

KRISTIN LUKER

SALSA DANCING INTO THE SOCIAL SCIENCES
RESEARCH IN AN AGE OF INFO-GLUT



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*Research in an Age
of Info-glut*

Kristin Luker

HARVARD UNIVERSITY PRESS

Cambridge, Massachusetts

London, England

2008

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Printed in the United States of America

Library of Congress Cataloging-in-Publication Data

Luker, Kristin.

Salsa dancing into the social sciences : research in an age of info-glut / Kristin Luker.

p. cm.

Includes bibliographical references and index.

ISBN 978-0-674-03157-9 (alk. paper)

1. Social sciences—Research.
2. Social sciences—Methodology. I. Title.

H62.L7995 2008

300.72—dc22 2008018302

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SALSA DANCING INTO THE SOCIAL SCIENCES

1



Salsa Dancing? In the Social Sciences?

I'm serious. Really I am. About the salsa dancing part, that is. After years of teaching incredibly smart graduate students at Berkeley, UC San Diego, and Princeton, I've come to believe that salsa dancing (or any other enterprise that makes you hot and sweaty and takes your mind off your work) is absolutely essential to successful research in the social sciences these days, from sociology to political science, from anthropology to education, from social work to psychology. It might seem that dancing doesn't have a lot to do with social research, and doing social research is probably why you picked this book up in the first place. But bear with me. Salsa dancing is a "practice" (a word I'll be using a lot in this book) as well as a metaphor for a kind of research that will make your life easier and better; and if you stick with me through this first chapter, I promise to tell you why.¹

Actually going to a salsa dancing club has been known to work wonders for untangling knotty research problems in the social sciences. As your partner holds you close, the Internal Censor (that

part of you that tells you not only are you terminally stupid, but no self-respecting social scientist would even *consider* doing the research you have in mind) gets lulled off-guard by the seductive rhythms of salsa. As your shoes trace new patterns on the dance floor and your hips start to swivel, a different part of your mind takes over and you find yourself drifting into new intellectual areas, making connections across boundaries and even across disciplines. You suddenly find yourself knowing things you didn't know you knew.

It is my deeply held conviction that the very best social science research of the coming era will be exactly this kind of research—research that draws on the kinds of bold and interdisciplinary insights you can get when you salsa-dance. But be warned: you can't just show up at a salsa dancing palace and expect to have as much fun as you should without having done at least a little training ahead of time. For all its improvisational nature, salsa dancing builds on some very specific steps. It's just that kind of training which this book aims to teach you as it trains you to be a “salsa-dancing social scientist.”

What do I mean by being a salsa-dancing social scientist? Over many years of struggles with my own research and with helping those aforementioned very smart graduate students, I've come up with a certain way of doing research that aims to hit that sweet spot between the rigor and theory-building capacities of canonical quantitative social science research, and the emergent, open-ended, and pragmatic capabilities of traditional field research.² Another aspect of this sweet spot where salsa-dancing social science exists is that the questions we address using this method fall at the intersection of what has come to be called “public sociology” and “critical sociology” (feel free to insert the name of your own particular discipline here). We ask questions that have important implications for the larger society (i.e., “public sociology”), but one of the ways we ask those questions is to start out by assuming that at least part of the problem is that the way the question is traditionally asked is problematic (i.e., “critical sociology”). If I had to free-associate a list of terms that define salsa-dancing social science, I would say that it's

holistic and attentive to context, conceptually innovative, methodologically agnostic research that sees itself as socially embedded, is strongly committed to building theory in a cumulative way, and is deeply attentive to questions of power.³

I plan to show you how to do this salsa-dancing research by carrying out three different enterprises in this book, and, like salsa dancers themselves, the three tasks I have set for myself (and you) will wind themselves around each other in the course of the pages that follow.

I start out by taking seriously the cliché among philosophers of science these days that the ways we go about studying the social world are themselves deeply embedded in that very world. This book will use the history of social science research methods to argue that most of what we think of as “good” social science, as well as many of our ideas about how to go about achieving it, are shot through and through with what Pierre Bourdieu calls “doxa”—those taken-for-granted ideas that are so much a part of our social world that we rarely even notice them.⁴ In concrete terms, I will argue that the political, social, and historical context in which specific “research methods” grew up, as well as the power relations among different kinds of stakeholders, have indelibly shaped what we have come to think of as “scientific” and “rigorous” research.

You might think of this as a paralyzing insight, since it stands to reason that the kinds of research you and I are about to embark on are socially situated as well. I actually find it quite a liberating insight, and I hope that by the end of the book you will too. Which brings me to my second goal in this book: I plan to teach you concrete guidelines on the equivalent of those few key steps you need to know in order to salsa-dance in the social sciences. I will teach you how to go about doing rigorous, compelling, and intellectually honest research, even in the shadow of the knowledge that “research methods” are not now, and never were, pristine, ahistorical social practices that were handed down from on high.⁵

Finally, my third and last task in this book will be to give you solid

advice about how to survive the process of doing high-quality salsa-dancing research in the shadow of uncertainty while keeping your sanity intact, and even how to have fun in the process.⁶ I am going to guide you through all the phases of a research project, from conceptualizing it, to executing it, to defending it from people who may hold a more orthodox view of how to do research.

You see, it's not just the orthodox methods of doing social science themselves that have become frozen with an excess of "doxa," but a rigid and often puritanical set of beliefs have grown up about the best way of getting that research done. Taken-for-granted assumptions in academia about how hard to work, when to work, and how and what to read, just to mention a few of the sacred cows I plan to push gently off the road, are often relics of a very different era. All they do is make you miserable and slow you down from the exciting and meaningful work you should be doing.⁷

My own personal trajectory is that I was trained in quantitative methods but discovered early on that the kinds of things I wanted to know about weren't easily amenable to those kinds of methods. But then when I turned to qualitative methods, I found that I was missing some of the rigor and logic that I had grown to appreciate from quantitative research. And as the issues of modern life became more pressing, I worried about the incapacity of qualitative methods to fully address questions of power. Over the years, that missing piece, that sweet spot I mentioned between the two traditional forms of social science, came to bother me more and more. The rise of postmodernism and what I call info-glut meant that my craving for something different became even more acute. I'll mostly be talking about qualitative methods in this book, because they are best fitted to the kinds of research my students want to do, but there is no reason for you aspiring quantitative researchers not to take the guidelines for salsa dancing in the social sciences and use them in any way that works for you.

Moreover, I hope to convince you that the dichotomy between

qualitative and quantitative methods is simply silly, and that a good salsa-dancing social scientist should be open to whatever methods will help you understand that part of the social world that challenges and intrigues you.

I'll be using salsa dancing as a metaphor for the kinds of social science methods I'll be teaching you in the pages to come, as well as a shorthand for a set of non-intellectual practices I want you to engage in. In terms of methodology, for example, there are only two steps in salsa dancing, and only two steps in my kind of social research, theory and method. In both cases, the beauty of the form is in how you do them.

At the same time, I'll also be urging salsa dancing—and yoga and movie-going and weight-lifting and myriad other forms of fun—on you as sweaty and thrilling and liberating “practices” that are absolutely basic to rigorous social research. You can't think like a salsa-dancing social scientist unless you are willing to play like one.

Who am I to be bossing you around like this? My credibility comes not so much from having done years of research, although I have done that, as it does from having mentored flocks of now happily employed graduate students, helping them to get a handle on what excites them in the world of research and how to make that research real without going crazy in the process.

For many years I taught the first-semester required graduate methods course in the department of sociology at UC Berkeley, and in recent years I've taught a similar course in the Jurisprudence and Social Policy Program (JSP) in Berkeley's law school. In both of these courses, I've found myself working with countless students who want to do a certain kind of research, but just don't know how to do it. They come from all of the social science disciplines—not only the ones represented in JSP (history, sociology, psychology, philosophy, and political science), but also from the fields of city planning, education, business, public policy, and anthropology.

When I speak of social science “methods,” by the way, I mean a

set of guidelines about how to conceptualize and execute a systematic and rigorous intellectual inquiry into something that lets you get as close to the “truth” as possible. For me, the twin goals of “methods” are to create a research design where (1) you can be surprised by your findings and (2) others can be persuaded by them.⁸

You’ll be hearing a lot about “truth” in the social sciences in the pages to come, so you should probably know that both truth and its handmaiden objectivity are going through some tough times right now. Lots of social scientists see no problem at all with the concepts, and think they are as reliable and trustworthy as they ever were. Others dismiss even the attempt to approach something resembling the “truth” as hopelessly misbegotten, a relic of older and less sophisticated times. As for myself, I see the search for objectivity in the social sciences as something along the lines of Zen enlightenment: I don’t personally expect to achieve either of them, but I do find the pursuit worthwhile. Let me tell you two different parables to prove the point.

It turns out that the ancient mariners (the Portuguese, not the one in the poem) accomplished extraordinary feats of navigation with bad, albeit beautiful, maps. They circumnavigated the globe with maps that had large sections around the edges inscribed “Here There Be Dragons.” So you and I can do the same. We can get a much better picture of the social world than the one we have at present, and we can get much farther than we ever imagined possible even if our maps do have the modern equivalent of dragons sketched all over them.⁹

The other parable is one told to me by my colleague Steve Epstein, to the effect that even the most radical, postmodernist, social constructionist Act-Up AIDS activist still wants to know if AZT works.¹⁰ In other words, sometimes *we just need to know*.

But let’s get back to my students, the ones who keep getting stuck on the way to doing the research they really want to do. It’s not that these students aren’t smart—quite the contrary. And in many cases

it's not that they haven't been taught "methodology," statistics, and research design. Still, these very smart, very hardworking people keep getting stuck year after year, and always in the same predictable ways.

I've heard the quote attributed to lots of people, among them Martin Buber and the Twelve Step folks, that one definition of insanity is doing the same thing again and again and expecting something different to happen.¹¹ Watching my students over the years, and watching the same problems crop up again and again, I've come to believe that there is something wrong—in fact, insane—with the way we teach "methods." To be blunt, we keep teaching methods in the same old ways, and students keep getting stuck in the same new ways—new, that is, in terms of the kinds of problems my generation of researchers faced.

And in turn, I've come to believe that the problem in how we teach "methods" is that the world that "methods" were meant to study has changed profoundly. Not only has social reality changed, but our assumptions about how to think about and explore that social reality have changed even more, and our "methods" don't reflect that change yet. I'm willing to bet that the majority of people in my shoes who teach students how to think about social research have at least three things in common. First, we ourselves were trained in a pre-Foucauldian era, and we have not really come to terms with what Foucault has done to our taken-for-granted ways of thinking, much less with what he has done to theory, and still less what he has done to methods. Second, people like me were raised in an era of information scarcity, not information overload, and this, too, has consequences in how we think about the world. Finally, people like me were raised in a much more linear world than the one students currently live in. I'll be coming back to all of these themes many times in this book, but for the moment let me say a little about each one.

When I refer to a pre-Foucauldian era, I mean that more as a

label for a zeitgeist than a specific reference to Foucault himself. Just as the early years of the twentieth century were permeated by “Freudian” thinking, often in ways that would have made the historical Freud blanch, so all of us in the early twenty-first century are “Foucauldian” in ways that may not necessarily be faithful to the text and letter of his actual written work.¹² When I say Foucauldian here, I mean that all of us who try to say something about the human condition in an empirical way have become much more skeptical of the enterprise than we used to be. We can no longer take for granted—if we ever did—that social reality exists “out there” in some uncomplicated way and that we can measure and study it without undue fuss.¹³ At the very least, we have to accept that writing about the social world is to fix an ambiguous, shifting, complex, multicolored reality into a single black and white sketch.¹⁴ In postmodernist lingo, we “privilege” our own account of what happened, our own point of view. Knowledge is power, and social scientific knowledge is a special kind of power, because much of the larger society still thinks we somehow embody truth with a capital T. We understand, accordingly, that to write social science is not just to passively “report findings” but to enter into a whole range of power relations. Thus, instead of the disinterested outside observer we sometimes imagine ourselves to be, we are in fact changing what we observe by the very fact of reporting it, if not in the actual observing itself.

If you are a picky person, and/or you have a background in either philosophy or the social studies of science, you are probably thinking that the process I am describing here actually started in the early part of the twentieth century, and merely accelerated and got more popular with the rise of postmodernism. You would, of course, be right. But in terms of how run-of-the-mill sociologists have come to appreciate these tendencies, my own experience is that it came with the enormous popularity of Foucault, and in particular, his *Discipline and Punish*. I myself have lived through the period in which virtually no one could pronounce Foucault’s name, to one in which

that same name comes up in every seminar, whether we are discussing sex, racial/ethnic relations, legal authority, or the welfare state, just to name a few locales where it has cropped up in my teaching recently.

In addition to the influence of Foucault and all he represents, the second theme I mentioned, that the extent of information available has begun to overwhelm the human capacity to process it, radically separates those who teach from those who need to be taught, but not in the way you might expect.¹⁵ At my own university, the library holds nine million volumes; with access to Inter-Library Loan (facilitated by computer), I can get my hands on millions more. It gets worse. At this writing, my library has 248 electronic databases of journal articles, reports, newspaper stories, and specialized scholarly resources, and in just *one* of those databases (LexisNexis) are lurking an estimated *one billion* documents. The information available to scholars has exploded in my own recent lifetime. Many of the people who are teaching you, however, unconsciously rely on and teach research strategies based on an assumption of information scarcity. It was one thing to tell students to “review the literature” in my day; it’s another thing entirely to tell students that when “the literature” is literally billions of documents.¹⁶

The third thing that marks people of my generation, that we were raised in a world more linear than the one you take for granted, means that much of what people like me teach young people rests on certain taken-for-granted assumptions—the “doxa” that I mentioned earlier—about the existence of a linear world which by and large has disappeared. A brilliant article by Jack Goody and Ian Watt argues that the rise of literacy fundamentally changed how the human brain worked and how social life worked as well.¹⁷ I see the same thing in my students when it comes to linearity, and I personally blame (or credit) the Internet.

In my experience, people who have come of age since the advent of the World Wide Web no longer think of the world as proceeding

logically from A to B to C. Rather, you are used to dipping into information and, by using hypertext, going where you want to go, rather than going along the straight line that a book or journal author wants you to follow.

This comes to you naturally, whereas a person of my generation gets a little nervous without the road signs to a beginning, a middle, and an end. I always feel a bit unmoored in the Web, wondering how I got from a page about evangelical Christians to a page about what's being served for lunch this week in a primary school in France (something that actually happened to me once).

Traditional research methodologies are based on an epistemology, that is, a taken-for-granted set of beliefs about how it is that we know what we know, and that epistemology in turn presumes a certain linear view of how the world is experienced and should be studied.¹⁸ If you have already taken a methods course, you will recognize this as having come up in the guise of independent, dependent, and conditional variables. If you have ever had to memorize a sentence like “A causes B; A causes B, but only in the presence of C” or “A and B are *spurious* correlations; A is really caused by C,” then you know what I mean.¹⁹ That is, you know that when we assume that A in some sense causes B, and that A is both logically and temporally prior to B.

This notion of linearity not only underlies our notion of how traditional social scientists think the world works, but also—and this is a key point, so underline it—it understructures, without our ever noticing it, our *practices* about how we should go about doing research in and on that world. Not only does A cause B, which means that in every good research project we must set up an independent and a dependent variable beforehand, but it also means that the actual process of doing research is usually taught as a linear one. We are supposed to design an inquiry, gather the data, analyze the data, and then, finally and blessedly, write up the data. When we come in later chapters to how to do salsa-dancing research, you will see that

more often than not, the attempt to specify an “independent” and “dependent” variable beforehand gets people off on the wrong foot entirely—it is a sure recipe for misery, and approaching our kind of social research as if it were an orderly and linear process will make you crazy.²⁰

Let me give you a more concrete sense of what I mean about all these processes reinforcing one another and making research much more challenging than it used to be. When I was coming of age as a social scientist in the early 1970s, I lived in a world in which information was scarce. Back in this world without the Internet and cell phones, where computers were as big as garages and you had to make appointments to use them, information, knowing something about something, was both scarce and costly, and the boundaries of what it meant to “know something” about “something,” although tacit, were pretty clear to all concerned. The “fields” in all of the social sciences were pretty well defined, and the few—but very few—interdisciplinary questions that then existed were mostly seen as crossing the boundaries of perhaps two or at most three fields. The path to success for an up-and-coming young scholar was to master an arcane but well-bounded area of human knowledge such that she or he knew more about it than anyone else.

Just how did one “master” an arcane but well-bounded area of human knowledge? One went to “the literature,” of course. And the literature itself was surrounded by a set of taken-for-granted “practices” (my colleague Robert Berring calls them “filters”) which, although invisible to most scholars, made tacitly clear what “literature” had to be taken seriously, and what could easily be ignored.²¹ For example, an article in a peer-reviewed, top-ranked journal in my own or any closely adjacent discipline had to be taken seriously, while articles in other disciplines, unpublished reports, or journalistic accounts did not. I’ll be talking a lot more about the rise of the Internet and its meta-effects on how we think about research, but for the moment just ponder the thought that there was once a day when sociologists

in good standing could happily ignore developments in the humanities, not to mention cultural studies, and only specialists kept up with what was happening in history.

Books that were clearly advocacy, not scholarship, were easily recognized as such because they were written by people not holding Ph.D.'s, and they were not published by peer-reviewed university presses. Taken together, these well-bounded areas of inquiry and "filtered" information meant that a young scholar could fairly easily map out the relevant literature and, with some luck, figure out where to make his or her own contribution.

I'll be talking a lot more about this, particularly in Chapter 5 where I take on in a deep way the question of what it means to do a "review of the literature" these days. But right now I want to give you a multi-layered example of the larger theoretical point I'm making, drawn from my own life.

I was in my early twenties, taking a graduate course in demography at Yale, when I realized one day that a woman's entire future could rest on the head of one microscopic sperm. As a nascent feminist—the year was 1968—I became intrigued by the question of pregnancy and how it was regulated, and I eventually settled on abortion as my area of inquiry. Within a fairly short period of time, I knew more about abortion than almost anyone except four or five other people on the planet, and I could tell you *exactly* how and why they knew more than I did.²² And this was not because I was a particularly brilliant sociologist, but because what it meant to "know" about abortion was clear, and "the literature" came to me already filtered. (Plus, as you might imagine, I was obsessed. And young.)

In concrete terms—because abortion itself was not a very big deal in 1968 (it only became legal nationally in 1973, which started all the fuss), and because it never occurred to me to think much about abortion in an international perspective—"the literature" itself was pretty limited. Moreover, because the literature was filtered, I could reasonably discern early on what was and was not relevant to me. If

something about abortion was published in *Demography*, for example, I knew I had to read it. But if it was published in *The Catholic Physicians' Journal*, or the *Wall Street Journal*, I probably didn't.

Because information was limited, deep command of it was valued over wide command. (This is one of those "practices" I was talking about.) The most telling example I know is Betty Fussell's account of her husband's oral exams in the English Department at Harvard, which he almost flunked because he did not know where William Wordsworth had gone to school as a child. Or Samuel Coleridge. Or Matthew Arnold. Mastery of trivia was the mark of a truly learned person, because if you knew the small stuff you could be trusted to know the big stuff.²³

Things have changed. In the olden days, both the way of thinking about research and the amount of information available mutually reinforced each other, since there wasn't that much information, and prowess was demonstrated by mastering more of it within a narrow set of parameters than anyone else. There still are fields like that today, and even within the social sciences there are still subfields like that. But for most of us reading this book, knowledge comes not from mastering esoteric facts or techniques, but in making connections *across* traditional boundaries—going wide rather than deep. It means mixing an insight from economics with one from history. Or one from demography with one from stratification theory. There are of course pitfalls in doing this, and we will be talking about them, but whether or not this is the best way to do modern research, it is the way, for better or worse, that most of my students think about scholarship these days.²⁴

In what I consider convincing evidence for my point, Richard Peterson and Roger Kern argued in 1996 that when it came to "distinction" in the larger social world, the marks of high status were in the process of shifting from, as they put it, "snobbish exclusion to omnivorous appropriation."²⁵ Although they were talking about the consumption of the fine arts, I think their argument still holds for the

way that people are beginning to think about scholarly research as well.

What Peterson and Kern found was that, whereas once one showed what a cultivated person one was by mastering one of the high arts in depth (“Ah, yes, Mozart in his later years, I think”), such displays now have to do with mastery of genres *across* boundaries (“Ah, yes, the Carter family. Reminds me of late Mozart”).²⁶

Think about it for a minute. The essential point that Bourdieu and his followers make is that the possession of “cultural capital” is what marks the boundary between the in-crowd and the losers (to use high school terminology).²⁷ But it is essential for “social closure” (that is, the capacity to exclude some while including others) that not everyone have access to the same kind of cultural capital.²⁸

The really useful thing about cultural capital is that it *appears* so democratic and meritocratic, so nominally open to all comers at the same time. Discrimination and sorting happen all the time, of course, but not in an obvious way.²⁹

Back to social research. As Peterson and Kern surmise, the spread of mass higher education, and the popularity of programs in the media that “explain” the high arts and how to appreciate them (think of Sister Wendy or Public Broadcasting), have served to deflate the cultural capital once associated with mastery of the appreciation of opera, modern art, and symphonies. So now a “cultured” person is one who can make connections across traditional boundaries. The pay-off here is that cultural capital has once again become scarce because *you have to make the kinds of connections across boundaries that key cultural arbiters will find convincing, “exciting” signs of mastery.*³⁰ In short, you have to use a mix of outer-directed and inner-directed insights to psych out your “intellectual field.”³¹ The proliferation of canned statistical programs that can be run on personal computers has, I suspect, deflated the status once associated with statistical mastery. Add to that the fact that the audience for studies of society has changed enormously over the past few decades; wit-

ness the enormous popularity of Malcolm Gladwell's books.³² The same spread of mass higher education that deflated the fine arts as markers of "distinction" has created an audience of people who want to know about social life, but who are impatient with the jargon and technology that sociologists have historically used in the service of their professionalization project.³³

Something else is up, too, I think. When I was coming up in my field, the only producers of "legitimate" findings or theory about the social world were academics with doctorates who were properly ensconced in prestigious universities. Today this group is, I suspect, at best a minority of those producing ideas about the social world, and probably a tiny minority at that. Non-governmental organizations (NGOs), think tanks connected to ideological positions, the federal and state governments, and contract researchers such as the RAND Corporation and Mathematica all produce both theory and findings. Often enough, these pieces of research (the "products," if you will) are produced faster in places like the Heritage Foundation and are often much spiffier and more inviting to read than the kinds of things that academics typically do.³⁴ Moreover, to tie into a theme mentioned earlier, a whole lot of truly nutty stuff can be found on the Web purveyed as "research." It seems as if every advocacy organization in America has a "research director" whose job is to invoke the mantle of "science" to claim that its position is the only possible one to take. If it's true, as the famous Peter Steiner *New Yorker* cartoon says, that "On the Internet, nobody knows you're a dog," it's also true that information on the Web lacks those "filters" mentioned by Robert Berring that once signaled the boundary between fruitcake research and reliable scholarship.

I've thought a lot about teaching smart people how to do research in this new world, and new ways of thinking about the social sciences; being a sociologist, I think about it sociologically. These factors—the role of Foucauldianism, the rise of info-glut and the decline of linearity—seriously undermine the historical traditions

on which research skills are taught. Understanding the changing social location of research—and researchers—gives you permission to break free from the traditional and often limiting rules about doing social research.

So what kind of person do I think you are, and what kind of research do I have in mind? Well, I presume that you are a graduate student in one of the social sciences, or perhaps a younger faculty member. You could be a precocious undergraduate, of course, and you might even be a more senior scholar who feels a bit estranged from traditional methods, be they qualitative or quantitative.

In terms of your heart and soul, you are likely someone who has come to social sciences for answers about the social world that surrounds you, and the astonishing changes you have seen in your own lifetime. You're curious about why organizations such as the World Bank are urging countries to turn communal water supplies into privatized ones. You want to know whether kids learn better in "all girl" and "all boy" schools. Or maybe you want to know why people do risky things like expose themselves to HIV/AIDS or fail to plan for retirement, or why there is a persistent "achievement gap" between whites and African-Americans. You'd like to know where the sudden opposition to affirmative action came from in the late 1990s, after it had seemed a staple of American life for almost three decades, and you want to know if affirmative action "works," however we might define that term.

In short, you came to one of the social sciences considered in this book in order to find answers. One of the most painful but ultimately liberating moments in my graduate classes is when I tell bright, hardworking students, often battle-scarred veterans of the social justice wars, that, alas, I don't actually *have* the answers that they are so desperately looking for, and furthermore neither does anyone else in the social sciences that I know of.

That's the bad news. But here's the good news: I can't imagine a better place than a graduate department in the social sciences to ex-

plore and ultimately *create* those answers for yourself. In all of the social sciences, there are some well-replicated findings, a set of tools, some terrific theoretical models, and some bracing intellectual disciplines (some of which will be explored in this book) to help you find out what you need to know. But to get there from here, you are going to have to ignore much of the conventional wisdom that some faculty in these departments (as well as fellow graduate students and even the general atmosphere of graduate school) will tell you about yourself, about the discipline, and about “the literature.” Most important of all, you will probably have to ignore most of what they tell you about how to do research.

Judging by my students, past and present, you are attracted to phenomena that grip you intellectually, but are hard to describe when you try to tell other people what it is about them that you find so fascinating. Many times you can’t even explain to your best friend in graduate school what is appealing about the thing you want to study, much less your adviser.

For reasons of simplicity, I’m going to call what you are interested in “public sociology”—or public anthropology, or public political science, or public city planning—you get the idea. By “public” (fill in the blank), I mean a theoretically informed, rigorous social science that permits itself to explore the big questions that beset society and preoccupy ordinary people, not just those questions that are thought to be the straight line to prestige within a small coterie of social science scholars.³⁵

Now here’s where we start in on the ignoring part. Every good story has to have a villain, and the one in this story is something that I will call “canonical social science.” In real life, I respect and admire the canonical social sciences, and think of myself as a somewhat black-sheep member of its extended family. I wouldn’t enjoy research as much as I do today had I not been trained in canonical sociology, with a touch of demography thrown in for good measure. For the purposes of argument, though, I’m going to make “canoni-

cal sociology” a straw person, because it often still embodies the traditional way of doing research, reflecting a world that no longer exists. But take heart: I intend to teach you how to profit from the many very real riches of canonical sociology—and canonical political science and history and anthropology and all the rest of the list—for your own purposes. I’ll teach you how to take what you want and leave the rest, and I’ll show you what you gain (and risk losing) by doing so.

Let’s go back to that case you were interested in—the one about how the World Bank is privatizing water in Bolivia, or whether single-sex schools are better, or where the opposition to affirmative action came from in the 1990s. If your adviser is a member in good standing of my generation of canonically trained sociologists (or political scientists, etc.), he or she will ask you what your hypothesis is. If you flounder around trying to answer this question, he or she may follow up by asking what your independent and dependent variables are. Even more basically, he or she will ask what your research question is. You just go blank, feeling like a rabbit trapped on the roadway with the headlights bearing down on you, as you try desperately to explain what’s so interesting about, say, privatized water, or rising rates of imprisonment in America, or adolescent sexuality.³⁶ When you and your adviser part at the end of the time allotted to you, more likely than not, you part in mutual frustration.

I’ll be talking a lot about this in the pages to come, but the key here is that your adviser, Mr. or Ms. Canonical Social Scientist (CSS, for short), probably wants you to engage in an act of *prediction*, whereas you want to engage in an act of *discovery*. And the saddest part is that neither of you realize that you’re sliding past each other, each doing a different dance. CSS is trying to teach you how to do a classic Viennese waltz, while you want to salsa-dance, although you didn’t know that’s what you wanted to do before you picked up this book.

But suppose on the other hand that you have an easygoing ad-

viser, and you are permitted to go off into “the field” (even if that “field” is only the library) without answering his or her questions. An even more dreaded fate may well await you, worse than being tortured into producing independent and dependent variables on demand for your adviser, namely what I think of as the Damnation of the Ten Thousand Index Cards or the Ten Thousand Entries into your computer-assisted note-taking system. The Damnation of the Ten Thousand Whatever happens to unwitting graduate students who have spent many years (at least three, and sometimes more than ten) gathering data without having stumbled upon exactly what it was that they were looking for when they first went out to that fabulous field site (or juicy library question). There they sit, doomed and damned, in front of the computer screen, wondering how to make a story out of the ten thousand entries. Or, worse yet, they finally do stumble onto a story as they pore yet again over the ten thousand entries, but the single piece of information (or the body of data) which they need to really nail the point beyond quibbles is back in the field and they didn’t know they needed it, or it’s disappeared, or they can’t afford to go back. Or they do find it, and realize that eighty percent of the data they have gathered is irrelevant. As the great feminist sociologist Pauline Bart used to say, “Data, data everywhere and not a thought to think.” An in-between outcome, one that I myself suffer from a lot of the time, is that you may actually find the research question, come up with the data that you need to make the case, and have a compelling and I hope well-written story to tell. The only problem is that you have eighteen boxes of data left over, and the entire enterprise took you at least four years longer than it should have. Not bad, compared to the other outcomes, but not good either.

All is not lost. I think I can promise you that if you read this book carefully and practice the disciplines it suggests in a mindful fashion, you won’t have to contort your interesting, juicy case study into a format of independent and dependent variables that don’t quite fit. Nor will you have to spend three to ten years of your life gather-

ing data, only to become a victim of the Damnation of the Ten Thousand Index Cards. This book will show you how to think (and dance) like a social scientist in the new millennium, and why the dilemmas you confront are deeply sociological ones and not really personal.

Better yet, I promise that the sociological tools we will discuss along the way, adapted to the kind of person you are and the kind of research you want to do, will liberate you.

The book is arranged in roughly the same sections that I cover in the courses that I teach on methodology, and they in turn track more or less sequentially the steps you need to complete to get a research project launched. But as with a lot of things in life, there are many different ways to configure a research project, and you'll find that we will revisit a few key ideas and principles over and over again. (Remember my discussion a few pages back of non-linearity? Here it is in practice, so to speak.) I hope that this will turn out to be something like learning a yoga pose, in that you feel stiff and creaky when you do it for the first time, but as we keep building on the basics, you will find out more and more about the pose and about yourself in the process.

Exercise for Chapter 1: Introductory Exercise

Throughout the book I'll be giving you exercises to do. I *strongly* recommend that you do them and *write them down*. I know what you are thinking right now: whenever someone in a book tells me to do an exercise in writing, I promptly go about ignoring him or her. I always figure that reading the exercise and doing it in my head amounts to pretty much the same thing.

For what it's worth, I've learned through trial and error that this is a very serious mistake. Something magical happens when you write things down. (When we get to the section on field notes, I'm going to counsel you to *write it down* before you even talk to anyone else,

even your loved ones.) My own theory is that writing engages a very different part of the brain than reading and talking do, and that writing is the door that opens out to the magic. Someone once asked Balzac, who supported himself by writing reviews of plays, how he liked a play he had just seen. “How should I know?” he is reported to have answered. “I haven’t written the review yet!” Balzac was onto something: I find that when I write things down, I write and think things that I’ve never really thought before. Novelists sometimes say that their characters do things that surprise their authors, and I guess this is the sociological version of that phenomenon.

Enough theory. Set a kitchen timer for fifteen minutes, and write about what question concerning the social world you would like to investigate if you were absolutely guaranteed you would not fail. Be as ambitious and wide-ranging in your thinking as you want. Spelling and grammar do not count, and you should feel free to cross things out and rewrite as you go along.

Save this document, because it has important clues in it, clues that you will want to come back to all through the book and, more important, throughout your research project (maybe even your entire research career). I like to keep a research journal (any old three-ring binder will do), and I’d like to suggest that you do the same: this should be your first entry.



What's It All About?

We're going to start this chapter with a pop quiz: Name the only American sociologist to win a Nobel Prize. Stumped? Want a hint? Okay, she was a *woman* sociologist. From Chicago. Still stuck? The answer is Jane Addams, founder of Hull House, who won the Nobel Peace Prize in 1931.¹

But wait a minute, you might object, Jane Addams wasn't even a sociologist. Well, yes and no, and that's the entry point to talking about what we're doing here and why it's important to know about Addams and sociology if we want to think like social scientists in the new millennium. (This story comes from sociology, but I could tell parallel stories for other social science disciplines.)

Here are the facts: Addams, better known these days as a social reformer, thought she was a sociologist. She taught at the University of Chicago at a time when the department of sociology there was creating the discipline of sociology in the United States.² Albion Small, the department's first chair, offered Addams an affiliation with the department, and later offered her an official half-time appointment.³ A charter member of the American Sociological Society, the forerunner until 1952 of the American Sociological Association, Addams re-

mained a member until just before her death in 1935. She published at least five articles in the *American Journal of Sociology*, and saw her own books regularly—and favorably—reviewed there. Luminaries such as George Herbert Mead, Charles H. Cooley, W. I. Thomas, W. E. B. Du Bois, and John Dewey were among her personal friends.⁴

So how is it that we don't think of her when we think of the founding members of American sociology? Why isn't she part of the lineage we learn about when we study Marx, Durkheim, and Weber, not to mention our own homegrown sociologists such as William Graham Sumner, Lester Ward, Herbert Blumer (in addition to Addams's friends whom I've already mentioned)?⁵

There are three reasons, I think, and all three are relevant to the inquiry at the heart of this book. First, as Mary Jo Deegan has shown, Addams was a woman, and as sociology came to think of itself as tough-minded and scientific, women seemed too, well, *feminine* for the tough-minded science that was central to the professionalization project early sociologists had in mind.⁶

Second, Addams was a social reformer, bent on making America a better place to be. The whole idea of Hull House was to bring together young people of privilege to live with and understand the urban population, many of whom were immigrants.⁷ Social reform, with the passion and commitment that this implies, was at odds with the notions of scientific objectivity that were to triumph as sociology came of age in the academy, and it was particularly problematic in the period of intellectual and political repression that followed the First World War.⁸

Finally—and most unpredictably, and most central to the issues of this book—(male) sociologists of the time thought that Addams's work was methodologically suspect, because they found it—are you ready?—too *quantitative*. Addams's enterprise was heavily influenced by the settlement house movement in England, which in turn was greatly influenced by the emerging Victorian obsession

with counting and measuring.⁹ Under the rubric of “social arithmetic,” Addams and her colleagues used surveys and other quantitative methods to do something close to what we moderns might call a “needs assessment” of the community in which she was located, although that doesn’t begin to do justice to the scope and vision of *Hull-House Maps and Papers*.¹⁰ The men in the sociology department at Chicago, however, found social arithmetic (that is, quantitative methods) much too rigid, not subtle enough, and too “feminine” to do the real sociology they had in mind, and turned instead to life documents, participant observation, and *verstehen* (“sympathetic understanding”), a methodology that reached its apogee in the monumental, much-cited (though not often read these days) *Polish Peasant* of W. I. Thomas and Florian Znaniecki.¹¹

If you wanted to think about this sociologically, you might argue that this disagreement was shaped by the different material and ideal interests (in Max Weber’s terms) of the men and women undertaking social research in and around the University of Chicago in the early years of the twentieth century.¹² In terms of material interests, many of the men, having had advanced training in philosophy, often in Germany, possessed the kinds of cultural capital likely to be enhanced and valued should *verstehen* become the dominant way to view social reality. Or as Albion Small put it, “It should be said that sociology is a pursuit which should be undertaken only by those whose philosophical talents and training are of the first order.”¹³

The different ideal interests of the men and women, moreover, tended to reinforce their material interests. That same education in philosophy that had given male academics forms of cultural capital amenable to *verstehen* had at the same time sensitized them to debates about epistemology, and in particular the much-discussed “objectivity” then being actively debated by the leading nineteenth-century philosophers with whom they had studied.¹⁴ As a consequence, they were more likely to be concerned about what they saw as the intellectual aridity of what seemed to them to be mere enumeration.

But today, if you have taken a methodology course as an undergraduate or a graduate student in an American sociology department (or most of the other social sciences), it's likely you did not get a great deal of training in the study of life documents and participant observation, the kinds of things these early professional (male) sociologists took to be key to social inquiry. (Anthropologists are the only social scientists I know of who are expected to engage in participant observation, and historians sometimes study life documents, but my highly unscientific survey of top departments in these two disciplines suggests that "methodology" is something that students are expected to pick up along the way, rather than something they study formally.)¹⁵

In fact, if your training was anything like mine, you may well have picked up quite the opposite message—that qualitative techniques are unscientific, soft, and (although people rarely say this out loud) *feminine*. *Verstehen*, the sympathetic understanding of Weber, now has connotations of tea and sympathy—a methodology not quite masculine enough for a "real" science.

"Rigorous" training in the social sciences is now defined as quantitative, and the more abstractly mathematical, the better. A typical course in methods briefly covers how to execute a piece of survey research, the brevity premised on the notion that most people will either "outsource" any surveys that they do or will take *lots* more courses on survey research. Then the bulk of the course, a course designed to teach everyday social scientists how to do research, is spent on learning how to do the statistical manipulation of the data thus gathered, with a particular emphasis on linear models. (I exaggerate to prove my point, but not by much.)

The "Swiss Army knife" used so much in social research these days is linear regression, which, along with its relatives, forms the basis of a set of techniques that seem to be at the core of most "methods" courses. Three interwoven ideas anchor traditional courses on method: (1) the best and most reliable data are quantitative data ran-

domly drawn from a universe of all possible elements; (2) nothing is meaningful unless it is statistically significant; and (3) losing your mind trying to make SPSS, STATA, or SAS (three of the popular statistical manipulation software packages) do what you want them to do will make you understand the entire enterprise better.

I have my doubts about all of these statements, and you'll hear about them soon enough, but let's turn back to the key point I was pursuing—when did numbers change gender in sociology? That is, when did numbers go from being unsophisticated, something associated with reform, and something that women did (and were presumably destined by nature to do) to something hard, rigorous, and upstanding?

Some scholars think that this began to happen as early as the 1930s, but my own reading of the history of sociology is that the big shift happened during and after World War II.¹⁶ Already in the decades before the war, a new methodology, namely survey research, had been developed which permitted scholars to gather some key data about a reasonably large number of people. (Reasonably large these days means somewhere between one and two thousand, which doesn't seem like a lot until you think of doing one or two thousand sessions of participant observation or that same number of interviews.)¹⁷

Moreover, the eventual development and adoption of random sampling by survey researchers, formally defined as choosing elements from a known universe in such a way that every element has a statistically equal chance of being selected, meant that under certain conditions these reasonably large (by qualitative standards) groups of people could be used to stand in for even larger groups. Nowadays, something on the order of a mere fifteen hundred to two thousand people can be interviewed and their answers used to predict a wide range of social outcomes, from the likely winner of an election in which millions of people will vote, to the risky behaviors of all American teenagers, to educational and occupational outcomes of entire generations of Americans.¹⁸

It was the coming of World War II, however, which turned survey technology into *the* dominant technology of the social sciences. War is the mother of states, as Bismarck famously said, and the Second World War saw the vast expansion of the federal, and to some extent the state, governments in America.¹⁹

Hand in hand with the expansion of what people like Stephen Skowronek call “administrative capacity,” that is, the institutional willingness and capacity to get things done, comes the development of what Foucault calls governmentality.²⁰ By this term Foucault means a way of looking at and experiencing the world such that it becomes ordered in terms of the priorities of government. (A telling example comes from James Scott’s *Seeing like a State* where he describes the newly minted occupation of “forester,” people who tramp through the German forests in order to measure not their size, nor their majesty, nor the number and kinds of wildlife in them, but their productive capacity in terms of linear feet of lumber.)²¹

Meanwhile, back in the United States it was the government itself that had laid the technological groundwork for numbers to become something new in American life. In 1890 the U.S. Census employed for the first time a technology known as the Hollerith card, a punched card modeled after the paper cards first used to direct weaving on mechanical looms.²² The Hollerith card, which still existed well into the 1980s under the name of the “IBM card,” permitted numbers to be manipulated and set the stage for statistics in the modern sense of that word.

Like anything else in social life, defining the exact starting point of the role of what I have been calling canonical social science in “governmentality” is a matter of opinion. The Great Depression in the United States (which effectively lasted from “Black Friday” of 1929 until the start of World War II) created incentives for the government to measure and assess both the damage wrought by the Depression and, more important, public response to it. To push the “governmentality” theme just a bit, it is clearly in the interest of elected leaders to be apprised of and familiar with the kinds of politi-

cal and social upheavals that will change the way voters look at the world, and the Depression was exactly that kind of phenomenon. The rise of the attitude surveys in the 1930s created a technology with which to do this.

President Hoover, for example, gave social scientists a significant boost by officially supporting the research that made its way into the White House Committee on Recent Social Trends, published in 1933 and directed by William Ogburn. Ogburn was president of the American Sociological Association, and the man who finally brought quantification to the University of Chicago's department of sociology. Similarly, the Roosevelt administration supported modest surveys within the Works Progress Agency and the Federal Emergency Relief Agency, both expansions of the federal government meant to address the enormous social, political, and economic impacts of the Depression.²³ For historical reasons, having to do with the very large presence of farmers as a political constituency, many of these early forays into sociology were conducted under the aegis of the Department of Agriculture.²⁴ Jean Converse, the foremost chronicler of the history of survey research, calls these early surveys "protosurveys" and notes that they had more in common with the kinds of "surveys" undertaken by Jane Addams than with later public opinion surveys.²⁵

What would later become survey research as we know it gained new respect in the aftermath of the Japanese attack on Pearl Harbor. Interviewers in the Department of Agriculture from Reidsville, North Carolina, Okmulgee, Oklahoma, and Norfolk, Virginia were sent to the West Coast to tap public sentiment. One week later, on December 14, 1941, they handed in a report entitled "Immediate Developments After Pearl Harbor." In so doing, they made the usefulness of this new form of information gathering immediately apparent.²⁶ (Just to get a feel for the incredible impact of such a report, imagine if social scientists had been able to turn in something similar one week after the attack on the World Trade Towers on September 11, 2001.)

Evidence for the dramatic increase in the status of both sociology and sociologists in the immediate aftermath of World War II (and for my contention of the rise of “governmentality” in this period) is provided by the fact that the membership of the American Sociological Society increased two and a half times in just the nine years between 1940 and 1949.²⁷

The Second World War was followed by the Cold War, and then in turn by the Great Society and the War on Poverty. Each of these enterprises implied a vast expansion of the government’s reach, and hence its need to measure, assess, and respond to an increasingly complex society—which is another way of saying that global conflict with another superpower abroad and expansion of the welfare state at home together dramatically increased the state’s drive toward governmentality.

As a small sample of the kinds of tasks undertaken by the government in the 1960s, take two examples of federally funded research from that period. On the one hand, the federal government planned an ambitious interdisciplinary project to investigate how peasants in Third World countries could be induced to resist the appeals of revolutionary social movements; on the other, a million-dollar experiment here at home compared how a group of people given an annual guaranteed income fared vis-à-vis a similar “control” group left to fend for itself in the marketplace.²⁸

When questions are of this scale, and when the stakes are so high and the methodologies associated with them are so expensive and relatively arcane, they tend to become the province of the “expert,” and more often than not, especially in the 1960s, the expert was male.

