neat manuscripts.) I have written as many as three ten-to-fifteen page manuscripts—rough drafts of articles—in a three week period.

So I would return from California with these rough drafts and spend the school year tinkering with them. I often put them away for several months and seldom thought of them as the routine of teaching—attending meetings, talking with students and colleagues—took over my daily life. That helped me redo the papers because, during the interim, I would forget why a particular point or way of expressing it was so necessary and find it easier to change them. I might not take any of these folders out and begin rewriting until the Christmas vacation. I always began by fixing sentences: cutting excess words, clarifying ambiguities, amplifying telegraphic thoughts. As I told my class, doing that invariably brought up the theoretical difficulties I had papered over, so that I soon had to reconsider my whole analysis. When I could, I wrote a new version of the parts that didn't work. If I couldn't, I didn't. In either

case, I usually put the paper away again, for months or sometimes years.

From here on, the description fits my new computerized habits as well, and I will speak in the present tense. Eventually I make another draft. I can do this kind of work any time and usually spend no more than a few hours a day for three or four days at it. After a second or third draft, I have something I can send to some friends who might have helpful thoughts or harsh criticisms. I prefer hearing those criticisms in private from my friends rather than publicly in a "Letter to the Editor."

Some papers never get finished, but I hate to waste anything I write and never give up hope, not even on pieces no one likes. I have had some things in my files for twenty years (in fact, I am still nursing an even older paper on the Abbey Theatre that I wrote for Everett Hughes's class in ethnic relations in 1948).

When I get criticisms and comments, from friends or from editors who have rejected a paper, I assume that I have failed to make my points clearly enough to forestall the objections they make, and look for what I can do to meet the objections without changing my position, unless the criticism convinces me that the position requires changing. This revising and rethinking goes on until I can't think of anything else to do with it, or until some home for the piece presents itself (that is, until I am asked to prepare something for some occasion or volume, and what I have been working on fits the specifications). I have sometimes thought I was done with a piece of writing and then discovered that I wasn't. How do I know that? When I see something that can be done better than it is, and see a way to do it, I know that I will have to go through the manuscript one more time. (I twice thought *Art Worlds* was finished before it really was.)

As I accumulated experience and became more cocksure, I began to set myself writing problems. Becoming dissatisfied with the long, complicated sentences I was

writing, I started experimenting with short ones. How few words could I use? Very few. I also began searching for alternatives to the third person (too pompous) and the first person (tiresome in excess and often inappropriate). That led to an orgy of second persons, stage whispers to the reader: "You can see how this would lead to . . ."

Such a routine presupposes that the writer can afford to wait as long as I habitually do to finish things. When you write to a deadline—if, say, you have agreed to contribute a chapter to a book, and the deadline is approaching, or you have agreed to give a paper at the annual meeting of the American Sociological Association—you don't have that luxury. You don't have it, either, if you need publications to convince your colleagues or some administrator that you deserve promotion. One way around the latter problem is to do something that necessity forced on me early in my professional life. Because I had research, rather than teaching, jobs for many years, I always had to start new projects before I had finished old ones. As a result, I was always working on several generations of writing simultaneously: roughing out an initial draft of something new, rewriting initial drafts from an older project, making the final revisions in something ready for press. That is easier than it sounds. In fact, it makes every step of the process easier because when you get stuck on one job you can turn to another, always doing what comes easiest.

When I started making photographs in 1970, the standard photographic practices I then learned gave me more ideas about writing. I learned, as all photo students do, that the most important thing a photographer can do is photograph and that making thousands of bad photographs is no disgrace as long as you make a few good ones too and can tell the good from the bad. Students learn to "read" a contact sheet, made by printing a cut-up roll of film on one sheet of paper, so that each frame is reproduced at its actual size. You see

every exposure you made, and you learn how to tell which one has an idea worth pursuing. It's the perfect way to learn that all that counts is the final product and that no one will criticize you for false starts and wrong ideas if you find something good in the process. I learned to be prodigal with film, paper, and my time. That carried over to my writing. I became more willing than ever to write down any damn thing that came into my head, knowing by analogy with photographing that I could always weed out what I didn't like or couldn't use.