Let me make the sociological point more explicit, as an example of what I promised you in the last chapter.²⁹ A new technology (survey research) emerges, and is useful to powerful stakeholders (the government, and later some large philanthropic foundations.)³⁰ This technology takes a great deal of money to deploy, and in order to justify its existence, develops a set of skills and techniques associ-

ated with it which are not easily accessible to the layperson.³¹ The individuals in command of these skills acquire the capacity to “gatekeep,” that is, to say who may and may not be admitted to the club of those entitled to use the new technology. (This, in a nutshell, is what sociologists call a “professionalization project.”)³²

If I may bring up a question that may well have occurred to you already: how is any of this relevant, if at all, to you? It’s fascinating history, to be sure, and it’s nice to know about Jane Addams, but what are she and the rest of this history doing here in a book about research methods? To make a long story short, it’s because research methods—what this book is about—*have* a history. And a politics, and a set of relationships to power and money. Whether you know it or not, the way you think about research, and about research methods in particular, is intimately shaped by the way numbers got gendered, and the taken-for-granted assumptions that surround all methods, not just numbers. We forget our history at our peril.

Don’t get me wrong—it’s not *just* about power; nothing about this particular historical outcome was, as sociologists say, determined. The actual technologies of quantification are, I believe, a significant step forward in understanding social life in the right circumstances, and I want to say right here and now that you are missing an important bet as a social scientist if you decide to ignore the results of it entirely. Quantification is, as Sam Johnson said about hanging, a great force for concentrating the mind. As you may have guessed, however, at this particular moment in history in this particular society, quantification has become something of a cross between an honor society and an old boys’ club. It is meritocratic in that people of modest social backgrounds can compete to enter it, and if they measure up, they are granted full and honored membership in the club. But others, perhaps equally talented, were shunted away from membership in that club with a series of small shoves, and over time this deters people from having the qualifications to perform well in what is, on the surface at least, a meritocracy.³³ So membership in the

club is earned in a meritorious competition, but successful members need to remember that others were turned away long before the competition began, and both the “rigor” and “scientific” nature of numbers and those who produce them are socially situated.

As a result, whether we know it or not, we are guided by our taken-for-granted assumptions about what constitutes “good,” “rigorous” methods whenever we undertake to do research. How could we not be? The studying of the social order is itself a social process, so how could the process of doing it not be surrounded by assumptions, fetishes, beliefs, and values that are not simply mirror reflections of objective reality, if there is such a thing? It is a problem that we will be confronting over and over again in this book—we are fish studying water, and our very fishiness shapes how we think about it. Not only are our assumptions about the social world themselves socially influenced, but so are our assumptions about the best way to go about investigating the social world. It’s enough to make you want to lie down on a couch and put a cool cloth on your forehead.

For reasons that should be clear from the previous discussion, when I learned “methods” there was the taken-for-granted assumption that quantitative methods were inherently better than qualitative methods.³⁴ Because the former have been associated with money and power since World War II, they naturally took on the prestige that generally comes with money and power. And look what our friends the economists have done with these kinds of methods!

So qualitative researchers have tended to slink off into the margins, mostly interested in studying the unusual and the deviant (or, as they are sometimes unkindly known, the “nuts and sluts”). Or, if they have bolder and more theoretical aims, they may search down on the micro plane, trying to do “theory generating” using firsthand observations.

Since I’ve been so snide up to this point about canonical (that is, quantitative) sociology, it’s only fair that I critique qualitative methods as well so you will believe that I really am looking for the sweet

spot between the two. My main complaint about qualitative methods is that they so often lack a method. More times than I can count, qualitative sociologists encounter a rich field site, settle in for the duration, undertake participant observation and/or interviews, and see as their task the writing of a fluid, graceful, and compelling narrative. Or as my colleague Loïc Wacquant once complained, “People think that all they need to do ethnography is to tell a good story!”³⁵

Now there certainly are exceptions to this generalization, but I’m not entirely happy with the main ones, because even though they have been extremely important to me and quite formative (you will see their influence over and over again in the pages of this book), they don’t really speak to the kind of research that I—and, I suspect, you—want to do.

First and most central is the Glaser and Strauss “grounded theory” method of research, in which, using the “constant comparative” method, you systematically let categories of social life “emerge” from your investigations and begin to fill in what those categories are. My intellectual life was in fact saved by Anselm Strauss, so I’d like to think that what I’m doing here is building on his accomplishments, rather than just carping.

Specifically, I was a graduate student at Yale, properly trained in canonical sociology (haphazardly trained, but that was my fault, not the department’s). In fact, I was a budding young demographer, in love with epidemiology and biostatistics. I actually thought in my heart of hearts that God must be a mathematician, since so many things in social life can be described with amazing accuracy using numbers. I’ll never forget the beauty of seeing a damped sinusoidal curve marking the decline of infant mortality in inner-city Chicago.³⁶

But I wanted to figure out something that did not respond easily to survey research, namely why so many women in California were having abortions when contraception was so widespread and easily

available. (This was in the late 1960s, before *Roe v. Wade*, but after California had substantially liberalized its abortion laws.) There was no way that I could design a survey, hard as I might try, that would answer this question for me.³⁷

I stumbled across Glaser and Strauss's grounded theory book when I was grappling with this question in San Francisco one summer, and I promptly took myself over to Anselm Strauss's office at UC San Francisco. He showed me enormous kindness and gave me what I'm hoping to give you—the permission to follow my own instincts, as well as a set of impromptu tutorials about how to do “grounded theory.”

I still respect and admire the method, but over the years I've come to see some of its shortcomings. The major one is that, as traditionally practiced, it's so relentlessly *micro*. Because of Strauss's own training at Chicago (you notice a certain connection here?) he was steeped in the idea that you went to the field in order to discover the categories of the social world as the participants themselves experienced them.³⁸ As sociologists working in a medical center, much of Glaser and Strauss's most important work was about the social experience of illness and health, death and dying.

These are such essential experiences in the human condition that Barney Glaser and Anselm Strauss and their colleagues and students could engage in what we might think of as basic research in the most fundamental sense of the word: they wanted to know how we die, or experience pain, or how parents of disabled children come to terms with their children's limitations, or how the chronically ill experience their lives.³⁹

As both a feminist and a veteran of the 1960s at Berkeley, however, I was keenly aware that these intensely personal experiences are also embedded in a certain social and political framework, a framework largely outside the lens of traditional grounded theory. What effect did health insurance have on how people died? How did gender affect death? What did managed care do to the handling of

pain? (In all fairness, the bulk of Glaser and Strauss's and their students' work that I'm citing took place before these massive changes in the nature of the health care system, but I still think the criticism holds.) Power in the original formulation of grounded theory is localized, and the analyst is not encouraged to see the daily micro-interactions going on before his or her eyes as reflective of anything larger.⁴⁰ This is not to say that grounded theory cannot in principle be used to connect the personal and the political, the micro and the macro, but some modifications are in order, adaptations that we will explore later on.

I know that some poststructuralists reading this will argue that I have been unfair—that the task of the social sciences (and really, the only thing the social sciences can do) is to investigate how people make meaning out of their surroundings. True, people do make meaning out of their surroundings, but, to paraphrase Karl Marx, not as they please. In other words, they are constrained by (and in some cases facilitated by) the social, and all too often the social is invisible to them. That to me is the task of the social scientist—not just *reporting* on how people make sense of the world around them, but *investigating* it as well. In other words, we spend all this time learning how to be good social scientists in order to see those things that people in everyday life cannot.

If Strauss and his colleagues are too relentlessly micro, the opposite problem afflicts the other model of scholarship that has been most important to me, that of Michael Burawoy and his Extended Case Method. Burawoy has had an enormous influence on me (and others), so you'll understand when I say that just as in the case of Anselm Strauss, I see myself as extending his ideas rather than just criticizing them.

For Burawoy, a prolific and energetic researcher, all qualitative research—and ethnography in particular—is deeply linked to the macro. He's not particularly interested in the intimate social psy-

chology of human experience, but rather in how those experiences illuminate a larger social order. In his *Manufacturing Consent*, for example, he takes on the question of why workers not only comply with production demands, but press themselves harder than they might otherwise do. His participant observation shows that workers “game” the system of production in order to make life on the assembly line more interesting and less deadening, and in doing so they inadvertently take part in the extraction of their own surplus value.⁴¹

His method is a nice counterpart to the Glaser and Strauss method because it is so theoretical. Observations are always keyed to theory-building, but not in the way that Glaser and Strauss have in mind. Rather, Burawoy starts out with a grand theory, namely Marxism, analytic Marxism in particular, and he draws from it both questions and propositions that he tests by observation. He sees the scholar’s task as using hands-on empirical observation to explore and extend the reach and vigor of theory, analytic Marxism in this case.

But what if you are not by temperament an analytic Marxist? What if you, in some secret little corner of your being, reject grand theories in principle? What if you want to explain something that either doesn’t fit into the analytic Marxist framework, or has a strong element of “culture” in it, or both?

Even more problematically, many of my students want to explain things that often deal with “squishier” aspects of the social world than Marxism traditionally looks at, such as identity, sexuality, mobilization, how humans develop a sense of efficacy, and the like, and the Extended Case Model often is an uncomfortable fit. As two of Burawoy’s students, Nina Eliasoph and Paul Lichterman, point out, the Extended Case Method “sits uncomfortably with cultural investigations; it squirms, reluctantly acknowledging that such research is possible.”⁴²

To be fair, Michael Burawoy both in person and in print is extremely ecumenical in terms of what theory you should choose,

and entirely open to the study of culture. His article on the Extended Case Method in fact suggests, “We start with our favorite theory . . . ,” which at a minimum suggests a pretty wide-open range of theories; and as all of his students will testify, he supports their choices of whatever theory works for them, while also setting no boundaries on *what* they will study.

But because Burawoy has such a powerful theory at his disposal, his being ecumenical about what theory to use doesn’t translate into telling you how to find one that works for you, and one where you can do what I keep urging you to do, namely build more theory.

Accordingly, in the course of this book, I will combine insights that are deeply indebted to grounded theory with the insights that Burawoy provides us in his several discussions of the Extended Case Method, and that’s the model I hope to share with you.

Let’s review where we are. I’ve criticized quantitative sociology for being (whether it knows it or not) profoundly shaped by its historical relationship to governmentality. Perhaps the kindest way to think of this is to think of canonical (quantitative) sociology as engaged in a kind of commensalism with governmentality—“commensalism” being a term that describes a situation where two beings live together to the mutual benefit of both. (Think of those birds that pick parasites off the wildebeests on the African plain.)

I hope I’m clear that I don’t think that quantitative methods are necessarily complicit in anything our government does. Rather, commensalism sets limits on how far either party can stray from the relationship. In terms of quantitative methods, this means that the expense of the technology involved will limit research either to the kinds of data that the government in its many forms is willing to fund, or at the least to something a large philanthropic organization such as the Ford Foundation or the Rockefeller Foundation will pay money to do.⁴³

Qualitative methods, in contrast, don’t run into the govern-

mentality problem as often, but that's because of their historical *weaknesses*, not their strengths.⁴⁴ Qualitative methods tend to emphasize good writing and the importance of telling a resonant story, and in their best incarnation they build "middle range theories," but often enough they stop short of using that good writing and fascinating story to build any kind of a cumulative body of theory. Moreover, as noted, they tend to bracket power.

But it seems to me that it is still the exception rather than the rule for a qualitative social scientist (apart, that is, from Burawoy and his students) to see his or her task as primarily one of building theory, except in some kinds of micro ways. More often than not, researchers do what I confess I did, namely undertake a qualitative study in order to answer a question ("Why is there so much abortion in a state like California with lots of available and low-cost contraception?") rather than to build theory, in part because, as we are the first to tell you, we are engaged in an enterprise of discovery rather than verification.⁴⁵

The point of all this discussion is to convince you that the history of both qualitative and quantitative methods, at least as practiced in this country, leaves the aspiring researcher like yourself (and myself) somewhere between the devil and the deep blue sea.

If you are trained as a quantitative, canonical, logic-of-verification social scientist, this means that the kinds of questions you can ask are limited to those that have relatively clear answers that can be known ahead of time. What you are looking for is the *distribution* of a population among the already specified answers. For example: "How happy are you with the President's performance? (1) Very Happy? (2) Happy? (3) Neither Happy nor Unhappy? (4) Unhappy? (5) Very Unhappy?"

Reinforcing my point about the need for more emergent, qualitative methods, an astute Republican pollster named Frank Luntz noted that survey methodology doesn't work very well when the cat-

egories themselves are in flux, and any attempt to survey the distribution of sentiment is faulty because people themselves don't know how they feel. As he put it:

If . . . polling is so clear-cut and conclusive, why is there a tremendous discrepancy between polling firms in their reported data on abortion? What do “pro-choice” and “pro-life” mean anyway? . . . polling can't answer that question because voters themselves can't explain in 30 seconds . . .

In today's post-partisan politics, there are too many shades of gray, too many, “yes, but what I really think is . . .” attitudes, too many voter priorities that cannot be prioritized . . . The elements that have made up public opinion have changed, so must its measurement.⁴⁶

Luntz is speaking here of public opinion polling, but as a man who regularly bets his political fortune on his ability to make good predictions about the social world, he is onto something larger, I think. It's not just that Americans' *opinions* are in flux; so is their social world. Part of living in a globalizing world and a globalizing economy is having many of your taken-for-granted assumptions shaken up.

So the job of the qualitative researcher is to figure out not only *how* what Luntz calls “the elements that have made up public opinion” have changed, but *why*.

To put it more formally, in the salsa-dancing model of research that I will pursue during the rest of this book, you will not be trying to assess the distribution of individuals (or groups or institutions) across a known number of categories whose boundaries are clear; rather, you will be trying to discern the very shape and elements of the categories themselves.⁴⁷ But since you won't know the shape and elements of the categories until you are finished, or virtually finished, you are not well placed to build theory in some kind of a

comprehensive or cumulative way. Unless you practice some serious self-discipline, you are at risk of finding yourself as just one more qualitative researcher with a good story to tell. A theoretically-flavored story, to be sure, but still a story instead of a building block of theory.

What I plan to do in this book is to take what I want from *both* traditions in sociology, the macro and the micro, the quantitative and the qualitative, the logic of discovery and the logic of verification. My goal is to have us approach qualitative research in a spirit of expectancy, one informed by the theories we want to contribute to.

So, holding on to this bit of hope, and keeping an open mind about numbers and concepts, let's embark on that exhilarating enterprise known as social research.

Exercise for Chapter 2

As a follow-up to what you wrote as the last entry in your research journal, I'd like you in this section to write down what kinds of questions you might want to spend several precious years of your life investigating. What kinds of things about the world worry and provoke you? What would you like to know that you don't already know? Most important, what kinds of questions do you find interesting enough to get you out of bed in the morning with energy and excitement, at least on most days?

List them all—don't edit and don't censor. Just get them down on paper and see what you come up with. When I say write down questions, by the way, I don't mean things like "What causes inequality?" but somewhat more specific questions, such as why do African-Americans still, on average, have less wealth than whites?



An Ode to Canonical Social Science

For all that this is a book about salsa-dancing social science, I think canonical social science has a lot to recommend it. For better or for worse, canonical social science is the local culture of most social scientists. Your work, and indeed the “methods” you are learning here as an aspiring salsa-dancing social scientist, will be challenging some of the most cherished assumptions of the dominant culture—not just the dominant culture of the larger society in which you live, mind you, but the dominant *professional* culture in which you will work. Not surprisingly, most members of the dominant cultures—both of them—won’t like it. Especially when it comes to your professional peers, I can almost guarantee that many of them will not entertain the idea that you are engaged in a different intellectual enterprise with different epistemological underpinnings. They will simply assume that you are doing what they are doing, only badly. *You must always be prepared to defend your research method at the outset, and you will have to be much more thoughtful and better prepared than any of them will ever be.* So it’s very impor-

tant to know the taken-for-granted assumptions of canonical sociology and to be able to show how your work is something different, not simply a bad version of what they do. In short, you have to learn how to be bilingual.

My goal for you is to help you become a multi-method kind of person. I know, of course, that all of us are intuitively drawn to some methods over others. My observation over the years is that most of the people who are drawn to my method have rejected canonical social science because of the hidden, taken-for-granted culture that has grown up around it. Either the gender, or the “toughness,” or the “hardness,” or the built-in epistemological assumptions turn us off, and we—without ever thinking about it very carefully—simply turn our backs on it.¹

This is a loss. As you’ve probably gathered, there are quite a few things in this world (although not nearly as many as canonical social scientists sometimes think) that respond just beautifully to survey research and/or canonical methods.² You, as a person who wants to know as much about the social world as possible, cannot afford to snoot methods that can be very useful to you in their place. Even if you don’t *do* survey or canonical research yourself, you must be prepared to use it when it helps illuminate the world and the problems you are trying to explain.

Finally, and maybe most importantly, canonical social science has embedded within it certain rules, certain disciplines if you will, that I think lead to good social science, whatever the method ultimately chosen. When I once told a Zen practitioner that I sometimes meditated lying down on the bed, he was shocked. “You have to take responsibility for your spine!” he almost shrieked at me, losing his Zen composure for an instant, so grave was my misbehavior. So Zen folks take their *practices* very seriously: they wear medieval pajamas, sit on black zafu and zabuton (pillows and mats) that have not changed one iota in centuries, and hold their hands in exactly the position the Buddha used before he became enlightened.³ I

don't know if any or all of these practices make enlightenment more likely (I myself am still waiting), but I do know that doing them concentrates the mind and lets you get down more quickly to listening to—and getting some more perspective on—the busyness of your own inventive, self-important mind.

People spend a lot of time learning the methods of canonical social science, but I think that there are only three core practices which are at the heart of all of them—sampling, operationalization, and generalizability—and that everything else is commentary.⁴

First, canonical social scientists worry a great deal about sampling. Building on nineteenth-century discoveries in probability theory, survey researchers insist that observations be drawn from a sample that has been randomly selected from some larger population, itself defined by something called a “sampling frame” (which can be either a list or a set of procedures). Indeed, their commitment to random sampling is so deep that when a canonical social scientist says “sample,” what he or she really means is a *random probability* sample.⁵ As far as they are concerned, if it ain't random, it ain't a sample. We salsa-dancing social scientists won't be drawing random samples because we can't—we don't know the parameters of the larger population from which our sample will be drawn, and are therefore clueless about what kind of sampling frame to use—but that doesn't mean we don't have to worry a lot about sampling. We do.

Second, because survey research is so expensive, they have to make very, very sure that they are asking exactly what they think they are asking. In the language of canonical social science, they have to have operationalized their variables ahead of time, and done so carefully and well. What that means is that whatever concepts will show up in the questionnaire have been defined in such a way that everyone reading (or hearing) them accesses the same mental category when they answer. In other words, survey researchers need to be pretty sure that when they are asking people about something, they and the people being surveyed mean more or less the same

thing. (In this context, keep in mind Frank Luntz's point from the last chapter about how complex and ambiguous people's feelings about abortion are, a point I have spent much of my career making.) In fact, the concern about operationalization is so serious that often survey researchers use time-tested questions to tap their concepts, on the grounds that these "items," as they are called, are "well-validated."

Here's an embarrassing story to prove the point about operationalization. I once gave a sample of abortion clinic patients a questionnaire and, being a "politically correct" sociologist, I listed "native American" (that is, a person of American Indian heritage) as one of the ethnicities that people could choose. Imagine my surprise when 90 percent of my sample said they were "native" Americans, which to them meant people born in the United States. And I swear that I pretested that questionnaire first. Obviously not very well, though, and not with people essentially similar to the clinic patients.

In my own chosen field of sexual and reproductive behavior, to take another example, operationalization is particularly tricky. It's not at all clear that adolescents, especially young ones, understand "having sex" in the same way that you and I might. It's not just that teenagers, like Bill Clinton, don't define oral sex as sex, but that teenagers are accessing a very different social world than the world researchers are accessing.⁶ Researchers want to know if an adolescent is exposing himself or herself to activities that might give rise to either a pregnancy or a sexually transmitted disease, while teenagers are dealing with concepts like "virginity" and "going all the way" that do not overlap neatly with the concerns researchers have.⁷

Finally, canonicals worry about the overarching philosophical concept of generalizability, sometimes called by the name of its evil twin, "bias." Generalizability is the Great White Whale of canonical social research. It is what the elaborate apparatus of random sampling, careful questionnaire construction and administration, and thoughtful hypothesis testing are all designed to yield. I mentioned

in a previous chapter that a mere one thousand likely voters can “stand in” for the entire population of all likely voters, so that asking them who they are thinking of voting for can produce quite accurate predictions of how the election that takes place a week later will come out. That is “generalization” in a nutshell.

Because we salsa-dancing social scientists do not have data that come from a random sample, we cannot, strictly speaking, generalize, at least in the statistical sense. But that does not mean that our case has to be a case of one. In fact, as I will argue in subsequent chapters, if we sample carefully, operationalize fastidiously, and stay connected to theory at every single juncture of our enterprise, we can be more than just a single case. While we cannot generalize *statistically*, I think we can generalize *logically*.

You knew this intuitively already, of course. You are attracted to your juicy case study or interesting question not because you think it is idiosyncratic, that is, a “case of one” as I just called it, but because you think that it stands in for something larger. I always think of the poem by William Blake at a time like this, the one about seeing the world in a grain of sand.⁸ Your case is the grain of sand, and you want to use it to tell me about the world. You just *know* in your bones that this case is not merely a story about, say, water privatization, but about the New World Order, neo-liberalism, the role of the United States as a neo-Empire, the cascading effects of globalization, and more. (I say this knowing absolutely nothing about water privatization, but to prove the point about how we have to “bump up” a level of generality.)

When canonicals generalize, they do so because they think (or assume) that they know certain key things ahead of time. To wit, they know the question they want to ask (i.e., the hypothesis they want to test); they know what concepts (i.e., variables) are likely to be important; they have managed to get those concepts down into “items” that have a certain consensually-agreed-upon face validity; and—here’s where the sampling bit comes in—they know the parameters

of the population that are likely to be important, and/or they have some kind of a sampling frame from which to sample randomly.⁹ (“Parameters” in this case just means features, or variables as we call them in canonical social science.)

All this means that you have to strive to be parsimonious when it comes to dimensions. Let’s say that you think that variable A is causally linked to variable B, and you want to draw a sample of people to test this hypothesis. Do you think that this relationship will hold for both men and women? For white people, African-Americans, Latinos/as, Asians and others? For straight and gay? For rich, poor, and in between? For young and old? And so on and so forth.

The problem is that if you don’t know ahead of time the answers to these questions, the number of possible combinations is the combination of all these possible categories or variables. For example, if you have 2 genders, 4 categories of race/ethnicity, 3 categories of sexual orientation (straight, gay, and bi), 3 categories of class, and 2 categories of age, you have $2 \times 4 \times 3 \times 3 \times 2$ possible combinations, or 144 “cells,” and even this is a simplified way of looking at things.¹⁰ The point is that the larger your possible set of combinations, the larger will be your sample size, and since size is money, you naturally want to start dropping categories left and right that are not relevant to your particular inquiry.¹¹

For canonical types, this is not as big a problem as it looks—they know from prior research that sexual orientation, or class, or religion, or lots of other parts of the social world are not likely to be relevant to how (or if) A relates to B in their particular population, so they can just sample in ways that make sure that their main categories are represented. (The one variable that canonical sociologists almost always think will be important, race, is so important that surveys typically oversample this group to ensure that there will be enough people of color to yield respectable statistical results.)¹²

Other features that might potentially be interesting—sexuality, immigration status, unwed parenthood—are simply left out, mean-

ing that either they are not asked about on the survey, or, alternatively, they appear as often as they do in the larger population. As a result, the numbers of people in these categories are too few to do any kind of serious analysis. So, to take one example, if it turns out that sexual orientation is unexpectedly important in predicting how A and B are related, you'll be hard put to figure out if it matters for all gay people, or only white gay people, or only coupled gay people—you get the point.

But since ours is a voyage of discovery, not verification, we cannot possibly do a random sample. We don't really know what our research question is, we don't know how to operationalize our relevant variables ahead of time, and most important, *we don't know what to leave out*. More formally, we don't know the parameters of the population from which we want to draw our sample, and in fact, part of our enterprise is to ascertain which social features are important and which are not. Which means that we, too, worry about sampling, and will worry about it a lot. While we don't want to generalize in the way canonical sociologists do, that is, predict what the distribution of people among different categories in our population is within a known error term, we *do* want to protect ourselves from the complaint that our small group of observations is totally idiosyncratic for one reason or another.¹³

This brings us to bias. Bias these days has gotten a particularly nasty rep. It's come to mean the willing, conscious application of racially-or-other-based standards to the detriment of one group or another. Some time ago my then six-year-old twins were fighting in the back seat of the car, and my son told my daughter that she was a "crazy girl." My daughter, who like my son goes to public school in Berkeley, turned on him and hollered, "Miss Barnes [her first grade teacher] says that's sexist bias!"

When canonical sociologists talk about bias, they are usually referring to the statistical sense, namely that something not measured ended up systematically affecting the resulting outcome. One could also argue that the two more recent definitions come together, in

that what is and is not measured is often the product of a gap in the “sociological imagination” of the person doing the measuring, and that gap in turn is often socially produced. In other words, reigning cultural narratives (“doxa”) help shape what it seems logical to ignore and what it seems logical to measure.¹⁴

The take-home lesson from this discussion is that along with sampling and operationalization, both bias and generalization are things that you and I have to worry about, and worry about a lot. When I speak of taking the good things from canonical sociology, it’s the foregoing practices and the overarching concern with generalization and bias that you and I need to worry about when we do research.

The way that these three practices shape up in my work is that I think a lot about sampling, I think a lot about operationalization, and I think from the very first moment of the research onward about how the question I’m asking enlarges theory, that is, how I can generalize.¹⁵

I’m going to give you specific guidelines about how to sample, operationalize your variables, and deal with generalizability in Chapter 6, but right now, let me suggest a rule of thumb. The one question I *always* try to think about, as I make every single decision in my research, is what would my smartest, nastiest, most skeptical, and meanest colleague think of this particular decision? How can I persuade someone who does not share my taken-for-granted assumptions about the world that my research is valid? If it’s true that I’m a feminist, bleeding-heart liberal with small children who loves dogs and worries about the environment, what would the aforesaid mean, smart, nasty colleague say about why I chose to interview these people and not those? Investigate this area and not that? Look only at what people told me rather than what the “official” sources said? In all of those questions is the implicit accusation that I consciously or unconsciously chose a group, a theme, a strategy, or an analysis so that I could make things come out the way I wanted them to—in other words, that I am biased.

Be forewarned: those who don’t like your research, or who are

nettled by it, will announce that your results are either (a) “spurious” or (b) biased. In either case, what they mean is that *the way you chose the sample (or operationalized your variables) influenced the findings*.

This is a prime example of how canonical sociologists will think that you are doing what they are doing, only badly. You will choose a case, investigate it thoroughly, and show how certain social practices and patterns shape what people do. Aha! your critics shriek happily. It’s a “convenience sample.” It’s not *generalizable*! You haven’t *proved* anything!¹⁶

Well, exactly. And you didn’t set out to prove anything in the sense in which canonical sociology means it. But that doesn’t mean that you didn’t prove anything at all—in fact, quite the contrary. *What canonicals want to estimate is the distribution of a population across categories, whereas you want to analyze the categories involved.*

Stay calm. In the chapters to come, you and I will carefully go over the things you need to know to sample in a way that is defensible to canonicals, to operationalize in ways that make sense to them, and to do some theory generating that they will find plausible, and maybe even exciting. Bias and generalizability will be things we will learn how to deal with.

But before we do that, we need to figure out what our case is a case of.

Exercise for Chapter 3

In this exercise, lay out for me the key features of the research interest that calls out your name.

Given your research interest, how would a canonical sociologist design this research question? Given the steps that you have undoubtedly already learned, or at least gotten a sense of from this chapter, how would a traditional, quantitatively-minded social scientist “set up” this question? What kinds of data would he/she use or

gather? How would he/she test his/her hypotheses? What kinds of steps would he/she take to guard against bias, and to make sure that his/her data were generalizable?

And now for the \$64,000 question: *Why is that way of studying your particular question not adequate for the question you want to ask?*

Example: When I first set out to do my dissertation, my research question was “Why do women in California have so many abortions, when contraception in this state is low-cost, easily available, and completely legal?” (Remember that this was in the early 1970s, after California had de facto legalized abortion, but some years before *Roe v. Wade* de facto legalized it for the whole country.)

I did my best to do a canonical piece of research in order to answer this question. Working with my advisers at Yale, I carefully found out the names of every non-Catholic hospital in the San Francisco Bay Area (my “sampling frame”) and made plans to randomly select every *n*th woman having either an abortion or a live birth. (The logic here was that women having abortions should be compared to women having live births. As I was later to learn, this was and is not the case). The lag was built in because women having abortions have been pregnant, on average, less than 3 months, whereas women giving birth have been pregnant on average 9 months. At this stage of the research, I had wanted to compare women who had gotten pregnant at roughly the same time.

I had even begun to draw up a questionnaire when I realized that I could not *test* theories about why women were having abortions, because there just weren’t any.

I had done everything expected of me as a canonical social scientist. I had set up a research question, a random sample of abortion patients, a random sample of women giving birth, and then I was going to collect data on the two randomly sampled groups and compare how and if they were different.

It was only when I realized that this case was so much richer than

my puny little questionnaire could get at that I turned to qualitative interviewing. In my case, canonical social science was inadequate because:

- There were no consensually accepted theories I could test about why so many women were having legal abortions, because the phenomenon was so new.
- I had no reason from theory or prior research to think that the proper comparison group was women having births; it could very well have been women who successfully *avoided* having births, that is, who did not get pregnant in the first place.
- I did not have any direction, either from prior research or from theory, about what relevant comparative variables I should use to predict why some women had abortions and others, equally pregnant, did not.

So, there, in a nutshell, is why canonical social science was inadequate for my question.

Now in this exercise, tell me why it would be inadequate for *yours*.



What Is This a Case of, Anyway?

In a salsa-dancing social science project, somewhere along the line and preferably sooner rather than later, you will be faced with the single most challenging task of this kind of research, namely, having to transform your research *interest* into a research *question*. Almost every student I've ever worked with comes in to see me with what they are convinced is a research question, but in reality it's only a research interest.

This will no doubt lead you to ask, exactly what is a research question? In my experience, four features set off a true research question from a research interest. First, a true research question proposes—even in a preliminary, confused, and sometimes just plain wrong way—a set of *relationships* between or among concepts (call them “variables,” if you like). Second, *understanding* that relationship (or those relationships, if there are more than one) helps us to explain something important about social life. Third, a true research question permits a *range* of possible answers that can be empirically or at least logically examined in order to see if some answers fit the data

better than others. Fourth and finally, a good research question, properly answered, *advances the state of play in one or more intellectual conversations that are already going on in some part of the scholarly world that matters to you.*

Please postpone that panic attack you were about to have after reading that last paragraph: as I've reminded you, the research question in a lot of salsa-dancing research becomes clear only toward the end, but it's important to keep in mind at all times that the goal of getting a clear and productive research question is a key one. (And you need to keep reminding yourself that you should never confuse a research *interest* with a research *question*.)¹

I'm a little embarrassed to tell you that earlier in my career, given my background as a quantitative sociologist, I was much more like the advisers I described earlier in this book than I would like to admit. I had seen all too many graduate students suffer from the Damnation of the Ten Thousand Index Cards, and my heart went out to them. More to the point, as you already know, I am committed to adapting the best parts of canonical sociology, using sampling, operationalization, and generalization as a way to help us create a qualitative social science that can be rigorous, theory-building, and cumulative.²

So, in an attempt to be helpful, I used to start out by asking students what their independent and dependent variables were. This lasted for some time, until I finally noticed that it didn't seem to help students get any closer to having a clear notion of what their research question was. At this point, borrowing a term from Walter Wallace, my former colleague at Princeton, I started calling these concepts the "explanans" (the explaining thing) and the "explanandum" (the thing being explained).³ This was a step forward, because it seemed to free students from some of the taken-for-granted assumptions about "causality" built into the terminology of "independent" and "dependent" variables, but it still didn't do the whole trick. The real tripping place, invariably, was when I tried to get them to tell me what their research question was.

Here's a quick tip to see if you have a real research question or are still in the realm of the research interest. When you tell someone else about your work, does the conversation include something being explained and perhaps something explaining it? (The first part is critical; the second part is gravy at this point.) Even more fundamentally, when you say out loud what you're interested in, is there a question mark audible at the end?

These are not entirely trustworthy tips: you can have something being explained and something explaining it and even a question mark, and still not have a research question. I think of this as the "faux" research question, something that looks like a research question but really isn't upon closer inspection. ("Faux" is French for false, and I use it here in what has come to be its modern meaning, namely a "classy fake.") Faux research questions can be treacherous, in that they commonly have all of the benchmarks of a real research question except one: they do not have a set of possible answers that permit you to judge one answer as better than another.⁴

For example, you could say that you wanted to study how poverty affects child development. Although you do have a thing being explained (child development) and a thing doing the explaining (poverty), this is still too vague to qualify as a real research question. What about child development? What about poverty? *Where is the link?* Fundamentally, you lack a proposed set of relationships between poverty (thing A) and child development (thing B).

As a consequence, you are probably also lacking another item, too, namely a sense of what conversations you are hoping to contribute to, because it's not exactly news that poverty is not the ticket for optimal child development. (You are a bit vague on operationalization as well—what is your measure of poverty and what specific parts of child development do you care about?—and we'll get there, too.)

Here's a little story to cheer you up. I once had a student who came to me and said that she wanted to research women in rock and roll bands. "Fine," I said, "it's a good research interest, but what is

the research question? What is it about women and rock and roll bands that you want to explain? What set of relationships between concept A (women) and concept B (rock and roll bands) do you want to investigate?" My student gave me the fish-eye and disappeared for two months.

Three months later she was back in my office with a new research project, this time a study of cocaine dealers. I pointed out that there might well be issues involving human subjects to contend with, but even after we got over that hurdle, I would still need to know what her research question was.⁵ Again, what was it about cocaine dealers that she wanted to *explain*? She disappeared from my office once more, this time for six months.

Finally she came back to talk to me about still a third project, the acquisition of sexual identity in the workplace. At this point I had to laugh, and told her that while we had now covered sex, drugs, and rock and roll, she still didn't have a research question! (She eventually, in the face of my nagging, got a research question and a good one, and today is happily employed.)

Why does it matter whether or not you have a research question? *Because you cannot do research without it.* If you try to do research without a research question, you will only end up with the Damnation of the Ten Thousand Index Cards, a lot of frustration, and—if *you are lucky*—a lousy research project. The other problem is that without a question you can't do theory, because without a question, you can't explain. And in modern-day sociology, *explaining* (or, as it is more formally known, analyzing) is held in much higher regard than simply describing.

Let me be clear about a distinction I've been harping on from the beginning: if you are a canonical social scientist, you have or have inherited a working model of how two (or more) things are related, and you are setting out to test whether your version of the model is better than the going one. But if you are a salsa-dancing social scientist, you may well have only half of the equation, namely something

(call it “B”) that needs explaining. Neither you nor anyone else has a good model of why B is happening the way it is (women having a lot of abortions in a state with low-cost or free and readily available contraception, to take my own example). Neither you nor anyone else has a firm grasp on what it is that you are explaining, either—is it that some women (and men) avoid pregnancy entirely while others turn to abortion? Or is that some women (and men), once a pregnancy happens, give birth while others seek abortions? This is what I meant when I said that at this point all you really need to have is an “explanandum”—a puzzle, paradox, or conundrum about the social world that in one way or another upsets our expectations, and for which there is no ready answer. But this is not at all a trivial accomplishment, and those of us who want to *explore* models of the social world rather than test them are, if you follow my advice, still very much social scientists in good standing.

Sometimes I ask my graduate students what the difference is between sociology and journalism. In fact, I share with them the mean joke that people sometimes tell, about sociologists just being just slow journalists. (Slow journalists who can’t write very well, I might add.) My students almost always have a hard time with this question because although they intuitively know that there is difference, they can’t quite put their finger on it. For my part, I think the previous paragraph captures the essential difference between the two. Journalists tell us the who, what, where, and when, but only sociologists tell us the *why*. And only sociologists (or social scientists more broadly) come equipped with a body of theory (even if much of it is of an intuitive rather than a formal nature). A body of theory or theories enables one to test various versions of why things happen the way that they do, and to choose the most robust one.⁶ But even with all the theory in the world, if you don’t have a research question (or acquire one along the way) you can’t possibly come up with a “why” answer, much less competing ones, *because you haven’t properly narrowed your focus so that you can tell signal (the “why”) from noise*. To

put it less metaphorically, if you're not looking for something, you won't find it.

In traditional sociology, the problem with the research question is that it came trailing behind it a whole set of assumptions about the world, about social life, and about science, assumptions that are all in transition today. In the olden days, people went out to read "the literature," found little holes in it, and set about doing their research in order to fill in those intellectual holes. (Alternatively, they sometimes thought of themselves as "extending" a line of argument, or "refining" it.) This linear model of how to do research was sometimes known as "normal science."⁷

The assumptions built into that model were the following:

- You know what the variables are.
- You know how to measure them in a way that will seem logical and already well-accepted to other people working in the area.
- You have a pretty good idea of how the variables are related.
- The literature drives you to think of some of them as independent (causal, or at least prior in time) and dependent (caused, or at least subsequent in time) to the independent variables.

In contrast, those of us who are attracted to an interesting case (an "explanandum," or puzzle seeking an answer) face one of two potentially ruinous dilemmas. On the one hand, you may be tempted to force your question into the procrustean bed of canonical sociology, and twist your research interest into something that has—ahead of time—an independent and dependent variable or variables. Or you may be tempted to forget all that, and just jump into your field site—your case—hoping that you will know what you are looking for when you see it. Either way, you're probably in trouble.

The logic of verification, based as it is on rigorous operationalization, usually presumes quantification—the assumption that we can reduce categories into their composite elements and assign a

numeric value to each of the elements. (In canonical sociology, that last sentence would read, “. . . that we can reduce variables into their composite values, and assign a numeric value to each.” Same deal.)

This presumption in favor of quantification grows out of our presumption that we can or should engage in something approximating a “natural experiment.” Turning to the natural or “hard” sciences, we know that the way to find out if something causes something else is to apply the “cause” (explanans) to the “outcome” (explanandum) and see if anything changes, compared to when we didn’t apply the cause.

True experiments are hard to come by in the social sciences, so we have historically tended to cobble something together that we call a “quasi-experimental” design: we statistically manipulate a large database to compare some people who have a particular attribute against others who do not, “controlling for” other differences between the two groups.⁸ If our statistical manipulation reveals that the two groups are different beyond a certain cutoff point, or the two groups are different once we have “controlled for” other variables, then we conclude that our independent variable caused the outcomes we see.

The shortcomings of quasi-experimental design are reflected in the discussions in the previous chapter about how canonicals leave out (or don’t measure) things that they don’t think will be important, which in turn leads to what they call “unobserved heterogeneity.” A perfect example of this from the medical field is the debacle that hormone replacement therapy (HRT) has turned out to be for so many women.⁹ For years, doctors told postmenopausal women that HRT would protect them from woes both big and small—“hot flashes,” loss of bone tissue, vaginal dryness—on the basis of data from 122,000 women surveyed in the Harvard Nurses’ Health Study. With such a large dataset, epidemiologists were able to control for what they took to be confounding factors, and they found that women on HRT had lower risks of heart attacks.

So convincing did the data appear that healthy women without obvious signs of menopausal problems were urged to take hormone replacements simply as a preventive measure.

The only cloud on this particular horizon was that a subsequent study, this one with a real experimental design rather than a quasi-experimental one, came to the opposite conclusion. The Women's Health Initiative, which randomly assigned some women to HRT and some to a placebo, found that women on HRT faced an *increased* risk of heart attack and other cardiovascular problems, not a diminished risk. As a result, physicians and the media began to urge women to go off HRT, or to use it for the shortest possible time. It appears that the confounding effects were not in fact controlled for, and that "unobserved heterogeneity" led to a situation where physicians were urging women to take a medication that may have harmed at least some of them.¹⁰

A preliminary examination of the HRT data leads me to think that there may have been a fairly powerful selection effect at work, meaning that the nurses who took HRT may well have been more health-conscious, more aware of current medical thinking on preventive measures, and more likely to have had better medical care than the nurses who did not take HRT—factors that made the first group of nurses less at risk of heart disease *independent of the HRT* they were taking. I bring this up because in my gloomier moments I sometimes think that every social science finding based on a quasi-experimental design (as in so much of canonical social science) is really a selection effect waiting to be unmasked.

The quasi-experimental theory testing model represented by the Harvard Nurses' Health Study was as popular as it was and unquestioned for so long because it appeared to conform to the standards of scientific method. Notice that I said *appeared*. When I was first trained as a sociologist, we were all taught that while sociology was not yet a science, if all went well it might just grow up to be one. It was, perhaps, a "proto-science," as Thomas Kuhn called it.¹¹

But new work within the history of science and the emerging field known as the Social Studies of Science has called into question the traditional epistemology on which the scientific method as traditionally understood rested. These scholars argue that at best the scientific model describes only experimental physics, and probably only nineteenth-century experimental physics at that.¹² Starting with the work of Thomas Kuhn, people have come to realize that modern working scientists typically meld a logic of discovery model with a logic of verification. More seriously, philosophers of science have also come to question whether the social sciences can ever meet traditional standards of objectivity and operationalization, given that the observers are socially located individuals observing other socially located individuals and institutions.¹³ For that matter, the natural sciences themselves are not as completely detached from the social world as the traditional account would have it.

I hope I have already made clear that there are some things that canonical social science does very, very well, and I'm all for using it for the questions it is well adapted to. Although I rarely do data runs myself, I take for granted that I need to know what canonical social science has already discovered about the thing I am exploring in my own way. I strongly urge you to do the same: to look at what canonical social science has to say about your area of interest before you head out to the field.

But canonical social science doesn't have much to offer you, at least at the outset, if you, like many of my students, are attracted to an interesting field site that you intuit will tell us something about the social world, but you can't tell me exactly *why* it's important, or worse yet, *how* you are going to go about researching it. You know that something about this case fascinates you, both as a person and as a social scientist. You can just *feel* yourself in Bolivia, Sri Lanka, New York, or somewhere else where a fascinating social process is unfolding itself. But people like me keep asking you what your research question is. What are you to do?

If you're like many of my students, the first thing you try to do is to shoehorn yourself into the canonical sociology model, searching and scratching until you come up with something that looks like an independent and a dependent variable. Because many of you are so smart and write so well, advisers like me don't get at first that you are missing some of the key items from the checklist of canonical sociology must-haves. The truth is that you have no idea what your variables are, you are faking your causal assumptions, and you don't have the foggiest idea what data will be relevant to you once you get into the field. So we let you go on about your business, not realizing that you are like a backpacker off for a three-week camping trip without any clean socks.