Sometime in the seventies, I began to develop literary pretensions and ambitions. I think this started when a friend who was a "real writer" (a writer, that is, of fiction) said kind things about some drafts of an essay I was writing on art worlds. I began to wonder if I couldn't make the writing better in a more extended sense than just clarity. I began experimenting with a kind of organization I had barely been aware of before. I began to plant the seeds of ideas to be explored later in the early sections, and to introduce examples that I would later use to recall a complex point for readers. I quoted Anthony Trollope's story (from his autobiography) about relying on an old manservant to bring him coffee before he began writing and his comment that he thought that servant deserved as much credit as Trollope himself for the resulting books. I let that stand for the artist's dependence on the help of others for getting the work done, and later in the book I just referred to Trollope and his servant, expecting readers to recall the theoretical point.

Perhaps as a result of my experiences in teaching, I have become more and more convinced of the importance of stories—good examples—in the presentation of ideas. I used to be irritated when students told me that what they remembered from my sociology of art course was the story of Simon Rodia and the Watts Towers, which I told in enormous detail and illustrated with slides. I wanted them to remember the theories I



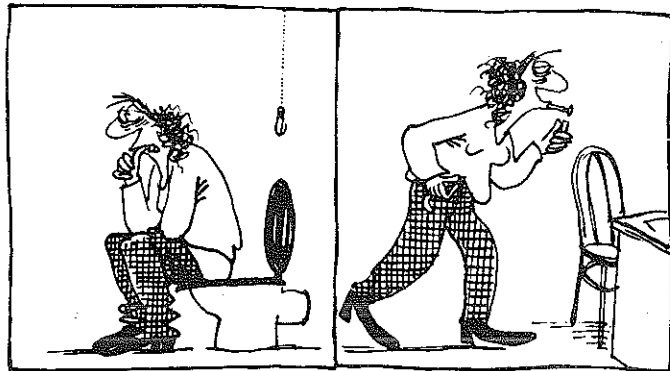
was so slowly and painfully developing. Later I decided that the stories were more important than the theories. In a way, I should have known that, because I always began writing reports of field research by picking out representative incidents and quotes from my field notes and arranging them in some order, then writing a commentary on them.

*Art Worlds* also introduced me to the problems and opportunities of illustrations. It was obvious that a book on the arts should be illustrated. I first experimented with that possibility in a mischievous way. The *American Journal of Sociology* had accepted, after many revisions, an article called "Arts and Crafts," which dealt with the way some craft media got taken up by worlds of art. In the course of the paper, I described a number of art works that illustrated my analytic points. When the article was accepted, I called the managing editor and asked if she didn't think that some illustrations would be appropriate. The *AJS* almost never published pictures, other than portraits of deceased members of the University of Chicago Sociology Department, and I think I assumed that she would say no, and I could then feel discriminated against. Naturally, she said that she would ask the printer and the editor, but thought they would say yes, as they did. Now I had more work to do, finding pictures that really made the points I wanted to make and for which I could get prints at a reasonable cost. The text had referred to Robert Arneson's ceramic sculpture of a teapot whose spout was an erect penis, and to a photograph of a nude woman by Edward Weston. I thought that perhaps there would be trouble over these (the Weston photograph included pubic hair, which had only recently made *Playboy*) but my prejudices were wrong again.

When I put the book together, I knew that it would have pictures. Grant Barnes, my editor at the University of California Press, gave me a wonderful piece of advice. He said, "Don't put captions on the pictures that just identify them. Say at least a sentence explain-

ing what the reader should see in the picture." Since I followed that advice, a reader can get the gist of the book just by looking at the pictures and reading the captions. All this has increased my interest in the visual aspects of writing and bookmaking. I expect my new computer's ability to produce pictures and unusual typefaces to be a help with that.