What happens to you is that you have a rich, juicy research case, and you've let your advisers nag you into doing a *logic of verification* study when the case calls out for the *logic of discovery* method. What's worse, you come to believe yourself that you're following the right path (after all, it's what everyone seems to take for granted), and so you find yourself getting angry and frustrated, not making much progress and watching all the juice being sucked out of the project.

Meanwhile, the curse of info-glut kicks in. At the same time that you're getting blisters from your lack of metaphorical clean socks, you're probably also sitting in front of your computer doing "research." For example, if you were interested in the privatization of water, you might think the World Bank had something to do with it, and so you would Google "World Bank" and would get 1,250,000 hits. (I just tried it.) Add "water" and you get 376,001 hits—something of an improvement, but still unmanageable. Adding "privatization" cuts you down to 44,402 hits, but you obviously could still spend a lifetime reading those forty-four thousand hits. Here's the Zen paradox: you can never know what you're looking for unless you know what it was you were looking for in the first place.

By the way, the instinct to Google is a modern version of an old

practice, from the days when people could browse through library stacks and sometimes come away with a research question. “Googling” feels like research, but it all too easily can take you further away from your research question, not closer.

If you’re lucky (and far away from your advisers), you might just get discouraged enough to have the strength to throw away your “methods” once you are out in the field. If not, you’ll keep pulling and tugging to fit yourself into the mold of an older era. You are, whether you know it or not, embarked on an intellectual inquiry in which you must sort through your tool chest from canonical sociology and decide what to keep.

The truth is that you do in fact need a research question, that you should put it as a high priority on your “to do” list, but you should ignore the taken-for-granted assumption that it comes first. Actually, the research question often reveals itself at the end, or close to the end, of the research (this is, after all, a voyage of *discovery*)—but you must never forget that you need it, or you will fall into the Damnation of the Ten Thousand Index Cards.

The next thing to do is to free yourself from the tyranny of the independent and dependent variables, although these, too, are useful tools to bring along, as long as we keep in mind that they are from a very different model of doing research.

Here’s the tricky part, and if you want to take a break and go salsa dancing just to clear your mind, feel free.

Doing the kind of research I advocate is an *iterative* process, where I’m going to give you a set of steps (steps actually adopted from canonical social science, by the way) and ask you to follow them. The hard part is that you have to do them not just once and for all, as is often the case in canonical social science, but over and over and over again until it comes out right. And this will be true in every one of your research projects!

So, to go back to the case that attracts and involves you, what is it about this case that strikes you as interesting? I sometimes ask stu-

dents to tell me what the “intellectual itch” was that brought them to the question. Alternatively, I ask them to imagine talking to someone like my Great-Aunt Lucile, a retired school teacher who was very, very smart and very, very impatient with sociology jargon. Why would a smart layperson be interested in this question? What is it about your case that turns you on? What, in short, do you want to explain? (This is where Exercise 1 comes in.)

What are the various questions that came to mind as you explored your intellectual itch? If you did Exercise 2 faithfully, you will see that you have a great many questions, and many of them will circle around that intellectual itch of yours. Likewise, Exercise 3 will have illuminated how it is that you can’t really do a successful canonical inquiry, so you know at least that you are doing a theory-generating study, not a theory-testing one.

Here’s where the iterative process comes in. Once you’ve gotten down in words all those insights from having done your exercises, and thought about what you would tell my Great-Aunt Lucile or a similar person in your life, you have to figure out how your question fits into a bigger picture, a process that paradoxically helps to narrow your question, or turn your interest into a genuine question. This is the fourth step: What is it about your question that illuminates questions that other people in your field, or other related fields, have been asking?

The quirky genius Erving Goffman popularized the notion of “frame analysis” in 1974, by which he meant what cognitive psychologists were later to call “schema.”¹⁴ My favorite story about schemas and frames is that some cognitive psychologists once showed subjects a videotape of several people tossing basketballs to one another, and asked the subjects to count how many times the ball changed hands. In the middle of the experiment, a person in a gorilla suit ambled through the gym, right where all of this was happening. Later the researchers queried the subjects about the gorilla. The subjects looked at the researchers, befuddled. “What gorilla?” asked half of

them. They had been so caught up in the task (that is, they were using their schema so intently) that they simply hadn't noticed it, since it was, for all practical purposes, outside of the "frame."¹⁵

What do gorillas wandering among basketball players have to do with us? What does the notion of "frames" have to do with translating a research interest into a research question, which is where we are now?

The answer is that this fourth step—finding what I am calling a "frame"—goes a long way toward narrowing down your research interest into a research question. You are interested in a substantive question, something that mesmerizes you. Because you are a pioneer in the age of info-glut, and you are casually crossing boundaries every place you turn, your substantive question is only partly about the nitty-gritty of what you are interested in.

Let me put it to you another way. My freelance writer friends constantly confront the need to come up with "hooks," that is, a new angle on things that magazine or newspaper editors will find interesting. For example, let's say that you are interested in HIV/AIDS in Africa. There are literally thousands of articles being written about AIDS and Africa, and your task, if you are a freelance writer, is to come up with something new to say about the subject. Unless you can come up with something new and fresh, with a "hook" (what I am calling a "frame"), the editor will simply toss your query letter into the trash basket, because we've all heard far too much about AIDS in Africa.¹⁶

Remember when I told you earlier that your question should be important because *it tells us something about the social world*? In step four you need to know under what rubric that "something" is generally found. It's not an easy or a self-evident question, although it might appear so. If I asked you to name the "something" your question is connected to, you would probably reiterate the reasons why the question interested you in the first place. Or, if you have come up with some independent and dependent variables, you might

well be tempted to tell me whatever line seems to sound best in your social world of grad school: “Oh, it’s a study of globalization” or “it’s a social movement study” or “it’s about the acquisition of identity in a racially complex world.”

Please note that none of these are really either (a) a research question or (b) a frame that will help you get to a research question. Here’s where things get a little tricky. Remember when I said that the steps in this process were sort of like yoga poses, in that you had to do them over and over again? Well, this is where we start. We know that you have a research interest, and that you are desperately seeking a research *question*, partly because I told you that you had to, and partly because you rightfully fear the Ten Thousand Index Cards.

So here you have an interest, and there you have a whole world of colleagues and other people who think about the social world, and your task is to get to a meeting of the minds between the two. You need to “frame” your research interest in one of the available frames in your field, and doing so will help you move from research interest to research question.

I see this task as having two different dimensions. On the one hand, you have to decide what you are; eventually you will be job-hunting and people will want to know what you are a scholar *of*. Of course you are a sociologist, or a political scientist, or a law and society scholar, but within these broad categories, there is a set of commonly accepted pigeonholes that people use to put aspiring academics into. You can be a sociologist of organizations, a sociologist of development, a political scientist specializing in public law or international relations, and so forth. On the more micro-level, there is a set of people you will be in dialogue with as you pursue your question: eventually you will have to tell me (and other readers) why the people who have explored either your area or adjacent areas are wrong, or incomplete. Those scholars are your intellectual reference group.

Once you decide *who* you are, you need to decide *what* your research question fundamentally is. Now, few projects come into the world with their subspecialty or their intellectual reference group tattooed on their foreheads. Your project will address many more than one subspecialty, but if you want to keep from going crazy, you need to identify a large clan of scholarly inquiries more or less related to yours, and within that, a smaller family of people with whom you are in more or less direct dialogue. This need not be ascertained at the outset, of course, but it's something you have to keep in mind from the earliest days of your research project, and it's a key part of getting to a real research question.

You may have noticed that I use a lot of metaphors to talk about research, and when I try to tell students about this process of locating yourself in a subspecialty and an intellectual reference group, I find myself using lots of different ones to try to get the point across. But maybe the best metaphor is that of an intellectual cocktail party. The idea is that you walk into a crowded room, and there are grouplets of two, three, four, maybe five people chatting about something in common, and everyone has a drink in his or her hand. You, the newcomer, go over to the bar and get a glass of wine or a soda, and start drifting around the room, hoping to strike up an interesting conversation with some interesting people. But all around the room, people in their little groups are speaking to each other as if you don't exist (although the friendlier and less intensely involved ones sometimes give you a friendly look to indicate that you are welcome to join in). How do you insert yourself into the conversation?

Well, we all know that the first rule of sliding gently into a conversation is not to appear to be changing the topic abruptly. If the group is chatting about Michelangelo and you say, "Do you believe in aliens?" all eyes in the group will be riveted on you instantly, and each and every eye will be beaming very bad vibrations your way.¹⁷ It's as if you had strong-armed your way into the conversation and tried to hijack it.

Socially adept souls know that the secret to joining an ongoing conversation is to slither in gracefully. And the way to slide in gracefully is to *pretend that your topic of interest is connected to what people are already talking about, even if it's not*. So back to our little group talking about Michelangelo. You stand there, listen politely, and then during a lull in the conversation, you say quietly, "You know, Michelangelo was so talented it almost seems as if he came from another planet." People murmur assent, and speak of his prodigious and multi-faceted skills. Then you gently ask, "I know it sounds outlandish, but have you ever considered the possibility that artists whose talents are so outstanding might really be from another planet? Of course, I don't believe in aliens, but how do we explain these extraordinary humans?" More chatter, and then you are free to say, "You know, I read a very interesting article on aliens the other day . . ." And off you go, talking about what you want to talk about.

It's exactly the same deal with intellectual conversations, including those that are frozen into print in journals and books. For our straw person, the canonical social scientist, sliding into an ongoing conversation in print is not usually a problem. The canonicals are raised in a rather formal tradition of what the socio-linguists call "turn-taking." They read the journals religiously, they know what narrow area of social science they are interested in and good at, and they know the kind of data that they have access to and which questions it can be used to address. As at a Quaker meeting, there are long periods of silence, and then new speakers share their thoughts as the spirit moves them.

But you are not a canonical social scientist, and you are in a noisy, competitive, and maybe slightly tipsy cocktail party where there are lots of conversations going on, and you need to figure out where your Burning Question best fits.

In more recent years, social movement theorists like Robert Benford and David Snow have expanded Goffman's notion of "frame analysis" to argue that social movement wannabes only get other people

to listen to them when they come up with a “resonant frame.”¹⁸ In other words, most people walk around with a set of rather incoherent and contradictory feelings (as Frank Luntz, the Republican pollster, told us in an earlier chapter), and the winners in a social movement will be those groups that come up with a short, snappy sound bite that embodies a particular political position, one that people are happy to sign on to.

In much the same vein, how you “frame” your question is something of key importance. And if you are not doing canonical, “normal” science, there are a multitude of ways to frame your project. Take any of the examples I gave you in previous chapters. While what you may *really* be interested in is the rules and regulations of flirting at work, or water privatization, or high levels of incarceration in the United States, you—like our friend at the cocktail party—have to insert this interest adeptly into an ongoing conversation.

You have a case that interests you, and you want to interest other smart people in your case, too. You can’t just announce that what you’re interested in is, in fact, really interesting. You have to make the connection for people.

The important thing to remember is that there is no single right way to insert your interest into an ongoing conversation. Your interest in flirting at work, say, could plausibly fit into a conversation about sex, about gender, about work, about organizational behavior, about law, about feelings, and the list goes on. In other words, are you a scholar of work, a scholar of gender, a scholar of organizations, a scholar of emotion, a scholar of law? So how do you know how to frame your question? Basically, the answer is trial and error, but I think there are some preparatory exercises that can make it easier.¹⁹

The first and most important thing is to remember to have an orderly and well-prepared house.²⁰ Make a practice of reading everything even dimly related to social science that catches your eye, or perhaps better yet, your inner ear. In direct contrast to what canonical sociologists will teach you, *everything is grist for the mill*. So what

if you find a fascinating book about epidemics when what you are really thinking about is flirting at work? Or how about a really well-written book on a heat wave when you think you are interested in how gender roles are changing? Any good book about any aspect of the social world can teach you something, and if your id is drawn to read something, trust your id.

So read, and read often, and don't worry that it's not on point, or at least doesn't seem to be just yet. Read what catches your attention, and read what other people in your world are excited about. This is what Zen practitioners have in mind when they talk about sweeping the grounds of the temple while you are awaiting enlightenment. Be sure to take good notes on what interests you in your reading.

To use the old metaphor of the forest and the trees, you are interested in bark, but the more you can show people how your particular bark is related to their trees, and to the forests (and, better yet, the whole woodland ecology) that all of us really care about in one way or another, the better off you will be, and the larger the readership for your research.

But how do you, passionate about your particular case of bark as you are, frame your work in terms of the trees and the forests that others are (or will be) interested in as soon as you publish? This, like entering into a cocktail party conversation, is actually a delicate piece of social behavior. As with a lot of social practices connected to power, there is a strong expectation that you should just intuitively know it. And as Pierre Bourdieu shows us, knowing how to decode it *without looking like you've decoded it, but just came by it naturally* is one way that the profession separates the "gifted" from the "less gifted."²¹ People who intuitively know how to frame interesting questions in ways that others will find interesting are typically thought of as really, really smart.

But how about the rest of us? Precisely because this knowledge is what Bourdieu calls transparent, that is, mastery over it is supposed to be a mark of innate talent rather than of figuring out, it's some-

thing that I'm still puzzling out for myself. I'm not sure I have the whole story (this is the trial and error part), but I can suggest some general strategies that will help you get started.

Let's begin with a couple of questions that will narrow down the field. Although it may seem paradoxical at this point to ask you about your publishing plans, thinking about them can actually help you clarify your research question. Are the kinds of questions that came to you in Exercise 2 mostly appearing in peer-refereed journals, or are they discussed mostly in university press books, or even in books for a popular audience? All of these settings imply different frames for the case you are interested in, and each of them has a different set of costs and benefits.

If the questions that interest you are mostly being written about in peer-reviewed journals, you are in luck. For better or for worse, these journals are the gold standard for much of the academic system. Particularly in academic settings dominated by hard scientists and social scientists aspiring to be hard scientists, peer-refereed journals have a lot going for them. For openers, you can rank them by their prestige—there will be a rough consensus among various parties about which journals are “better” than others. Second, the very form and content of peer-refereed journals pay homage to the “normal science” version or canonical model of social science, a model that most natural scientists are extremely comfortable with. Third, a good, hard-working scholar can turn out a number of these articles during a review period, thus testifying to a “pattern of scholarly productivity.” (A more cynical observer calls it “salami science.”)²² And finally, something called the Social Science Citation Index permits others to track how often peer-reviewed articles are cited. In short, publishing in peer-reviewed journals fits beautifully, almost hand in glove, with the institutional need for bureaucracies to weigh, measure, and promote scholars on seemingly objective and “intersubjective” criteria.

So why would anyone ever publish anywhere else? Well, all the

reasons that make peer-reviewed articles a thing of beauty to your average academic bureaucracy and to epistemologically innocent natural scientists and economists are precisely the things that often *don't* work well for salsa-dancing social scientists. If you are not interested in incrementally adding to existing theories, but in creating new ones, or if you don't want to slide gently into ongoing conversations, but you want to tell people that they've been talking about the wrong things all along, journals may not be your best bet. And as far as journal editors can tell, you may have interesting data, but not a recognizable theory (that is, a priori), which is their way of saying that they just don't know what slot to put you into.

Do not despair. Although it may not be obvious *where* you fit into journal conversations, this does not mean that you don't fit at all. You, like our cocktail party person, need only convince the listeners that your interest in aliens (or water privatization or flirting in the workplace) is really connected to their interest in Michelangelo. While the connection may not be obvious to them (i.e., editors and reviewers), it's obvious to you, and you will explain it.

Ah, there's the rub.

Here's what I suggest: go do some anthropological fieldwork. If there are journal articles that have excited you in the area of your research interest, investigate the journals they were published in. How did the articles that you like frame *their* question? Usually in journals the framing occurs within the first one or two paragraphs, but often people just skim that part. Go read, and read carefully. Whom do those articles that you love cite in turn? How do they frame their questions? Read the journals that keep coming up as the location of articles that excite you, and check to see if there are any hidden rules of the journal that might get in the way of your publishing there.

Again, do fieldwork. Read the last five years of the journal that you think you might want to publish in, or at least are curious about. Do an anthropological/Bourdieuian analysis of the journal. What are people talking about? What are the formal rules of the jour-

nal? (This can usually be found in the mission statement, generally printed on the inside front cover of the journal, and like all mission statements, it should be taken with a grain of salt.)

More important, what are people in this journal talking about? What are the *informal* rules regarding what you can talk about and how you can talk about it?

For example, one of my students did the following analysis of the journal *Demography*:

I reviewed 116 articles from the last 11 issues of the journal *Demography*, published by the Population Association of America, the professional association of demographers. The articles published in *Demography* tend to rely on statistical and demographic methods; sometimes an entire article is devoted to exploring a particular method. Though the articles often use these methods to explore a specific concept or question, most demographic research that relies on other methods (such as ethnography) and/or asks broader questions is found in the pages of another journal (*Population and Development Review*).

The not-so-hidden subtext is that numerical data drive the research presented in the pages of *Demography*. The manipulation of data is central to these articles, and sample sizes are of the magnitude associated with large-scale surveys. While surveys are a main source of data, administrative records are also utilized. The methodology used includes everything from straight demographic methods to statistical techniques. Getting an article published in *Demography* appears to require a willingness to delay the detailed discussion of theoretical implications for publication in another journal.

Another student looked at *Theory and Society*, a journal at the other end of the epistemological spectrum:

This bimonthly journal is one in which I might like to publish—compelling, well-crafted articles and book reviews by

both prominent and less established scholars, with topical as well as methodological breadth. It generally publishes from two to four 30–60 page articles in each issue, on political, cultural, social, and economic matters ranging from Bourdieu’s habitus to queer theory, path dependency and development, urban ghettos, and critiques of rational choice. Its approach to social science seems methodologically reflexive and rigorous, yet not stultified. My main disappointment is that it doesn’t seem to draw from an international pool of scholars, but mostly from the U.S.—which makes sense given the mostly U.S.-based editorial board.²³

A quick review of these two analyses suggests that the kinds of articles that would get published in the first would not get published in the second. Some things are obvious and could have been learned by reading the journals’ mission statements or instructions to contributors, for example, page length and the like. What is less clear is what these two analysts have picked up on: that *Demography* tends to take its methods as given (and indeed, seeks in the normal science way to continue to improve the methods), while *Theory and Society* insists that authors be aware of the methods of social research as themselves social products. (This is what my student means when she says, borrowing a term from Bourdieu, that this journal is “reflexive.”)

Another thing that you will not get from simply reading the mission statements of a journal are the rules about what you can and cannot talk about, or more to the point, how to “frame” something that has not yet been a topic of discussion in that particular journal. Particularly with journals that aspire to the normal science or canonical model of social science, introducing a new topic is extremely challenging. My favorite example of this is an article by another group of my colleagues, Mike Hout, Clem Brooks, and Jeff Manza, titled “The Democratic Class Struggle in the United States, 1948–

1992,” which was published in the *American Sociological Review*, the flagship journal of the American Sociological Association.²⁴ While the substantive issue is extremely interesting, namely how different classes in the United States have changed their loyalties vis-à-vis the Republican and Democratic parties in the period between the end of World War II and the early 1990s, there had not been much writing on the effects of class on voting for some time. In fact, as the authors themselves note, the conventional wisdom was that there was a “declining significance of class” as a factor influencing voter choice in elections.

So the article slides into a much larger and more interesting substantive discussion about class and politics, by presenting itself in large part as a *methodological* critique. The reason class doesn’t seem to matter, the authors argue, is that people have simply not measured class adequately and have failed to take advantage of new statistical techniques (in this case, multinomial logistic regression) that permit a better measurement. Thus, smuggled under the rubric of a methodological discussion, this article presents a nifty and elegant discussion of what has happened to the two main political parties in our century.

The good part of this is that you don’t have to do this alone. Somewhere in your department is a faculty member—maybe more than one, and maybe a lot—who has published in your journal of choice. Better yet, you may well have an *editor* of one of your desired journals in your department.

Go talk to him/her about the specific question we are discussing here, namely, what kind of “hook” will make your substantive issue look relevant and timely to the journal editors?

Again, I must urge you to use a very large grain of salt here. *The trick of figuring out how to “frame” a piece for a key journal is itself a social practice, one that—as Bourdieu would have us know—is something that is supposed to look easy, but be hard to access.*

Here’s my last piece of advice on this. Put on your overcoat (you

might well get rained on) and just start submitting papers to journals. I even tell my students that the only reason to write a class paper that is *not* destined for a journal is that they might want to write a “think piece” for their own purposes. So for every single class paper that you write, think about where you would send it for publication, and how you would frame it in a way that would be interesting to that journal.

I know that this is potentially one of those “you’ve got to kiss a lot of frogs before you meet a prince” scenarios, but look at it this way. Even in the worst case, which is where you get a scathing letter from a journal, you have (a) met new people with whom to correspond and (b) had some reasonably smart people look at your work very carefully.

Surprising as it may seem, journals need you more than you need them. Good journals are always short of good papers, and in theory, dedicated editors (and bless them, each and every one) will be happy to work with you to help you learn what it takes to get published. What you get is a terrific professional education, and it’s free.

But let’s say that, for all the reasons outlined above, journals are just not your cup of tea. In fact, you’ve done the anthropological investigation of the main journals in your field, and they leave you cold. Your interests don’t fit into what the journals are talking about, and you can’t imagine any way to smuggle your interests into the things that they are interested in. So, here’s the deal: you write a book. Well, in fact, you have to write a book anyway, since that is essentially what a dissertation is—a scholarly monograph that is close to the first draft of a book.

To think about how to frame your topic in a book, rather than in journal terms, you have to practice a bit of “black belt” sliding into ongoing conversations, by examining what is going on in “the literature” and keying your contribution to it. Which brings us to the next chapter, rather conventionally called “Reviewing the Literature.” Doing a review of the literature is, as we’ll see, reasonably straightforward in the case of an article, but considerably more complex in

the case of a book. Still, knowing how to frame your story in the context of an ongoing debate in the literature is crucial to success in either of these realms.

The next chapter gives you important suggestions about how to find out what books (and articles) might be relevant to you, but let me say that if you think you will be writing a book, you should still do the key things I spoke of earlier in the context of journals. Read the books that excite you and keep you interested. Look at how they frame the question. Read the key people these books cite, and look at how they in turn frame the question. In short, grab all the frames (or “hooks”) you can get your hands on.

Exercise for Chapter 4

This is perhaps the most important exercise in the whole book: What is the “frame”—the “hook”—of your research? What kind of research question is emerging from your research interest? I know that even after all of this discussion, it’s possible that you still are having a hard time seeing exactly what I mean by a research question, but not to worry—we have a few more exercises coming up that will help clarify your thinking.

In this exercise, along with the development of your own research question, write down a list of all the ways that people in journals have “framed” questions close to the things you are interested in. Are those frames useful to you? Can you “slide” into a journal using one of those frames?

Conversely, if the journals are just not doing it for you, how do the books that you like and are inspired by “frame” their questions? (Remember that you have far more latitude in a book than in an article.)

Be systematic: excavate and express in the most succinct terms exactly what the frames are in work that you like.

So, after all this, what’s *your* frame?



Reviewing the Literature

Once upon a time, in the days when canonical social science was all that there was and information was scarce and costly, doing a review of the literature was a stroll in the park. Your adviser told you what the main writings were in your specific area; your task was to master the oldies but goodies and the up-and-comers. You went out and gathered data (theory-testing data, that is), and after you did most of your analysis, you went back and checked over the relevant literature one last time, just to make sure that you were up to date with anything new that had been published in the interim.

And because of the rules of canonical social science, you didn't really have to cite everything—you just needed to cite the canonical writings in your subfield. That is, your contribution was assumed to build upon a series of prior contributions, and there was a very high consensus (at least among the heavy hitters in your area) regarding who should be cited as the “authors” of each of the previous contributions that you were going to build on.

But we salsa dancers have no such luck. Your article that eases its way into a journal and subtly changes the conversation, or your book that creates a new field or area of inquiry, likely covers not one, but

many fields. And here is the very worst part: you don't actually know what fields those are at the beginning of your research, so you can't "review the literature" as a way of finding out what your field is. Moreover, the literature is so overwhelming these days that if you reviewed all the literature—meaning everything interesting that relates to the large, juicy case study that has caught your interest—you would sit in front of your computer until cobwebs started to collect around your ears.

So for us salsa-dancing social scientists, a "review of the literature" is not something we do once, but many times. We do it at the beginning of the process, as we try to figure out how to "frame" our research; we do it many, many times in the middle of our research, as both our frame and our research question become clearer; and we do it one last time at the end to see if there is anything we missed.

Sounds daunting, doesn't it? But let me give you a few salsa-dancing tips about how to undertake this first review of the literature, the one that helps you frame your research or, to put it in salsa-dancing language, insert your interesting case study into one or more intellectual conversations going on in your (and maybe several) fields. This model, with modifications, should stand you in good stead to the very end of your research project.

Because of the way information is gathered and stored these days, it is harder than ever to find your "frame" in the work already done by other people. Precisely at the moment when you need a frame more than ever, canonical social science and its frames, which are mostly located in journals, have become less relevant to your interests as a salsa-dancing social scientist in a rapidly changing and globalizing world.

Once upon a time, information (of the sort that interests us, namely scholarly information) was processed and stored by virtue of passing through a set of social practices devised by and drawing on human intelligence.

The early years of the twentieth century were ones much like to-

day, with a vast expansion of information and, more to the point, the expansion of a scholarly apparatus in the form of the modern university, as shown in the story of Jane Addams and the rise of the department of sociology at the brand-new University of Chicago. It turns out that what was happening at Chicago was happening all over the country, with the rise of new institutions of higher learning and the spread of new disciplines within them. Thus early twentieth-century info-glut was a challenge to scholars of that time, just as ours is to us.

In the face of this earlier info-glut, new social practices arose, in two parallel and separate forms. In Washington, D.C., at the birth of modern governmentality, and again in Massachusetts, smart people began to confront the need to store and retrieve information on a scale hitherto unimagined.¹ The “schema” that both locations hit upon was to imagine that information was like a set of continents, each neatly separated from the others by a clean border and lots of water.

With the development of this schema, people could be trained in the system, and could assign each piece of knowledge (for example, each book) into a specific “slot” in the system, which was thought to be mutually exclusive and exhaustive such that each book would be located in one and only one classification.

You’ve probably guessed already that what I’m describing is the system of book classification within libraries, known either as the Library of Congress Subject Heading List or the Dewey Decimal System. (No, it was not devised by John Dewey, friend of Jane Addams, faculty member at Chicago, and founder of progressive education.)² Although they differ in their details, both the LCSH and the DDS assign books to a classification based on their subject matter. Thus, once you have decoded the subject matter that a book pertains to, you need only locate that classification in order to find out the whole conversation going on around that particular form of intellectual inquiry.

This was a lovely system, and I am almost willing to bet you a lunch at a fine restaurant of your choice that you have intuitively navigated this system at least once in your lifetime by using the taken-for-granted assumptions built into it. Although I am less certain of another point, and hence won't bet you a lunch, I'm pretty sure that you are probably unconsciously still using these systems and their underlying assumptions to do research in a world where they no longer hold sway in the way they once did.

For example, because each item of information (that is, a book) was presumed to be about something, and only that thing, it was shelved in the library next to other items of information of broadly similar background. Now confess—how many of you have gone into a library to “browse the stacks”? Your having done so proves my point, reflecting the taken-for-granted practices I mentioned and highlighting the best features of the system.

First, you assumed that similar books would be physically close to one another. If you were in a university library, and were interested in “marriage,” say, you could cruise right on over to HQ 536 (“Marriage—United States”) and start picking up books likely to be about things you were interested in. And that's not all. Each of the items of information had two handy finding aids built into them, namely tables of contents and indexes, so that you could quickly ascertain whether or not the book was relevant to your interests. Better yet, you could take down the book, which was a full-text version of what you were interested in, and browse through it to see if it held other items or issues relevant to your interests, ones you had not yet thought about until you saw them on the printed page.

Moreover, the system had a number of wonderful features that were probably even more invisible to you than the assumption that materials on like topics would be physically shelved close to one another. For example, the system was *hierarchical*, meaning that items of more narrow interest were located under topics of broader inter-

est. In fact, if you were one of the few people outside of professional librarians trained in either of these systems, you knew there was a set of volumes available for your perusal which permitted you to locate a single term describing your area of interest (a “frame,” if you will) known as a Library of Congress Subject Heading (LCSH); each LCSH was described carefully and precisely within a hierarchy of broader and narrower terms, signaled to you by the notation BT (Broader Than) and NT (Narrower Than) in relation to the term you were looking for.

If I sound nostalgic, it’s because that system has in large part been superseded, for lots of different reasons. First, an increasing amount of information is now found not in books, but in other formats: articles, congressional hearings, government reports, international statistics, Web pages, and the list goes on and on. Next, the system assumed that knowledge was like countries, cleanly separated from other areas of knowledge by nice, clean borders. As Thomas Mann shows, and as I have been arguing throughout this book, knowledge just isn’t like that any more.³ The example that Mann chooses to prove his point is a wonderful book written by the late Barbara Christian, *Black Women Novelists*.⁴ Is this a book about African-Americans? Women? Novelists? Literary criticism? Short answer: yes.

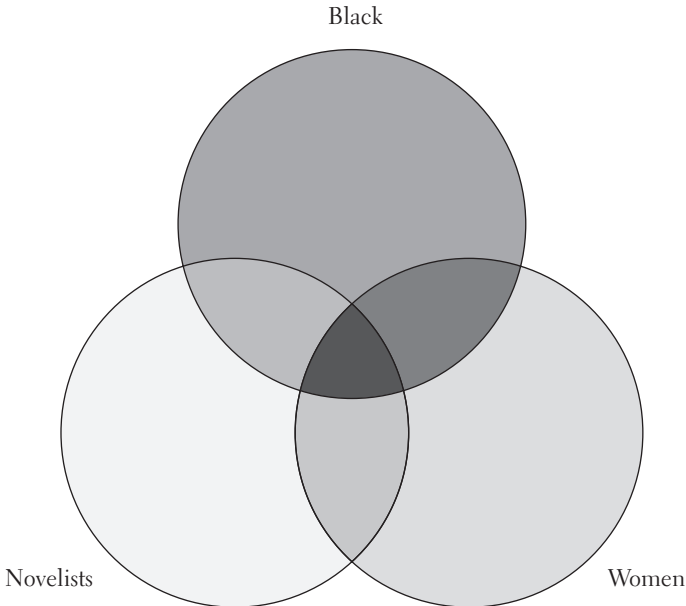
Today, instead of a human intelligence thoughtfully sifting through and coding all available information, we have tons of stuff out there, and no one has put a label on it, anywhere. (This is, in short, the problem of “full text” databases—it’s all there, but unless you can remember a reasonably uncommon word in the article, or some key proper nouns, it’s likely to be very hard to find.) The whole process is like being in the world’s best and most interesting tag sale with trash and treasure all mixed up together and no road map in sight.

The advice I’ve been giving you is to go to the literature to find a

“frame.” But the literature is a mess, and so is your case study. Where are we going to begin?

After many years of trial and error, I think I have an answer and a set of tips to go with the answer. It involves what I call the “bedraggled daisy.” Take a deep breath, sit down with your juicy case study, and pull out a piece of clean paper and a pen or pencil.

To get the Zen of the “bedraggled daisy,” you have to know something about Venn diagrams. A Venn diagram is a visual representation of two or more “sets” of things. Going back to Barbara Christian’s book, there are three “sets” involved: blacks, women, and novelists. Think about this for a minute. The set of “black” includes men and women, novelists and non-novelists. Likewise, the set of women involves blacks and non-blacks, novelists and non-novelists. Finally, novelists can be white *or* black, male *or* female. In a Venn diagram, the subject of Christian’s book would look like this:



But what Christian's book is *really* about is the intersection of those three sets, that is, the set where everyone is black AND female AND a novelist, or, that place on the Venn diagram where the three larger sets intersect, and *only* where they intersect.

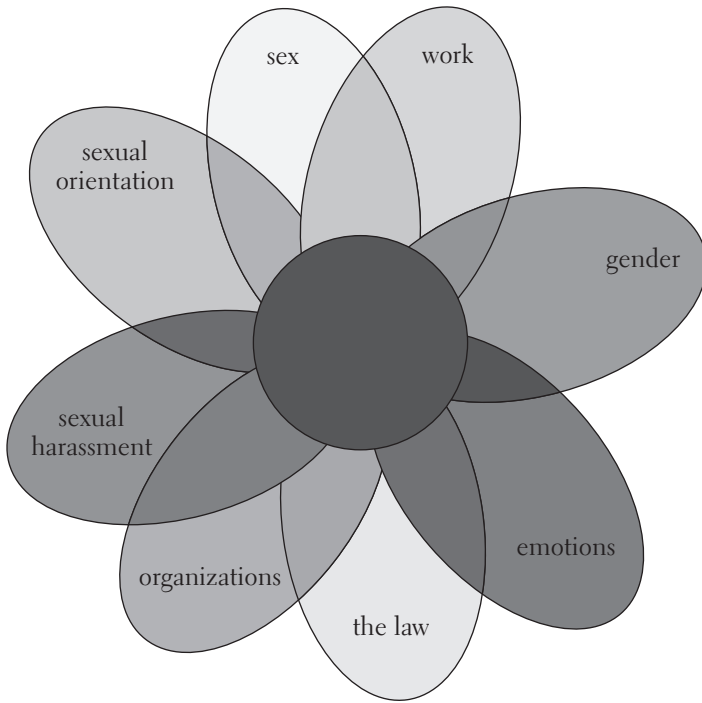
To expand this idea into finding a frame for your work, you need to write down all the things that your case study is about. To use the "flirting in the workplace" example, a study about flirting in the workplace is (or could be) about:

- Sex
- Sexual orientation
- Gender
- Work
- Organizations
- Emotions
- Sexual harassment
- The law

Then put all of these areas into a Venn diagram. Unlike Barbara Christian's lovely and neat example, yours (ours) looks more like a bedraggled daisy.

We, like Christian, are looking *ONLY* at the intersection of the sets, that is, the places where the relevant literature addresses sex AND sexual orientation AND gender AND work AND organizations AND emotions AND sexual harassment AND the law. Well, guess what? This is probably what the symbolic logic types call a "null set"—there isn't anything in it, or at least not at this writing. *Of course not!* You are the pioneering person writing in this area. But there will surely be some intersecting sets where two or more of the petals of the daisy overlap: sex and gender, perhaps, or sex and work, or sex and organizations and the law.

What the daisy does is to let you focus on where those interesting intellectual conversations adjacent to, but not exactly the same as,



your work are taking place. Now you need to go out and evaluate those conversations in the literature and hook your frame to the smartest ones that are also most relevant to your field. Here's an example: "Most of the research to date has focused on how the law conceptualizes sexual harassment, but it implicitly assumes that (a) sexuality is heterosexual and (b) that it is really gender domination in disguise. The present study will show . . ."

Unfortunately, because the intellectual classification system has broken down to such an extent, you can't just go to your friendly computer, type in "sex and gender and work," and get what you need. This is where you need to work smarter, not harder. Here are some key tips about how to get started *before* you go to the computer:

1. *Find a “nodal point.”* You know how there is someone in your life who *always* knows who has a cheap apartment they are giving up because they are going far away to do fieldwork? Or the one who always knows who has a reliable used car for sale? Or who wants to get rid of a perfectly nice couch? That person is a “nodal point,” meaning that he or she connects many strings of a network together.

Now you need to find yourself the equivalent kind of person in the scholarly arena, sort of an intellectual “nodal point.” Ask yourself this: Who in your department (or adjacent departments) knows about any area that covers two or more (the more the merrier) petals of the bedraggled daisy? Say sex and work? Or sexuality and organizations? This person is an intellectual nodal point. He or she will know who is doing important research in this area, who is considered a “biggie,” what the key ideas in the field are. (Note that this person may not necessarily be a faculty member. Particularly when you are interested in unconventional combinations of daisy petals—work + sex + sexual orientation + emotion—graduate students are far more likely to have thought about those combinations than the faculty have, since other graduate students and young faculty are, on average, more likely to be part of the zeitgeist that gave rise to your interests. On the other hand, older generations are more likely to have the long view, to be able to help you position your work in a wider context.)

So go and talk to these intellectual nodal points who are interested in two or more petals of your daisy. Talk to them early and often. One drawback of having a very smart and pioneering research project, however, is that everyone you talk to will think they understand it too quickly (“What you are *really* studying is . . .”), but listen to any and all advice that this nodal point gives you and just discard what doesn’t float your intellectual boat.

One way I find out what my juicy case studies are about is by talking to smart people. They tell me “What you really mean to say is . . .,” and more often than not, I tell them that what they’ve

just suggested is not, in fact, what I'm trying to say. But in the process of telling my friends and colleagues what I'm *not* interested in, I get closer and closer to articulating in a lucid way what I *am* interested in.

2. *Make friends with a reference librarian.* To my mind, this is the single smartest thing you can do to advance your research. The reason more people don't do this, I suspect, is because of ancient prejudices. When I was coming up as a young scholar, it was thought be faintly shameful to ask a librarian for help. Weren't you a *researcher*? And didn't researchers do *research*? What were you doing hanging out with a librarian? Trying to cheat or something?

Librarians, along with pediatricians, are among the greatest human beings in the universe. One of my colleagues at Berkeley calls them the "pit bulls of democracy"—as our government increasingly tries to hide things from us, librarians are among the few souls fighting back.⁵ They love the thrill of the chase as much or more than you do. More to the point, they are experts in what is now being called "information retrieval and storage." In fact, in my own university, the library school is now part of the computer information program.

If it was stupid thirty years ago to avoid reference librarians, it is downright suicidal now. Information has become a commodity—it is being bartered, sold, and arranged in more ways than anyone except a professional librarian can keep up with. Your job is to *analyze* information; a librarian's job is to help you find it in the first place. So once you have started talking to your intellectual nodal points, go and find your own personal reference librarian.

Information these days is like Europe after the fall of the Roman Empire. It's entirely in disarray: every little principality mints its own money, passes its own laws, speaks its own language, and has its own rituals. Imagine trying to be a trader selling your wares as you go from town to town, each one under the sway of another prince (or princess) and his/her personally tailored ways of doing business.

It's the same thing with information. It is being sold, purveyed, arranged, and contracted by so many different "vendors" that you couldn't begin to name them if you wanted to. More to the point, some of them are "full text," some of them have their own idiosyncratic subject headings (often known as a "thesaurus"), and they all have their own rules. Take "truncation," for example. If you want to search for "adolescent pregnancy," for instance, and you want to include the words "adolescent," "adolescence," "adolescents," and "pregnant," "pregnancy," and "pregnancies," most systems will let you "truncate," that is, use some form of adolescen* and pregnan* which would include all six terms. This is sometimes called a "wild card" convention and will permit you to look at all phrases that have the letters before the asterisk, hence pulling up all the terms you are interested in. In the systems I regularly use, truncation is signaled sometimes by an asterisk (*), sometimes by an exclamation point (!), and sometimes by a tilde (~). You can't just take your conventions from one system to another. Only a reference librarian can teach you the tricks of the various systems and direct you to the ones you most need.

I often say, and I am not entirely joking, that you should court your reference librarian as you would court your future (or present) spouse. They are as overloaded as anyone else these days, all the more so being the pit bulls of democracy. So say "thank you" often. Write thank-you notes. Bring them coffee and cookies and chocolates. When they help you a lot, write a letter to the head of the library about how inventive, creative, and helpful this person was. *Always* thank them in the "acknowledgments" section of your book or articles. Behind every good research project written by a salsa-dancing social scientist stands a great librarian, and maybe even a phalanx of them. Keep this in mind as we navigate the next tips.

3. *Find a "synthetic article."* Now that you've narrowed down at least what the questions adjacent to yours are, and found yourself a good reference librarian, you can go look for what I call a "syn-

thetic” article. You have a short list of places to start, so look to see if someone has written about those places where at least two or more of the petals on your daisy overlap.

I’m a great fan of the *Annual Review* series.⁶ As the name implies, these are journal-like articles which, rather than being bent on bringing new information to the reader, provide their own reviews of the literature on important, controversial, or emerging fields. As we will see later when we come to your own personal review of the literature, they are not just recitations of what’s out there. *Annual Review* articles are deeply theoretical, showing you the topography of an area, or sketching out a new frontier of an emerging idea. If you are lucky enough to find an *Annual Review* article which covers two or more of the petals on your daisy, or covers an adjacent topic, you can be sure that if you read the cited material, you will be in command of the key literature in this area. You will probably also get some smart ideas about what kinds of things we *don’t* know (but should) in this area, ideas that cannot help but make your own research better.

Here’s another tip that I spent far too many years of my life ignoring. Every social science discipline has a bevy of encyclopedias and dictionaries dedicated to that very discipline, and on top of that there are a number of general encyclopedias of the social sciences. I like to keep a couple of these paperback dictionaries on my shelf, because you’d be surprised how often I think I know what I mean when I use a social science concept, and then when it comes down to operationalizing it for something I’m researching, I find that I don’t. In addition, the “work smarter, not harder” mantra can be deployed by using these dictionaries and encyclopedias to get a smart overview of the development of the literature in either a theoretical or an empirical area. Of course, these tools are just a starting point, but I, at least, do much better when I have a road map and I know how and where to plug things in. That’s what these resources are for.

You don’t need to buy all of the many dozens of dictionaries and

encyclopedias that might be on point. The reference section of your closest research university will have a shelf of them, and what you don't find in one encyclopedia, you'll likely find in another. Better yet, since you are a boundary-transgressing type of person, you will find, shelved not that far away from the encyclopedias and dictionaries dedicated to your discipline, dictionaries and encyclopedias for the other social sciences—political science, politics, anthropology, and the like—so you can “contrast and compare” if you want. By the way, any good dictionary or encyclopedia will have a bibliography attached to each entry with the most classical works cited there, so if you want to find out, for example, where Anthony Giddens first explored “structuration theory” (the idea that social structures and individuals are reciprocally and mutually constituted), you can find that out, too.⁷

Because the entries are so brief, I have to admit that reading just *one* dictionary or encyclopedia is frustrating, but after I've looked at three or four, each of which looks at a concept or a person from a slightly different angle, I feel that I at least know where to start building my own road map.

4. *Find a relevant journal article, or articles, or books.* Remember when I told you earlier that journal articles had to review the literature in an economical way and thus cite only the most accepted sources? Use this fact to your advantage. If your nodal point or reference librarian, or your encyclopedia or dictionary, refers you to an article or book, go and read them. If there is an *Annual Review* article which is helpful, read all the literature it cites, and—working smarter, not harder—glean from this *Annual Review* article which articles are being cited over and over again.