To repeat the moral, the only good reason for talking so much about myself, you learn to write from the world around you, both from what it forces on you and from what it makes available. The institutions scholars work in push them in some directions, but also open up a lot of possibilities. That's where you make a difference. I have been relatively open to the possibilities, perhaps more than most, and resistant (again, perhaps more than most) to the pushes. The world does push and sometimes it hurts to resist. But my story, I think, for all its historical and personal peculiarities, shows that the opposite is truer than most people think.



## Risk

by Pamela Richards

The bulk of this chapter is by Pamela Richards, a sociologist who teaches at the University of Florida, but it needs some introduction and explanation. I had been very pleased with the results of asking Rosanna Hertz to write to me about what she meant when she said that some ways of writing were "classy." I was therefore on the lookout for a chance to see what else I could discover by persuading people to write to me about what they meant by their offhand remarks. I didn't have long to wait.

I have known Pamela Richards since she began her graduate work at Northwestern. After graduating and beginning her teaching career at Florida, she continued to do technical statistical studies in criminology, in the style of her dissertation. After several years, she decided to try something different and use her substantial fieldwork skills to do a study of the Florida state women's prison located near Gainesville. She thought

the study would be more difficult than it turned out to be. The prison officials made her entrance easy, and the residents, initially suspicious, soon talked to her freely and gave her access to most prison activities.

After a year she had accumulated a substantial file of field notes and knew a great deal about life in this prison. She thought she ought to begin writing up her results. We had corresponded earlier about her fieldwork problems, so she confided that she was having trouble getting started. Since she had successfully written up her earlier research, she thought there might be something about qualitative materials that required a different approach, and she asked me about it.

I brought out my standard remedy, mentioned earlier, suggesting that she sit down and write whatever came into her head, as though the study were done, but without consulting her field notes, the literature on prisons, or anything else. I told her to keep typing as fast as she could. When she got stuck, I suggested, she should type in "I'm stuck" and go on to another topic. Then she could read the results and see what she thought was true. In that way, she would find out how to analyze her field materials, because she would have to check them to see if what she thought was true really was and, if not, what was. In any case, I said, she could produce a lot of rough draft quickly, and that would be a start.

I have given this advice to many people over the years. Not many take it. They don't argue with me, they just don't do it. I had always found that hard to understand, but the results of my advice to Pamela helped me to see why they were so balky. She wasn't balky, but, because she was reflective and articulate, she could make clear what others had found troublesome.

For a while, I heard nothing from her. Then she wrote to say that she had followed my advice and was enclosing the fifty pages she had written in ten days as a result. That tickled me, of course. It's rewarding to see

your advice pay off. But her accompanying letter raised what turned out to be an important question, one for which, with a little prodding, she provided a wonderfully detailed answer.

She wrote that she had rented a cabin in the woods to live in while she tried the experiment of writing the draft. "Even though I knew it would be a very high-risk operation," she said, "I decided to try it anyway." I couldn't understand what she meant. She was a well-established professional who had published in respected journals and coauthored a book. She gave papers at professional meetings and had just been promoted and given tenure. She had, in other words, been through the scariest trials that afflict young academics. Where was the risk?

Here was my chance to use the "research method" that had been so successful with Rosanna Hertz. I wrote Pamela, asking her to explain what was so risky about sitting at a typewriter for ten days and writing any damn thing that came into her head. At worst, I pointed out, she would have wasted the time she had spent on it, but that can never be much of a price for someone who otherwise might not have written anything at all.

Again I didn't hear for a while. Then I got the letter that follows, explaining honestly and personally what lay behind that casual remark. I originally intended to use what she wrote as raw material for an analysis of the problems of risk. As I reread what she had written, however, it was clear that I could add very little to her story and analysis. So I asked her if she would be author of the body of this chapter, for which I would simply write an introduction and whatever else was necessary to relate it to the rest of the book. She agreed. It's an unorthodox way of doing things, but it seems the best and most honest way of getting what needs to be said said. What follows is her letter answering my question.