These articles in leading journals in your field will tell you two things. First, if you read the “introduction” part carefully, you will get a feel for how published articles in areas that you care about are framing related questions. What kinds of frames are being used? How do they introduce the question? How would you slide your

topic into the intellectual conversations(s) that these articles are addressing? Second, because of the implicit guidelines that shape canonical social science, the review of the literature in these articles will tell you what is considered the *crème de la crème* of “the literature” in that particular area. Once you have read these articles, and absorbed their frames, you are well on your way.

Books, in contrast, being less attached to the “normal science” model of scholarship, are generally more leisurely in their attempts to review the literature, but more often than not, they will also be more theoretical.⁸ More theoretical, that is, not only about the substantive material that the book is about, but more theoretical about the literature itself.

To take just one recent example that has crossed my desk lately, Gene Burns has written a book, *The Moral Veto*, about the role of the Catholic Church historically and at present on such touchy topics as birth control, abortion, and so forth.⁹ (As you may gather from the title, he argues that the Church has expressed its opinion on these matters by exercising a “moral veto.”) The point for us here is that if you happen to be interested in social movements, Burns provides a splendid, smart, economical review of social movement theory in just a few pages in his book. It’s like taking the best and smartest professor’s seminar in social movements, and all you have to do is read a few pages! Now you may in fact disagree with the way he thinks about “the literature” in social movements, but what you have after you’ve read this section of his book is a road map for understanding that literature, and you can plug in your own landmarks, even redraw the map if you want to.

Burns is not alone. Many (one would hope most) books by people who really love what they are writing about will have these kinds of smart, broad-reaching overviews of the material that they think is important. If you ask around, you will find a treasure trove of books that can educate you about a particular area. Books that do this very well are often, like Burns’s book, outstanding books in other

ways, so the “greatest hits” in your field, if at all relevant to your question, may well have these kinds of concise, succinct reviews of the literature.

Unlike journal articles, books like Burns’s will give you more of a theoretical road map of the literature. Thus books, *and dissertations in particular*, are very good places to get a “looking down from the mountaintop” view of a particular body of literature. (If you didn’t know this already, a good place to find out if there are dissertations in your area is to check the University Microfilms—now known as Digital Dissertations-ProQuest—for materials that match the overlaps of the petals in your daisy.)¹⁰

5. *Use your discoveries to track down the key suspects.* Let’s say that someone—a nodal point, a friend, your professor—happens to steer you to a book or an article that *really* moves you intellectually. Gosh, if only you could have written that book or article! Boy, (or girl), does this person understand where you are coming from! Here is an intellectual kindred spirit, one you could just read all day.

Should this lucky accident happen, there are several ways to make sure that your excitement doesn’t stop there. First, you might try to write or e-mail the author of that book or article. Now, don’t get your hopes up. Most teachers and researchers are quietly going crazy these days with overload and a speedup of the pace of things. Where once I carefully answered questions about my research, and engaged in wonderful correspondence with long-distance colleagues-to-be, I mostly don’t these days. For me, given that I was trying to mentor my graduate students in the way they deserved, raise a family, keep up with the literature, do my own scholarship, and be a good citizen in my department and university, something had to give, and what gave was thoughtful e-mail exchanges with other scholars. But not everyone has my priorities, and rumor has it that some people actually have wives to help with the “second shift.”¹¹

On the other hand, given how rare that intersection of particular petals on the daisy probably is, you and this other person may be the

only two people on the planet interested in flirting in the workplace, or the privatization of water, or whatever your passion is, and he or she may be as excited to hear about you as you were to read his/her work. So give it a try.

If that doesn't work, however, you can still use this key book or article as an "open sesame" to the place where other work like this may be hiding. If this wonderful piece of work is a book, then we have one of the few places where that beloved but overwhelmed system of intellectual organization, the Dewey Decimal System or the Library of Congress Subject Headings, can help you. (For the rest of this example, I will assume that it is the LCSH that you will be using, since that is more common in research universities.)

Remember I said earlier that while almost everyone has used the LCSH system, practically no one uses it well? Here's an example of just what I meant. When you find a book that is so on target you just can't believe it, that book has a hidden (well, not so hidden, but mostly overlooked) code tucked within it which will direct you to the place where other books like it are stored. (In most cases, that means "digitally stored.") Again, harking back to our earlier discussion about how once upon a time, actual human intelligences arranged information into logical and orderly categories, it turns out that for books, they still do!

If you call up the book that you have found so interesting, and ask for the "full record" of that book, you will find (often hyperlinked) all the Library of Congress Subject Headings that apply to that book. Now, you can do one of two things. You can use those terms to refine your own daisy, knowing that at least as far as books are concerned, you will be using the most appropriate terms. And, better yet, you can click on all of those subject headings which seem relevant to you.

As an example, let's take the flirting at work case. When I put in the title of a book that seemed on point, and clicked on the "long record," I found a new Library of Congress Subject Heading I hadn't

known about, "Sex in the Workplace." Clicking on that gave me four more titles located in my UC Berkeley library. Then I did something that I recommend to everyone who has Internet access:¹² I put that same subject heading into OCLC: WorldCat (a consortium of research university libraries) and got 353 more books. If I were in a more adventuresome mode, I would have moved on to the Library of Congress itself, which by congressional decree holds every book published in the United States, and a lot more besides.

It's a little more complicated, but you can do the same thing with journal articles, sort of. Because journal articles build on the model of normal science, you will find that your key article, if it is as central as you think it is, is cited again and again in subsequent journal articles, most of which will presumably in some way be addressing the material or question which you are interested in and which this article is about.

It turns out that you can track who is citing the article which excites you, provided, of course, that it wasn't published just this month. Using the ISI Web of Knowledge, you can enter what used to be called (and somewhere in this digital terrain, still is) the Social Science Citation Index. You put in the name of the person who wrote the article, the journal where the article was published, and presto, you get a list of all the people who have cited it. Those citations are, in theory, related to what you are interested in. In addition, in my own library at least, I can access the full text of some and maybe many of those citations by just clicking something called "e-links."

In much the same way, when I access the electronic journal database known as JStor (www.jstor.org), which most research libraries subscribe to, a pane to the right of the article accommodatingly shows me, via Google Scholar, all of the articles that cite the article I am reading. Once you have found one way into the forest of "the literature," the job just gets easier and easier. You can follow the trail of bread crumbs (like Hansel and Gretel) to find almost all of the people who are interested in that small center section of your daisy.

And if you keep good records (see the next tip), before long you will have a very good sense of who the most-cited “experts” in your area of interest are, not to mention a good feel for what the fights and debates are.

6. *Keep good records.* This one should be obvious, but you’d be surprised how long it took me to find it, and how grateful even my graduate students are to be reminded of it. For many years, especially with the explosion of info-glut, I never kept very good records of where and what I searched. Oh, sure, I kept good records of what I *found*, but not what I was looking for, nor where I had found my prize items.

Big mistake. There are certain elaborate searches that I have done at least three times, if not four. And no one has that kind of time. One of my colleagues told me about a great website, designed by the librarians at UCLA. (Didn’t I tell you that librarians are the nicest people on the planet?) It’s an incredibly helpful, user-friendly guide to doing research, called “Bruin [that’s what UCLAers call themselves] Success with Less Stress.”¹³ Along with terrific material on intellectual property, and how to use the various kinds of information you can find on the Web with discernment, they also suggest creating a Search Log to remind yourself of how and what and where you searched, and what worked and what didn’t. In short, you need to write down the database, the search terms you put in, whether or not the search was successful, and ideas about what to do next. Start doing this tomorrow and you will thank me later, I promise. (I have provided an example of a blank search log in Appendix Four.)

7. *“Harvard,” don’t read.* A dear friend of mine went to Harvard, and within the very first week of his time there, he discovered that the faculty gave (and probably still give) poor unsuspecting students more reading assignments than a human person could ever read. It took most of them just about a week to figure out that it couldn’t be done (the rest had nervous breakdowns), and the smart ones learned how to work smarter, not harder.

So I married this very dear friend of mine, and that is how I found out about what I call “Harvarding.” You cannot imagine how depressed I used to get before I learned how to “Harvard,” confronted with stacks of books four feet high, knowing that I would never get around to reading them all, and feeling like a failure. Often enough, confronted with such a huge pile, I just took a nap.

From here on out, you must never, ever read a book again unless you have “Harvarded” it first. If you are like me before I learned to “Harvard,” you read a book with painful intensity. You underline things; you put notes in the margins; you take notes on your computer or in longhand. And you move very, very slowly. It was not uncommon for me in the old days to spend the better part of an afternoon on ten pages of a book, unless, of course, I fell asleep first.

This is nonsense! Very few books deserve that kind of attention, probably including this one.

Even the relatively few books that are well written and to the point are often hard to follow, because it’s a great leap of imagination for the author, once she has been immersed in an area for many years, to figure out what her reader needs to know. At this point, the author probably knows *everything* there is to know about the topic, and sad to say, most academic authors seem totally incapable of making that leap of imagination. (A mistake you will not make in your own book!)

So here is your mantra, which I want you to repeat before you sit down with any book, including this one: “If I’m not getting it, it’s her (or his) fault.” If you’re like me, you flagellate yourself when you read a dense, badly written book, especially if it’s one that your colleagues hold in high regard. Such a book has failed in its most elemental job, namely, that of bringing you to the point where you can understand what’s at issue. (In all fairness, in a globalizing world of info-glut, I suspect that *no* author can truly connect with all possible readers, but I’m struck by how rarely most scholarly authors even bother to try to connect to any readers, except three or four that the author considers his or her peers.)

For these reasons, this is the time when you should “Harvard” a book. You know what your question is, because of the previous steps. You know what the adjacent areas are because of your “daisy” diagram. You have found a particular book in one of your searches, and you should not spend very much time on it unless it is totally, entirely, on point for your project. By being on point, I mean two things specifically: it has a theoretical “frame” that could be useful for you at least in one section, and/or it has some empirical data that could be useful.

So how do you find out whether the book has a theory or data that would be relevant to you? You “Harvard” it. You look at the Table of Contents and the Index, focusing your laser-like attention on those topics closest to what you care about. You skim the introduction and conclusion. You skim the chapters that might seem relevant. If, and only if, this book seems to be exactly what you were looking for, come back to it; but for the moment, treat all books as if you had only twenty minutes to get everything useful to your study out of it, and then it will disappear in a puff of smoke. Make a book *earn* more of your precious time.

Sometimes, by the way, a book is held in very high regard, and you can’t figure out why. Chances are that it either solved a theoretical problem in the field or advanced the state of play in an area. But it’s a little hard to figure that out when you are a newcomer to the territory.

What to do? My advice is to put the title of the book into JStor’s search engine, and restrict the search to reviews. Read four or five reviews of this book, if you can find them. A good review will not only tell you what the book is about, but it will also usually tell you why the book matters (or doesn’t). However, academics, like everyone else, get tics about things, so it’s important to read three or four, or ideally six or seven, reviews. At the end, you will know how this book fits into the literature, and you can come back to it.

Once you’re back to it, you can still “Harvard” this highly regarded book, if you like, but you can also use it to clarify how your own

thinking differs from the author's, and whether any of your data contradict hers or his.

While we are on the topic of "Harvarding," I'd like to suggest a great resource. Reading is a key practice in academia, but, as this section has made clear, "reading" a book can mean many things. I advise you to go out this minute and buy yourself a copy of Mortimer Adler and Charles Van Doren's *How to Read a Book*, originally published in 1940.¹⁴ Adler and Van Doren have some great tips. One that I will be forever grateful for is the idea of writing your own index on a blank page in the back of the book or elsewhere for ideas or data that really grab you, so that you can find them again when you need them. How many times have you needed to know where an author discusses (fill in the blank here), and you can't find it in the index and you really, really need it? This will not happen again once you learn to write your own index.

8. *Be kind to your readers.* Let's jump ahead some months into the future. You have carefully refined that neat area within the heart of your daisy; you've got a much clearer sense of the various frames that other people are using to address questions like yours; you are happily corresponding with people who are similarly interested; and you've discovered a number of relevant and exciting books and articles. You have "Harvarded" the relevant books and articles, and you have come back to a very, very few and have read them carefully and thoughtfully.

Congratulations! But keep in mind that this is just the first of many times that you will be doing a review of the literature. As your project progresses, and you gather data, it's entirely possible that your frame will shift as well. (T. H. Huxley speaks of the "tragedy of a fact killing a theory," and the same thing happens to frames. Once you get into the field, and find some actual, factual data, you may well shift your frame.)

But let's say, just for argument, that you haven't shifted that much; you have gone out and gathered your data, and you are now at the

happy place where you want to write up your book. (Remember? You are writing a book, no matter what anyone else says or thinks.)

Eventually you will come to the point where you have to provide an official review of the literature, one that is written up. (In fact, in dissertations, it is traditional to have a whole chapter on the review of the literature. I don't always agree, but I do think that *somewhere* you need to show how your work advances the state of play.) What I'm about to say may seem totally obvious, but you'd be surprised by how many of my very smart students get a block when they come to writing this part up.

Here's the flash: *you don't have to write about every book and/or article ever written that is remotely relevant to your question.* You only have to do what Gene Burns did when I recommended him to you as a model, namely give readers an intellectual road map of the existing literature in a smart and critical way, and show us that, however elaborate or intelligent or extensive it might be, that literature doesn't really answer the question that your book will answer for us.

I also find myself wanting some information in the "review of the literature" that is not considered canonical. Because I argue throughout this book that it's important to talk about our work as an example of something larger, I really want to be situated with respect to that "something larger" early on in your book. I want to know how much, how big, and how often. And I want to know, right at the outset, why I should care. So back to our example of flirting at work. Do we have any idea how much flirting at work goes on? (Probably not, since this is such a fresh take on a topic.) But are there any data, no matter how incomplete, that would give me a sense of the scale of the phenomenon? How many people work in gender-integrated jobs where there are both men and women? (Yes, I know, even when a work force is 99 percent one sex and 1 percent the other, there can still be flirting.) How about sexual harassment cases, which we might think of as flirting gone awry? How many of these are there?

Finally, somewhere in this review, I want to be reminded why I should care about your question. If your case of particulars illuminates something about the more general world in which we live, make the connection for me. Show me why I, who may not have any particular interest in flirting in the workplace, should know about this as a good social scientist. I know that the first chapter of your book, the one entitled “Introduction,” “motivates” the research for me by telling me why your question passes the “so what?” test. Here, just refresh my memory.

That’s it. If you’ve done all these steps, and maybe more than once, you’ve done your review of the literature.

Exercise for Chapter 5

You guessed—the exercise for this chapter is to draw yourself a daisy. I know I asked you to do this earlier in the chapter, but I bet you didn’t. Here’s where you get to do it for the first time. Put in all of the items that you think your study covers as petals of the daisy, and then see where there are overlaps that someone is writing about or has written about. (I’m taking for granted that there probably won’t be an area in the center of your daisy that lots of other people are writing about, but I could be wrong.)

If you really want to get a head start on your book, then label this daisy something like Daisy 1.0. Then update your daisy as your work goes along, and you’ll get to Daisy 2.0, 3.0, and so on. Writing the “review of the literature” section will be a cinch.



On Sampling, Operationalization, and Generalization

So now you have a case, or perhaps a wonderful and vexing research interest, and by proper application of the disciplines and exercises in the previous chapters you've begun to coax it into something resembling a research question. You are starting to have an idea of how you are going to frame your research in terms of the intellectual conversations you are interested in (and the subdiscipline you want to be employed in), and you have developed an acquaintance with what *you* have defined as the relevant literature.

The next step is to get some data. But how do you figure out *what* data you need? And how do you figure out *where* to get it? (The question of specifically *how*, that is, what kind of data-gathering *method* you will be using, comes in the next two chapters.) In order to do this part of your research project, we have to go back to the practices we've borrowed from the canonicals—sampling, operationalization, and generalization—and discuss them in some detail.

Sampling

I will repeat myself here because this is so important: when canonical social scientists say the word “sample” what they really mean is a “systematic random probability sample,” one drawn from a population where each and every element has a statistically equal chance of being chosen. Many canonical sociologists, by the way, don’t actually sample, but they do perform secondary analyses on data drawn from random probability samples.

An old saying goes that people should never look too closely at how laws and sausages are made, and in that same spirit, random samples when looked at closely are not all that they are cracked up to be. Because national random probability surveys are expensive, many social scientists use the surveys undertaken by the federal government or large organizations such as the National Opinion Research Center (NORC) who have the means and motivation to do such studies and to repeat them year after year. To give you a brief flavor of what I mean, the Feds undertake such workhorses of social research as the Current Population Survey (CPS), the National Survey of Families and Households (NSFH), the National Survey of Family Growth (NSFG), the Youth Risk Behavior Surveillance Survey (YRBSS), and the Survey of Income and Program Participation (SIPP), to name just a few. As a result, many social scientists only do secondary analysis of these big datasets. BUT, and this is a big but, you are then limited to what *they* wanted to ask about. One of the examples I often mention is that the General Social Survey, an ongoing survey on public attitudes carried out by NORC since 1972, has codes for only three races for most of its history: white, black, and “other.” Particularly for my students here in California, who live in a multiracial state, the idea of limiting “race” to these three values makes pretty clear to them that surveys have their limits.

What all of this means is that when you, as a case-oriented researcher, talk about a “sample,” these kinds of folks are likely to

give you the fish-eye. They take for granted that you mean a random probability sample, even though that's not at all what you had in mind, and because you are both using the term "sample," you both think you are talking about the same thing when you're not. So although you and I will be talking about a "sample" in the following pages, you should keep in mind that while I have ample epistemological and historical backing for my use of the word, it is one that you must use very carefully (and with full explanation) around canonical social scientists.

How do we sample? In fact, all of us sample in the more general sense of the word every single day. You don't have to sit through the whole movie to figure out it stinks, you don't have to take an entire aerobics (or salsa dancing!) class to know you love it, and I bet every single one of us has nibbled a little hole in one piece of candy in a box of chocolates as a way of finding out which ones have the icky cherries inside.

So we will sample, too, because there is no way we can gather all of the possible bits of information that would illuminate our research question. But while we can't do it the way the canonicals do it, and we don't want to do it as mindlessly as someone nibbling corners off chocolates, we do have to do it, and the more mindfully the better.

Let's keep in mind why we want to sample in the first place. Obviously, the first reason is that all of us have limited time and energy, and real life in real time is lots of noise and not very much signal. Quantities of information wash over us at all times, and we have to decide what subset of facts, observations, people, and so on we will pay attention to. So we have to sample, whether we like it or not.

Recall that the canonicals sample in order to take advantage of some brilliant work done at the end of the nineteenth century in probability theory and statistics. Canonicals sample so that they can *generalize*. If 6 percent of their sample do X, they can predict that 6 percent of the population (i.e., the group from which the sample

was randomly drawn) will do X , plus or minus an error term. Thus, having randomly sampled, the canonicals can draw on a huge body of elegant mathematics known as “parametric” statistics to estimate the likelihood of X in the population as a whole.

At the risk of belaboring the point again, I want to remind you that canonicals want to know the *distribution* of a *population* among *known categories*, as estimated from a properly drawn sample with a known error factor. We salsa-dancing researchers, on the other hand, want to discover the *relevant categories at work*, not the distribution of some larger population across categories that we have a priori chosen. We have turned to our kind of research because we have a question that canonical social science can’t take on, or can’t take on very well.

Here is another metaphor, if you will bear with me, about the differences between the two kinds of sampling: some years ago I trained my golden retriever in Canine Search and Rescue (K-9 SAR). There are two very different kinds of K-9 SAR, and it turns out that they are pretty good metaphors for the ways that field researchers think about sampling compared with survey researchers.¹ In wilderness search and rescue, the terrain is carefully divided into grids, and a dog and handler team together search each section of the grid and report back as to whether they have located any scent or item that suggests that the lost person passed through. In earthquake search and rescue, however (which is what my dog and I trained in), it would make no sense to divide up the terrain into grids. If someone is buried in rubble after an earthquake, you know without much thought that you should be looking in places where there are (a) buildings (b) in a state of collapse and (c) where people are likely to have been inside at the time of the quake.

Obviously, as my story makes clear, both kinds of sampling (a.k.a. “searching”) are necessary and proper in the right circumstances. When we want to know the distribution of a population among known categories (that is, when we are trying to answer the kind of

question best approached by survey research), in fact we do want to draw a grid and search systematically, and that's what drawing a random sample in effect permits us to do.

But in some cases, we may either have prior knowledge to the effect that the thing we are looking for is not equally likely to be in all parts of the field, or, as often happens, we have actually found something, and don't exactly know what it is we have found. In either case, what we are dealing with is what I call a *data outcropping*. I often tell my students that if you are looking for ancient fossils, it probably makes no sense to do some kind of a survey of a vast swath of terrain. Rather, you are better off looking for them in a setting where others have already found the kinds of fossils you are looking for. Instead of doing an aerial survey of Indiana, say, you'd be much better off going to the Olduvai Gorge in Kenya, or the Gobi Desert in Mongolia.

So, given that we are looking for a data outcropping, where do we look? The answer to this question, in turn, relates to *why* we sample in the first place. To put it simply, I would argue that we sample for the same reason that the canonicals do—so that we can tell a bigger story. (In more formal terms, we hope to be able to tease out some generalities about social life from our particular research.) So our task is to find a case or set of cases that is (or are, when we have multiple cases) reasonably representative of the larger *phenomenon* that we are investigating. Listen carefully: not representative of the larger *population*, but of the larger *phenomenon*.

Two different kinds of samples grow out of the two different ways of doing salsa-dancing research. The first way, the one that has probably been most common to the book so far, is where you have a juicy case study and know in your heart of hearts that it tells us something important about the social order, but you just can't put your finger on it yet. In this kind of case study, your first stage of sampling has already happened. You have your case, and you want to get started right away in the field. And you should. But this does not, alas, ex-

empt you from the trouble of sampling. As you continue to work on your field site, you find that it is a *theoretical* case of some important social process. (At this point, I remind my students of the question that came up when they first proposed the site, namely, *what is this a case of?*)²

As you continue to work in the field, you may find that it is a case of something like Michael Burawoy's "Zambianization," where political pressures (concretely, black nationalism in a newly independent country) forced the country's copper mines to take on African managers and directors. At the same time, however, the government's desire to keep the mines profitable, combined with racially based taken-for-granted practices, meant that the elevation of such workers to positions of power and authority was paralleled by a set of processes which stripped power and authority from these positions, now newly filled with Africans.³ What this is a case of, then, is what my legal colleague Reva Siegel calls "persistence through transformation"—the idea that when a system is disrupted, it will tend to reconstitute itself in ways that recreate previous positions of power.⁴

If you want to make absolutely sure that you have found what you think you've found, you might well want to sample another case, either formally or not, to make sure that you were looking at all of the key variables. (This is another case of what Barney Glaser and Anselm Strauss call "theoretical sampling.") Given the micro focus of their work, they often mean sampling events or people that would extend your understanding of the theoretical processes involved, but I think, in keeping with our constant commitment to "bump up a level of generalization," that you might want to think about another case of "persistence through transformation" to see if your theoretical insights hold up.

This will come up again when we look at the topic of historical and comparative methods, but for the moment, I want to argue as we discuss sampling that *all* the work that you and I are contemplating doing is *inherently* comparative, and our sampling demonstrates

that. For all practical purposes, you can conduct your comparisons in any of three ways.

First, you can compare what you are finding in your case to the conventional wisdom. For example, you might look at Robert Michels's prediction that organizations, even radical ones, will tend toward becoming oligarchies, more interested in meeting the needs of the organization than in pursuing the social goals that gave birth to them. But wait, you say: Here's an organization (say, a labor union) that does *not* seem to conform to what Michels called the Iron Law of Oligarchy. Thus your comparison is with what Michels predicted was an "iron law," and your task is to discover what it is about your case that makes it different. (If you want to see this played out, you can look at either Seymour Martin Lipset's classic *Union Democracy*, or Kim Voss and Rachel Sherman's recent "Breaking the Iron Law of Oligarchy.")⁵ Either way, conventional thinking, as embodied in classical works of sociology, is your comparison.

Next, I wasn't being sloppy a few lines ago when I said that you might want to "think about" another case; I actually meant it quite literally. The second way of sampling in order to do a comparison is what I sometimes call a "tacit control group," that is, a case that in certain respects allows you to test your theoretical understanding of what is going on.

In my first book (which was a revised version of my dissertation), I came up with an algorithm that explained why people who had previously used contraception in the past did not use it subsequently, even though they did not necessarily want to become pregnant. My sample was a group of women seeking abortions in the San Francisco Bay area, in a period of time when abortion was legal in California but not nationwide. I concluded that women seeking abortions by definition did not want to have a baby, at least at the time when they were seeking the abortion. (In fact, many of them did want to have a baby, but that "wanting" got revised in the face of later events.)

If my theory explained the decision-making around what I called

“contraceptive risk-taking,” that is, not using contraception you had used previously to prevent a pregnancy that you did not want, then I figured that other women with pregnancies that they presumably did not want would also fit into my theory. I figured that a group of women living in a home for unwed mothers (as they used to be called) would also be a group who did not actively want their pregnancies. (This was the bad old days, when a considerable amount of shame and stigma was still attached to unwed motherhood; in fact, many of these young mothers in the home were there because their families had thrown them out when the pregnancy was discovered.) I did interviews with a number of these women, and sure enough, the variables I had found in the abortion group worked in understanding the decision-making chain of how the unwed mothers had come to be unwed mothers. Although these interviews never showed up in my dissertation or in the book that grew from it, I was much more confident in my conclusion that there was a socially produced and rational set of risk-taking behaviors common to the two groups.

Finally, the third way of doing a comparison is to do a formal, *theoretically driven* comparison. If you go back to your research question, as it is evolving and taking on more formal theoretical properties, you will notice that you are beginning to argue something along the lines of A is related to B, and helps make it happen, and that A and B are a *case of something*. (Notice how I’m avoiding the complex epistemological issues of “cause” and “causing” here?)

But because you are making a *theoretical case* and using *theoretical sampling*, you need, once again, to bump up to that higher level of generality. Whatever it is that you are arguing about A and B, you need to find another case of A and B, and explore whether or not A and B are related in the way that you think they should be, given your case.

This brings us to the second way that you can do a research project. You can have a question that is sociologically interesting (and,

one hopes, important), and your task is to find an exemplar that lets you examine in close detail how that question gets played out. (For more on this, see Appendix One, “What to Do If You Don’t Have a Case.”) Take, for example, the perennial question of how working-class kids get working-class lives, more formally known as “class reproduction.” Annette Lareau answered this question by virtually living with school kids, black and white, male and female, working class and middle class. Her short answer is that middle-class parents (both black and white) inculcate a set of “practices” into their children’s daily lives, practices that intersect well with the school culture. Working-class parents inculcate other practices which lead their children to feel alienated from and hostile to school, that “ladder of ascent.”⁶

How did Lareau “sample”? She selected two schools, one in the wealthy suburbs and one in the inner city, and chose students who would permit her (and her team) to observe the kids, observe the families, and even spend the night.

Now there are lots of questions that could be raised about how she sampled, including whether or not there was a selection bias resulting from who was willing to let a researcher observe them for days at a time. But Lareau’s explanandum (class reproduction) and explanans (parental practices) show no obvious relationship to whether or not parents would permit observation, so, while this is the kind of thing that Monday morning quarterbacks like to bring up, I would argue that the burden of proof is on the critic to show how a selection effect (which is, after all, what we are talking about here) would have changed the *theoretical* case being made.

Lareau chose a setting that was intuitively generalizable, since we presume (rightly or wrongly) that public elementary schools are pretty diverse. It’s true that we do not get to see the upper-class children who go to private day schools, nor the children in parochial schools, nor that growing and mixed bag of children who are home-schooled.⁷ Still, this setting is as diverse as one could hope to get un-

der the circumstances, and a would-be critic needs to show how the findings would be different (not just extended) if Lareau had done her study in Tulsa, or Toledo, or had included these other groups of parents and children.

But what about you? What if you have a really important question, in contrast to a juicy case study, and you have to choose a place to explore that question? Let's go back to flirting in the workplace. There are millions of people employed in thousands of workplaces—how are you to choose among them? Or say that you want to study how parents exercise school choices for their children. Again, at last count, there were approximately 55 *million* school children in the country, distributed across public, private, and parochial schools, not to mention all the children who are being home-schooled. Where do you start?

Here are some guidelines that I've found helpful in choosing a "data outcropping" that will permit you to think about the kinds of questions I've just posed.

First—and it may sound obvious—you need to have a setting where the variable that you are trying to explain *varies*. You probably have already intuited this, but if you want to study (for example) the privatization of water, then you probably need to find a community where water is being privatized and, ideally, one where it is not. Likewise, if you wanted to study how parents choose schools for their children, you would not be well served to interview only the parents of children in parochial school, because they are parents who have already made a specific choice; and while you might want to interview them about how they made this choice and not another, this is not exactly the question you started out with. (You wanted to know how *parents* make decisions, not how parents of *parochial school students* made that choice.)⁸

The second guideline is what high-flown literary types call "synecdoche," which is where the part stands in for the whole.⁹ In other words, to take the case of flirting at work, we may be telling the story

of sixty-four people in a high-tech workplace somewhere in Silicon Valley, but we want these people to stand in for—imaginatively at least—a much larger group of workers who find themselves in workplaces more or less similar to the ones we studied.¹⁰

This second guideline rests on what I think of as a moral obligation. If we are going to present our case as being meaningful about something other than the life events of sixty-four particular people in California's Silicon Valley, or one small town in Bolivia, we are duty bound to make sure—or as sure as we humanly can—that in fact our people are likely to be, as far as we can tell, reasonably representative of a lot more people than just themselves. While we can never *statistically* prove that they are, we can—like Lareau—*logically* show the ways in which they do (or do not) resemble people elsewhere, and why we have theoretical grounds for thinking that it does or does not make a difference.

Which brings us to guideline number three: let theory tell you how to sample. This one is a bit tricky, but I have already confessed that I personally am not a follower of Grand Theories. So your task (and it goes back to the question of framing your research question—you *did* do the exercise for that chapter, didn't you?) is to let the array of theories that you considered when you wrote that framing exercise drive you. In our flirting example, let's say that by careful reading of the organizational literature (which, incidentally, contains almost nothing on flirting) you discover that there is a consensus that high-tech firms are "flatter," less hierarchical, and, in California at least, more laid back than other companies.¹¹ Moreover, good high-tech workers are in high demand, so we might expect that managers will look the other way at flirting unless it seems likely to lead to a lawsuit or lowered productivity, in order to keep scarce and valuable workers from leaving for greener pastures.¹² Well, you reason to yourself, if there is anyplace on earth that is likely to be open enough to tolerate quite a bit of flirting on the job, then high-tech firms are likely to be that place.

Now keep in mind that you are *not* making claims about workers on the assembly line, or in mines, or in service occupations like waiting on tables. In fact, to be perfectly clear, you are not making a *statistical* claim about either the workers you studied, or workers in any high-tech firms other than the ones you studied. But if you were reasonably diligent in making sure that you talked to a wide range of people in each of the firms, and kept talking to more and more people until you were not learning anything new, you can, if you want, make a *logical* claim, namely that barring unforeseen complications, your analysis of how these sixty-four people in high-tech workplaces think about, reason about, and decide about flirting is likely to be a good starting place for other researchers wanting to investigate how professional people in organizationally-relaxed jobs think about, and make decisions about, flirting.

Let me digress to warn you about something. Whenever you find something really, really wonderful in your research, people will have one of two reactions to it. If they like it—or at least if they are not offended by it—they will say, “I knew that!” Well, they do know it *now*, but the odds are they didn’t know it before you did your research. One of the tasks of salsa-dancing research is to elicit the deep meaning structures that people in a given situation hold, and how those meaning structures map across external reality. So often, when we show the deep logic of what once seemed odd or incomprehensible behavior, bystanders often nod knowingly and say, “It’s obvious.” But it wasn’t before you showed them! You just have to live with this kind of criticism, and show by your elegant review of the literature that in fact no one did know this before you published your article or book.

On the other hand, if they don’t like your finding, are nettled by it, or are in principle offended by salsa-dancing research and regard it as useful for nothing but local color, they will announce that your results are either (a) “spurious” or (b) biased. In either case, what they mean is that *the way you chose the sample influenced the outcome*. This is the thing that we must avoid at all costs.

In the face of potential criticisms of obviousness or bias, we have to do two different tasks in selecting a sample, and once again, both are related because the principles are. First, we have to find a rich mine of whatever it is that we think we are looking for. We have our first graders, or perhaps—once again—a village in Bolivia where water is being privatized, or workers in Silicon Valley. But these are, of course, research *interests*, not research *questions*. What is it about the first graders (or the World Bank) that we are trying to explain?

Presumably we are drawn to a case or a question because our own inner conjectural theorizer finds something amiss in a setting. In my own case, I wrote my dissertation because in one four-week period as a volunteer for Planned Parenthood, I ran into three different women who had recently had an abortion and came back to the clinic because they had reason to believe that they might be pregnant *again*. I didn't know a lot about abortion at the time, but I did know enough to know that something was amiss when not one, nor two, but *three* women found themselves at risk of another unwanted pregnancy so soon after having terminated a previous one by abortion. Whatever your attitudes toward abortion may be, you have to admit that the procedure is expensive, disruptive, and, at a minimum, uncomfortable. So why would otherwise reasonable people put themselves at risk for the same experience again in such a short period of time?

I had an alternative conjectural hypothesis, of course, namely that these women were nuts. But the problem was that there were *three* of them. I tell my students that one person who does something totally unexpected (or at least what *you* consider totally unexpected) is a flake; two are a flake and a friend; three are a social phenomenon.¹³

So there was my case (or perhaps, phenomenon)—three women risking another abortion just after they had had one, and I knew from clinic records that each of them had left the clinic with a prescription for birth control pills firmly in hand.

So my tentative question became, why do people risk pregnancy

when we know that they have skills to avoid it? My tour of the literature made clear that scholars thought getting pregnant when you didn't want to was due to either ignorance (demographers and sociologists) or neurosis (psychologists and clinicians). I knew that these women were not contraceptively ignorant in the usual sense of the word, because before leaving the abortion clinic after the first abortion they had obtained both a session of contraceptive counseling and a prescription for pills. I suppose they could have been neurotic (my alternative hypothesis), but there were just so many of them. And not just in my volunteer office. A quick look at California abortion statistics showed that abortions had doubled every year since the law was liberalized in 1967, until the number leveled off at a rate of about 100,000 abortions a year in 1972. That's a lot of neurosis.¹⁴

How would I sample such a question? What kind of setting was both intuitively persuasive and also broadly representative of the larger group of people seeking abortions?

I was lucky. Abortion was still pretty tightly controlled by the medical profession in those days, and there were only a limited number of settings where abortions were performed. I just chose the biggest, which happened to be a Planned Parenthood enterprise, where I already had an entrée.

But before I launched into interviewing people, I took a look at the clinic records. I examined a series of 600 medical records from the previous year, and looked at the patterns of contraceptive use among the women seeking abortion. It turned out that 60 percent of them fell into the category of women I had already met, namely women who had gotten pregnant after a period of successful contraceptive use. I coded as a "previous user" only people who had successfully used some method of contraception to prevent pregnancy at some earlier point in the past, but who had used nothing in the month when they got pregnant. (I had only *assumed* that the three women I had met as a volunteer had access to contraceptive information because I knew, and they confirmed, that they had been given contraceptive counseling after the first abortion.)

So how would I sample? You know from earlier chapters that I tried the canonical way of sampling first, and then gave up. Finally I turned to the Planned Parenthood clinic and started doing interviews.¹⁵

From there, my sampling was fairly straightforward: I interviewed anyone who had a past record of successful contraceptive use and who would talk to me. (And they all did—luckily, I did not have to worry about sample selection.)

Operationalization

In addition to worrying about problems of sampling in all of the methods that we use, social scientists also have to worry about how we “operationalize” our concepts, or “variables” as they are sometimes called by the more canonical types.

This is a lot harder than it looks at first. As the European philosophers have been telling us for a hundred years (and as Foucault has told us in a way that really seems to hit home for a lot of social scientists these days), we can only know things through the medium of the terms we use. (Nietzsche speaks of the “prison-house of language.”) But language and terms are themselves shifty and unpredictable things, because they track changing social practices, and they may well be tracking a practice which is being challenged.

I think one reason that feminist scholars of my generation were so open to what has been called “the cultural turn” is that we saw in very real ways the power of what Foucault calls “discourse.” When we came of age, discrimination based on gender was considered entirely natural. For example, when I was first in graduate school at Yale in the late 1960s, it was considered perfectly legitimate to have a quota on admissions for women, not to let women use the gym except on the three mornings a week when faculty wives could use it, and to conduct university business in an establishment—Mory’s—that would not permit women to enter. Grabbing, groping, and bedding women graduate students and staff were just taken for granted

as the perks of being a senior and distinguished male member of the faculty.¹⁶

In just a few short years after I arrived, perhaps two at most, as the women's movement hit Yale like a hurricane, all of these practices—quotas on women, denial of gym use to women graduate students while male graduate students could use it any time they wanted, making women who wanted to enter Mory's in order to *conduct university business* enter through the back door—had both a name and moral opprobrium to go with it: it was called sexism. Later, thanks to the work of Catharine MacKinnon, the groping, grabbing, and bedding part had a name, too—sexual harassment—and it was actionable.¹⁷

So how we operationalize (that is, define or name) something is very important, and it is even more important to those of us who do case studies than to the canonical types.

I sometimes illustrate this point to my students by asking them rhetorically how much rape there is in the United States. Well, it all depends on who you talk to. Some years ago, the FBI's Uniform Crime Report claimed there were 102,560 completed forcible rapes; the Bureau of Justice Statistics reported a slightly higher number, or 130,000 rapes, in that same year.¹⁸ Christina Hoff Sommers, who is a critic of rape statistics, has added more numbers to the mix: a Harris poll at roughly the same time came up with a figure almost three times higher than either of these, an estimated 380,000 "rape victims or victims of sexual assault"; and the National Victim Center, an advocacy and support group for the victims of crime, reported an estimated 683,000 forcible rapes in 1990.¹⁹

Men are raped, too, of course, but the traditional conceptualization of the crime imagined a female victim and a male perpetrator. More recently, some advocacy groups have come to speak more broadly about "coercive sexuality" to include both a gender-neutral stance and a wider range of behaviors. This is my point entirely.

Rape, and how much of it there is, lies at the heart of its own little

“culture war” between feminists and their critics. Feminists say that rape is so normalized in American society that it is taken for granted, often not recognized as such even by the victims themselves, and wildly underreported. Critics of the feminist view say that rape is a heinous crime, is actually rather rare, all things considered, and is one of the *most* reported crimes of all.

In the last two decades people like Hoff Sommers, Camille Paglia, Katie Roiphe, and Heather MacDonald, while carefully denouncing what the feminist theorist Susan Estrich calls “real” rape, argue that the new rape advocacy has created a victim mentality that leads women to imagine situations as rape that really aren’t. My colleague at UC Berkeley, Neil Gilbert, makes much the same point in a more sober and considered fashion.²⁰

What’s going on here? Is rape relatively common, or is it relatively rare? And why can’t all of these “experts” come up with some figures that everyone can agree on?

The short answer is that lots of things are going on. Some sources, such as the FBI’s Uniform Crime Reports, take only reported cases passed on from local police departments who have judged these cases to be “well-founded,” while the Bureau of Justice Statistics and the other sources listed above do household surveys which include rapes never reported to—or taken seriously by—the police.²¹ But the key thing that’s going on is at the heart of my point about operationalization—how much rape you measure depends entirely on how you define rape. And rape itself is undergoing a political “discursive shift” that even the participants have had a hard time articulating until the dust has started to settle.

Briefly, traditional views of rape grew out of a notion that rape is about women as sexual property owned by men—owned by fathers until women were married, then by husbands subsequently. We see remnants of this view around us every day, although we may not recognize them as such. Fathers “give away” brides in the marriage ceremony; women often go from bearing their fathers’ names to their

husbands' names. Thus, before feminism, rape was a binary category—a woman was either raped or she wasn't, her virtue (or virginity) was compromised or it wasn't. If she was raped, it was a heinous crime—although it was considered a crime against a man's property—and the perpetrator was often punished by the death penalty. But there was a hitch: while men (remember, for much of the period I'm talking about, women were not legal persons) agreed that to “rob” a woman of her chastity was a serious offense, they also agreed that women had just enough agency or wile to entice men into illicit sex. As the carriers of this “sexual property,” women were less like a bag of gold coins carefully hidden away in a secret place and more like cows who tended to wander into other people's fields, creating confusion as to whether they had been stolen or lost.

So in an earlier era it was not uncommon to insist that a rape be witnessed, or at least that the resulting injuries be corroborated, or that women were obligated to resist to the utmost, even at the risk of their lives. Most centrally to the point I am making here, in many jurisdictions only a woman of “previously chaste character” could be said to have been raped. If a woman had previously “given away” sex (or sold it, in the case of prostitutes), it could not by definition be stolen. So if a woman was wearing provocative clothing or was drinking or using drugs with a man who subsequently sexually assaulted her, many people (including many members of the legal establishment in an earlier era) thought she had simply gotten what was coming to her. (This last assumption still comes up with amazing frequency even today.)

Until recently, for example, police in Oakland, California, routinely dismissed any rape complaints made by prostitutes or drug users as “unfounded.”²² And until the last two decades or so, police, district attorneys, and the general public alike refused to take seriously what we now call “date rape,” where the parties know each other, being even more skeptical when the couple have been drinking or using drugs together.

But feminists began to challenge these practices, and in so doing challenged the very definition of rape itself. Instead of regarding rape as the theft of a man's sexual property, feminists began to work out, case by case, a new model of rape as a crime against female sexual autonomy—sex was rape unless the woman involved actively wanted it. This changed paradigm of rape has become so pervasive, so widely accepted, that it's only recently that we have come to realize what a revolution it was. In the feminists' view, prostitutes, drug addicts, lovers who have already had sex, married women, and teenage girls who have been passionately kissing their boyfriends in a dark lover's lane do not automatically lose the right to refuse sex.

In the "sex as property" model they emphatically did lose this right, as each of these acts raised questions about a woman's sexual "innocence," but in the "sex as autonomy" model they don't, because the question is not whether the woman has lost her chastity, but whether the woman *wants* to have sex.

What feminists understood in a visceral way was that in its deep structure, traditional rape law was built on the presumption that women's sexuality really belonged to someone else and that the injury involved was actually an injury to another man. In challenging the practices surrounding rape law, they were challenging small-p patriarchy at its heart, namely the idea that only crimes among men mattered.²³

From a violation of a man's sexual property in his wife or daughter, over time rape came to be defined, case by case, as a crime against a woman's right to choose with whom and when to be sexually intimate.²⁴

All of this is a long (but I think necessary) way of saying that when we talk about what rape "is" nowadays, in fact we are actually debating, whether we know it or not, what women are, what sex is, and what female sexual agency is. And most of us don't even know that's what we're doing.²⁵

This is a classic example of what I meant in earlier chapters when

I said that the boundaries of social categories are shifting in this postmodern era, and although this is a particularly dramatic case, in that the boundaries of this one were both highly politicized and contested, the lesson of this section is that because boundaries are shifting, you must take the problem of operationalization (that is, definition) very seriously.

So back to where this section started. What is rape, and how much of it is there? The answer is still the same—it depends on who you talk to—but at least now you have some feeling for what is at stake. The FBI and the law enforcement apparatus from which it gets its data are government institutions, and thus they tend to change more slowly than other parts of the culture. They still tend toward a modified version of the old assumptions about rape. They can most easily recognize what Susan Estrich calls “real rape”—where a stranger jumps out of the bushes and has violent and cruel sex with a woman, particularly when the rapist and his victim are of different races. Because they are drawing on a property model of rape, they assume that few women willingly consent to violent sex with a man (of a different race) whom they do not know.

But once your model for rape becomes one of sexual autonomy, things get a lot more complicated. What kinds of standards do we hold for consent when (a) our models of consent are based on how men consent, (b) we have generations of women raised under older practices of sexuality, and (c) we want to move toward the future while still respecting the past?

Moreover, if what we are really protecting is not men’s sexual property in women but women’s sexual autonomy, the logical conclusion is that we have to start worrying about *men’s* sexual autonomy as well. Even though our old models of sex assumed that men wanted sex all the time, once the issue becomes one of sexual autonomy, the law can at least recognize that men’s sexual autonomy can be violated, too. (Hence the rising activism over what was once an invisible part of men’s experience, namely male rape. While it’s

hard for us as members of a culture with pretty fixed assumptions about men and women to imagine a woman raping a man, we have slowly begun to recognize that men can and do rape other men.)

So, to come back to the question of operationalization, it strikes me that these days you can't just rely on official statistics, or even on whether an individual him- or herself says he or she was raped. (Note that I've started including both men and women, now that we are talking about sexual autonomy.) Since we've agreed that what feminists have done is to make rape into a continuum, conceptually speaking, each of us is now drawing the "bright line" in a different place. And, whether we know it or not, how and where we draw that bright line rests on deep and unexamined notions of gender, agency, and sex. So if a woman, for example, gets really, really drunk, and her date for the evening has sex with her, has she been raped? The law on the books is pretty clear about this: in most jurisdictions a person who is not able to give meaningful consent (that is, a person who is developmentally delayed, in a coma, under anesthesia, or drunk or drugged) is in fact raped if someone has sex with her. But most laypeople think that there is some measure of agency involved in taking drugs or consuming a lot of alcohol, and therefore the sex that can follow this kind of intoxication doesn't really seem in the same category as that violent stranger of a different race who leaps out of the bushes.

Which view is "right"? I'm not sure. I personally think that all of us should err on the side of positive and affirmative and meaningful consent, the absence of which is rape. Still, whatever my personal values might be, I can't just assume that you and I share the same definition. As a researcher, I am entirely allowed to (and indeed must) tell you what I think constitutes rape or sexual assault, or whatever category you choose to call it, but I have to *operationalize* that category self-consciously, aware that not everyone agrees with my operationalization.

Let's go for a moment to Mary Koss, a researcher who started

much of the current controversy in a 1985 *Ms.* magazine–sponsored survey of college students. Koss found that college women faced a one in four chance of being raped (or faced attempted rape) during their four years on campus, based on a random sample of 3,000 women at colleges nationwide. Here’s how Koss *operationalized* rape:

- (a) Have you had sexual intercourse when you didn’t want to because a man gave you alcohol or drugs?
- (b) Have you had sexual intercourse when you didn’t want to because a man threatened you or used some degree of physical force (twisting your arm, holding you down, etc.) to make you?
- (c) Have you had sexual acts (anal or oral intercourse or penetration by objects other than the penis) when you didn’t want to because a man threatened you or used some degree of physical force (twisting your arm, holding you down, etc.) to make you?²⁶

Now people can (and have) quibbled about the operationalization of rape in this fashion. What does it mean that a person “didn’t want to” have sex with someone who had given her alcohol or drugs? What does it mean if the woman *provided* the alcohol or drugs? What if the questions had asked “Have you ever had sex *against your will* when . . .” But the main point here is that Koss is being straight with us. She told us how she defined rape (that is, if a person answered yes to one or more of these questions, she was raped as far as Koss was concerned). And she even went further, by showing us how big a discrepancy there was between her definition and that of the people involved: Koss thought that 15 percent of the college women interviewed had been raped, with another 12 percent having been the victims of attempted rape. And here things get interesting. Of those people that Koss judged to have been raped, only a little over one in four (27 percent) agreed. Half of these

women thought that what had happened was “miscommunication,” 14 percent said that it was a crime but not rape, and 11 percent said that they didn’t feel victimized at all.²⁷

The final point of this discussion is that no one these days can assume that there is a consensually-agreed-upon definition of rape that almost all of us share. In fact, there is a wide range of behaviors that might or might not be called rape by the individual—or individuals—concerned, and the assessment of the individual might or might not match the assessment of an objective observer. Moreover, we know that the way people assess a given piece of behavior is deeply dependent on where they see things from. Edward Laumann and his colleagues, in the first nationally representative study of sexual behavior since Kinsey, asked people if they had ever been “forced to do something [sexual] that you did not want to do.”²⁸ They then went on to ask people if they had ever forced anyone else to do something sexual that they did not want to do, asking both men and women about any such behaviors with either men or women. If the person being interviewed said yes, he or she was given a self-administered questionnaire to fill out privately. Laumann and his colleagues found that 2.8 percent of the men surveyed had forced a woman to do something she did not want to do; 1.5 percent of the women had similarly forced a man. But while 1.3 percent of the men reported sexual coercion by a member of the opposite sex, 21.6 percent of the women did.

One thing that jumps out at you as you read those figures is the vast discrepancy in the experiences of men versus women. One woman in five says that she has been forced by a man to do something she didn’t want to do in the sexual realm, but only three men in a hundred confess to having forced a women to do something sexually. Only two conclusions are possible: either those three men in a hundred are doing a lot of sexual coercing with a lot of different women, or many men walk around blissfully unaware that what they have just done with their lovers was experienced by the aforesaid lov-

ers as coercive. Just to make the point even clearer, Laumann and colleagues found that almost half of the women who had had coerced sex were “in love” with the person doing the coercing at the time the event took place; one in five (22 percent) knew the partner well; and roughly the same number knew the partner as an acquaintance. One woman in ten said the perpetrator was their spouse, and only one in twenty (4 percent) said it was a stranger.²⁹

So these data support both views: according to Laumann and his colleagues, the kind of rape where someone is attacked by a stranger (Estrich’s “real rape”) is relatively rare, happening to only 4 percent of this population. (Remember that the question asked people if they had “ever” been forced, so it is a cumulative figure.) But the way I read this table, there is also evidence for the fact that male sexual preferences fairly often trump the wishes of their female partners, and that what men see as persuasion is often seen by women as force.

Back to operationalization. How would *you* operationalize rape if you wanted to study it? If you are a salsa-dancing social scientist, you would want to interview a sample of people and find what they think rape is. What are the mental maps that people carry around in their heads with respect to rape? In more formal terms, as both the grounded theory people and the cognitive science people would put it, what are the *elements* that comprise the category of rape? Before you do this, however, you might want to operationalize your *own* definition. This will give you a framework for examining what the taken-for-granted elements are in other people’s categories, and it will also sensitize you to things you have taken for granted.

So, just for practice, what do you think are the elements of the category “rape”? I think we would all agree that the stranger jumping out of the bushes and having violent and often injurious sex with someone he did not know would qualify. Thus the *elements* are: stranger–sudden–violent/assault/force–lack of consent. Note how we are already getting into murky terrain. I clumped “violent/assault/

force” into one element, but now I have to consider whether injury in and of itself is an element of the category, or a separate element. Likewise, I am looking for something *not* there, namely consent. Because my model is one of female sexual autonomy, I will put a lot of weight on that single element. So even if a lover of many years—or a husband—forces him- or herself on someone against that person’s will, then in my eyes it’s still rape. (However, since rape is a continuum for me rather than a bright line, I begin to think about aggravating and mitigating factors—you might say that I have an implicit notion of first-, second-, and third-degree rape.)

How would someone measure “force”? Again, it’s a continuum going from violent and potentially lethal physical force to psychological pressure. Since my model is one of female (and male, for that matter) sexual autonomy, I would hold people reasonably responsible for their capacity to resist on the “softer” side of the continuum. Advocates and law enforcement people are in conflict about whether or not resisting violent, “real” rape is a good thing, but both men and women, in my view, have a moral responsibility not to give in to a lover just to stop her/him from nagging.

You may or may not agree with how I list and evaluate the *elements* of the *category* of rape. More to the point, the people I will be interviewing may not (and probably won’t) agree with my list and evaluation. But the point is that I have carefully thought about the components of my conception of rape, and thus have operationalized my concept as clearly as possible. My experience is that I will surely have to revise this operationalization over and over as I go out into the field. In fact, my task in research is to solicit and analyze how *other people* think about something as controversial as rape. But I start out with a sure knowledge that the category is not self-evident, and that I myself have certain elements I want to check out in my interviews. Does force have to be there? What is force? How do you know it when you see it? When people know each other, what point distinguishes rape from simply being obnoxious and pushy sexual

behavior? I also hold open the possibility that the people I interview may have other categories that I have not thought of, each with their own elements. So, like the women in Mary Koss's survey, some people may feel that an action is a crime, but not rape. Or that it is wrong, but not criminal. My job is, by careful interviewing, to figure out what elements lead people to put one behavior into one box ("rape") and another behavior into other boxes ("wrong, but not rape," "miscommunication," etc.). Then I also have to figure out why and how people differ in their opinions about the *same* behavior. If I'm really lucky, between my interviews and the data that I gather on my face sheets (data sheets I give people at the end of the interview which elicit certain socio-demographic things about them), I can begin to explore whether there are any systematic patterns in who uses what categories and what elements. Do men and women differ on their assessment of the categories and elements, or do they disagree about what kinds of behavior belong in what kinds of boxes?

In short, before I have even undertaken my very first interview, I have begun to think analytically. And operationalizing my variables is the most important doorway into analysis.

Generalization

This is a tricky one. This is the part where I absolutely think I'm right, but I'm hard-pressed to prove it. This was implicit in our earlier discussion of sampling, but I want to make it explicit. Canonicals do all the things that they do in order that they can extrapolate from their findings to some larger population. Thus in canonical social science, you do the following steps in order: You come up with a hypothesis you wish to test, based on prior research in your area. You operationalize your variables, and draw your (random probability) sample. You administer your "instrument" to those you would like to study, collect and clean your data, and then analyze it, typically with some kind of linear model, most likely linear regression or its rela-

tives. (In real life, most people skip from step one to the last step, analyzing data from large, expensive, national datasets.) When all of this is done, the canonicals can then *generalize* from their sample to the larger world, which usually means other Americans. Who speak English. And answer the phone. And consent to be interviewed.

Salsa-dancing social scientists want to generalize, too, but we can't do the kind of generalization that the canonicals do because we don't have the necessary statistical props, namely random sampling.

Remember earlier in this book when I mentioned William Blake and his seeing a world in a grain of sand? Like Blake, we want to examine a grain of sand very, very closely, and show how the world is reflected in it.

Keep in mind, of course, that we are doing a different enterprise than the canonicals are. They are doing a logic of verification study, and we are doing a logic of discovery. Another way of putting this is that they are doing theory testing and we are doing theory generating. When we talked earlier about sampling, I made the case that we want to sample (that is, choose the things we will study) in such a way that *logically*, if not statistically, we can generalize to some larger population. To return to Annette Lareau's book about how social class gets reproduced in schools: there is no *logical* reason to believe that the two schools she studied are not typical of public schools all over the country, or that the families she studied are somehow different from all the other families in America. Of course, she cannot, on the basis of her case study, claim that these families are *statistically* representative of all families in America, but *logically*, these kids and classrooms seem pretty typical.

You need to choose your cases in the same way. Anticipate the kinds of criticisms that people will make of you. Many of my case studies take place in California, and most people don't think California is typical, so I always do a few observations or interviews outside of the state to reassure myself (and others) that I haven't just interviewed oddballs.

But there is another way in which we can pursue generalization,

and that is to “bump up a level of generality.” By that I mean, what does your case study tell us at the highest level of abstraction? In the case of Lareau’s book, for example, I would claim that it illustrates a general proposition that schools tend to favor the kinds of behaviors and actions that middle-class people inculcate in their children, because there is a complementarity of “habitus,” as Pierre Bourdieu calls it, between middle-class institutions such as schools and middle-class families. In fact, at a broader level, this study suggests how classes reproduce themselves without seeming to, and with the reproduction seeming legitimate. And at the very broadest level, it’s about how a reciprocity between the middle-class parental habitus and the habitus of the school makes some children (mostly middle-class) seem “gifted” while others, mostly working-class, seem “disengaged” or troublemakers.³⁰

It’s this level of generality to which I would encourage you to aspire. Once you know—at the most abstract level—what your study is about, consider how it is informed by other studies that think about things on this same level of abstraction. To use Lareau’s study as an example, are there studies out there that examine how classes seem to reproduce themselves invisibly in contemporary society by using subtle cultural measures of what it means to be “smart,” “talented,” or “deserving”?³¹

When you bump up your study to this level of abstraction, you are doing two things. First, you are bringing to bear on your own study important theoretical insights from other scholars who you might not have thought were relevant, because they aren’t interested in the same substantive areas that you are. Second, you are guaranteeing an audience for your book far beyond those people who are interested in your particular substantive areas. To come back to Lareau’s book one last time, I’m not really all that interested (except in a personal sort of way) in public schools. What I *am* interested in is the puzzle of legitimacy in modern society. As Max Weber wrote a hundred years ago, everyone in a well-functioning (i.e., “legitimate”) so-

ciety needs to believe that the people on the top of the social heap got there deservedly. So despite my general lack of interest in the sociology of education per se, I read Lareau's book because it has a fresh take on how schools tend to reproduce the existing social order, lending it continuing legitimacy.

I cannot stress enough how important this advice is to bump up a level of generality. Not only will it inform your book and make it richer, it will mark you as someone that a wide range of colleagues will want to read, and more to the point, hire (or promote). We all have our little areas of interest, our "inside the beltway" manner of looking at things. And if you are a canonical social scientist, you can simply address your research to the in-group of other canonicals, and they will know exactly how to assess your contribution. But if you are a salsa-dancing social scientist, it's up to you to show how your study of water privatization, or flirting in the workplace, or rising rates of incarceration (a) is not idiosyncratic, that is, a case of one, and (b) is really something that illuminates one of the Big Questions of the social and behavioral sciences.

So, as in sampling, we generalize theoretically, holding our findings up to other studies of, say, social reproduction, in order to see how our findings illuminate, contradict, extend, or amplify existing theory.

Exercise for Chapter 6

In this exercise I want you to describe how you will sample and how you will operationalize your variables, keeping in mind my advice about generalization.

First, where will you sample? If you already have a case, what will be your "tacit control group," that is, the people that you will be keeping in mind as you gather data about your case? As you'll recall, we use our "tacit control group" to make sure that things we are claiming are unique to our group are not just a function of our case.

For example, when I studied how people become right-to-life advocates or pro-choice activists, my “tacit control group” was animal rights groups. It seems strange almost thirty years later, but in the early 1980s the animal rights movement (then known mostly as “anti-vivisectionists”) wasn’t going anywhere much. This permitted me to clarify in my own mind what was specific to the *abortion rights* movement, and what was specific to *movements* in general.

If you *don’t* have a case, then is there a “bounded sample”—such as a schoolroom, or a factory—that seems likely to have a lot of whatever it is that you are studying? If you can think of such a group, how would you defend it against the claim that your findings are merely a product of your sampling strategy?

Finally, even at this early stage, you need to start thinking about what your variables (“elements”) are likely to be. You need to remember that in a theory-generating study, you usually don’t start out with a priori variables. But thinking about *your* study, how will I (and more to the point, you) know when something is inside the category of things you want to study, and how will you know when it’s not? At this early stage, it might be easier for many people to simply write down what elements the study will *not* contain. This is one of those exercises that you will have to do over and over again, so think of this as just your first iteration.



Getting Down to the Nitty-Gritty

As is probably clear by now, I have strong opinions about how to do the kind of research that you and I are interested in. I'd like to spend a minute talking about the Big Picture of doing research in a salsa-dancing way, and then in the following chapters I'll walk you through the intricacies of "salsa methods": salsa participant observation/ethnography, salsa interviews, salsa focus groups, salsa textual analysis, and so forth. (For the record, I'm going to tell you about the parts of each of these methodologies that you *can't* get from a good textbook on the topic. There are lots of resources that cover the basics of participant observation, interviews, textual analysis, and the like; see Appendix Three.)

Then, in Chapter 9, I'm going to talk a little about salsa-dancing and historical-comparative methods. My logic for putting this whole group of methods near the end of the book and in a separate discussion is twofold. First, I think that most discussions of historical and comparative methods already come pretty close to the principles I've laid down here as "salsa-dancing suggestions." Second, and most im-

portant, I'm going to argue that if you use salsa-dancing methods well, then you are de facto *always* using comparative and/or historical methods, whether you know it or not. Finally, in Chapter 10, I'll show you how salsa-dancing social scientists analyze the data we have gathered by these methods.

Let's start with the big conundrum of this kind of social science research: you don't really know what it's about until the very end. You don't know—although you might well have some good hunches—what part of the ecology of the forest this case of yours is going to illuminate until you've spent a great deal of time gathering and studying far more bark than you ever thought possible. But then at the end, after you've gathered all the bark in your particular part of the forest, you suddenly realize that you know a lot more about this part of the forest (i.e., social world) because you did this study. Alas, you are exhausted, and not very happy about the thought of going back and doing any more work, the work necessary to connect the dots from your bark back to the whole ecology, so to speak. But if you *don't* go back and make the connections, then salsa-dancing social science fails to cumulate (that is, create a better and more accurate picture of social life) with each additional study. We run the risk of just telling a good story, as Loïc Wacquant warned.

Compare us again for a moment with our friends and alter egos, the people we have been calling the canonicals. They have a small, well-defined set of questions as defined by the leading journals, and expanding that well-defined list of questions is devilishly difficult, although what constitutes an advance in the field is reasonably clear to all or most concerned.¹ Moreover, they usually have, by consensual agreement, a relatively bounded list of canonical sources that one cites in order to make the case. (Because canonicals hew to the "normal science" model of research, where accumulation is all, they see themselves as building on already-proved pieces of data, so there are strong pressures to consensually agree what those already-proved pieces of data are.)

When a canonical sits down to do her work, she typically has a well-bounded set of questions and an equally well-bounded set of theories and previous findings upon which to draw. Her job is to figure out how to make a contribution at the margin, as an economist might say.

You, however, have a unique and precious case, or you have sampled in a salsa-dancing (that is, theoretical and purposive) way. You are beginning to think analytically about what larger class of social phenomena your case (the one that chose you or the one you have sampled yourself into) is an example of. To go back to two of our mantras, *What is this a case of?* and *How do you bump it up another level of generality?*

The kicker here is that the larger class of which your case is a part is not engraved in stone, or handed down on twin tablets. It is, in the ultimate sense, arbitrary. Arbitrary, that is, but not random; and here is where we get down to the hard intellectual work of salsa-dancing social science. Any case that you find of interest will have, of necessity, a very wide range of elements involved in it. As a social scientist, you can almost always count on power, and stratification, and culture, and institutions, and the like. The challenge is to determine the larger set of categories that the elements in your case relate to.

The cognitive scientist Eleanor Rosch points out that categories (“dogs”) have elements (“hair, four legs, snouts, tails, bark”), so in Rosch’s terms our task is to (1) define the *elements* of our category, (2) define the *category*, and (3) see how these categories *fit into a larger conversation* among other social scientists.²

The trick here is that you have to do this pretty much simultaneously. I said a moment ago that the problem with our kind of work is that we finally find out what our study is about at the end, when we are all tuckered out and want to take a break—but now the hard work really begins. To take the terms borrowed from Eleanor Rosch, what I’m describing in this worst-case scenario is that you end up at the end of your time (months? years?) in the field, and you have

finally figured out what your *elements* are. (This is the Damnation of the Ten Thousand Index Cards in a different version.) But now you are confronted with the need to weave your conversation about the elements you found relevant into a larger social scientific conversation about *categories*. Which brings us back to the question we have seen before: *What is this a case of?* (Translation: *What category or categories* does your juicy, wonderful case illuminate?)

As you know from previous chapters, the path to mental health and professional success is to start worrying about this from the very first days of your work. When you zip off to Latin America or to a workplace where people are flirting with each other like mad, you need to be pondering on a regular basis the answer to this question of what *category* (or theory) of social life your research illuminates.

I hope by now you are somewhat prepared for this. After all, my discussions about how to do the review of the literature and how to think about sampling, operationalization, and generalization were (although you may not have known it at the time) *practices* (of the salsa-dancing sort) to get you to start thinking about the Big Picture from the very beginning.

In fact, I'll go further. If you look at the research notebook that you started in Chapter 1, you'll see what we've been building up to. In each of the previous exercises, I've asked you to meditate on (from different angles) what the main elements are of this question that intrigues you so. When you go to Latin America or to a workplace or out to do your interviews, I hope you will *always* be thinking about "What is this a case of?" (I'll cheer you up by reminding you that ever since you first started Exercise 1, you have been circling around this question, getting closer all the time.)

An important thing to keep in mind is that the notion of relevant categories is not something that arises full-blown from the head of Zeus. Remember when we talked earlier about sliding into ongoing intellectual conversations? Well, here is where you start practicing your moves. The first task is to figure out what the available catego-

ries of discussion are that might be relevant to your own inquiry. In order to determine that, you need to figure out what *elements* play a role. In your water privatization case, for example, it strikes me that at a minimum, you are interested in globalization, development (and maybe what used to be called “dependent development”), the role of multinational organizations, governance, power, and authority. (Recall that in the original formulation of this case, it was the World Bank that was pushing water privatization.) Similarly, to take flirting in the workplace, it seems to me that at a minimum the case is about gender, sexuality, the boundaries between the private and the public, and how institutions function in a modern economy.

Because I know next to nothing about your area, I will come up with only the most minimal list of elements, as in the examples given above; surely you, who are so interested in the area and may have actually spent time in the field, will have a much longer list. Now, armed with your list of key elements, your task is to go and read the literature in an open-hearted way to find out who is saying interesting things about the elements you have listed, *even though they may have nothing to do with your specific research question*.

When I say “open-hearted” here, I really do mean it. If you read the way they teach you to read in graduate school, you’ll miss the boat. I speak from harsh experience here, as I myself spent all too many years reading the wrong things and ignoring the right things, because I thought someone out there had a User’s Guide that they just hadn’t let me in on. Worse yet, I spent lots of years forbidding myself to read things that my heart wanted me to read, because it “wasn’t relevant.”

Let me be clear: as I warned you earlier, this is just the first of many times you will do a “review of the literature.” In fact, you may have done one already, in preparation for even starting to think about your case study.

This is where the various parts of salsa-dancing social science come together. I’m trying to teach you a new practice (how to read

open-heartedly, at a new level of generality, by making a list of the *elements* of your inquiry), while leaving behind an old practice (reading in a mechanical way everything that either your adviser or Google tells you might be relevant), while situating both of them in a historical, social, and yes, political context.

At the same time, I am asking you to bump up a level of generality. In other words, I'm asking you to assess *at a more general level* the elements of your case. Once you have decided that yours is a case of Element A and Element B and Element C, you can answer the question, "What is this a case of?" and you can then go out and read books and articles by smart people that have nothing to do with your case per se, but are very illuminating about Elements A, B, and C in other situations, and better yet, about the interrelationship. Remember, the one thing that all social scientists are doing is looking for pattern recognition. Thus a study about gender in the schoolrooms of Oaxaca or Bangladesh (if gender and organizations are your elements) or about detecting, preventing, and managing drug use on the shop floor (if management and organizational behavior are your elements) can be extremely illuminating to a study about flirting in the workplace, even though none of them has anything to do with your particular study, at least at the level of the particulars.

This new practice (and resisting, mindfully of course, the pull of the old practice) takes a lot of work, more than you know. I once took my first dog, a Doberman, for a run in a park in San Diego, only to discover that a marathon was also going on in the same park. The dog and I ran with the marathoners for a while, but because he was a Doberman, and Dobermans have an entirely undeserved reputation for nastiness, we were distracting some of the runners. So in kindness to them, Sam (the Doberman) and I switched over to the other side of the roadway and ran the opposite way. I thought Sam was going to have a doggy nervous breakdown. Every single ounce of his canine DNA was shrieking, "The pack's going THAT WAY," while I insisted that we run the other way. It strikes me that this is a

great metaphor for graduate school and even assistant professorhood. You are so new, so afraid of trusting your own judgment, that you, like Sam, will be tempted to run with the pack. Don't.

But this is not to be a case of foolish love. At every moment as you are drawn to your case, you should be thinking, "How will I make my case interesting, and persuasive to other social scientists?" The short answer is, drawing on the above, you will show them that your case illuminates elements in the categories they care about in a way they never dreamed possible.

Which brings us back to salsa dancing, in the literal sense of the word. You will recall that earlier I urged you to take up salsa dancing, or some other physical activity, preferably an unfamiliar one, that makes you sweat. Here's why. Leaving the scholarly shore behind (although always with a map in hand) is anxiety-provoking. Anxiety is not your friend. Anxiety will tell you, as my dog's DNA told him, to go back and run with the pack. Sometimes, of course, anxiety is a true signal, warning you that you are getting lost or, at a minimum, not paying attention to something deep that is going on with respect to your research. But often enough it's just a sign that you are worrying about doing something fresh, unusual, and not the kind of thing that people have done before.³

How do you tell the difference between a true signal ("Look out! There are problems!") and a false one ("No one has ever done this exactly this way before")? Go salsa dancing. Or run. Or play a killer game of pick-up basketball. When you come home, the odds are that you will know in your gut whether this is true anxiety, which means a course correction is needed, or false anxiety, which just means that you are breaking new scholarly ground.

One argument for doing something new like salsa dancing (swimming? running? pumping iron?) is that when you do something new, all of your neurons are preoccupied trying to figure out where to put your feet and hands in the new enterprise. I truly believe that keeping those ego-neurons busy opens up space in the brain for

some deeper part of your brain to make connections. This brings us back to another key salsa-dancing precept I keep reminding you of: more and more often in the future, research contributions will be made by bringing an insight from one category into another. To continue with the language that we have been using so far, it takes a certain level of relaxation, of letting go of control, to perceive that despite their surface lack of similarity, the elements in Category A are fundamentally similar to those of Category B, and hence insights from Category B can be used to make sense of what's happening in Category A. Recognizing that the elements are similar means—you know this by now—bumping up to another level of generality. Thus while I may be studying chickens, and you may be studying African hyenas, as long as I bump up to the level where you and I both recognize that chickens and hyenas are animals, I can fruitfully examine your research to see if it has any relevance to mine. Of course, this means I have to read your work with a salsa-dancing mind as well, discarding everything you say that is specific to hyenas, and focusing on the places where you make claims about animal-kind in general, about evolution, about ecological relationships, and so forth.

So here are the key steps, the PowerPoint Presentation if you will, of what you need to do to get ready to salsa-dance. Keep in mind that these steps are iterative—or, as I like to put it, they are intellectual yoga poses that you will find yourself cycling through sequentially many times before your research is finished.

Tell Me about Your Case, a.k.a. the “So What?” Question

In traditional social science lingo, this is called “motivating the project.” In salsa-dancing terms, I want you to tell me why I, a busy social scientist, might remotely be interested in your case or question. I know *you* want to know about women cocaine dealers (or the recent opposition to affirmative action, or all-girl and all-boy schools), and I

admit to a certain interest, but as that T-shirt says, “So many books, so little time.” Why would I take hours or days out of my busy life to read the book that you’re just getting ready to write? I know that you don’t know all the details yet, because you are early on in the journey. But from the very outset, I need to know why I should care.⁴

Specify Your Case

Where is your case located? Why did you choose this example of it and not another? We will come back to this in a subsequent step, the one about *sampling*, but for the moment, tell me as specifically as you can where, what, and when your case takes place, and why it matters. In an ideal world I should be able to go there and see for myself what fascinates you, even if you decided to go work on your tan on a beach in Tahiti. As you specify your case, tell me its boundaries. How could I tell what is a part of your case and what *is not* a part of your case?

Specify the Elements of Your Case That You Think Are Theoretically Important

This is probably the hardest step in all of salsa-dancing research. It is in this step that you move from a research interest to a research question. Much depends upon it. Here is where you tell me what it is about your case that makes it an inquiry in social science rather than one in, say, journalism. It’s a shame, I know, that the World Bank is disrupting local Third World communities by insisting that they privatize what was once a communal resource, namely water. But while your outrage may be grist for a protest movement or an exposé, it is not a priori material for a sociological inquiry. Because I trust you, however, I’m ready to bet that if you sit with your case long enough, you will see that there are elements within the case that beg

for explanation. In the words of the canonical social scientists, you are sorting out what your *variables* are, and you are beginning to think about *relationships* between and among them.

This brings up another key precept of salsa-dancing social science, namely, constructing your study so that you are in a position to be surprised. In the case of water privatization, it may be the case that if you specify the theoretically relevant elements and then go educate yourself about what other smart people have said about the arrangement of those elements in other contexts, you may well find yourself being surprised. It may turn out that privatizing water, though disruptive and painful to those involved, is a necessary first step to getting to a new level of economic development, one that promises some modicum of human dignity and comfort to the locals. I don't know if this is true, of course—you are the one who is the expert on this—but my point is that if you can set aside your initial emotion of outrage or concern and specify the elements, bumping them up to a new level of generality, you may well change from whatever was your first, pre-research position. Then again, you may not, but you will have a much firmer grasp on why you believe what you do, *and you will have submitted alternative explanations to some empirical test.*

Explain What This Is a Case of, a.k.a. Bump Things Up a Level of Generality

You now have outlined the elements of the case that you think are theoretically important, and you are beginning to propose relationships between and among your variables. You have told me what your elements look like on the ground (“flirting”) and what they look like at a more general level (“private behavior in public places” or “non-work-related behavior in a work environment” or “emotionally intense interactions in a location formally defined as emotionally neutral, e.g., Weber’s bureaucratic rationality”).

Operationalize Your Elements

This goes back to the discussion of “operationalizing” that we borrowed from the canonicals. What I need to know are the *boundaries* of your elements and how to determine what is in and what is out, both on the ground and at the more general level. What, specifically, is flirting? Does it count if it just happens inside someone’s head and never has a visible, behavioral component? Likewise, what is water privatization? Does it count if someone proposes it but a community resists? At the more general level, if yours is a study of gender, what precise aspects of gender do you have in mind?

Decide How to Sample, If You Don’t Already Have a Case

Going back to the exercise you did in Chapter 6, you already know that we are going to be doing a version of what Barney Glaser and Anselm Strauss called theoretical sampling. But how, specifically, do we do that? The short answer is that since we now are beginning to know what our elements are and what they are a case of, and we are exploring relationships between and among our elements, we need to think about a location where we can find a good proliferation of these elements in the approximate configuration that our theory tells us we are likely to find them.

Recall our earlier discussion of “data outcroppings.” Where are you most likely to find a lot of your data? What kinds of settings would seem intuitively obvious as a logical place to find your kind of data? Do you know of a “naturally bounded” case of your phenomenon of interest? To go back to our old friend water privatization, is there some more or less emblematic setting in which that privatization is taking place—a setting that could be “everywhere” and “anywhere” at the same time?

In fact, real working sociologists often choose settings because they are convenient, but they do so only after they have done their

homework. Remember those smartest, nastiest opponents of yours who will *hate* your findings? Rehearse to yourself all the cutting, minimizing, hostile things that person could say about your choice of setting. Imagine yourself at some public occasion in which Mr. (or Ms.) Smart-and-Nasty accuses you of having chosen your setting *only because it proved your point*. Now imagine your retort. (“Well, actually, Ms. S-and-M, I thought about that carefully when I chose my setting. I looked at all of the communities in Bolivia that were having their water privatized and chose only that—or those—communities [and here you fill in all the theoretical reasons why your community is not idiosyncratic, and does not bias your results].”) Now, using that ahead-of-time theoretical defense, choose your setting.

Preparing to Gather the Data

So now you’ve figured out, by practicing the salsa moves we’ve been discussing, what the *elements* are of your study, and what your study is *a case of*. It’s time to go out and get some data. Let’s go through the steps you’ll need to practice to get the data you need, and then in the next chapter we’ll talk about actual data-gathering methods.

If you’ve been doing the exercises in the earlier chapters faithfully, then you have some notion about what relationships between and among your elements (what canonicals call “variables”) are worth exploring. Keep in mind that you are exploring relationships, not testing them, but you are still honor-bound to consider the possibility that other relationships might well account for the patterns of data that you see.

Also keep in mind that what follows is the expurgated version of what real social scientists do. In real life, social scientists, whether salsa-dancing or canonicals, make mistakes. We miss the beat, we step on our partner’s toes, we sometimes fall on our derrieres. I’m writing here about the “clean” way, which you’ll get closer to the

more you do this sort of thing. But before we get into the nitty-gritty of actually *using* the traditional methods—ethnography, interviews, focus groups, archival data, and the like—let’s review the Big Picture of what it is we are doing. That is, I’m not going to teach you stuff about ethnography that you could get in any good book about how to do ethnography—or interviews, or focus groups, and so on. (If you don’t have ready access to good textbooks on various methods, see Appendix Three, where I have hand-picked some books that I think have a good effort-to-payoff ratio.)

At this point, you will have chosen a method that you feel intuitively comfortable with (for me it’s interviewing), and you will have taken yourself off to the field setting that calls out your name. (But remember to make sure that the method you are intuitively comfortable with is matched to your question. You don’t want to be in the situation of asking a participant observation kind of question and doing interviews to get your data.) You will be running around Latin America looking at water policy, or hanging out in workplaces where people are flirting, or [fill in your own other examples here]. You will be using your favored method—doing interviews, or participant observation, or what have you—and for a while you will feel happy and accomplished because at the end of each day you will be able to point to a pile of audio tapes (or their equivalent), or pages full of P.O. (participant observation) notes, or a tidy stack of pages you’ve written on your chosen historical question, and you will feel the glow of a job well done.

Then you hit the wall. All of these interviews and observational sessions don’t seem to be adding up to anything much, after the thrill of gathering the data has worn off. Now what?

Now we go back to the earlier steps. What are your elements? What categories do your elements add up to? What is this a case of? Here are the steps you need to get over that wall. (On the off-chance that you are one of those cautious types, someone who needs to know what they are looking for *before* they go out to the field, then

these are the steps to take before you go. Keep in mind again that “the field” could just as well be your nearby library as a distant and exotic field site.)

Figuring Out What You Think You Know

You have done the work of the earlier chapters. You’ve framed the question that you are interested in, and you’ve identified your potential audience or audiences. You’ve thought carefully about sampling, and you’ve chosen a research site where there are rich “data outcroppings.” You are either in the field, or contemplating going there shortly.

Now you should write. Start taking for granted that writing is something that you will be doing intermittently all through the research process, *and not just at the end*.

So for this first step, write me a brief memo about what this is a case of, what the key elements of the case are, and how this case illuminates the larger social world. Go further: tell me a story about your data, but a *theory-driven* story.⁵ (Hint: all your exercises up to now should have positioned you to be able to do this pretty easily.) What’s going on in your story? What elements are related to what other elements? Put another way, what are the *moving parts* of your story, and how do they move?

The wonderful thing about this exercise, if you do it right (and that means simply that you keep doing it until you get the results I’m about to describe), is that you’ll find that your story starts to take shape before your very eyes. As you keep trying to tell the story to a smart, critical listener, you’ll find that you will start hearing the story from his or her point of view. What makes you think that A and B are connected? What leads you to suspect that when C happens, D often follows in its wake? In fact, how do you support the claim that this is a case of X?

Once you have written this for the first, or first few, times, you will

begin to see the architecture of your emerging argument. Which brings us to the next point:

Figuring Out What You Need to Know

This step actually has two parts. The first and easiest is the one that you've already done most of the work for, when you did your review of the literature. It has to do with what other, smarter minds than yours and mine have thought about your area. So if in answer to the question "what is this a case of?" you have concluded that it's a case of upward redistribution of profit as a result of neo-liberalism, or of the "politics of motherhood," or what have you, what are the smart people in your and adjacent fields doing?

I think of this as the can-opener response. I don't much like grand theory, but at the same time I'm deeply committed to it. I found studying theory in graduate school one of the most boring things I ever did, particularly because in my graduate school we were not encouraged to figure out what the sacred texts meant to us—we were supposed to *guess* the canonical readings that were held by our professors.

So how did I get to be such a theory-junkie while hating theory as I do? I needed can openers. Here I was with great data, and I needed to know how the smart people in my field thought about how the world was organized, and whether that map could do me any good in understanding my own data. I only like theory when I can connect it to real-world problems that interest me.

This brings us to part two of this step: What pieces of evidence would convince someone who is not already predisposed to agree with you that your argument about what this is a case of is compelling? You say that your case study is really a case of X. Let's say that I am your meanest, smartest, most obsessive opponent, ideologically speaking. Whatever you claim, I'll be predisposed to disagree with you. What data would you need to show me in order to win me over?

This is actually a bit more complicated than you might think. It's not only that you need to convince me that this is a case of X, but that cases of X tend to work as some larger theory predicts, or that they don't.

Let's take one of my earlier books as an example. I argued in *Abortion and the Politics of Motherhood* that access to legal abortion dissolved the traditional relationship between gender and a woman's roles, specifically that of motherhood. I argued, much as Susan Moller Okin did, that historically women were all regarded as mothers or potential mothers.⁶ Once abortion became legal, however, motherhood became a *voluntary* choice, and as such, lost status—it became something that was a private choice, rather than a woman's destiny. As a result, I claimed, the fight over abortion was really a fight about motherhood and its role in women's lives.⁷

What kind of evidence did I need to make that claim? In my case, it was rather easy, since the people I was interviewing said as much, and I was able to show that women activists in at least one state were fighting over the *symbolic* meaning of motherhood. But I'm not sure I would have been able to hear what they were saying had I not read Okin's book, or the work by other feminist theorists about motherhood, gender, and the state.

What did I examine as contrary evidence? For one thing, I looked at how the genteel, professional right-to-life movement in the years before *Roe v. Wade* conducted itself. I confirmed that it consisted mostly of professionals (including some professional women), but that precisely because it was professional, it was limited in how radical it could get without alienating fellow professionals and hence losing status.

I also showed that women activists who came to this issue after *Roe v. Wade* thought about the issue in very different terms than did the professionals who came before them. Finally, I documented that all of these people were living in a state (California) that had de facto legalized abortion *before Roe v. Wade*, but that the women in

particular mobilized only after the Supreme Court case put the symbolic issue on the political and social agenda, by declaring that “the unborn is not a person under the 14th Amendment.”⁸

Figuring Out Where to Get It

You’ve already “sampled” at least once to get where you are now in your project. Now that you think you know what’s going on there (and keep in mind that this is just your first of many versions of “what’s going on”), and you have a feel for what kind of data you would need to persuade someone not already committed to your kind of research, the next task is to think about places where you are likely to run into a lot of that kind of evidence.

If you have come to decide, for example, that your case of water privatization in South America is a case of the privatizing of what were previously communal assets, a modern “closing of the commons,” how does your case illuminate/problematize/complicate existing theories about the closing of the commons in the eighteenth century? Or about globalization? Or about property?

In my case, it seemed to me that the people most involved in the abortion debate would be very likely to have a lot of thoughts and opinions about what abortion meant, and therefore these activists seemed a natural “data outcropping” to look at. (I did not, in fact, start out thinking that professionals—mostly male—and activists—mostly female—would think about abortion differently; I discovered that in the course of interviewing.)

Getting In

At this point, you’re at the front door of data gathering. You know what you think you know, at least as a first approximation; you know what kind of data you need; and you’ve identified a place or a set of documents or actions or places or people that you think would be

rich data outcroppings for you. *Now you need to get in that front door.* This is much harder than it looks at first glance, and it is the kind of thing that conscientious professors should tell their students about, but mostly, in my experience, don't.

In almost any kind of qualitative methodology, with the possible exception of historical and comparative methods, in order to gather data, you need to *get in*. The official name for this is "gaining entrée," and like a lot of qualitative methods, it seems mostly to be taught by experience, if not downright trial and error.⁹ Michael Burawoy provides an apt description when he says that entrée "is often a prolonged and surreptitious power struggle between the intrusive outsider and the resisting insider."¹⁰

If you are planning on doing interviews, you have to find out whom to interview and get them to let you talk to them; if you are doing participant observation, you usually need some "gatekeepers" to give you permission to hang around your site. Even with historical and comparative methods, often enough, you need to persuade gatekeepers to help you find the archives or data records you want, and then to let you use them once you have located them.

In my own experience, gaining entrée is harder than it used to be. People are more sophisticated (my respondents routinely tell me that they have looked me up on the Web), and they are more wary of having a social scientist commit to print a version of their reality. Marginalized groups in particular (teenage mothers, criminals, prostitutes, homeless people) are rightfully getting fed up with being the "object" rather than the "subject" of social research, and are not that thrilled to be put under a social-scientific microscope one more time. Even survey researchers are having a much harder time of it. When I was first doing research, a response rate of over 80 percent was de rigueur, and if you dipped much below that, other social scientists began to worry about non-response bias. These days an 80 percent response rate is something that most survey researchers can

only dream about; results in the range of 30 to 40 percent are more common.¹¹

Gaining entrée is an art as well as a craft, and because it's getting harder to do it than ever before, I'll give you some very important advice: remember what your mother taught you, and remember the Golden Rule.

To put it another way, no one gives a fig for what *you* want out of the data, so your task is to figure out what's in it for others to participate. Some researchers have it easy, in that they have so much money that they can afford to pay a stipend to everyone they study, and thus they don't have to worry that much about creating cooperation. (There is a caveat, however. At a certain point, too large a stipend will create problems with your Committee for the Protection of Human Subjects; the very size of it can become coercive in that people really, really want the money and are unwilling to say "no" to your research.)

But if you don't have tons of money to smooth the way, then consider what your mother taught you when you were in kindergarten, namely to think about other people's feelings. (More formally, this is "taking the role of the other," as George Herbert Mead put it.) Specifically, before you ever set off to gather your first bit of data, you need to sit down and ask yourself what earthly reason a busy, perhaps harried individual would have for wanting to give you an hour or two (or more) of his or her precious time.

Many of these same caveats apply to getting into an institution, for the purpose of doing either participant observation or interviews. Many, if not most, institutions are a priori opposed to research, despite what they might tell you. Think about it from the point of view of the institution: researchers are all cost and no benefit. We take up time and space, and we hold the potential of discovering embarrassing things about organizations that they would rather we not see. I take for granted that no organization, like no human being, lives up

to its ideals all of the time. Organizations, like people, cut corners. But would you want someone checking out your closets and kitchen drawers to see how clean they are? That, in essence, is what you are proposing to do when you ask an institution to let you come in and hang around.