Dear Howie,

I just finished two cups of coffee while thinking about the issue of risk. My meditations have to start with three dreams that I've had in the last week. Two are about risk (among many other things, I'm sure) and one is about pushing through the risk. Actually, only two are dreams, the other is a different sort of midnight event that I suffered through right before I received your letter.

In my first dream, I had sent copies of three chapter drafts to a close friend I've known since graduate school. They were the same drafts that I'd sent to you. (I haven't really sent her anything yet.) She and I met at the American Sociological Association meetings in San Francisco, and she brought a huge stack of written comments with her. She was angry with me, and the comments were scathing. They went on for page after page: "This is absolutely the stupidest stuff you've ever written. . . . How could you say such things? . . . Don't you realize the politically objectionable nature of what you've said here. . . . What's wrong with you, haven't you any sense at all? . . . This is nothing but bullshit. . . ." As I read through the stack of comments, she sat there and simply glowered at me, and I felt like she wanted to take me by the shoulders and shake me till my teeth fell out. Naturally I began to cry—silently, with the tears running down my face. I wanted to wail and keen and run away, but because we were at the meetings and there were all these colleagues around, I had to keep as good a face on it as possible. I felt terrible. Betrayed, perhaps, but mostly as if I had let her down. I felt that I had failed to measure up to what she expected of me, and that this preliminary work had somehow demonstrated that I was a shit—intellectually, personally, politically, and morally. I struggled up from the table where I was reading the comments. She leaned back in her chair and watched me. Her face was cold and the anger had turned to disgust. Then somehow I was pushing my way through a crowd of

conventioning sociologists (none of whom I knew), trying to get out. I kept bumping into them, saying "Excuse me," but no one responded much. They didn't even really look in my direction when I ran right into them. Then I woke up.

Now for some balance. I had a second dream that night, it seemed to be right after that one. (I'd been reading Lillian Hellman's *An Unfinished Woman* and *Pentimento*. Over and over and over. I don't quite know why.) In the second dream I was sitting in a chair composing things for the book on the women's prison. I'm not sure what chapter or what topic, but the words were flowing beautifully. I wasn't writing them down; instead I was speaking them, and they just rolled out of my mouth. Everything was perfect, the style was gorgeous, and I was conscious of the fact that it all sounded as if Lillian Hellman were writing it—it was exactly the same style, the same marching sentences, the same feel and expression. It was wonderful. I felt very powerful and fully in command of what I was doing. I knew it was good stuff, knew it was elegant, and even began gesturing as I was speaking, almost as if it were oral interpretation. When I awoke, I just sort of floated up into consciousness slowly and comfortably, very pleased with myself and what I had accomplished.

But then, two nights ago I flashed out of a deep sleep (no dream this time) with a perfectly formed, crystal-line conviction. I knew, absolutely and with complete certainty, that I was a fraud. The knowledge wasn't constructed through some explicit argument; it didn't develop out of anything I recognized; it was just there. So I began turning it over in my mind, trying to see what might be on the underside, and it began to take on better form: "I am a fraud because I don't work the way everyone else does. I don't read the classics as bedtime reading; hell, I don't read anything except weird novels and stuff that has nothing to do with my 'work.' I don't sit in the library taking notes; I don't read the journals cover to cover; and what's worse, I don't want to. I am

not a scholar. I am not a sociologist because I don't know any sociology. I haven't the commitment to steep myself in the ideas and thoughts of The Masters. I couldn't converse meaningfully about The Literature on any topic including those in which I am allegedly a specialist. Even worse, I have the temerity to claim that I am doing a study of women's prisons, when in fact I haven't done it right. I don't know all sorts of things I ought to know, and can't seem to force myself to do it the way it ought to be done. Worse still, I know I have to go back soon and do another data push, filling in the holes, expanding things, and doing it right this time. And I don't want to. I'm too tired."