You may be tempted to get into an organization the easy way—by getting a job there, or volunteering there, or temping there. Don't. Both Human Subjects Committees and good manners insist that you must never do research on individual people without getting their permission ahead of time.¹² (To go back to the closet and drawers metaphor, think how you would feel if you walked into your own bedroom during a party and found one of the guests rifling through your sock and underwear drawers. That sense of betrayal should give you a taste of how betrayed organizations and individuals feel when you study them under false pretenses.)

This is not to say that you—like organizations—can't cut *some* corners. For example, when my students tell me that they want to observe or do interviews in a public school, my regular advice is to plan for a good two-year gap while you try to get permission to do your research. Public schools are appealing venues for researchers (as I mentioned earlier, they keep all those kids in what is a pretty heterogeneous “bounded sample,” and they are an excellent way to get to a wide range of parents), but they are even touchier about letting researchers in than most institutions. They are busy, under-resourced, and really worried that you will “out” them in some way they can only dimly imagine.

But as our sociological colleague Mark Granovetter tells us, seemingly “weak ties” between people are actually very strong.¹³ Just because I've told you that you can't sneak in to a site, and that getting in formally can be very time-consuming, this doesn't mean you can't use the weak ties that you have at your command to facilitate the process. To take the public school example, surely you know someone who went to a public school, or has a child in public school, or

is a public school teacher, or is a friend of a public school teacher, or is married to a public school teacher . . . you get the drift. Ask this person to intervene on your behalf, explaining how nice you are, and how they can vouch for you.

And, nice person that you are, I know you would never cynically go and volunteer at an organization just to get entrée. But many a social scientist *has* volunteered in organizations that have caught their fancy, and having spent time there (and having become a known quantity there), they go on to do research there. If you've been paying attention, you will remember that that's how I got my own dissertation written. I got into my volunteer work merely because I had time on my hands and thought that the local Planned Parenthood organization was doing an important job. The next thing I knew, I had a dissertation and a published book, both growing out of that rather unformulated desire to be useful over a long summer.

So, once you have a foot in the door, how do you get the rest of yourself in? Or get to that first person who will lead you on to your highly desirable "snowball sample"?

I think we sometimes get so excited about what *we* want to know about a social situation that we make the all-too-human mistake of thinking that everyone else cares as much as we do. So use your imagination now and think about what might be interesting or even, if you are lucky, useful to those whom you are studying. (This is what I call one of the "hooks" of actually doing research.)

Practice in your mind telling someone on the staff of your chosen organization, or the first interviewee, exactly why your research is fascinating *from the point of view of that other person*. Think about how you would convince him or her (or the organization) that his/her/its participation is vitally important to the successful completion of your research.

Have in your back pocket (metaphorically speaking) two items that you can pull out when the time comes. First, have a short (two-page max) summary of your research *written in plain language* for

real people. Put in writing the same set of ideas I just urged you to develop, namely a quick overview of why this research is interesting from the point of view of those being researched; why you really, really need *them* and not someone else or another organization; and how their participation will help advance human understanding.

No one is immune to flattery, so part of this statement can contain your astute observations about why this person or institution is so central to your research. Once you have a draft of this summary, ask your mother to read it, or your best friend outside of academia, or some other “civilian,” and ask them to be brutally honest about whether it sounds obscure, pretentious, or jargony (all those sins that graduate school teaches us so well).

The other thing you need in your metaphorical back pocket is an idea of how you will repay the kindness of the people who agree to be researched. One common way, of course, is to offer to give them a summary of the research after you have finished, or, in the case of the organization, a briefing to whatever set of people might like to hear about your findings. But you can't always do that, and even if you do, it's likely to be many years down the road. So, to use Lévi-Strauss's term, you have to think if there are other ways that you can show reciprocity to your research site.¹⁴

Since people are putting themselves out, either a little or a lot, to accommodate your research, how can you give something back? Be creative: can you do some filing, or run errands, or do something else to help the organization? If you are interviewing people, can you make clear in the interview how much you appreciate their contribution? Do you have resources you can share with them? (In my own case, when I was interviewing women who had had abortions they had not planned on, I was given entrée to one of the few abortion clinics available at the time. The clinic gave Rh-negative women a shot of Rhogam, something that would protect their future pregnancies from the antibodies that a pregnancy can stimulate in Rh-negative women. The shots were expensive, but free to the wo-

men, so my way of giving back was to donate blood to the clinic's account, thereby making those shots cheaper for the clinic.)

To take the question of reciprocity and understanding the role of the other one step further, do yourself a favor and have someone observe you as you are actually doing research. Have someone watch you do participant observation, or ask a friend to run a (fake) focus group with your interview schedule, or have her actually interview you one-on-one using your instrument. Don't just take on the role of the other by thinking about it—really get into the spirit of the thing. If it's boring to hear someone else interview you, then you will bore other people.

The only way I can give you a concrete feel for this is to tell you how I've done it. For my first book, I interviewed people who found themselves with pregnancies they did not want (operationalized as people seeking abortions) even though they had successfully used contraception in the past to prevent pregnancy. So I was studying "contraceptive risk taking," and my task was (a) to convince abortion clinic gatekeepers to let me in to study the records, and (b) to convince individual women to let me interview them. It turns out that my "hook" was an intuitively compelling one. California had substantially legalized abortion some five years before *Roe v. Wade*, and almost everyone involved was surprised by how many women were seeking legal abortions, given how widespread and available contraception was. So my question, "Why is there so much abortion in a state with such widespread access to low-cost contraception?" was one that piqued the interest of almost every clinician I talked to. Another version of that same question was one I used with the women themselves: "I'm interested in how women get pregnant when they don't want to be." This question was equally intriguing to the women involved, who almost seemed surprised to find themselves pregnant.

When I came to doing my second book, the question I was interested in was again one that the interviewees themselves found fasci-

nating, namely, “How do some people come to be pro-life, while others are pro-choice?” This question was of interest to people on both sides of the issue, because each side assumed that “the facts” dictated their position, and they couldn’t begin to understand how other people could look at the same facts and come to a different conclusion.¹⁵

You should be prepared, by the way, to answer questions yourself about where you stand on whatever issue you might be researching, as well as how you came to be interested in this topic and other details about yourself. My theory here is that this is only fair—you will be observing, asking, interviewing, and otherwise prodding *them*, so you owe them some answers, too. There are other reasons as well not to do interviews or observations without having thought through some answers to these questions. If you are caught off guard, you will mumble and stumble, and people will lose trust in you. If *you* don’t know why you’re here, why should people open up to you?

This brings up another key matter—if you lie to people, they will lie to you. My own sister, an investigative reporter with a great deal of experience, some years ago tried to do a story on a local homeless shelter. Tired after a long series of deadlines, she skipped for once the often-long-drawn-out process I’m describing here, where you persuade the gatekeepers (in this case the people who ran the shelter) to let you in, and then persuade the people you are observing (in this case homeless women and children) to let you talk to them. She arrived at the shelter late at night, in shabby clothes, and asked to be admitted. She was admitted, but one look at her made both the residents and the people who ran the shelter incredibly suspicious, and no one would talk to her all night long. When she called me the next morning, she ruefully acknowledged the truth of the maxim, “If you lie to them, they’ll lie to you” —or in this case, they will not say a word. A better approach in this case, I think, would have been to persuade the homeless women that you wanted to do something other than just one more story about how pitiful they are, or how

they got to where they were, or “what’s a nice girl like you doing in a place like this?” In other words, as my more “po-mo” students say, to persuade them that for once you had no intention of “othering” them.

This does not mean, however, that you always need to tell the truth, the whole truth, and nothing but the truth. Actually, you *should* do two of the three—you must tell the truth and nothing but the truth. It’s the question of the *whole* truth that gets a bit tricky. My own opinion is that it’s all right not to tell people my musings about what the elements of this study are, or what this is a case of, and all of the other things that we’ve talked about in this book so far. For example, in my study of abortion activists, the question of why it is that some people end up being pro-life while others, ostensibly similar, turn out pro-choice was sufficiently interesting to people that they were willing to explore it with me. I said to the interviewees, and it was 100 percent true, that I was studying this question because I didn’t know how people could look at the same facts and come to such different conclusions. What I eventually concluded—that the debate over abortion was a debate over motherhood—was not something I knew for sure at the very beginning, and it would only have put people off. So I don’t share the *whole* truth, or, for that matter, all of my musings and thoughts and other kinds of materials I’m working on.

So now we know, at least in a preliminary way, what we are looking for, how to get in, and how to recognize it when we see it. Let’s go get some data.

Exercise for Chapter 7

We’ve been talking about elements and “what is this a case of?” I know we are still in the early days here, but what elements have you come up with so far, at least in terms of the things you have found interesting?

Likewise, whereas in earlier exercises your study could have been a case of almost anything, what is the “short list” of things this could be a “case of” that has emerged as you’ve worked on these exercises?

As you begin to write (and remember that we, unlike the canonicals, have been writing since the very first chapter of this book), what story line do you see (or hear) emerging?

One last thing. Keep watching your body and your emotions. When anxiety rears its ugly little head, start practicing telling the difference between useful anxiety (“Look out! There are problems!”) from false anxiety (“No one has ever done this exactly this way before”).

In the former case, take a deep breath, look those problems squarely in the eye, and come up with a plan; in the latter case, go salsa dancing.



Field (and Other) Methods

So here we are. We've discussed the history, the politics, and the power relations involved in the search for data. Now it's time to get started on the adventure of gathering data, and adventure it is. In this chapter I want to (a) show you how to use traditional methods in a salsa-dancing way, and (b) tell you some things I've learned the hard way, but which I've never seen discussed in any books.

Participant Observation

First I'll discuss participant observation (PO), or ethnography. In theory (and I do mean *theory*), these methods run the gamut from watching children at play in a school yard while standing on a sidewalk on the other side of the fence (which Janet Lever did to write her classic study of gender and games), to actually moving to a distant site and staying there for months or maybe years at a time, as anthropologists are accustomed to do.¹

As these examples suggest, I see these methods as a continuum, running from specific and delimited observations to answer a specific question ("How do little boys and girls play differently, if at all,

in the school yard?”) to understanding an entire way of life. On the far end of the continuum lies anthropology, since sociologists rarely feel so bold as to take on an entire society.²

I tell my students, only half in jest, that if you get to go home at night, it's participant observation, and if you don't, it's ethnography. When you are enmeshed in a different culture, everyday life becomes problematic and challenging. You don't get your regular morning coffee in your familiar coffee mug, you don't read the comics in your local paper, and you probably don't eat whatever it is that you usually eat every day for breakfast at home. Then the day goes downhill from there.³

When you live in a different culture, you are constantly off-kilter, a little befuddled, and everyday transactions become puzzles that you must untangle. It's something like living in an altered state of consciousness, and it reminds us how much of our everyday lives consists of habitual and unnoticed acts. So whatever you are observing in this context will be inflected by your own new awareness of yourself as an observer, someone who watches and notes the things that everyone else takes for granted. You are, by definition, an observer.

Living in a society in which you are an observer is thus one end of the continuum. No matter how acculturated you become, there will still be those moments when you realize that you were not born into this setting, and you'll always be a bit tone-deaf to its nuances.

The risk of this kind of ethnography, particularly in the early days, is that *everything* is grist for the observational mill. You are overwhelmed with data—sights, sounds, smells. You tend to note the exotic and weird, and have to force yourself to notice the daily and the taken-for-granted. In short, you have to stop being overwhelmed in order to become systematic.

On the exact opposite end of the continuum is the case where you do observations in a place that you know well, perhaps too well, where you are a full-fledged participant. Many a social scientist has

started out getting a job somewhere, from driving taxis to being a paralegal, and has then decided to study it intensively.⁴

Here the problem is quite the opposite of the ethnographer's situation. Because you know the setting so well, you overlook what to the outside observer would seem quite strange. ("Yes, we admit a considerable number of students to elite colleges because they hit balls well or because their fathers went here.") You know the rules of the game so deeply in your body that you never even notice that there *are* rules.⁵

Even (and especially) when you choose to study something—as most of us do—within our own culture, but something which we don't know very much about, I think you end up getting the problems of both ends of the continuum. You tend to highlight the weird and unusual, and you faithfully document the trivial, *but you are hard-pressed to see any connection between the two*. This is so for a deeply sociological reason. It is one of the cardinal rules of social life that groups try to turn people who sit on the edges observing them into proper, law-abiding, norm-following members of the community. Thus there are powerful forces, on the part of both you and the other people, to make you "one of them." But the price of becoming one of the gang, often enough, is to agree not to notice precisely those things that you went in to study.

The important thing to keep in mind is that this is not a personal problem of yours—some deep need to be liked at all costs. It is a social phenomenon, one that is all the more powerful for being in large part unconscious. Part of being a member of a group is taking for granted the things that they take for granted, and becoming inured to what once seemed to you very strange patterns and practices.

Speaking of practices: after years of teaching observational methods, but not being entirely clear in my own mind why someone would want to use them, I finally had an "aha" moment. I took it on faith that observational methods were valuable, since some of my

smartest colleagues use them, but I could never quite get the point myself.

One day, reading Lynne Haney's excellent study of teenage delinquent girls and teen mothers in a group setting, I finally got it.⁶ Haney presents us with the problem that "the state" in feminist theory is under-theorized, and that women in fact do much of the work of the state, at least when it comes to other women. Both delinquent girls (in this context, read "sexual" girls, or "girls being sexual with the wrong guys") and teenage mothers are deviant from the point of view of white, middle-class observers, but when "the state" intervenes and puts them on probation or in homes for unwed mothers, it is women, and often women of color, who are their "keepers." Haney's finding is that these girls paradoxically flaunt either their sexuality or their motherhood *against* these officers of the state (officers in the loose sense of the word), *even when these women hold feminist values*. To reduce an elegant piece of research to the bare bones, delinquent girls and unwed mothers reinvent and deploy private and public patriarchy as a way of opposing "the system"—something like "the lads" of Paul Willis's famous book.⁷

That's when I understood that for our kind of social science, the point of doing observational methods is to document "practices," those moments when belief and action come together. So when Haney paints a portrait of "delinquent" young women pulling out hand mirrors to check their makeup at the exact moment when their probation officers are lecturing them on the foolishness of depending on men, we see in that moment one "practice" upon which Haney's claim is built—namely that these young women are using "private patriarchy," that is, their youth and sexual attractiveness to men, against an ideology that says that young women should acquire skills, stand on their own two feet, and never become dependent on men. These young women, quite insightfully, claim that their older probation officers lack the sexual resources that they themselves have, and are therefore preaching a model of very differ-

ent resources—resources that these young, poor, and minority women have only tenuous access to.

Once I got it, I realized I had been seeing these pieces of evidence for a very long time, without actually understanding exactly what they were. In Elijah Anderson's *A Place on the Corner*, when Herman goes to the inner sanctum in the place where he is known and has friends, we see what Anderson has been telling us all along. In his world, Herman belongs and is a somebody.⁸

Or when Julie Bettie tells us that “las chicas” in the high school she studied in the Central Valley of California wear dark nail polish and mature makeup in contrast to the college prep girls, who go for the more “natural look,” we see that it’s because those in each group are signaling their intended futures. The college prep girls are aiming for an extended adolescence, with many more years of schooling and financial dependence on parents ahead, while “las chicas” are planning to assume the role of adult women in the relatively near future.⁹

Once I got to that key place, I went back to Howard Becker’s classic article about how to make sense of participant observation data.¹⁰ Not surprisingly, I found an elegant and parsimonious set of guidelines about what to do with your data. Becker argues that you start by “discovering if the events . . . are typical and widespread, and seeing how these events are distributed among categories.” Then you try to decide how central these events are to the thing you are studying. Next, you try to find other pieces of evidence to validate what you think you are finding theoretically; and finally, you try to build a model of why the people (or other units of analysis) are doing what they are doing.¹¹

All these points reinforce what I’ve been saying all along: we observe in order to build theory. To return to Lynne Haney, I know for a fact that she did not choose her field sites—that is, “sample”—in order to “test” theories about private and public patriarchy, or about how life on the ground is much more complex when it comes to

gender roles and the welfare state than classic feminist theory would allow. She did what you are probably doing—she chose a juicy field site, a “case,” and kept observing and observing, all the while thinking theoretically until she finally had a theoretically informed argument to make. She was *generating* theory about feminist theories of the state, not testing them. In short, she “salsa-danced” her way to a theory. That theory will and does change over time, as you realize that what you think you are seeing is not in fact what you are seeing. But writing down what theory you think you will be using—even if it’s only “conjectural theory”—is a good habit which serves to remind you at every step of the way that you are not just gathering details about colorful people, but building theory.

The surprising thing to me is that, although there are legions of fine books on ethnography, as far as I can tell not much has been written on how to do PO in a salsa-dancing way, that is, where we take for granted that the point of the enterprise is to systematically gather evidence about social practices that in turn is used to build theory, just as Haney did. So here are my key points for salsa-dancing methods of gathering data from either participant observation or ethnography.

First, you need to go through the earlier steps of salsa-dancing social science that we have worked on in previous chapters of this book. What do you think this is a case of? Or, if Burawoy is your model, what theory do you think your study will illuminate? Compared to what? If you don’t have a theory because you are not yet sure what this case is really about, write down your conjectural theory, that is, how and why something in this case contradicts something you thought you knew about the social world.

Next, you should think very deeply about what kind of sampling you will be doing. Some of you may be lucky—you may be attracted to a case where sampling will not be an issue for you at the outset, although it could be later on, as you begin to build a more robust theory about what is going on in your case. But let’s say that you are one

of those sociologists for whom “case” is a bit more abstract at this point. That is, you want to explore the relationships between different kinds of social phenomena, and you don’t know where to start. This is where the question of sampling comes up.

The rule of thumb in this case is simple, but not easy: you want to find (and ideally, define theoretically) a location in which the phenomena you are interested in are likely to be found. Remember the difference between our kind of sampling and the kind canonical sociologists do? In canonical sampling, it is assumed that elements (people or organizations or what have you) are selected from a sampling frame where each and every item has a statistically equal chance of being selected. But for field research (participant observation, interviews, focus groups, intensive study of historical documents), you want to go where there are data outcroppings—places where you have good reason, either from previous theory or logic or personal experience, to think that there will be a lot of what it is you want to study.

Thus, in the salsa-dancing model of field research, you get to sample on two levels. At the outset you sample in terms of *venue*, which is what the canine search-and-rescue metaphor addresses: where are you likely to find the kinds of data that will let you build theory? If the practices you are interested in are well-nigh universal, then you should look for what I called earlier a “bounded group,” that is, a setting where you and everyone else are pretty clear about who belongs in the setting. For example, if you are interested in the “reproduction” of class and racial hierarchies in American society, you would be strongly attracted to studying a public school. A public school classroom gives you a nice “bounded group” from which to sample. It solves the venue problem for you, and lets you concentrate on other kinds of issues.

As we saw earlier, public schools are not entirely representative of the range of *children* in a particular age group (because there are also private schools, religious schools, and home schooling), but un-

less we have evidence to the contrary, a public school is likely to be representative of the range of *practices* that we're concerned about. (In other words, practices, not children, are our unit of analysis.) And if it turns out that we generate evidence to the contrary, finding as we do our work that the practices shaping our theory need to be explored in all kinds of schools, then we go out and find some kids in private schools, religious schools, and in home schooling.¹²

Once you have found a data outcropping and managed to get entrée into it, you will have to sample in a second way, namely across time and space. Again driven (or at least shaped) by theory, you think of all the times and places where the social practices that you are interested in are likely to occur. To put it more formally, given that we want to observe in order to build theory, we want to make sure that we observe across the spectrum in which theoretically important practices occur.

To borrow an example from another piece of sociological research, if we wanted to know how parents and medical personnel make decisions about seriously ill newborns, we might look at Renée Anspach's book on how different kinds of people make decisions about how to treat (or not treat) very ill or seriously premature babies. In order to make sure that we are seeing the entire range of the kinds of interactions that shape this decision, we would want to try to be present for all the hospital shifts from the middle of the night to lunchtime, and everything in between.¹³ We would want to make sure that we observed all the possible places where such interactions could occur—in the Neonatal Intensive Care Unit, the quiet rooms, the waiting rooms, the lunch rooms, the chapel, and even the cafés and other “getaways” across the street and in the neighborhood.¹⁴

Having decided on a venue and done some hard thinking about how you will sample across time and space, your next order of business is to map the terrain of the place you will be studying. Not just the physical terrain, of course, but the social and emotional terrain as well. (You will see that many PO field notes contain almost poetic

descriptions of people and places. Part of it is to remind you who is who, but part of it is to bring back the immediacy of the setting in emotionally evocative ways.)

Mapping the terrain draws on all the social skills you have acquired over a lifetime, and at first this will likely be shaped by how you got entrée into the setting in the first place. You may well have come into your setting by the “front door,” so to speak, having gotten the permission of the powers-that-be to do your participant observation, but often that can be a mixed blessing. True, you’re legitimate, and no one can officially throw you out. On the other hand, because of the very fact that you came in under the aegis of “the boss,” people will often be suspicious of you, wondering what you are *really* up to. (Helping to plan layoffs, perhaps? Figure out who is goofing off on the job?)

In addition to the official power network which you must negotiate, in many kinds of settings you also have to negotiate the informal power structure—almost always if you are in a setting in which there isn’t one “boss” who can grant you official permission to enter. But the catch-22 here is that you can’t get any real data until you negotiate the informal power structure, you can’t negotiate that structure until you know who runs it, and you can’t know who runs it until you’ve gotten some real data.

A friend of mine who is a primatologist once told me how adolescent male baboons make their way into a new community after being thrown out of the group they were raised in, which is apparently something that happens quite often to adolescent male baboons. The way that a young male worms his way into a new group of baboons is that he hangs around the edges of the group and makes friends with the females. After a considerable period of being on probation and doing favors for these female baboons at the edge of the group, eventually some watchful mother will let the young male groom or otherwise entertain a baby, and from there the rest is easy.

As you might expect, I find this a splendid method for getting into

groups where you don't know the power structure. You sit around on the edges, not making a big deal of being there, being helpful in small ways, and sooner or later, if all goes well, you are part of the group.

One of the problems with this strategy, however, is the opposite of what happens when you come into a setting under the aegis of the official power structure—rather than being seen as a (nefarious) ally of the powerful, you are seen as a dupe of the powerless. And this situation is aided and abetted by the fact that relatively less powerful people often do in fact like to cling to researchers, using you as a resource to prop up their social power. (“I know what she's *really* doing/thinking/finding.”)

If all of this sounds just as stressful as deciding where to sit in the lunchroom on your first day in a new school, rest assured that it is. Just as when you first went to high school and had to decide whether to sit with the “in” group (while simultaneously finding out who they were), with old friends if any were around, or to risk sitting with the nerds and the geeks, you will have to call on all of your social skills to figure out how to negotiate your research setting. The difference between now and high school, though, is that you will do it self-consciously, and you will be taking notes every inch of the way.

Notes are the treasured and feared tools of the fieldworker. You need them. How are you going to analyze what is going on if you don't take daily and perhaps hourly notes? Depending on how intensively you are studying the social practices involved and whether you are focused on a smaller or larger set of practices, your notes will take more or less time. It's not at all uncommon for researchers to spend four or five hours writing up notes for an observation period of an hour or less. Every fieldworker eventually finds his or her own way of taking notes, whether it means scribbling a word or two to yourself in the setting that will guide you when you get home, excusing yourself to go to the bathroom to take notes, or discreetly finding an empty room and opening up your laptop.¹⁵ But the one thing I in-

sist on is that you absolutely, positively must write your notes before you talk to another human being about the work. I don't know why it happens, but I know that it does: something magical happens when you write, and if you talk about it first, that magic can't happen. A cognitive psychologist would know better than I, but my hunch is that once you've told your roommate/spouse/mother what happened in the field today, you've created a fixed narrative, one that has been constructed for a specific audience. Once you've made your observations into a narrative, the observing part of the brain thinks that its job is over, and it will obstinately refuse to serve up fresh data, memories, ideas, and theories. This is something that always happens to me, and it seems to be true of virtually every student I've taught, but of course, your results may differ.

So here you are—you've gotten entrée one way or another into your setting, you've cased the formal and informal power structure, you are resolutely coming home every night and writing up your field notes. Now what?

Well, it's basically back to the same core question I keep asking you: What is this case of? Put another way, you started out with a theory, at least a conjectural one, about what this particular venue represents in terms of the social world we all aspire to study. *Starting on the very first night you write up your field notes*, you need to start fine-tuning your theory. Did your observations today strengthen or weaken your conviction that this is a case of _____ (fill in the blank), best explained by a theory of _____? As you ask yourself this question *each and every day you gather data*, you will find that you are getting a firmer and firmer grasp on what it is that you are studying, what it is a case of, and what kinds of theories best explain it.

As your grasp gets firmer, and you become more confident, you begin to think about what Glaser and Strauss call "theoretical sampling." That is, what kinds of "practices" would illuminate/extend/complicate the theories that you bring to the case? In other words,

what are people doing, and when are they doing it, when they do things that you would not expect them to do?

In Lynne Haney's case, for example, she was obviously surprised to find that women probation officers and women who worked in homes for unmarried teens had feminist values. She seems to have been taken aback by the juxtaposition of women being at one and the same time agents of the state, disproportionately women of color, and feminists (feminists in the sense that they wanted the young women in their charge to be independent of both public patriarchy ["welfare"] and private patriarchy ["homeboys"]; these were not the "femocrats" of Australia who tried to change the state itself).¹⁶

You can tell that Haney was startled by the juxtaposition of these different categories, and by the additional phenomenon of young women aspiring to *reject* messages that in the lives of their elders (and perhaps Haney herself) had been empowering. Thus Haney had a beginning research question on her hands: why did these categories "clump" in this way, when one would have expected them to "clump" in another way?

Given that research question, the next question becomes: what kinds of "practices" can the researcher observe that illustrate the embracing of these values by the staff, and the rejection of them by the girls? I suspect Haney did what you have done or are contemplating doing, namely immersing herself in her research site and keeping notes on almost everything that she sees. But from the very beginning she also had to think about how she would theorize about what she was seeing, which is just another way of saying that sooner or later she had to tell a story about these girls, either to her professor, or to her colleagues, or to a potential journal editor.

As she began to think about *what kind* of story to tell, Haney found herself confronting the question of "what is this a case of?" and the answer became, "it's a case of welfare and probation officers, who are agents of the state, trying to control young women and encourage (coerce?) them into doing 'the right thing.'" The answer to the question "compared to what?" then became, "compared to the

classic feminist treatment of ‘the state,’ which is thought to recreate patriarchy.”

At this point Haney was set to look for practices that illuminated this contradiction between what the literature says that “the state” does to women, and what Haney actually saw on the ground.

Interviews

As is the case with participant observation/ethnography, salsa-dancing sociologists do interviews in order to build theory.¹⁷ By and large, we are not so much interested in the veracity of the interviews, in some cosmic sense of the word, as we are in the deep truth of them.¹⁸ Regardless of whether things happened the way people said they did, what interests us is that people chose to tell us that they happened that way.

This puts me in a tricky position with respect to both canonical sociologists and the post–postmodern types. I am under no illusions that interviews are in any way a realistic account of some aspect of social life (a criticism that canonical sociologists often level at interviewees). Likewise, I am entirely aware that interviews are “narratives,” stories about what the person being interviewed *thinks* happened, or thinks *should have* happened, or even *wanted* to have happened, as the postmodernists claim. But that’s exactly the point. I think that interviews are, almost by definition, accurate accounts of the kinds of mental maps that people carry around inside their heads, and that it is this, rather than some videotape of “reality,” which is of interest to us.

The point of interviews, however, is not what is going on inside one person’s head, but what is going on inside *lots of* people’s heads. When you hear the same thing from people all over the country who don’t know one another, you can be reasonably sure that you are tapping into something that is reliably *social* and not just individual.

My working assumption is that, as Ann Swidler and Elisabeth

Clemens tell us, people in a given society at a given time have only a limited number of cultural tools, or templates, available to them to make sense of the world in which they find themselves.¹⁹ (Or, since much of my work has to do with social movements, we could say that there are only a limited number of “frames” that people for or against an issue can bring to bear on the matter, and that much of what a social movement does is to help refine and popularize those frames.)²⁰ So when I do interviews, what I am looking for is how people put together the inventory of tools they have available to them. Much of my own work also has to do with how that particular toolkit evolved over time—how the “discourses” on it emerged and were modified.

How do you actually do an interview? Here’s the way I do it. First, I think long and hard about the “hook” I will use to explain my study to people I would like to interview, thinking about what version of it makes it compelling and attractive. I know what *I* want to find out from them, but how do I present it to them to make it worth *their* while to talk to me? I keep hammering home this point because it is one of the things that I spend the most time thinking about, and thinking about it forces me to do the intellectual task so central to our kind of sociology, namely looking at our study from the outside in.

Having done that, my next step is to take a pack of 3 by 5 index cards and write down every single question I want to know the answer to. If I could sit down with the single smartest, most knowledgeable, most thoughtful and introspective person involved in the issue I’m studying, what would I want to ask him or her? Of course, in real life it will take lots of interviews to get this material, but in my imagination there is some Perfect Interviewee out there, sort of like nineteenth-century literature’s omniscient narrator, who could tell me anything I wanted to know. What would I want to know?

I write one question per card, and I try to use the kind of easygoing, accessible language that I would use during an actual interview.

Again, I don't phrase the question in terms I would use, but in terms that would make sense to this imaginary Perfect Interviewee. So instead of writing "What motivated you to get involved in this issue?" which is the real question I'm interested in, I would get at this with a number of questions, on the assumption that few of us know our own motivations, and even when we do, we rarely think of them as "motivations" *per se*. So I would jot down a series of specific, concrete questions to get at this point: "When did you decide to get involved? What was going on in your life? Why then, and not earlier or later? Why this issue and not a closely related one? [I usually spend a lot of time thinking about an alternative issue that I present to them in the interview.] What have been the costs of getting involved? What have been the benefits?"

Then I take this stack of index cards (and here's why I use cards instead of doing this on the computer—although you folks, being less linear than I am, might well want to try it on the computer to see if it works), and I arrange them. This is the art part, not the science part, but I am absolutely sure that if you apply yourself to this stage hard enough, it always works.²¹

I sit down near a flat surface—for me, the living room floor has always worked just fine—and I lay out these cards in different orders. If you play around with your cards long enough, you will see that they start to "clump."²² By this I mean that there will be a sort of topic outline of the areas you're interested in, and a series of questions will fall into each topic area. Then, within each topic area, I try to arrange the questions as closely as possible to an approximation of natural language. I tell my students to imagine that they are sitting next to a person on a train or an airplane, and as they are chatting they discover, quite accidentally, that this person approximates the Perfect Interviewee. Of course you could not "interview" this person on the train or plane, but you could have an inquisitive conversation with her or him about your topics of mutual interest. So, getting away for a moment from the idea of a formal interview, how would a

natural conversation about your topic go? Obviously it would move from the more general to the more specific, and from the less emotionally threatening to the more emotionally threatening. You would be polite and charming, slowly building up trust and confidence. You would be very careful not to be too abrupt or intrusive, and you would constantly tell the person (either verbally or nonverbally, by your body language, alertness, little sounds of appreciation and admiration and the like) how much you appreciated what he or she was telling you. Then, as your station approached, you would want to “cool down” the interview, setting the stage for a friendly departure. All in all, this is not a bad model for actually doing the interview.

So as you imagine the “strangers on a train” discussion, think about how you would ask the questions that are now sitting in separate clumps on your living room floor.²³ What order would the clumps be in? What order would the questions be in within each clump?

Next comes the part that you would do naturally in a conversation, but that you have to do mechanically in your interview, and that is to put in what I call “turn signals” between the clumps. Because you are working at building theory, as opposed to just telling a story, there is a good chance that your clumps may not logically cohere from the point of view of the person listening to them. (They *do*, of course, logically cohere for you, because you have an underlying theory that you are trying to generate, and even though it is early days yet, your questions in all their little clumps represent, whether you know it or not, different proto-theories that you are trying out on this human person who actually knows the situation.)

But from the listener’s point of view, the clumps may not seem logically connected, so to keep from sounding abrupt and scattered, you will have to signal to the listener that you are moving to another clump. This is easy enough, and is accomplished by saying something along the lines of, “Up to now, we’ve been talking about your

childhood. Now I'd like to ask you about [fill in the blank]." This is just your way of acknowledging that you are going on to another topic area, and that you need to invite your interviewee to come with you. I know the phrasing here doesn't sound exactly like a "strangers on a train" conversation, but I start out by writing these "turn signals" in rather formal language, and then I play with the wording during the actual interview.

So now you have all of your questions sorted into clumps on the floor, and they are in a logical order that you could use to talk to the stranger on the train. Within each clump, there is a logical order of questions, and between clumps there are "turn signals." Two more steps, and you have the beginnings of an interview schedule.

At the start of this emergent interview schedule, you need to have your "hook." Yes, I know that you probably used your hook when you talked to your interviewees on the phone to get them to agree to be interviewed; you may well have told them the hook when you first wrote them a letter asking if you could interview them; and there may even be a version of your hook in a consent form, whether it is written or oral.

But you can never tell people too often what your study is about, why you are interested in it, why *they* should be interested in it, and most important, why the person you are interviewing is *the* key person needed to help you understand this puzzling case that you are studying with such intensity.

In my own case, I often do the kinds of interviews where I only need verbal consent, so I tell people what the study is about, and then after I have gotten permission from them to tape the interview, I reiterate what it is about and give them an opportunity to have an informed consent on the tape itself as proof that I have offered people the chance to consent in an informed way to the interview.

The final thing you need in order to have a good working interview schedule is a "cool down." This is especially important for researchers like me who work on sensitive issues like abortion and un-

wed motherhood and sex education, but my feeling is that everyone needs some form of this in an interview.

Whatever your topic may be, you have spent between an hour and several hours in intimate but asymmetrical contact with another person. You have asked questions about something that they are generally pretty interested in, or they would not be letting you interview them. You have been asking, and they have been telling, and in my experience this creates an odd kind of intimacy.

Then the interview ends, and the link is broken. It's your job at this point in the interview schedule to help the person come back to a more general, emotionally detached place and get ready to finish up and let go of the interview. So some "cool down" questions are in order. In my work, I like questions that focus on the future, or I ask people to assess their experience or perhaps to tell me what they think is the single most important thing they think I should know about the issue we have been discussing. (Many beginning interviewers close the interview by asking the interviewee if she/he has any questions, but since few people ever do, this is often an unsatisfying way of ending the interview.)

Then, of course, you must thank the person who has just given you some of his/her precious time, and thank him or her with the full-hearted sincerity of a researcher who may have just gotten the piece of data that will permit you to crack the whole thing open analytically. There's a case to be made, by the way, for keeping the tape recorder handy at this point in the interview, because almost every interviewer has had the experience of turning off the tape recorder and then watching the interviewee *really* open up, now free of the constraints of Being Interviewed.²⁴

Now that you have your hook, your logically ordered clumps, and your "cool down" questions, you have the first draft of an interview schedule, and you should go ahead and type it up. I stress, however, that this is just the first draft; if you're like me, this is an interview schedule that would require the Mother of All Interviews to com-

plete. (My list of questions is typically appropriate for a four-hour interview, and very few people will give you that kind of time.) So now you have to decide what you really, really care about, and be ready to toss the other questions overboard when the interview runs too long.

When you do your first few interviews, there is an argument to be made for letting your interviewees take the bit in their teeth and run. Keep in mind that your first questions will be general (it's best to go from more general to more specific, and from more emotionally—or politically—neutral to more loaded), so often a general question, the social-science version of “what's a nice person like you doing in a place like this?” will prompt your interviewee to chat on for what seems like hours.

Let them.

Remember that we are looking for the road map that this person carries around in his/her head, and while we have some theoretically grounded reasons to think that this mental map might be arranged in a specific way, these are only hunches. So let the first few interviews go in whatever direction they want to. Try to cover all of your clumps, but don't be too anxious about it.

The key point here is that we, as social scientists, are involved in an enterprise of *pattern recognition*. One person is part of a larger pattern, but you won't know until you have done at least a handful of interviews what is idiosyncratic to individual people and what are the social patterns that are of theoretical interest. In more formal terms, what I'm warning you against is premature closure. You have your clumps, and they are important, but you don't yet know the situation intimately, because if you did, you wouldn't be doing the study. So you should be open to the possibility of new topics (“clumps”) that you didn't know enough about to ask about.

I once read a study to the effect that the average physician waits only about thirty seconds after she/he has asked “How are you?” before she starts interrupting you. Although this is because the physician has a tentative hypothesis (that is, a diagnosis), people who

study these sorts of things point out that such a short time interval inhibits the ability of people to bring up relevant material, such as the fact that they just got back from the Amazon, or their partner just announced he has AIDS. These researchers urged physicians to come up with more “slots” (“Is that all?” “Is there anything else that you’re worried about in terms of your health?” “How’s your life going these days?”) in order to create places to drop new information into.

So try to create lots of slots, and wait for new topics to come edging out into the sunlight of your approving attitude. How will you know a new topic when you hear one? Several key indicators are:

- People use certain metaphors or expressions over and over again to describe something.
- People “punch up” certain ideas (i.e., they use expressive language or emphasis).
- People get emotionally agitated about certain things.
- People keep bringing up the same themes again and again.

This is, by the way, why I feel so strongly about tape recording interviews. Months and even years into a study, when I’ve finally figured out what the elements of my categories are, I go back to my very first interviews, and there they are, although my ear was not sophisticated enough to recognize them at the time.

Something I’ve worked on a lot is trying to capture those themes earlier rather than later, in order to cut down on the “idling time” I spend on my studies. I’ve become convinced that the best way to do this is to begin analysis of your data *immediately after your very first interview*. You’ve done the interview, you drive around the corner, and you start speaking into the tape recorder, spilling out whatever things surprised you, worried you, upset you, or gratified you. You start speculating on themes and elements. You keep reminding yourself of your research question, and you ask yourself how you are thinking differently about the question in light of this data.

As I emphasized in the section on participant observation, it is imperative that you write down your impressions of the interview—both the circumstances in which it took place and your first-cut impression of what was said—before you do anything else or talk to anyone. As you write, ask yourself the classic questions that all salsa-dancing social scientists have to answer sooner or later, and preferably sooner: what is it a case of, what kinds of theories might explain what you are hearing in the interviews, and what kinds of variables seem to be emerging? Maybe “variables” is too canonical a term here, but what we are interested in is what kinds of categories make sense to the people being interviewed. Particularly in the early interviews, these categories will be shaped and maybe even determined by the “clumps” that you developed sitting on your living room floor; but if you are patient and listen carefully, the people you are interviewing will start telling you about all the other categories that you didn’t know enough to ask about.

Just as it’s essential to write down your impressions and preliminary analyses of the interviews right away, you should also transcribe your interviews (or have transcripts made) as soon as possible, for the same reason: a written document calls on a different part of your mind than listening to a tape does. As you go through these early transcripts many, many times, ask yourself what surprised you, upset you, worried you, and so forth. Are categories perhaps emerging already? And, more to the point, how does this interview (or these interviews, if you do more than one in a single day) *change your theoretical view of what is going on?*

As you review the interviews, a key thing to look for is which questions worked and which ones didn’t. Some questions will set the interviewee off on several minutes of talking, and some of this talking will be right on point for what you are interested in. Other questions will run into the dreaded “Gary Cooper effect”: “yup,” “nope.” Monosyllabic answers like these are usually a sign that the question is too complicated, or too off point, for the interviewee

to connect with. My general observation is that there is an inverse relationship between the size of the question and the size of the answer. When I ask “how come?” I get pages of answers; when I ask a long, convoluted question I get a “yes” or a “no” or sometimes just a blank stare.

Keep in mind, however, that you should never jettison a question or a line of inquiry after a single interview, or even after a few. You need to do a number of interviews to verify that a line of inquiry or a question is a true dud, because it may be the case that the line of inquiry itself is valid, but something about the way you are phrasing the question just doesn’t work for the people you are interviewing.

When I have a line of inquiry (a “clump”) that’s just not working, I’m perfectly willing to ask my interviewees about this. Since I’ve spent a lot of time crafting the interview to make it interesting and accessible from their point of view, I will occasionally ask an interviewee something like, “I would have thought [blank] was something that people would be thinking a lot about, but in my interviews to date, it doesn’t seem to be. Am I missing something?”

This brings up an interesting point. In the canonical literature, which had to do with interviewing people in surveys rather than the long intensive interviews that we do, the idea was to ask every single person the same question in as close to the same way as possible. (The underlying concept was, I think, a “stimulus-response” model, with the hope that if you gave everyone the same stimulus, you would have “controlled” for variations, and thus any variation in the response must be a true variation.)

In contrast, the underlying concept in field methods is that people carry around mental maps in their head, cobbled together from the items (the “tools”) available to them in their cultural toolkits. Of course these maps are fragmentary, are rarely examined carefully, and often are internally contradictory. So what? What we really care about is getting at least a preliminary handle on what that cultural toolkit looks like, and how it is that some people use some tools to

craft a certain map while others use other tools to come up with an entirely different one.

Implicit in this model is the (perhaps questionable) assumption that these maps, at least for the things that are emotionally salient to people, are reasonable stable. To put it another way, if you are a pro-life person today, you are probably going to be a pro-life person tomorrow.

So I feel free to ask the kinds of questions that are anathema in the canonical tradition. I ask leading questions, provocative questions, and open-ended questions where I have no idea where we will end up. The particularly knotty item in this list from the canonical point of view is the leading question. It's sometimes argued that if you lead a person by asking a question in a way that suggests a certain answer, you will get that answer and it will be a product of the question, not of the underlying thoughts and opinions of the person being interviewed.

Nonsense! I have time and again in my interviews said to people, "What I hear you saying is . . . ," only to have them roll their eyes, sigh with exasperation, and tell me, "No, no, no! You've missed the point entirely! That's *not* what I'm saying at all! What I'm trying to get across is . . ." Which leads me to believe that in salsa-dancing social science, as opposed to the canonical kind, not only can you get away with a leading question, but sometimes it can be just the kind of question that irritates the interviewee so much that he or she will painstakingly outline for you everything you have been longing to hear in your interviews.

In my own interviews, these leading questions are reasonably low-risk because I have spent the entire interview trying to achieve rapport, that state where you and the person being interviewed feel comfortable with each other. This is easier said than done, especially when you are interviewing people with whom you disagree. But I have found that if you listen to the person you are interviewing with respect and deep attention, rapport usually emerges anyway.

One way of facilitating rapport is to make sure that the questions you ask are worded in a way that does not put the interviewee on the spot. This is where the canonical social scientists are onto something. In survey research, you take special pains to make sure that people are not forced to admit to socially undesirable behavior. So you ask something like, “Lots of people fudge their income taxes a bit [the ‘lots of people do it’ line] because it’s so complicated to figure out those IRS forms [the socially acceptable motive for doing it]; how about you—did you ever fudge your income taxes a bit?” (True-blue canonicals who write survey questions for a living would tell me that this isn’t a very good question because “fudge” isn’t very precise and may not be a word known to lots of people. But I’m using it to make the point that rather than saying “Did you ever cheat on your income taxes?” you have to phrase the question in a way that permits people to keep their self-regard.)²⁵

The only other thing that I think it is vital to know when you do interviews is that everything about you is grist for the mill. How you look, how you do your hair, what kind of shoes you have on, what bumper stickers are on your car—all these things lead the person you are interviewing to decide who and what you are.

And isn’t it only fair? After all, we look at what *they* wear, and how they choose to decorate their houses, and whether or not they offer us coffee, and how clean the house is, and so forth. The difference is that we, the researchers, need to be keenly aware of our image, and think about what message it sends to the people we are interviewing. In general, low-key, vaguely “professional” clothing is best, and it should be teamed with a low-key, vaguely professional hairstyle and (if appropriate) makeup. Especially when interviewers are young, it’s important to look old enough to do the job, unless of course you are planning on playing the “I’m just a student doing a project and I won’t get a good grade unless you help me” card.

I learned this the hard way once when a student was coming with

me to do an interview, my first step in training new interviewers. I had a long discussion with her about what to wear and recommended low-key, professional clothing, suggesting my own professorial style of dress as a model. (I like to think of my style as “classic,” although a stranger on a train once told me I looked like a librarian.) Imagine my surprise when I went to pick up this young person on the way to our research site two hours away, and I found her wearing a low-cut dress with a very short skirt, as well as a rather spectacular set of platform pumps. To top it off, she had a large and graphic tattoo on one of her thighs, the same thighs her dress was hard put to cover. Of course, this was the day we were interviewing a very conservative Mormon Republican, and it was also a day when I was running just late enough to make it impossible for her to go in and change. She spent the entire interview in the awkward postures of a circus contortionist, trying to cover up the tattoo. And wouldn’t you know it, the rather starchy interviewee turned out to be wearing a three-piece suit, highly polished wing-tip shoes, and a conservative tie. Now I make my students send me digital photos of what they are planning to wear to the interview.

Does this mean that you have to go out and get a haircut, buy a new wardrobe, and start pretending to be someone you aren’t, just to do an interview? Of course not. Or perhaps I should say, not usually. The point is that we telegraph our opinions, our class status, our religion, and almost everything else about ourselves when we go to do an interview. So it behooves us to be as conscious as we can about what image we are projecting, and to make sure that it fits into whatever it is that we hope to accomplish in the interview—that we think of our appearance as a tool, not unlike our interview schedule that we created while sitting on the living room floor.

If you don’t believe me about the extent to which we telegraph who we are, I’ll tell you about a set of interviews where I was interviewing right-to-life activists, who at the time were disproportion-

ately Catholic. I had the good luck to have a wonderful research assistant who helped me with the interviews. Although she had a Jewish last name, she was a “cradle Catholic,” as she termed it, and our interviewees knew this in a matter of seconds. Over and over again when I would listen to the interviews that my assistant conducted, early in the interview the person being interviewed would say, “And you’re Catholic, aren’t you?” And in my own interviews, they would likewise say, “You’re not Catholic, are you?” So if people can read something as subtle as religion from interviewers very early on in the interview, before we’ve revealed much about ourselves, you can bet that interviewees are tracking lots of other things about us.

The lesson to remember is that an interview, like any other form of salsa-dancing methodology, is theory-driven, that is, theory dictates where we will start interviewing, what we will ask, and when we will decide to come out of the field. On this last point, students often ask me when they have done enough interviews. My short answer is “when you can move your lips in every interview” — in other words, when the categories are so saturated, as Glaser and Strauss would say, that you are not learning anything new.

Focus Groups

Although you wouldn’t know it to read the *American Journal of Sociology* or the *American Sociological Review*, focus groups have a long and honorable history in sociology. Like much of our modern technology in the social sciences, they were first used during World War II, when Paul Lazarsfeld and his colleagues developed them to analyze the effectiveness of Nazi propaganda. Lazarsfeld and friends found that by interviewing people in groups, they could quickly begin to pull out what themes and aspects of Nazi ideology and claims resonated with people. From the standpoint of Lazarsfeld and his colleagues at the Bureau of Applied Social Research located at

Columbia University, focus groups were an inexpensive (relatively speaking) and efficient way to gather data quickly.

After the war, focus groups fell into disuse among sociologists and were banished to the hinterlands of market research, where they remained until relatively recently. During their long sojourn in market research, focus groups acquired most of the technical developments that we associate with them today. The use of a room with microphones and (sometimes) video cameras, the insistence that only one recognized speaker talk at any one time (to facilitate transcribing the recordings), and the use of one-way mirrors so that observers could watch the group—all of these came out of marketing research. (I hasten to add that the observation of the group by people behind a one-way mirror was done with the knowledge and consent of the people in the focus group. As far as I can tell, there was no covert research in which people did not know they were being observed.)

In the late 1960s and early 1970s, focus groups were reborn as “small group analysis” as social scientists tried to ascertain the whys and wherefores of group dynamics. Why do some people seem to take “natural” leadership in groups? Why do some people in small groups seem more reliable or trustworthy than others? What kinds of things are going on below the surface of small groups that shape the decisions people in those groups make? These were the kinds of questions that drove social scientists back to small groups.

After this resurgence of interest in focus groups in the late 1960s and early 1970s, focus groups dropped out of the social science repertoire once again and moved into psychology. But two people, political pollsters of differing stripes, brought focus groups back. In 1994, the Republican pollster Frank Luntz, whom we heard from earlier in this book, wrote a much-discussed article in which he warned his fellow pollsters that traditional political polling (read “survey research”) presumed that the categories of thought people brought to public opinion polls were stable and clear-cut. (He didn’t

say it in exactly this way, but close enough.) You may recall that earlier in this book I quoted Luntz as saying that “if . . . polling is so clear-cut and conclusive, why is there a tremendous discrepancy between polling firms in their reported data on abortion? . . . In today’s post-partisan politics, there are too many shades of gray, too many, ‘yes, but what I really think is . . .’ attitudes, too many voter priorities that cannot be prioritized. . . . The elements that have made up public opinion have changed, so must its measurement.”²⁶

Luntz proposed that one remedy for this was an increased use of focus groups, much along the lines we have been discussing. In such groups, the facilitator can probe, push, and provide people the venue in which to think out loud about what they really believe. In the language of social science, people in focus groups can use the opportunity to lay out the social categories that are relevant to them.

I believe the renaissance of focus groups was anticipated almost a decade earlier by another pollster, this one a Democrat. Stan Greenberg, who had been trained as a political scientist and had once been a marxist, was intrigued by the emergence of so-called “Reagan Democrats.”²⁷ These were folks who had voted Democratic most of their lives, but who suddenly defected to vote for Ronald Reagan in 1980. In searching focus groups with these people, most of them white ethnics, Greenberg developed an on-the-ground sense of what two political scientists, Edward Carmines and James Stimson, were calling “issue evolution.”²⁸ Not to put too fine a point upon it, in the wake of the 1960s many working-class white people had come to decide that the Democratic Party had become too attuned to the needs of poor African-Americans and had lost touch with the needs of people like themselves.

To reiterate a point I have made many times, canonical survey research, whether it is social science surveys or political polling, *can only find the distribution of a population into categories that we have defined a priori*. If a category exists that we have not thought to ask

about (the infamous “omitted variable” canonicals worry about all the time), we can never know how different things might look had we included that category.

Which brings us back to focus groups. In modern terminology, focus groups are much more “interactive” than surveys, and I would suspect more interactive than even the kind of interviews that I do. Remember how I asked you to create “slots” in your interview so that people could tell you things that you hadn’t thought of asking about? Well, the very structure of a focus group means that people are constantly creating slots for each other. While you might have to do a dozen interviews to find out that what people are really worried about is X (whatever X is), because you did not know enough about the situation ahead of time to inquire about X, a good focus group, run by a good facilitator, will tend to ensure that X will come up if it’s something that the people you are studying really, truly are worried about. (In more formal terms, if X is a category relevant to the social maps of people in social group Y, a focus group increases the chances that X will be brought up.) I often say that with small children, the level of chaos goes up with the cube of the number of children, such that three children are 27 times more chaos-producing than one child. It’s the same with focus groups: a nicely sized focus group will create an exponential number of slots for other people to fill in.²⁹

The only problem I see with focus groups is that mainstream social scientists, unlike political pollsters, tend to view them as unscientific. As I mentioned, the journals by and large do not publish articles whose data are derived from focus groups. But that’s all right. Even if you can’t use the data for publication, you can use it (as part of a multi-method approach) in your book, and you can use it to jump-start one of the other methods that you’ll use in your journal articles. If you can find a group of people likely to represent the kinds of data outcropping that you would use for interviews or participant observation, you could do a lot worse than to run a focus group at the outset, so that you can hit the ground running; in the

middle of your research, to make sure you are getting all the relevant categories; and even at the end, to make sure that you have now gotten all the relevant categories. You don't even need to tell journal editors that you have run focus groups at all—it can be our secret.

Assuming I have persuaded you of the utility of focus groups, at least as a way of jump-starting other research or checking your categories at some later point, what do you need to know to run a good focus group?

In fact, almost all of this will sound familiar to you from the methods we have discussed already. First, you need to write a guideline for the facilitator of the focus group to use. You will use the exact same techniques you used to write an outline for interviews, complete with 3 by 5 index cards and the like (or the electronic equivalent). You will think of all the things you would like to know about the people you are studying; you will write one or several questions about each of these topics on the index cards; you will sit on the floor and arrange these cards into clumps; and you will put in “turn signals” so the facilitator can signal when he/she is moving from one set of questions to another.

Next, as in every other field method we have considered, you have to think about sampling. Obviously, you want people who are part of the social world you want to study, so you might want to review the earlier sections on sampling to think about how to get people who would make up a good focus group. Political pollsters, for obvious reasons, usually select from voter registration rolls, and they try to stratify by variables they think will be important: working-class whites, high-status African-Americans, and so on. Again, do a thought experiment: if you could gather into a room the most knowledgeable people on the issue that you are studying, what kinds of people might they be? And where could you find a group of those kinds of people?

The ideal focus group, in my opinion, has between six and ten people. If it's much smaller, people start feeling shy; if it's larger,

people get into the “let George do it” phenomenon, where they sit back and let other people do all the talking. If you are going to run focus groups, it makes a lot of sense to have more than one, just so you can reassure yourself that you are picking up real opinions rather than something that is a product only of group dynamics.

In the ideal situation, you would have a room with a one-way mirror, of the sort engineered by market researchers. This permits you to have one person actually running the focus group (the facilitator) while another observes from behind the one-way mirror.³⁰ My own preference is to train another person to be the facilitator so that I can be the observer. The observer watches, ideally behind the one-way mirror, and can see “meta-themes” emerging that the facilitator, caught up in his or her written focus-group guide, sometimes can’t see. As you, in the role of observer, watch people in the focus group warm to a theme, you may find yourself thinking, “Whoa! These people are really talking about X.” Then you or another person can run into the focus-group room with a note instructing the facilitator to ask some more probing questions about X.

A good facilitator will not get flustered by this process. In her introduction to the group, where the facilitator tells people the location of the bathrooms, why only one person should talk at a time, and where the coffeepot and doughnuts are, she will tell people that there are observers behind the one-way mirror. In her most charming style, she will tell people that since facilitators get caught up in the moment, sometimes the observers can see or hear things that they might want to explore a bit more. When that happens, someone will come in with a note, and new questions might come up.

The main problem with focus groups is that they take a lot of footwork to set up. So do interviews, of course, but focus groups are much more time-intensive to get up and running. You need a central location, ideally with a one-way mirror room. (These used to be very common on the college campuses I’ve been associated with. Many have been decommissioned, but some have been taken over

by psychologists, so it's worth asking around.) Of course, you can do a focus group without such a room, but the group dynamics are often such that if you sit in with the facilitator, you too will get caught up in the immediate situation and will lose track of the bigger picture. Videotaping a session is an alternative, though it usually means you don't see the bigger picture until afterwards, but at least you can use those insights in the next group that you run.

Whatever room you choose should be in a location where people can park easily, and where they feel safe walking around in the evening. (If you run focus groups in the daytime, you will get students, elderly retirees, and a few lone dot-commers who have made a mint and are now a bit bored.) You should offer people coffee and doughnuts (or croissants, if you are feeling upscale), and you should make sure that a bathroom and a water fountain—or a few flats of bottled water—will be available. All of these make focus group participants feel comfortable and, we hope, talkative.

Your main headache is likely to be finding people to participate. Unless you have a small, bounded group (say, all the parents of fifth graders in your local public school), finding enough people for a focus group who can all meet at a mutually agreeable time is likely to take weeks of your time. If you can afford it, you might want to hire a scheduler, since this is probably the thing that will be most time-consuming for you.

Remember that whatever your sampling strategy may be (sampling in our sense of the word), you will have to—metaphorically speaking—kiss a lot of frogs before you meet a prince. Set out your parameters ahead of time: if you are interviewing parents of fifth graders, will you accept the ex-boyfriend of the stepmom, who has time to talk to you when the stepmom herself doesn't? (Of course the answer here is a theoretical one, depending on what it is you want to know.)

The good news here is that after you have done all the heavy lifting—finding a suitable space, finding suitable people, writing an in-

terview guide, training a facilitator, setting up some kind of recording system—you can relax, knowing that you are likely to get much more data than you imagined you could.

Content Analysis

Content analysis is simply a way of systematically surveying how often and in what categories things occur within texts. Like many of the methodologies that have fallen somewhat into disuse these days, this one has a long and honorable history.³¹ Paul Lazarsfeld (who seems to have invented almost every social science method known to humans in his spare time) and Harold Lasswell laid out the framework for doing content analysis as long ago as the 1920s and 1930s. Even forty years ago, content analysis was still all the rage, as the psychologist David McClelland tried to speculate about international levels of economic growth by analyzing the frequency of achievement needs (“nAch”) in textbooks for children. Serious thinkers in the period tried to link the frequency of items in texts to the larger context in which those texts were being produced.

Call me a neo-positivist, but while I am willing to concede that levels of ambition portrayed in elementary school textbooks might be related to levels of economic growth, I am distinctly skeptical that we can show anything other than a correlation, and I always want to know more directly how things are allegedly influencing other things.

On the other hand, content analysis really can be useful for making points that are difficult to make in other ways. For example, Deanna Pagnini and Philip Morgan wrote an article describing how African-American and white women viewed out-of-wedlock pregnancy during the early years of the twentieth century. Using WPA oral history interviews recorded during the Depression, Pagnini and Morgan coded every mention of out-of-wedlock pregnancy and how the woman and her significant others responded to it. The African-

American women were not pleased to be pregnant and unmarried; the white women were distraught. On the basis of this research, Pagnini and Morgan suggest that racial attitudes toward unmarried childbearing have been different for the better part of a century, although there is some evidence that now they are beginning to converge, with whites moving more closely toward African-American attitudes and practices.³²

What makes this article particularly interesting is that it draws on a relatively neglected resource (oral histories collected in a very different time for a very different purpose) and uses them to get at something that is important and timely, namely evidence as to whether there are different racial attitudes toward unwed motherhood, and if so, when they emerged. The Pagnini and Morgan article isn't "dispositive," as my legal colleagues say, meaning that it doesn't end the argument for all time, but it's still a useful piece of evidence about subtle attitudes that are hard to tap in other ways.

Similarly, I once used a content analysis to show how a report on teenage pregnancy published in 1976 by the Alan Guttmacher Institute (at the time, the research wing of Planned Parenthood) essentially created the modern notion of "teenage pregnancy" as we now understand it. I could show, with a fair degree of precision, that there really was no such thing as "teenage pregnancy" before the AGI published its report, known as *11 Million Teenagers*. True, there were young women under twenty getting pregnant before 1976—although the peak of young motherhood actually happened in the Eisenhower administration—but the particular configuration of young, unmarried, equal-opportunity teenage pregnancy that could happen to your daughter or mine was, as I was able prove, almost entirely a product of the Guttmacher Institute's report. Using the Readers' Guide to Periodical Literature, I could show that prior to 1976, there were "unwed mothers" (presumably of any age), "school age mothers" (presumably women under 18), and "premature parenthood"; after 1976, there was "teenage pregnancy" in the way we

have come to experience it today, with virtually all of the (exponentially expanding) coverage using not only the language and configuration of the Guttmacher Report, but often its figures and conclusions as well.

Finally, as people begin to get interested in both “discursive shifts” (like the one I just outlined) and “frame analysis,” content analysis is the tool of choice to find out when and how people start talking about things in a different way, and when a topic gets framed in a new way.

There are lots of really good books on content analysis. But the truth of the matter is that the intellectual tasks we do for content analysis are the very same ones as those we do for data reduction and analysis more generally. So, if you can’t wait to get started, go to Chapter 10.

Exercise for Chapter 8

Now we get down to the most fun of all. Given everything we have done to date, what method looks best to you? What calls out your name—ethnography, interviewing, focus groups, or the close examination of texts?

In a variation of something I said earlier in the book, write a few paragraphs about why this method (or combination of methods) is the absolutely, positively best way to get at what you want to know, and—equally important—why this one is better than the others.

Once you have settled on a method, the second half of this exercise is to start doing the prep work—write an interview schedule, outline where you will do PO and what you will look for there, or write a handbook for the facilitator of your focus group.



Historical-Comparative Methods

As methods go, “historical-comparative methods” are something like that drawer in your kitchen where you put all the useful stuff that doesn’t logically go in other drawers. Mind you, I’m not saying that it’s the *junk* drawer—quite the contrary. In my kitchen, there is a drawer underneath the Tupperware drawer where I put the pie weights, the jar-opener, the madeleine pans, and a whole bunch of other things that do only what they do, and nothing else. (This is in contrast to, say, the silverware drawer, where every piece of silverware does double and maybe triple duty—the spoons become trowels, the knives become screwdrivers, and so forth.) The things in this special drawer are unique, and therefore of high value to me because they do what they do better than anything else in the kitchen. So it is with historical-comparative methods.

As we’ve been looking at all of the previous methods, I have pointed out over and over that what we use methods for is to enhance our capacity for *pattern recognition*, which in turn is the first step to our building theory. I even started out this book with a

somewhat acerbic but on point comment by my colleague Loïc Wacquant to the effect that many people think the only point of using qualitative methods is to tell a good story.¹ But something in the very nature of historical and comparative methods urges people not to just “tell stories.” (I think if you just wanted to tell a story, that is, craft a narrative of what happened, you would have found yourself more drawn to history. The difference between a social historian and a historical sociologist, although the lines are getting more blurred every day, is that historical sociologists aspire to discovering *patterns*, a.k.a. *theory*, while historians in good standing tend to think that they will get their wrists slapped if they try to write anything resembling theory.)

So theory is, I think, bred in the bone of historical-comparative methods. To simplify a great deal, researchers turn to these methods to answer one of two questions: either (a) what events in the past shaped how this turned out in the present? or (b) why did things turn out this way in one place and another way in another place?

Implicit in both of these questions is a consideration of what we have been calling elements, and others call variables. Each question implies that some element or variable was present (or in some cases, absent) in the past, and that’s why things turned out the way they did, or that some element or variable was present (or absent) in location A and not in location B, and so that’s why things turned out one way in location A and another in location B.

A classic example of this is Theda Skocpol’s ambitious book *States and Social Revolutions*, where she takes on what is probably one of the biggest questions of all, namely why you get revolutions in some places and not others. In her comparison of the various revolutions that we know of, she concludes that in order to get a revolution, you need both pressure from below (the lower classes have to become mobilized) *and* disorder at the top (elites have to be conflicted or divided regarding what to do about the disorder brewing in the lower ranks). For Skocpol, then, the *elements* are (a) disorder in the lower

classes and (b) ambivalence in the elites. Pay attention here, because we will be coming back to this in the next chapter.

Now the more philosophically-minded among you will be thinking that what I'm really talking about here is *causation*, and that I'm playing fast and loose with it. You would be right. Much of historical-comparative research is much more nuanced than the model I've just presented, but I am using it to make a point, namely that inherent in most historical and comparative methods are taken-for-granted assumptions about theory building.

One of my dearest colleagues (actually, I'm married to him) has a term for this kind of sociology: he affectionately calls it "Big Think." To borrow his phrase, most historical and comparative methods are harnessed to the kinds of questions that would properly fall into Big Think. So questions like: what causes revolutions? or how did the American welfare state turn out so differently from the welfare states of comparable European countries? or do historic Supreme Court decisions such as *Brown v. Board of Education* or *Roe v. Wade* actually change people's lives in any material ways? are all questions that address *by their very nature* issues about the relationship of power and authority in society.

I said early on that one of the problems we salsa-dancing social scientists face is that we have to address the connection not only between the forest and the trees, but the connection of the bark to the trees, the trees to the forest, and the forest to the whole ecosystem. Well, the kinds of questions that historical and comparative methodologists are inclined to ask almost always *presume* that the real question is this key one, namely how the bark relates to the ecosystem.

So you would think that salsa-dancing social scientists would not have much to say to the average researcher using historical and comparative methods, but this is not the case.

I want to argue both sides of the street here. First, I want to point out that historical-comparative social scientists have intuitively

come up with most of the practices that I've borrowed as the key steps for my kind of social science; to put it another way, I learned most of the steps to salsa-dancing social science by thinking about historical-comparative questions. But second, I want to argue that the key word in the previous sentence is *intuitively*. Most historical-comparativists learn their methods in the same way that most field researchers learn theirs, namely by practicing them in the presence of a mentor or adviser who nags them into shape.

This is a time-honored tradition, and I would be perfectly happy to leave it alone except for the fact that (a) graduate school (or assistant professorhood) is expensive, and (b) graduate school (or assistant professorhood) is stressful. Who needs the extra expense and *tsuris*?² So I would tell the budding historical-comparativists among you exactly what I have told your field-working colleagues in other parts of this book, namely to go through the salsa-dancing steps as we have been developing them.

So, first, what do you think you know? What is your presumed theory about what is going on? If you, like Theda Skocpol, are taking on the question of why there are or are not revolutions in a certain time or place, what have others bequeathed to you as possible elements, that is, collections of elements? (You know from the earlier discussion that Skocpol argues that it's disorder at the top *and* bottom, so now you have at least two elements. What else? What other things have you come across in your reading and thinking that you think might be related to your question?)

Next, where are you likely to find a rich source of data to pursue your question? The ongoing dilemma of the salsa-dancing social scientist is probably even more acute for you than for the rest of us. Chances are, you've found a really juicy case—say a place where there was disorder among the lower classes and the elites, but there *wasn't* a revolution. Assuming that you have operationalized your variables properly—or in salsa terms, defined the elements of your categories well—then you have a counter-case that you want to ex-

plain. (Be sure that you have taken this first step, by the way. How did Skocpol and the many others who have followed in her wake define “disorder”? Are you defining it in another way? Are you *absolutely sure* that the explanation for why there wasn’t a revolution in your case, even though you have the requisite forms of disorder in both the lower classes and the elites, isn’t just one of definition?)

If you do have a juicy case, you need to do what the owners of all other juicy cases considered earlier do, namely explain to us, the readers, why your case is a good place to test the theory that both disorder in the lower classes and ambivalence and division among the elites are needed if you want to have a revolution.

We’ll leave aside for the moment the question of whether your elements (disorder in the upper *and* lower orders) are a case of something being a *necessary* but not a *sufficient* element. In other words, it might be the case that you need to have disorder in both the upper and lower reaches of society (i.e., these are *necessary* elements of a revolution), but having these elements doesn’t necessarily guarantee that you will have a revolution (i.e., they are not *sufficient*).

So what is it about your case (or the case you will be going out to look for) that makes it a good place to get data in order to think about the kind of theory you want to be building? In our hypothetical case, your revolution that you found that wasn’t one, it seems to me that this in itself could be your justification for studying it—it had the elements that prior theory thought would lead to a revolution, but a revolution didn’t happen. Once you tell me that in some detail, you’ve set up an interesting question for yourself. The one thing I would worry about here is that tricky problem of operationalizing your variables, that is, defining your elements. In much of the historical-comparative social science that I read, it’s hard to build theory because people don’t define exactly what it is that they are using as elements or variables. It’s probably true that you and I, like Justice Potter Stewart, know a revolution when we see one, but it’s less clear exactly what elements have to be present for us to define some exemplars (the French Revolution, the Russian Revolu-

tion) as being inside the category, while we define others as being outside (the “Glorious Revolution” in England in 1688).³ In the terms of my argument, Justice Stewart was throwing up his hands at the prospect of operationalizing the variable of pornography. That might be well and good for a Justice of the Supreme Court, but you and I cannot afford such high-handedness.

What I’m saying here is what I’ve said about choosing cases in the context of other methods: imagine defending your choice of case (either the juicy one that called out your name, or the one you found after much thinking about the aspects of sampling that we discussed earlier) to the meanest, pickiest critic, the person most inclined to hate your work. What *I* would do in the hypothetical example that we are using here, to defend my case from the accusation that I hadn’t properly defined my elements, would be to (a) find out what the people whose opinions matter in my field think are the classical works on revolutions and read them, and (b) look very carefully at how each of these authors (say, Billington, Tilly, Huntington, Moore, Skocpol) has defined what a revolution is. I would even note the page numbers on which these definitions occurred, so that I could cite chapter and verse to a skeptical person who thought perhaps I wasn’t measuring the same thing that these other people were.⁴

At the next stage, I would go through all of these classical books on revolutions and look at how their authors explain a revolution, that is, what categories in their opinion account for the presence or absence of a revolution. Again, I would write down obsessively and carefully *exactly* how these famous authors of the classical works on revolution define their causes, complete with chapter and verse. By the way, once you have done this, you can take one small additional step, and really have readied your house for the Muse to arrive. If, as you were scrutinizing how others had *defined* both revolutions and the causes thereof, you were to write down for each famous author how he or she proposes linking the relationship between their independent (“causes”) and dependent (“revolutions”)

variables, you would have what my JSP colleague Martin Shapiro calls a “propositional inventory,” namely a list of testable hypotheses on which you can unleash your historical-comparative methods. You are probably doing this somewhat intuitively, of course, but why not do it systematically, as it is very little additional effort for a large payoff.

In more formal terms, what we have just done here is to operationalize our independent and dependent variables, which once again shows how historical-comparative methods are a different breed from the other kinds of field methods that salsa-dancing social scientists use. You’ll remember that I promised to rescue you from the dilemma of an adviser who insists that you come up with independent and dependent variables, whereas most of you don’t have them as such but just have a good, juicy case study that begs to be explained. By a somewhat circuitous route I’ve come again to a point I made earlier in this chapter, which is that historical and comparative methods incline to theory, and thus often have, either readily available or lurking just below the surface, elements which can serve as independent and dependent variables.⁵

Thinking through how you would defend yourself against your pickiest critic is also a good means of defending yourself from getting dismissed in another way as well. Let’s say that the critic brings up the “necessary” versus “sufficient” causes, arguing that your case doesn’t prove anything except that the kinds of causes that Skocpol looked at were merely necessary but not sufficient causes of revolutions. If you have thought about these kinds of criticisms early on, you are entitled to shout “aha!” at this point, because, in fact, showing that something is a necessary but not a sufficient cause is itself a contribution to theory. (If your critic still looks dubious, just say something about black swans and Karl Popper, and he will likely be intimidated and quiet down.)⁶

I stand in awe of historical-comparative social scientists, really I do. Not only do most of them intuitively come up with the secrets of salsa dancing without breaking a sweat, but most of them excel at

the most difficult task of scholarship. I've been using different metaphors throughout this book to try to capture this elusive skill; it's the capacity to shift from close-up inspection of particular pieces of data to a wide-lens view of how those little pieces of data (the bark, I've called them) fit into the forest and, moreover, the whole woodland ecology.

I've spent a professional lifetime figuring out how to do this without getting vertigo, and I still have to go take a nap when I've done even a relatively short stretch of it. But the kinds of scholars who are drawn to these methods intuitively (and, as I've told you earlier, all of us are intuitively drawn to some methods and not others) don't even seem to know it's *hard*. So go study with the masters (you know who they are, or will within a short time), and come back to this chapter to remind yourself to work systematically and smarter.

Exercise for Chapter 9

Since you know from this chapter that I personally think almost all historical-comparative researchers are really salsa-dancing researchers, whether they know it or not, use this chapter to formalize some of the things you have learned earlier in this book. For example:

How did you sample your data and/or documents, and what potential biases could possibly have been introduced in the process?

How do you plan to generalize—logically, if not statistically—from your data to a larger world?

What is this a case of?

For researchers who are not bred-in-the-bone historical-comparative types, I suggest that you spend this part of the exercise thinking about how your own case (flirting in the workplace, water privatization) looks when you bring in both a historical and a comparative lens.

What did flirting at work use to look like, say, in 1950? Where is water privatization working out unusually well or badly, whichever is the bigger contrast to your case?



Data Reduction and Analysis

In my youth I was something of a Julia Child junkie, reading her books with gusto, sneaking out of graduate school lectures so I could hear her inimitable voice on television telling me to “flaMBÉ the chicken” (if you’ve ever seen even one of her shows, you can probably hear her voice right this minute). Somewhere in almost every show, Julia would introduce us to a “reduction,” where she would take the juices left in the pan after sautéing something, throw in a big splash of something alcoholic (while intoning “nevah use a wine to cook with that you would not drink”), and then, over high heat, boil things down until they became thick and syrupy. The essence of flavor, this syrupy “reduction,” was the basis of all good sauces.

So it is with social scientists. At this point we have accumulated a big pile (digital or otherwise) of data: we have participant observation notes, interview transcripts, focus group transcripts, or an endless series of notes from our comparative or historical research. What do we do next? Like Julia, we “reduce” them into the essence of what we are cooking.

We have two tasks after we have gathered our data, namely to reduce our data to something we can manage, and to analyze our data

in meaningful ways. Here is still another example of how the salsa-dancing enterprise is very different at its core from the work that canonical social scientists do. Although there will be variations in how canonicals work, in general their relationship to data is a linear one: they think about how they want to gather the data (issues of sampling, operationalization, and generalization); they gather the data; they clean (reduce) it; and then they analyze it. We do all of these things too, of course, but we do them over and over and over. And worse yet, for us, reduction and analysis are so closely intertwined that often we can't tell the difference.

Every salsa-dancing project will be different, of course, but there are some general guidelines I can give you to help things along. First, keep in mind that this process of reduction and analysis is really an ongoing one that begins the first night you come home from gathering data or even the very first day you start your project. As we contemplate that first interview, or set of field notes, or focus group experience, or day in the archives, we need to remind ourselves that our most important job today (and all subsequent days) is *pattern recognition*. Faced with the noise and discomfort and disorder of our very first batch of data, we should ask ourselves, "What am I seeing here that could possibly be a pattern?" We make private bets with ourselves about what features that have caught our eye in this first set of data will turn out to be actual patterns. We know that we will often be wrong—lots of stuff that looks colorful and interesting will just turn out to be noise—but we hang in there.

Each interview or set of field notes only refines our sense of what is going on. If we find ourselves confronting recalcitrant data that never seem to tell us anything, we redouble our efforts. We pore over our field notes or transcripts or historical-comparative data until we can recite them in our sleep. We read all the theory we can get our hands on. We give ourselves permission to read anything that our heart wants to read, on the theory that the id knows the book it wants to write. And finally, we talk.

I told you earlier that it was vital not to talk to anyone until you had gotten your field notes and/or your research memos written. But there will come a point when, confronted by data that insist on sitting there mutely, you will need to talk. Talk to your mother (or father), siblings, best friend, adviser, fellow researchers. Use copious amounts of caffeine and carbohydrates if that works for you. (Herbal tea and spirulina if not.) Tell everyone who will listen about what is going on in your data.

Almost without exception, the professional social scientists among your listeners will say, “What you’re really saying is . . .” When this comment comes from an adviser or other person in authority you have to beware, because there is a tendency for these people to try to hijack a research project and turn it into something more recognizable. This is where you get to say, “No, I don’t think so,” or just listen politely if the person has some authority over you. (This is also a good time to practice the “ignore 50 percent” rule, where you ignore half of what anyone in authority tells you.) But eventually, as you try to explain to people why they aren’t getting what you find so interesting about the data you’ve collected, you will, I promise, stumble onto a “hook,” a theme that ties your data together.

At the same time, however, you are steadfastly analyzing your data. That is, since the beginning you have been writing memos to yourself as to what is going on, and exploring those bets with yourself about what is and is not a reliable pattern. It may sound a little mysterious, but there comes a point in every salsa-dancing research project when you realize that you are hearing the same things over and over and over again.

When this happens, the time has come to code (i.e., reduce) your data. I wrote my first two books coding all my data by hand, my last two by computer, and I’m hard-pressed to tell you which is better. I’ve finally come over to the side of computer analysis (CAQDAS—computer assisted qualitative data analysis) only because I have become enamored of Charles Ragin’s method of Boolean analysis, which I will describe later in this chapter.

If you want to code your data by hand, you simply take all of it—transcripts, field notes, historical data, and the like—and photocopy it twice. The first one becomes the reference copy, the second becomes the coding copy; eventually you will copy this second coding copy and it will become the analysis copy. As you go through the coding copy, you will find once again that the same themes keep coming up over and over. Figure out a way that works for you to annotate these themes. (I'm a big fan of the outline style I learned in eighth grade, so I mark the major themes as I, II, III, IV, and so on.)

You can use different-colored markers to literally highlight each theme, and you continue in this way until you have coded all of your data. Now you take this coding copy, marked up with Roman numerals, and photocopy it one final time. Then cut apart each highlighted section and paste it on a 5 by 7 index card.

Now, take your stack of index cards (chosen so slips from your 8½ by 11 paper will fit on it) and place all the similar codes together, so that you have a pile of Roman numeral I's, a pile of II's, and so on. As you go through each of these major codes, it's my experience that you will eventually begin to see variations on a theme. In short, you will find subcodes. You should treat subcodes the same way you treated codes, so at the end of the process you will have a nice outline-organized set of codes and subcodes (and maybe even sub-subcodes).

A not trivial benefit of this is that when you have coded your material in this way, you have effectively outlined all the main arguments of your book. Add an introduction, a review of the literature, and a conclusion, and your book—at least a rough draft of it—is well on its way to being written.

If you are going to computerize this process, you will probably want to invest in a CAQDAS software package. A list of all the major ones, complete with free trial downloads, is available at <http://caqdas.soc.surrey.ac.uk/>, a qualitative research site located at the University of Surrey in the United Kingdom.

I have to tell you that although I use a CAQDAS package, and

have tried all of the major ones, I dislike them for two reasons. First, all of these programs are a labor of love produced by dedicated researchers who want to make life easier for the rest of us. They are not, repeat not, in a league with most of the commercial software you use in the rest of your life. As such, they have a steep learning curve, and they are often filled with bugs. For me, it's proved a real toss-up as to whether the colored marker system or the CAQDAS system is more labor-intensive and frustrating. If I knew for sure that I would never do another Boolean analysis of my data, I don't think I would invest time and money in a CAQDAS.

My deeper problem is a more philosophical one. I don't know whether it's the nature of the programs themselves, or the way I relate to material when looking at a monitor as opposed to working on paper, but I find myself working in exactly the opposite way with CAQDAS than I do with the colored marker scheme that I just described. With CAQDAS, I find myself coding lots and lots of subcodes that eventually add up to a major code, instead of first identifying big codes and then breaking them down into smaller parts. For some reason, I feel farther away from the data than I do when I am coding on hard copy.

The good news is that with all these free demos on the Web, you are in a perfect position to try several of them in addition to trying the marker system, and find out which one works best for you.¹

In all cases, however, one more labor-intensive step awaits you. As you've been coding, you have been creating a codebook that reminds you what each code covers and, more important, what goes into a code and what does not. Now you must refine that codebook such that a person who does not know anything about your project could code your data, and code it pretty much the same way you would.

Having done that, you need to go and find that person. In other words, you need to have a least a sample (and here is the only place I would use a random sample) of your data coded by an unknowing

coder, that is, someone who has no idea what hypothesis (or hypotheses) you are generating. There is a very handy statistic, Cronbach's alpha, that permits you to assess the degree to which you and your reliability coder are coding things in the same way, and you need to let us know somewhere what this value is. This is a technique that keeps researchers from consciously or unconsciously coding things the way we want, and equally important, it reassures others that we have been very, very careful.

Let's face it—we are all human. On top of that, everyone in the universe who cares about our area of research but hates what we have found will want to pick holes in it. Steven Shapin points out that scientific “truth” was once the province of the gentleman, who literally staked his honor on having been as careful about doing research as he could be.² And we all know how snotty people in the hard sciences can be when they tell us that someone has found a specific result, but “no one else has been able to replicate it.” The clear implication is that the scientist in question is either careless or downright dishonest.

So we have to protect ourselves and our work, and, not to be too grandiose about it, the integrity of the enterprise. Using a reliability coder *who does not know the hypothesis you are generating* is just one of the practices that we do to keep ourselves honest, and to keep our work as above suspicion as it can be.

So there you have it. While canonical social scientists have an easier job in some ways because they approach their data in a linear way, our salsa-dancing way of dealing with data means that we don't have to quail at the thought of “analyzing” our data because we have been doing this all along. And on that note, I'd like to introduce you to a very exciting new way of analyzing data.

From my point of view, it's one of the most exciting innovations in qualitative research (that is, the larger category of which salsa-dancing research is a part), namely the development of a systematic way of thinking rigorously about the data we are gathering. In fact, it's not

only exciting, it's possibly a paradigm-changing way of doing our discipline. Throughout this book, I've argued that sociologists are socially located, and that our curse and our gift is that we find ourselves in the complex and maddening position of trying to study the social world at the same time that we live in it—"the fish studying itself studying water," I called it earlier. Up to now, I've discussed this meta-problem on the level of the working social scientist, and have described some "practices" that I, as one working sociologist, have evolved to help both myself and my students work around the "fish problem." Now, I'll tell you how some brilliant and innovative sociologists have figured out a way for our *discipline* to work its way around the fish problem.

Remember how I told you in the last chapter that comparative-historical social scientists are doing salsa-dancing social science, but they (mostly) don't know it? Well, this innovation came about, as you might guess, from a comparative-historical social scientist par excellence named Charles Ragin.³

As you know from the early pages of this book, American sociology was born at a particular time and place, and hence found itself faced, like many another emerging profession of the era, with the need to execute a "professionalization project" to distinguish itself from the more activist, more religious, and more "feminine" enterprise that preceded it, embodied in people like Jane Addams. One of the first strands of the professionalization project was an embrace of what we would now call "holistic" methods, but in time, with the advent of World War II and developments in probability theory and mensuration, American sociologists turned to quantitative methods as the *sine qua non* of the scientific enterprise.⁴

Much was gained in the process. The capacity to engage in random sampling, and new ways of thinking about how to standardize the gathering and analysis of information, led to significant increases in precision and predictability.

But something was lost as well. Increasingly, as social scientists be-

gan to rely on a notion of scientific rigor that was deeply indebted to the natural sciences, they came to depend upon a model of causality that presumed a linear relationship between cause and effect. (In technical terms, sociologists expected relationships that could be expressed as vectors.) As Charles Ragin argues, social scientists, like natural scientists, began to expect that the sine qua non of scientific rigor was the existence of both a necessary cause and a sufficient cause at the same time.

Philosophers long ago distinguished between these two kinds of causes, and I'll try to make them clear (or clearer) to you. A "necessary" cause is something that has to happen (let's call it A) for something else to happen (let's call this thing B). That means that every time we see thing B, assuming a necessary cause A, we would also expect to see thing A show up as well. But—caution!—it doesn't work the other way. We could perfectly plausibly expect to see a whole flock of A's without expecting to see any B's at all. But if every A is followed by a B plaintively calling out A's name, then we have a "sufficient" condition. So this time we would not be surprised to see B's all alone, although we would expect every A to be part of a pair. The way I remind myself of which is which is to visualize it like this:

Necessary		Sufficient		Necessary and Sufficient	
A	B	A	B	A	B
A		A	B	A	B
A			B	A	B
A	B		B	A	B
A					
A	B				

Because canonical social science tends to expect a causal relationship which is both necessary and sufficient at the same time, then the notion of linearity follows in its wake: if every time we find cause A we find effect B, then we should expect that twice the amount of cause A will have some linear and predictable relation to effect B.⁵

Some things in social life do in fact work like this, a good example being the relation between education and income. Generally speaking, the more education one has, the more income one makes. There are important exceptions, of course (academics being one of them), but in general, using a random sample of individuals, we can predict that *on average* every year more of education yields a predictable amount of more income.

What drops out of this picture is an entire arena of social relationships, namely those where a cause is either necessary or sufficient, but not both. In other words, those times when there are, socially speaking, many ways to skin a cat.

In general terms, sometimes you can find cause A but not effect B, or conversely sometimes you can find effect B without cause A. (My own hunch is that this is true of most of social life, being as messy and complicated as it is. I once spent some time looking through the General Social Survey, a major sociological survey, only to find that only a handful of variables actually met the strict definition of the kinds of variables you need to explore a linear relationship.)

For years some sociologists, frustrated by the procrustean bed of survey research and the assumption of linear relationships, held on to the qualitative methods first pioneered by the Chicago School. As you'll recall from Chapter 3, as quantitative sociology became more muscular, qualitative sociology eventually came to be experienced as "softer," less "rigorous," than the mathematical stuff, and, in my own experience, came to be gendered as female. This is not to say that there weren't plenty of brilliant men who did qualitative sociology, many of them trained at Chicago, or trained by people trained at Chicago. But, to take an example from my early career, I was told many times that it was a good thing I was trained as a demographer, to offset my decidedly "soft" way of gathering data.

And although I hate to admit it, the people who said this to me may have been onto something. As quantitative methods came to take pride of place, and as qualitative researchers seeking funding

had to explain with great care how their qualitative methods were chosen only in desperation because so little was known about the population that random sampling and surveys were out of the question, qualitative research sometimes found itself more enchanted with description than analysis.

As I mentioned earlier in the book, the first hint of a new way of doing social science, in my own intellectual trajectory and that of many others, came with the publication in 1967 of Glaser and Strauss's *The Discovery of Grounded Theory*. In their development of the "constant comparative" method and their language of "theory generating" rather than "theory testing," the authors began to point to a new epistemology of social science. They were careful to say that this was not the deductive model implicit in survey research, where a hypothesis was tested in a random sample of a well-defined population, and the results were verified by way of powerful inferential statistics. Rather, their model was designed to explore those situations where the conceptual parameters of a phenomenon needed to be elucidated, not tested.⁶

But it was also true that, in part because of the traditions of the Chicago School, and perhaps because of the fact that both Strauss and Glaser were based in a medical school, grounded theory tended to remain focused on the micro. Careful reading of the key works that lay out grounded theory shows that virtually all of the apparatus of investigation is premised on what is often called "symbolic interaction," the behavior of two or more individuals observable in the moment.⁷

While individuals *do* engage in meaningful interactions with each other, actions that we can study with some diligence and rigor using the tools provided by grounded theory, they do so in a social, political, and institutional context that often seems to fade out at the edges in some grounded theory work.