Not too useful for the middle of the night, right? God, it was torture. I went round and round on these sorts of things, getting angry and frightened by turns. I simply couldn't shake the conviction that I was a fraud. The main reason? I don't "do sociology" the way all my colleagues appear to do it, and the way it's supposed to be done. (And I've had a dry period as far as writing goes—almost two weeks—which leads rapidly to the conviction that I am a lazy parasite who doesn't do anything, anything at all.) The fact that I know that no one works the way they say they do, and that no one hews the perfect methodological line doesn't help much because I cannot translate this knowledge into gut-level belief. I feel vulnerable. Others can get me if I let on that I am a misshapen lump of a sociologist, even if they are equally misshapen.

So what does all this have to do with risk? For me, sitting down to write is risky because it means that I have to open myself to scrutiny. To do that requires that I trust myself, and it also means that I have to trust my colleagues. By far the more critical of these is the latter, because it is colleagues' responses that make it possible for me to trust myself. So I have dreams of self-doubt and personal attack by one of my closest and most trusted friends.

God, it's hard to trust colleagues. There's more at

stake than simply being laughed at. Every piece of work can be used as evidence about what kind of a sociologist (and person) you are. Peers read your work and say, "Hell, that's not so bright. I could do better than that. She's not so hot after all." (And, by extension, they decide that your public act of sociologist is fraudulent.) The discipline is set up in such a competitive fashion that we assuage our own insecurities by denigrating others, often publicly. There's always a nagging fear (for those of us who are junior, unknown sociologists) that even peers can make offhand comments about us that will become part of our professional image. If those comments are critical or negative, it's dangerous. This makes it very risky to give drafts of anything to peers. Few people understand what working drafts are. They assume that first drafts are just one step removed from being sent out for review. So if you show up with a working first draft, you worry about what could happen. They could decide that it's shoddy work, poorly constructed, and really quite sloppy. Their conclusion? That you're not much of a sociologist if you pass around such crap. And what if they tell that to others?

But say you can convince them that a working first draft is indeed a working draft, that it has been whapped out in a stream of consciousness fashion, that it is truly just for ideas. It's still terribly risky because the reader may not be looking for great grammar and well-turned phrases, but she is looking for stunning ideas. In some ways this is even more terrifying. It's ideas that are on the line, not ability to write. How often have you heard someone say, "Well, she may not be able to write, but god, is she brilliant!" It is OK to write like a college sophomore if you are bright. If you give someone a working draft to read, what you're asking them to do is pass judgment on your ability to think sociologically. You're asking them to decide whether you are smart or not and whether or not you are a real sociologist. If there are no flashes of insight, no riveting ideas, what will the reader conclude? That you're

stupid. If she tells that to anyone else, it's the kiss of death. Hence the fear of letting anyone see working drafts. I cannot face the possibility of people thinking I'm stupid.

Most of these points also apply to letting sociologists other than your peers see your work, but with something of a twist. There are times when giving your work to senior colleagues seems even more dangerous than giving it to peers. Say you're an untenured faculty member. What is the practical outcome of getting known as a sloppy worker (scenario 1 above), or a concrete brain (scenario 2)? What if members of the tenured faculty reach this conclusion about you and your work? No grants, no job offers, no promotions. That's risky. Professional reputation is tied to professional position, and few of us have the power to say, "I don't care what you think."

To overcome these fears, to take the risk of being thought sloppy or stupid, you have to trust your colleagues. But the discipline is organized in a way that undermines that trust at every turn. Your peers are competing with you psychologically (ah, the perversity that allows me to feel better when someone else eats dirt) and structurally. Tenure, grants, goodies are becoming more and more part of a zero-sum game, as the academic world feels the current economic crunch.

So peers are hard to trust, especially those close to you: those in your department or those in your specialty. It's also very easy to fear your senior colleagues because you feel that they are constantly judging you. They're supposed to, because they are the ones who feel that they have the duty to weed out the good from the bad in this young crop of academics. They do talk to one another about your work and tell one another what they think of your potential. So how can you trust them not to tell tales when they decide that your work isn't very good?

This problem of trust is critical because it undermines the kind of emotional and intellectual freedom

that we all need if we are to create. Who can you trust? I imagine there are a few people who are so confident that they don't really worry about what colleagues think, but they're a special breed, a very uncommon type. They just charge ahead, dropping off manuscripts left and right, filling up people's mailboxes with page after page of interesting and useful ideas. How is it possible? Some of them have the kind of personality that gives them this ability; others (most) have the structural freedom that gives them more power to say, "I don't give a damn what sociologists are 'supposed' to do, I'm doing what I want." I've noticed a little bit of this (a very little bit, I'm afraid) in myself now that I have tenure. It's not that I necessarily trust anyone more, it's just that I can be less concerned about the impact of their negative judgments.

But trust—. Who can you trust? When I think about the people I trust to read my work, I realize that they are people who already know how stupid I can be: the people I went to graduate school with, the people who taught me sociology while I was in graduate school, and a few people since that time whom I have come to know as friends as well as colleagues. People who knew me in graduate school have seen it all, and I know that with them there's only one way I can go: up. They've seen my early attempts to write and think, supported me through that, and believed that there was something lurking there beneath all the confusion. So I trust them. And, not incidentally, they trust me. We share things back and forth because of those early bonds. After all, nothing could rival the pain involved in those first attempts to sneak out into the world, scribble a few notes, and then come home and try to make something of it. And nothing can rival the exhilaration of having someone tell you that those tiny, tentative offerings were good. The colleagues since then who have also become friends are few but precious. Our mutual trust comes from having struggled to overcome the structural barriers that originally divided us. Like all friendships,

they're the product of those cautious little dance steps that move you close together and then apart, near again and then farther away, each approach creating a bit more trust and concern. I have no prescription for creating those trusting friendships, though I wish I did. With me it's highly idiosyncratic, although it sometimes comes from working on a shared research project.

So these are the people I trust with working drafts. The professional risk is minimized by our common history. Their responses to me do something important, something absolutely critical if I am going to be able to continue to construct working drafts. Their responses convince me to trust myself, because for me, there's another great risk involved in writing. It's the risk of discovering that I am incapable of doing sociology and, by extension, that I am not a sociologist and therefore not the person I claim to be. The risk of being found out and judged by colleagues is bound up in the risk of being found out and judged by myself. The two are so closely interwoven that it is often hard for me to separate them. How can you know that you are doing OK, that you are a sociologist, unless someone tells you so? It's other people's responses that enable me to understand who I am.

These then are the twists of risk: I trust myself (and can therefore risk writing down my ideas—things that I have made up) primarily because others I trust have told me that I am OK. But no one can tell me that until I actually do something, until I actually write something down. So there I am, faced with a blank page, confronting the risk of discovering that I cannot do what I set out to do, and therefore am not the person I pretend to be. I haven't yet written anything, so no one can help me affirm my commitment and underscore my sense of who I am.

I need to mention something else about gathering confidence from the feedback of trusted friends. You have to trust these people not just to treat you right (not to be competitive with you, not to tell tales when you

mess up), but also to tell you the truth. I must believe *absolutely* that if I write crap or think idiotic thoughts they will tell me. If I can't trust them to tell me the truth, then their feedback will not help me trust myself. I'll always wonder whether my ideas are really good, or whether they're just trying to be nice. The feeling that someone is humoring me is more damaging to my sense of self than outright attack. Sure, we all tell little white lies to each other. But there's got to be an underlying honesty, or I really start spinning. We must believe that it's no sin to make mistakes and no sin to criticize, otherwise feedback is useless.

How do I try to deal with all this risk and get myself moving? To begin writing at all, I sometimes have to look backward. I say to myself, "Well, I may not have written about prisons before, but I did write about juvenile delinquents, and people seem to think that was acceptable." It's at least a small bit of comfort. Or I look far to the future: I call trusted friends and tell them about my work. I run on and on, they make appropriately comforting noises, and then I feel a bit stronger. Sometimes I feel strong enough to begin writing. There's something that I think many of us believe: talking about work is less of a risk than writing about it. In part that's because no one remembers the ideas that you speak. But it's also as if we have an informal agreement not to hold one another responsible for anything we say. So I can throw out some safe comments, gather reinforcement, feel better about myself, and maybe take that first risk. But there is a catch here too. Because what we say doesn't count, it is easy to think of these conversations as inconsequential bull. But if I think that, then the listener's positive feedback is not credible, because I conclude she is responding to my act, my sociologist's facade, rather than to any meaningful ideas. If, however, I can learn to take talk seriously, people's responses can help me get the first words down on the page.

In some ways, writing gets easier the more you do it,

because the more you do it, the more you learn that it's really not as risky as you fear. You have a history on which to draw for self confidence, you have a believable reputation among a wider number of people whom you can call on the phone, and best of all, you have demonstrated to yourself that taking the risk can be worth it. You took the risk, produced something, and *voila!* Proof that you are who you claim to be. Though I must also admit that it's not as easy as I'm making it sound. My writing history gives me some confidence, but I look at my past work with mixed emotions. It looks awkward and full of errors, and I tell myself that I must do better. My expectations change constantly, and I continually redefine what I consider to be good work. This means that every time I sit down to write I find myself wondering whether I can really do this stuff at all. So writing is still a risky activity.

But what I seem to be learning as I spend more time writing is that the risks are worth taking. Yes, I produce an appalling amount of crap, but most of the time I can tell it's crap before anyone else gets a chance to look at it. And occasionally I produce something that fits, something Lillian Hellman might have written, something that captures exactly what I want to say. Usually it's just a sentence or two, but the number of those sentences grows if I just keep plugging away. This small hoard of good stuff also helps me take risks. When I feel as if I simply cannot write, I sometimes go back and reread sections of something I've written that I like. It reminds me that there are two sides to risk. You can lose, but you can also win. I tend to think only of losing, and that makes me fearful. Rereading some good stuff can sometimes get me started when other stratagems fail. And I'm also seeing that the negative side of risk-taking isn't as bad as I fear. I can hide the worst of the writing I do. No one besides me need ever see it—and I throw it out as quickly as I can. What I show others are things that I think have some merit, and even the occasional paragraph that rolls beautifully off the

platen. In other words, I have some degree of control over the risks involved in writing and letting others see what I have done. I am not completely at anyone's mercy, not even the mercy of my own impossible demands for perfection. I am allowed to throw things away.

So. But it's the complexity of risk, its dual nature, that allows me to dream of being attacked by a friend and of writing like Lillian Hellman, both in the same night. As I write more and more, I begin to understand that it's not all-or-nothing. If I actually write something down, I'm liable to win a bit and lose a bit. For a long time I worked under the burden of thinking that it was an all-or-nothing proposition. What got written had to be priceless literary pearls or unmitigated garbage. Not so. It's just a bunch of stuff, more or less sorted into an argument. Some of it's good, some of it isn't.

*I have nothing to add to this analysis. Pamela Richards has explored in detail the organization of peers and superiors characteristic of the world of the young academic and shown vividly how it affects one's willingness to take the chances that trying to be a professional intellectual confront you with. Having two personal stories in this book gives you a feel for what is peculiar to the person and what is generic in the situation and process. I don't know how typical these feelings are of other fields. I think they afflict most academics and intellectuals.*

## Seven



### Getting It out the Door

**T**racy Kidder's *The Soul of a New Machine*, an account of an engineering team creating a new mini-computer, taught me a useful expression: "getting it out the door." People in the computer industry commonly use it to refer to the final stage in the development of a new product. It takes a long time to create a new product: conceive the idea for it; translate the idea into plans for hardware and get the hardware built; simultaneously create a software operating system to control the hardware and the applications and programs that will make the machine worth having built; write the instruction manuals from which people will learn how to use it; shrink wrap the books and disks; and finally see the product shipped out to dealers and users.

The industry has a special expression for completing the process because so many things can interfere with it happening. Many projects never get out of the door. The hardware doesn't work the way it's supposed to.