Into this picture came Charles Ragin, a historical-comparativist, whose interests lay precisely in those "big processes," as Charles Tilly once called them. With the publication first of *The Compara-*

tive Method in 1987 and then of *Fuzzy-Set Social Science* thirteen years later, Ragin has once again shifted the epistemological and methodological ground under the feet of working sociologists.⁸

What Ragin points out is that there can be patterns of social life that are not linear—in other words, each and every time we find cause A we do not find outcome B, and each and every time we find outcome B we don't find cause A. But—and this is his signal contribution—there is a way in which we can ascertain patterns of occurrences which are neither linear nor random. Like Glaser and Strauss's model, this is an inductive method, but unlike grounded theory, it can be applied to larger structures.

Ragin, quixotically enough, brings numbers back into the picture—but not linear ones. Ragin has adapted set theory and Boolean algebra from symbolic logic into a method which permits us to test whether or not there are meaningful patterns in the data we gather, even if those patterns are not linear in nature.

Developed by the Irish mathematician George Boole in 1847, Boolean algebra uses a form of mathematical analysis to analyze the logic of statements, and as such is closely linked to symbolic logic. Ragin's explanation of this is much more elegant than my own, and I strongly suggest that you take a look at pages 85–103 in *The Comparative Method*. Here I will simply say that this kind of algebra permits us to ask if a variable is present or absent, and what clusters of variables are associated with certain outcomes.

The Boolean algebra merely serves to mark in an efficient way whether an element (a “cause” for the purposes of this discussion) is present or absent, and whether other elements (an “outcome”) are as well. Ragin has even developed software that permits us to check through large masses of data in a reasonably short time.⁹

Ragin himself puts it more elegantly:

Boolean methods of logical comparison represent each case as a combination of causal and outcome conditions. These

combinations can be compared with each other and then logically simplified through a bottom-up process of paired comparison. Computer algorithms developed by electrical engineers in the 1950s provide techniques for simplifying this type of data. The data matrix is reformulated as a “truth table” and reduced in a way that parallels the minimization of switching circuits. . . . These minimization procedures mimic case-oriented comparative methods but accomplish the most cognitively demanding task—making multiple comparisons of configurations—through computer algorithms. The goal of the logical minimization is to represent—in a logically shorthand manner—the information in the truth table regarding the different combinations of conditions that produce a specific outcome.¹⁰

What this method, which Ragin calls qualitative comparative analysis, or *QCA*, does is to work out an algorithm that most economically describes the patterns observed in the data. For example, in the book I wrote about people who opposed or favored sex education, the data consisted of transcribed and coded interviews for 169 people, a set of socioeconomic indicators for a smaller group within this larger group, curricula, and other materials such as newspaper accounts; we were able to create a metafile with items from many different sources about the same community, the same person, or the same issue.

The main results from the *QCA* analysis suggested that two kinds of people opposed sex education in the public schools. On the one hand, there were the kinds of people that previous social science research led us to expect would be social conservatives: people with relatively fewer years of education, working in blue-collar or lower-white-collar jobs, whose fathers did not go to college. These people typically grew up in a conservative religious tradition, such as Southern Baptist or one of the evangelical churches. On the other hand—and this is where things got exciting—it turned out there was a clus-

ter of another kind of person who opposed sex education, people who looked very much like what we might think of as social liberals. They were relatively highly educated, they were often in white-collar or professional jobs, and if they were raised in any religious tradition at all, it was one of the more liberal denominations. In terms of social background, they were virtually indistinguishable from the kinds of people who supported sex education.

How did we find this out? Basically—and this is where the Boolean algebra part comes in—we coded everyone in the study in a “truth tree,” where we coded for the presence or absence of an element (or variable). Then, running the QCA analysis, we discerned what statements are true of people who support or oppose sex education.

QCA sidesteps (and most elegantly, I think) some of the more complex questions associated with causality, but what it does permit us to do is to make statements like: “outcome C can happen *either* when A is present *or* when B is present.”

To get back to the details of my research, we found, by doing a QCA analysis on the data we had on people opposing or favoring sex education, that people could become opponents of sex education in one of two ways. First, people who were from a lower socioeconomic group (as measured by their father’s education, or lack thereof) and who had a mother who stayed home when our interviewee was growing up were likely to oppose sex education.

But the story gets more interesting, and it shows the power of this kind of analysis. People who opposed sex education could *also* come from families that were upper-income (as measured again by the father’s education), but who had subsequently become members of a conservative religious denomination and who had opted for a traditional family where the wife stayed home to care for the children.

As in much quantitative analysis (remember, we had to reduce complex people with complex stories to dichotomous variables to fit the analysis, such that we coded the father’s education as either “college grad” or “not college grad”), what we got were hints about what

was going on, namely that there were at least two kinds of people who opposed sex education in our interviews.

When we cross-linked these two kinds of people to the narratives that people told us, the sparks began to fly, intellectually speaking. Now that we knew there was no single path to opposition to sex education, as revealed by these two clusters of people, we could go back to the interviews and find out if we could use the qualitative data to give us a hint about what was going on.

We did find a lot of evidence of the first kind of opponent of sex education—a person who is fairly traditional and whose encounter with liberal sex education is something of a “moral shock.”¹¹ In classic theoretical terms, these are people who had been sheltered from a broad set of experiences and values that sociologists sometimes lump into the broad category of “modernity,” and they didn’t like it when they saw it.

Alternatively, there were people who had been raised, so to speak, deep in the heart of modernity. They were educated, they were affluent, and they often worked very much in the modern sector. Many of the men we interviewed, for example, were in high-tech fields, and the women, if they worked before marriage, worked alongside them. What was different for this group of people is that they had experienced a period of emotional and social instability in their lives (often involving drugs, alcohol, or both) and/or a period of rootlessness, and they turned to evangelical religion, and the strict moral code that came with it, as a way of dealing with the overload of choices that life had confronted them with.

Theoretically, it seemed that we had found, at least with respect to opposition to sex education, that there are people who are born into what has come to be a classically “social conservative” position, and there are those who are converted into it. (In the book I wrote about this topic, I called the first group “birthright” sexual conservatives, and the second group “converted” or “born again” sexual conservatives.)¹²

One of the recurring themes of this book has been the idea that social science “methods,” though conventionally imagined as neutral tools to get at the truth, are in fact deeply shaped by the social reality they wish to examine. Unlike the humble pick and brush of the archaeologist, who uses them to sweep away the dust obscuring her vision of a fossil, social science research methods grow out of the very terrain they want to study.¹³

So it seems only fair, now that I’ve pitched Charles Ragin’s qualitative comparative analysis to you as the answer to many of the problems that have bedeviled me (and, I think, the whole enterprise of salsa-dancing social science), that I subject his method to the same kind of sociology of knowledge that I’ve brought to bear on what I’ve called canonical methods.

Earlier I argued that Ragin’s Boolean analysis is designed to pick out patterns in data that might not be visible to the naked eye. Moreover, drawing on Ragin’s own analysis of what his method does, I’ve agreed that his method moves us away from linear notions of causality, or those cases where causes are *both* necessary and sufficient, *and* where an increment in a cause can be neatly tracked against a comparable increment in the outcome.

It strikes me that this is the perfect method for these postmodern times. With all the whiffs of postmodern angst that have blown through the social sciences, we are more nervous than ever about what it means for something to “cause” something else. But, to go back to Steven Epstein’s example about the most radical, social constructionist AIDS activists wanting to know if AZT *works*, we also live in a world in which our ability to recognize patterns in the hope that we can do something about them is more pressing than ever.

I think the reason Ragin’s method is so tailored to the existential and social temper of the times is that, either explicitly or implicitly, it eschews what the literary types call “master narratives.” In contrast to earlier generations of social scientists, it doesn’t try to build grand theories or—worse yet—invariant laws of social life. In the core of its

Boolean heart, Ragin's method builds in contingency and human agency. Hidden in the Boolean expression I described to you earlier is the idea that people can turn out to be sexual conservatives in more than one way.

If you think about this for a minute, this is a far more compelling model of social life than the linear model built into the canonical methods. As I picture it, social life is like one of those old-fashioned pinball machines, where individuals (the little balls) are bounced around an area by things like history, accident, their own temperaments, and all the things that make us unique. BUT, and this is a big but, we find ourselves bumping up against pillars, and barriers, and even being flipped by flippers, which represent the social parts of life.

So far, this model presumes that you and I are looking at social life from a distance, and the little balls have no "subjectivity," as we now say. But we both know that those little balls, to the extent that they stand in for human beings, are busily taking those barriers and pillars and flippers and not only constructing the meaning of them, but constructing new barriers and pillars and flippers even as we speak. My take on social construction is a paraphrase of Marx: humans socially construct their realities, but not in unbounded and random ways.

To go back to Charles Ragin, I think his method permits us to see both the messiness and the contingency in social life, while at the same time recognizing the patterns. I know something about how people come to be sexual conservatives that I did not know before I did my research, and would not have known had I not applied a Boolean analysis to my data.

At the same time, if you had sat with me (and my wonderful research assistant) as we did this analysis, you would have seen that the hand of the analyst is much more visible than in canonical methods. To the extent that canonical methods aspire to be neutral, to simply measure what is "out there," it looks as if social reality is revealing it-

self to the social analyst, shyly taking off its clothes to show the inner essence. With Boolean analysis, however, the analyst has to be much more up-front about what theories are being generated in a study, and about how his or her treatment of the data may be affecting the emerging theory.

For example, to the extent that qualitative comparative analysis (in contrast to Ragin's more recent work on fuzzy sets) requires you to simply code whether an element is present or absent, you, the researcher, have to decide where to cut your elements into "present" and "not present." It sounds a lot easier than it is. In the example that I gave you from my own work, we had to decide how to code "education" as something that people had or didn't have. Of course (and here is my own critique of canonical methods coming back to tug on my pants leg) in real life, people have varying (and linear) amounts of education. I think it is fair to say that in the United States today, almost no one entirely lacks education. Even immigrants from countries where education is limited usually have *some*. But the Boolean form required me to divide the individuals I had studied into people who "had" education, specifically higher education, and those who didn't. For the purposes of the study, we decided that people who had graduated from college were socially, economically, and attitudinally a different kettle of fish from people who had not, but *that was our imposition of a schema on the social world*.

The difference here between QCA and canonical methods, I think, is that we agonized over how we constructed and then imposed our schema. We read as much literature as we could on education and attitudes, education and life chances, education and mobility. The point is that the method itself forces you to undertake this process in a much more mindful and self-aware way than canonical methods usually do. I know, of course, that it would be ideal if canonical methods spent as much time operationalizing *all* their variables as QCA does (and indeed methods textbooks for canonicals of-

ten urge them to do so), but I also know that life is short, and if there are well-replicated variables to measure socioeconomic status or education or religion, you use them.

The main thing to remember, however, is that defining the elements (or, in canonical-speak, operationalizing the variables) is so integral to the theory that you hope to generate using QCA that it's not a luxury, it's a necessity.

I said earlier that the canonicals have it easier—they design a study, gather the data, clean the data, and then, hey presto, analyze it. Writing it up is just a short step away. We salsa dancers, on the other hand, have no such clear and linear path ahead of us. On the bright side, however, we don't really have to dread “analyzing” our data, since analyzing it is something we have been doing since our very first interview, or PO session, or what have you. By the time we know what theory we are generating, we have essentially analyzed most or all of our data.

Exercise for Chapter 10

This one is easy: tell me how you are going to code (reduce) your data. The ideal, as I discussed in this chapter, is to write a codebook so clear that someone else can use it to code your data independently.

Drawing on work by the Centers for Disease Control, I now write my codebooks in Microsoft Access, so that I can track the following:

CODE NAME (a short memorable name for something)

BRIEF DESCRIPTION (what the code covers)

WHEN TO USE (instances of when the code covers some aspect you care about)

WHEN NOT TO USE (draws boundaries between elements of this code and elements of other codes)

EXAMPLE (a verbatim example from your research to give an intuitive feel for what is at stake)

The CDC suggests using *Access* because you can go in and tweak each of these dimensions fairly easily without disrupting the others. If, on the other hand, you don't have access to *Access* (so to speak), any old text file will do just fine.



Living Your Life as a Salsa- Dancing Social Scientist

Let's review the state of play. I've told you that "methods" in the social sciences are historically, socially, and politically located in both time and place. I've also told you that the methods most commonly taught (canonical social science, "normal science") grew out of a particular time and place, namely postwar America. I've tried to convince you that in this new postmodern, globalizing world, those old methods don't work as well as they used to, at least on the kinds of problems that most of us are interested in these days. Finally, I have argued that a whole set of "practices," that is, taken-for-granted ways of doing things that aren't even at the level of consciousness most of the time, grew out of those old methods and now must be rethought by those of us whose contributions will consist of making connections across boundaries, rather than following the normal-science way of making incremental contributions to a deep but narrow part of our field.

Which brings us to living your life. Here is the single most important thing I want to tell you in this chapter: anxiety is NOT your

friend! Now I know, because I was there, that anxiety is a core part of the graduate school experience. My theory about this is that being in a situation where there are powerful pressures to “be good,” combined with very few guidelines about how to do so, makes us all revert back to the last time we were around powerful people who wanted us to be good without telling us how to do it, namely early childhood. So, as the Freudians would tell us, graduate school creates the kind of deep transference that would thrill a psychoanalyst. The trouble is, in a good psychoanalysis, that kind of transference would be acknowledged, honored, and used as grist for the psychoanalytic mill. In graduate school, however, it is ignored, unacknowledged, and generally left to fester in such a way as to make you miserable—and to make you very anxious.

But you can’t make the kinds of connections across boundaries that I’ve been urging you to make if you’re very anxious. Unless you are very different from me, anxiety makes you tight and rigid, which is exactly the opposite psychological state from what you need to make those intuitive leaps across boundaries which are true in a deep way. To put it another way, no one can salsa-dance when they are frozen with anxiety.

All along I’ve been telling you that what makes a salsa-dancing social scientist different from the canonical kind is that we have to think at a higher level of generality—we have to think horizontally, not vertically. But you can’t do that when you are anxious. So here’s the paradox: to be a good salsa-dancing social scientist, you have to be loose, relaxed, “playing out of your shoes” as the basketball players say. Yet by its very nature graduate school makes you tight and anxious, and nowhere does it do so more toxically (and I speak as a recovering graduate student as well as an adviser) than when you are meeting with the person who is supposed to be helping you, your adviser. (This is especially true, as I’ve noted, when he/she is harassing you to name your independent and dependent variables.)

What’s a nice person like you to do, besides pressing a copy of this

book into your adviser's hands? Well, salsa dancing for starters. Or running, or yoga, or cardio boxing, or anything that makes you sweat and not think about your work, your adviser, your colleagues (if you're a faculty member), or anything else. I also strongly encourage the students I work with to go to at least two movies a month, and I'm trying my hardest to do that myself.

The next thing is to remind yourself on a daily basis that anxiety is not your friend. This is a perfect example of one of the practices I was talking about. When academic contributions were narrow but deep, I think everyone took for granted that anxiety was a *good* thing, spurring you on to early-morning and late-night obsessiveness, helping you get ahead of the competition. Thus you will find yourself, more often than is healthy for you, around people who actually think anxiety is good, or at a minimum, part of life. *Do not let these people get to you!* Write down in large letters (and maybe paste it on your bathroom mirror), "Anxiety is NOT my friend," and repeat it to yourself on a regular basis whenever you feel your breathing becoming shallow and your shoulders inching up toward your ears. Then go for a run, or a salsa-dance, or something.

Here are some other tips to help you live your life as a salsa-dancing social scientist. The first is, *have* a life. Make sure that you have friends outside of your work, and interests not directly connected to it. I can't stress the importance of this too much. When I was a young assistant professor, I was lucky enough to fall in with a group of wonderful political scientists at UC San Diego, and it was the best thing that ever happened to me. I got friendship, support, intellectual excitement, and I never, ever got the Black Hole at the Pit of My Stomach feeling that I got when I talked with my sociology colleagues. (I hasten to add that my colleagues in sociology were wonderful people; the Black Hole feeling was entirely on my side and had nothing to do with what they were doing, which was being, by and large, genuinely kind and helpful people.) In addition, I was fortunate to have friends who were artists, psychotherapists, journalists,

full-time moms, and other people who could make me laugh at the absurdities of academe.

To come back to a point that I've been making throughout this book, my need for friends outside of my academic department wasn't just personal, it was structural. It takes a very psychologically sound person to take big intellectual risks in front of people who will eventually be called upon to judge you. So I strongly suggest that you find people and/or a setting where you can feel safe and take risks. (Sometimes, if the chemistry is right, graduate students can find that kind of support in a dissertation group made up of people in their own discipline, but my experience is that being in groups that are made up of serious scholars from adjacent disciplines is a good thing.)

Another piece of advice about living your life comes from Annie Lamott, who says that "perfectionism is the voice of the oppressor." She's right. Lamott counsels you to write "shitty first drafts," and I second this idea with enthusiasm. One of the things that happens to all of us—especially when we get anxious—is that we forget where to start. Add perfectionism into the mix, and you end up with a real psychological mess. (I should say that I take for granted that you are perfectionistic, because very few people get to graduate school without an abundance of this particular character trait.) In my own case, I didn't sit down to write my thesis until I was suddenly told that the grant I was working on had run out of money, and I would be out of a job in a month. As I started to hyperventilate, the director of the grant reminded me that he had arranged a post-doc for me to fall back on. But what I hadn't told him (or anyone else) was that I had written only one chapter of my thesis, and I knew that you have to "doc" before you can "post-doc." The stress of being out of money was sufficient to get me to sit down and write the thing, which I did, as I recall, in six weeks. (This would sound more impressive except for the fact that I had spent the last four to six years *not* writing my thesis, so I had thought about it a lot.)

That experience was hard on me, and you shouldn't have to find yourself under that kind of pressure to get writing. And, for all of the reasons I've talked about earlier in this book, you really need to let go of the old model where you gather your data, analyze your data, and then write it up.

You need to start writing very early in the project, and it is your writing, in particular writing about things that surprise you, that will point you toward new research directions. The hard part of this particular piece of advice is that you will write, write, and write again, and then you will rewrite. But on the plus side, since you know that you are just getting some thoughts down on paper, you know that you can mess up without anyone being the wiser.

Finally, Lamott, whose book is required reading for all my graduate students, says—although she is talking about writing fiction—that you should write something the size of a one-inch picture frame.¹ (In fact, she keeps a one-inch picture frame on her desk, to remind herself of this idea.) Borrowing from her, you might want to think about the thing that you found most surprising/disturbing/disorienting in your most recent day of doing interviews, taking field notes, examining documents, or whatever it is that you did to gather your data. Keeping in mind the metaphor about that one-inch frame, write about it in great detail. Write about what you found. Write about why it surprised/disturbed/disoriented you. What “conjectural theory” or common-sense expectation was unseated by what you saw or heard or discovered? How was that conjectural theory or expectation related, if at all, to a relevant social scientific theory or theories? What would you need to do to prove to yourself and others that what you found was a real “social fact,” as Durkheim might put it, rather than an idiosyncratic improvisation that some creative soul came up with?

Write about all of these things, and if you write them with as much passion and self-reflexive criticism as you can muster (“why did this surprise me?” “what kinds of things might be going on that

I'm ignoring?" and most to the point, "what is this a case of?"), then you will have written a really big chunk of whatever it is that you will be writing.

Two more suggestions along these same lines: a useful strategy is to set the kitchen timer and write for only an hour per day, or limit your writing to only fifteen minutes if you are feeling particularly tight and anxious. In fact, I wrote a fairly large part of this book in exactly this way, so I speak from experience when I tell you to let your timer become your new best friend. If I can write an entire book an hour (or fifteen minutes) at a time, so can you.

The other thing is to have *fun*. Not just in the research part (I routinely tell both my undergraduate and graduate students that if they are not having at least some fun, they are doing it wrong), but in the writing part as well. I often tell my graduate students that graduate school is a hothouse for writers' blocks. Only the most mentally healthy, confident individual can withstand the social pressures of graduate school—or assistant professorhood, for that matter—without developing a full-blown case of the willies about writing. Freud once said something to the effect that when two people make love the bed is crowded with the ghosts of parents and others who have been significant in one's erotic development. I'm here to tell you that when it comes to those unseen others who surround you—interrupting you, offering to "help" you, and proffering unasked-for and often harsh advice—the bedroom is close to a zendo, compared to whatever room you usually write in.

Your parents, your advisers, your eighth-grade English teacher, the author of the most important book on your topic, the graybeards in your area, all crowd around you trying to get a word in edgewise. It's a wonder you can hear yourself think over the cacophony of all these other voices, and often enough you can't.²

This is where I take a page from the other book that I insist every one of my graduate students must buy, namely, Jane Anne Staw's

Unstuck.³ Staw points out that every single writer who comes to her needing help getting, so to speak, “unstuck” is convinced that all he or she needs is a kick in the pants, someone to treat him or her brutally and force the writing to pour out.

Staw argues—and this is confirmed by my experience both with my own writing and that of my students—that this is exactly the wrong tack. Writers need more kindness from themselves, not more abuse. Let’s be honest: writing, even if it’s a dissertation that never gets published (an outcome that will never happen to you), is *terrifying*. It’s standing up in the public square and claiming (and probably demonstrating convincingly) that the emperor over there is as naked as a jaybird. I said earlier in this book that all of us, like my beloved dogs, have a deep and probably hard-wired need to be part of a group. What could be more dangerous than pointing out, however politely, that the group is wrong in some important respects of how it does things?

And that’s not even the worst. Not only are you getting ready to stand up in *public* and say something controversial, but if you have a canonical social scientist as your mentor, she or he is insisting that you say it in a particular way, one that does not comport with the salsa-dancing impulses surging through your soul.

What’s a body to do?

You have to be kind to yourself. I’ve seen firsthand how this advice can change the lives of my students. Yes, it can really change your work life forever if you practice being extraordinarily kind to yourself. Practice treating yourself as you would treat a friend who is engaged in a dangerous, challenging, spiritually demanding adventure. Treat yourself as you would someone planning on climbing Mount Everest or hitchhiking around the world with only \$200. Lavish attention on yourself. As one of my old friends used to say, “Nothing is too good for the working class!”

I personally think that all writers are mildly manic-depressive at

heart, and I hate to break it to you, but as a salsa-dancing social scientist you are a writer at heart. I watched myself in my early years get high when the writing was going well, convinced I was God's gift to social science. Then just hours (and sometimes minutes) later, I would become suicidally depressed when I hit a writing snag, convinced that my Ph.D. had been given to me as a complete oversight on a day when the Registrar of Students in charge of such things was home with the flu. And it totally messed up my mind: on days when the writing was going well, *everything* I had written, even days before, looked good to me; when it was going badly, I figured that every single word on every single page was a travesty.

What I learned from Jane Anne Staw is the need for regular practice—a commitment to sitting down and getting the hands hovering over the keyboard (or the pen dashing across the paper). Once you have made a commitment to write regularly, even for just fifteen minutes a day, you get a bit more distance on the ebbs and flows of writing, thinking, data collection, and the like. So try to set up a writing time so regular in your life that on those days you don't write, you feel like you've forgotten to brush your teeth.

Which brings us back to being kind to yourself. It's my conviction that the capacity to bring things together across boundaries, to make those leaps of faith, comes from the very deepest part of ourselves, and this is much more true for salsa-dancing social scientists than for canonicals. That deep part of you is no fool, and has no intention of showing up regularly just to be abused and judged. So practice the art of kindness to yourself not only by salsa-dancing and going to the movies, but by treating even your intention to sit down with your data or write a research memo with great respect. Staw argues that the only remedy for writer's block is kindness, discipline, and respect for the writing self, and she is right. If graduate school has gotten you stuck, go out and buy her book (and Lamott's) right now. You'll be glad you did.

Exercise for Chapter 11

I was tempted to tell you just to go salsa dancing for this exercise, but I think I have a more useful idea.

Go through this book and write out all the mantras that seemed to work for you—“Perfectionism is the voice of the oppressor” (Anne Lamott); “Anxiety is *not* your friend”; “Work smarter, not harder”—and print them out, say in a 24-point font. Tape them to your bathroom mirror and your computer. Look at them each and every day, but replace them with others once they become part of the scenery. (I have good cognitive science on my side here; I’m not just being touchy-feely.)

Now go salsa dancing!

Appendixes

Notes

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Appendix One

What to Do If You Don't Have a Case

The overwhelming majority of this book is written on the assumption that you have come across a fascinating phenomenon which your instincts tell you could make a great dissertation (or book). The reason I've done this is because that's how most of my students—the ones who convert to salsa-dancing social science—approach me. They see something; they sense that it's a good case study, one that social science should pay attention to; but they don't know how to turn it into a dissertation. (Or a next book, in the case of a young faculty member who needs some mentoring.)

But for the sake of argument, let's assume that you don't have such a case. Instead, you have a very broad general interest in something, say globalization, and you don't know how to turn that into a viable research project. You look with mixed envy and loathing at your fellow students who do have case studies, because that looks so easy compared to what you're up against. At least *they* can go to some exotic locale, or even just a set of offices, in order to start gathering data. “But what about *me*?” you say to yourself. “What am I supposed to do?” This section is for you.

You will be doing—more or less—the same steps that those folks

who have a case get to do. You start out by thinking about what your *research interest* is. If you said “globalization,” think again; you know by now that “globalization” is a label, not a research question, and you probably just said that—if you did—because you are desperate.

Stay calm. While your colleagues who have found themselves a nice juicy case study have to work from the case to the elements (see Chapter 10 for discussion of this), you have to work from the elements to the case. So what *exactly* is it about globalization that interests you? Often I ask my students what the “intellectual itch” was that brought them to their interest in a research topic, and sometimes if I and they push hard enough, we find that there is a case lurking in the background, one that perhaps the student was a bit too embarrassed to mention. (“Well, I watched a ten-year-old kid selling chicle on the streets of Tijuana, and I found myself wondering why she/he was not in school, given that northern Mexico, and the border area with the U.S. in particular, are economically vibrant places right now. Why hasn’t school caught up with him or her, and why isn’t there mandatory elementary education?”)

Once the hidden case comes to light, it becomes much easier to outline what the elements of this particular research interest are. Again, it’s your research project and not mine, but at a minimum, I see questions about economic development, state building, investing in human capital (i.e., schools), and policy decisions to take young people out of the labor market. I will remind you, of course, that once you have the elements you don’t necessarily have the research project yet, because you could research the interconnection of these elements in literally hundreds and maybe thousands of ways. You could interview bureaucrats in the Department of Education; you could do a history of obligatory education in other industrialized countries, and show how Tijuana does and does not fit into models of state building and the expansion of education; you could interview individual parents to see how they decide (or don’t) to put kids into school; and on and on and on.

So now, at least, you have some blocks (“elements”) to move around. You can stretch out on the floor and start moving things around on butcher paper (or using Inspiration, if you have chosen to go the electronic route) to begin to see the story you want to tell. Inspiration is a clever mind-mapping program that lets you do electronically what I used to do on the floor with butcher paper and colored pens, namely visually plot out the moving parts of your argument. You can draw free-association flowcharts, messy little back-of-the-envelope visual approximations of your thoughts. Because I’m mostly a right-brained person, it really helps me to be able to visually represent relationships. Then, with the touch of a key, I can toggle back and forth between a classic eighth-grade English teacher kind of outline and my visual images, allowing me to see imbalances and missing parts of an argument. (For more information go to www.inspiration.com.) I’ve recently been introduced to a similar product called Mind Manager (www.mindjet.com), whose “Lite” version is priced about the same as Inspiration. Both have free downloads, so try them out.

But let’s say for the purpose of argument that not only do you not have an “intellectual itch” that reveals a case study lurking underneath, you don’t even have a repressed case study, that is, an incident which caught your eye and started you thinking, but which you have forgotten until I asked you to start thinking about it just now.

So you have a big research interest, but no case study to go with it yet. In this case you should do the salsa-dancing steps that I outlined in Chapter 10 in a slightly different order. You specify your elements, as best you can at this early stage. You “motivate” your study by telling me why I should care about the particular combination of elements that you think will show up. You think seriously about where you might find a “data outcropping” of the kinds of elements that you suspect you will need. You look for a “bounded” case within that data outcropping that has some of those elements in it.

It sounds scary, and it is, being marooned alone with a research

interest, especially if there are no salsa-dancing social scientists around. The pressure for you to squish your study into one with an independent and a dependent variable, specific a priori, is almost overwhelming.

The only thing between you and betraying what your heart wants to do is to listen to your gut: every time someone tortures you into telling them *ahead of time* what your model is, smile politely and go get some coffee. In fact, drink lots of coffee (or herbal tea, or sparkling water). Then go and talk to lots and lots of people, refining each time what you find interesting.

Take a page from Walter Wallace's book: think of a paradox, a conundrum, an "explanandum" that needs to be explained. Many an excellent research project has grown out of trying to make sense of a puzzle. A variation on this is to look for a deviant case, an example in which things are not happening as theory tells us they should.

It has been my experience that if you sit with your interest long enough, with enough kindness and patience, a research question will in fact emerge. Trust yourself and your work, and all will be well, eventually.

Appendix Two

Tools of the Trade

Although in most of this book I've been talking about “tools” in the metaphorical sense, in fact there are a couple of real-life tools that will make living your life as a salsa-dancing social scientist easier. I've learned about these the hard way, and I share them with you in the hope that they will be helpful.

First, it's really worth your while to invest in some bibliographic software—a relational database that lets you type in the material for a citation once and only once. This kind of database is one where the parts—or “fields”—of a citation—or “record”—are electronically connected, so that when you sort one, you sort them all. This may sound obvious, but in the early days of databases I once, thinking to make my life easier, entered grades for two hundred undergraduate students as they handed in their papers. Then I sorted their names alphabetically, and the software happily sorted the column of names, totally ignoring the next column of grades. In short, it gave everyone a new grade!

But in the context of bibliographic software, a relational database means that can't happen—that once you enter the author, title, publisher, city, date, pages, etc., you can manipulate them in any order you want, and all of the “fields” in a “record” remember that they are related.

Specifically, this means that once the information is entered, you can push a button and these “fields” neatly array themselves in the order expected by the *American Journal of Sociology*, or the Modern Language Association, or almost any other format known to the publishing world. Better yet, with a few keystrokes, you can make your own format, as I did for the notes and bibliography in this book.

Even better, such programs can inhale (I think the proper word is “import”) citations straight from the computer. If you use JStor (an electronic journal storage system) or any of the many library electronic databases available to you (such as my beloved Melvyl here at Berkeley) you can—again, just with pressing a few keys—direct your computer to download the citation directly into your bibliographic software. So not only do you never have to type a citation more than once, in some cases you never have to type it at all!

There are lots of proprietary packages for bibliographic software—I happen to use one called EndNote, but there are lots of others with names like Bibloscape and ProCite and the like. There is also some freeware, at least at my university, called RefWorks (note how these people like to capitalize in the middle of the word?) that you can download and use for free. As with everything else in life, the more you pay, the more you get, but on the other hand, most of my software is so loaded with features I never use that I feel as if I’m surrounded by a whole host of talented children I am not helping to live up to their potential.

Next, I would spend money on the kinds of things that make doing research easier and reduce what we might think of as the friction level. A good digital tape recorder for those of us who do interviews is worth its weight in gold, although I do have quite a few students who swear that they get good-quality sound by attaching an inexpensive mike to their iPods. Not my experience, but worth trying.

Finally, if you don’t have good word processing software and a good spreadsheet program to enter your data into, you probably

should. As I told you earlier, a friend of mine always says that nothing's too good for the working class, and I suggest you take this to heart. Anything that makes you happier and more productive as a salsa-dancing social scientist is a good investment, and could be the most profitable investment you will ever make.

Appendix Three

Special Resources for Specific Methods

I've said several times in this book that I would not waste your time telling you things you could learn in any ordinary book on methods. To guide you in this area, here is a list of resources for specific methods. These are books that you should find helpful in figuring out how to design a study using qualitative methods in a salsa-dancing way.

I should mention at the outset that I am a big fan of the Sage series on methods and methodology, produced by Sage Publications in Thousand Oaks, California. They are typically short, well written, and very much to the point. Feel free to browse their catalog on research methods (www.sagepub.com); the books here will be ideal starting points to discover the nuts and bolts of key methods.

The books listed below discuss the meta-issues of doing research; they are arranged in sections that generally correspond to the order of issues considered in my book.

“Big Think” (Epistemology and the Like)

Abbott, Andrew. 2004. *Methods of Discovery: Heuristics for the Social Sciences*. New York: W. W. Norton. This book asks us to think about what

makes an explanation a good one. The author is a well-respected theorist, and the book is remarkably readable and helpful.

Abbott, Andrew. 1988. "Transcending General Linear Reality." *Sociological Theory* 6: 169–186. I cited this article in the text; it is a critical appraisal of the weaknesses of the main quantitative methods (linear models and their kin) from an expert in the field.

Brady, Henry E., and David Collier. 2004. *Rethinking Social Inquiry: Diverse Tools, Shared Standards*. Lanham, Md.: Rowman & Littlefield. This book is written in direct dialogue with King, Keohane, and Verba, cited below. If I had to do research on a desert island, and I could only take one book, this would be the one. It has very little "how to" advice in it, unless you count how to think about research as a "how to." The sections debunking the assumptions of quantitative research are worth their weight in gold. Brady and Collier are prominent political scientists, so their debunking has to be taken seriously by the quantitative critics.

Burawoy, Michael. 1998. "The Extended Case Method." *Sociological Theory* 16: 4–33. I've cited this article many times in the text; it outlines how to link the micro with the macro in ways that make that process seem much less daunting.

King, Gary, Robert O. Keohane, and Sidney Verba. 1994. *Designing Social Inquiry: Scientific Inference in Qualitative Research*. Princeton, N.J.: Princeton University Press. These are three of the best-known political scientists of our time, whose only fault from my point of view is that they think qualitative work would be much better if it were only more like quantitative work. Still, like Zen monks, they have some habits and practices that we would all do well to emulate. I use the book to keep myself honest, and to have contact with some very smart and orderly minds.

Lieberson, Stanley. 1985. *Making It Count: The Improvement of Social Research and Theory*. Berkeley: University of California Press. This is a smart and feisty book, written by one of the outstanding quantitative researchers in sociology, who also thinks deeply and hard about the knotty issues of argument and evidence.

Smith, Dorothy E. 1998. *Writing the Social: Critique, Theory, and Investigations*. Toronto: University of Toronto Press; Smith, Dorothy E. 1981.

The Experienced World as Problematic: A Feminist Method. Saskatoon: University of Saskatchewan. These two books, while a bit dated in some respects, changed my life. They take on, in a deep and feminist way, what I have elsewhere in this book called the “fish studying fish in water” problem.

Steinmetz, George, ed. 2005. *The Politics of Method in the Human Sciences: Positivism and Its Epistemological Others*. Durham, N.C.: Duke University Press. This edited volume looks at the various social science methods in their political and historical contexts, much as I have tried to do in this book. It is really a “Social Studies of Science” book (in this case social science), and all of it is worth reading. Perhaps because I’m a sociologist, I found Philip Mirowski’s essay very thought-provoking, as is Steinmetz’s own essay on sociology.

Research Design

Alford, Robert R. 1998. *The Craft of Inquiry: Theories, Methods, Evidence*. New York: Oxford University Press.

Becker, Howard. 1998. *Tricks of the Trade: How to Think About Your Research While You’re Doing It*. Chicago: University of Chicago Press. Howard Becker practically invented sophisticated qualitative research, and this book shares some time-tested ways of thinking about your work in a clever and smarter-than-you-thought-you-were way.

Charmaz, Kathy. 2006. *Constructing Grounded Theory: A Practical Guide through Qualitative Analysis*. Thousand Oaks, Calif.: Sage Publications. A lucid and modern-day exposition of the grounded theory method. A bit too focused on the micro for my research needs, but the hands-on advice is very good and very useful.

Creswell, John W. 1994. *Research Design: Qualitative and Quantitative Approaches*. Thousand Oaks, Calif.: Sage Publications. Creswell’s book is the Swiss Army Knife of handbooks. Nothing especially fancy, but it contains most of the tools you will ever need.

Denzin, Norman K. 1978. *Sociological Methods: A Sourcebook*. New York: McGraw-Hill.

- Glaser, Barney G., and Anselm L. Strauss. 1967. *The Discovery of Grounded Theory Strategies for Qualitative Research*. Chicago: Aldine.
- Lofland, John. 2006. *Analyzing Social Settings: A Guide to Qualitative Observation and Analysis*, 4th ed. Belmont, Calif.: Wadsworth.

General Aspects of Fieldwork

- Lofland, John. 1971. *Analyzing Social Settings: A Guide to Qualitative Observation and Analysis*. The Wadsworth Series in Analytic Ethnography. Belmont, Calif.: Wadsworth. This book provides a good, if brief, overview of interviews and participant observation. Chapter 6, “Materials, Mechanics, and Analysis,” in the 1971 edition has particularly useful information for managing and analyzing qualitative data.
- Shaffir, William, Robert A. Stebbins, and Allan Turowetz. 1980. *Fieldwork Experience: Qualitative Approaches to Social Research*. New York: St. Martin’s Press. This book mostly assumes that the researcher will be doing more anthropological research, as opposed to theory-generating work. However, there are good materials on things you need to know about “learning the ropes,” such as how to seem well prepared when interviewing people in power, and how to leave the field. (Note: several of the authors in this book—published in 1980—are far more tolerant of covert research than I am, and than most Committees for the Protection of Human Subjects would be.)

Participant Observation

- Emerson, Robert M., Rachel I. Fretz, and Linda L. Shaw. 1995. *Writing Ethnographic Fieldnotes*. Chicago: University of Chicago Press. While nominally about taking field notes (something it describes very well), this book also teaches you a great deal about analysis. This is not too surprising—you know that for salsa-dancing social scientists, analysis and data gathering go hand in hand—but *Writing Ethnographic Fieldnotes* does an especially nice job of it.

Interviews

Gubrium, Jaber F., and James A. Holstein. 2002. *Handbook of Interview Research: Context and Method*. Thousand Oaks, Calif.: Sage Publications. These folks are modest. This is not a “handbook” of interview methods, it’s the *encyclopedia*. Weighing in at more than most laptops do these days, it’s the “Everything You Wanted to Know But Were Afraid to Ask” about interviews.

Focus Groups

Krueger, Richard A., and Mary Anne Casey. 2000. *Focus Groups: A Practical Guide for Applied Research*, 3rd ed. Thousand Oaks, Calif.: Sage Publications.

About Writing

I may have said this before—graduate school is a virtual petri dish for writer’s block. Unless you are extremely lucky, if you didn’t have it when you came, you are very likely to catch writer’s block while in graduate school. Everything you need to be a good writer—confidence, patience, playfulness, a structured approach to writing—is typically stripped away from you while you are in graduate school (and/or while you are an assistant professor, an associate professor, and in some extreme cases, a full professor). I’m particularly struck by how often elite graduate schools turn out people who think that because they can’t be the next Max Weber, they have nothing to say. Here are some remedies:

Becker, Howard. 1986. *Writing for Social Scientists: How to Start and Finish Your Thesis, Book, or Article*. Chicago: University of Chicago Press. Howard Becker is one of the most distinguished qualitative social scientists of our time, and his thoughts on how to conceptualize (see above) and write social science are well worth attending to. In this book, Becker

advocates and demonstrates the lucid and deceptively simple prose he is famous for, and shows us the hard work that goes into it.

Lamott, Anne. 1995. *Bird by Bird: Some Instructions on Writing and Life*. New York: Anchor Books. This is a funny, compassionate, and wise guide to writing. Although Lamott aims her book at fiction writers, I find that virtually everything she says can be useful to writers in the social sciences. A wonderful book.

Staw, Jane Anne. 2003. *Unstuck: A Supportive and Practical Guide to Working Through Writer's Block*. New York: St. Martin's Press. This is the best book I know for doing exactly what the title promises, namely getting you unstuck. Staw says that everyone she has ever seen with writer's block assures her that all they need is a kick in the pants. What they *really* need, she says, is more compassion. And in practical chapters you can use, she shows you how to get unstuck.

Zerubavel, Eviatar. 1999. *The Clockwork Muse: A Practical Guide to Writing Theses, Dissertations, and Books*. Cambridge, Mass.: Harvard University Press. In contrast to Staw and Lamott, Zerubavel is sort of the Nike ("Just do it!") coach of getting writing done. When I'm feeling hard on myself as a writer, Lamott and Staw comfort me and urge me onward. On those rare occasions when I feel I'm being too easy on myself, I turn to Zerubavel to stiffen my spine, inspire my gumption, and get me going. Read closely, though, all three books advocate a mix of compassion and discipline. I recommend keeping all three of them side by side on your shelf.

For additional and updated resources to help salsa-dancing social scientists, please consult <http://sociology.berkeley.edu/faculty/luker/publications.htm>.

Appendix Four

Sample Search Log

Date	Databases Used	Search Terms Used	Hits	Useful?	Next Steps	Comments

Source: Adapted from "Bruin Success With Less Stress" (<http://unitproj.library.ucla.edu/col/bruinsuccess/04/06.cfm>) and used with permission.

Notes

1. Salsa Dancing? In the Social Sciences?

1. These days the word “practice” —like “structure” in an earlier day— is one of the most complex and resonant in the social sciences. To make a long story short, a “practice” in this book is where belief and action come together, where people produce meaning by thinking something and then acting on it. See Pierre Bourdieu, *Outline of a Theory of Practice* (Cambridge: Cambridge University Press, 1977), especially p. 164, and Pierre Bourdieu and Loïc J. D. Wacquant, *An Invitation to Reflexive Sociology* (Chicago: University of Chicago Press, 1992).

2. I’ve been talking for a while about the “sweet spot” between these two kinds of research, so I was delighted to discover in Wikipedia that “sweet spot” can mean “a place, often numerical as opposed to physical, where a combination of factors suggest a particularly suitable solution.” Yes!

3. Over the course of the book, you will get a much better sense of exactly what I mean by these terms; for the moment, just ponder what they might mean for you. To be sure, these terms describe *all* good research, but as we go along, I hope to show you guidelines for getting there without getting too bogged down with conventional assumptions about what good research should look like.

4. True, Plato used “doxa” first to mean taken-for-granted wisdom, but I have in mind more what Bourdieu describes, as meaning parts of our world so commonly accepted as true that to even question them seems ludicrous (Bourdieu, *Outline of a Theory of Practice*).

5. Doing good research while simultaneously acknowledging that the very act of research is socially situated is what sociologists mean when they say that good research is “reflexive.” And I have to tell you that I personally think that the only kind of research worth doing is reflexive research. Much too much research is just confirming what people thought before they began their research, and not only is this not very helpful for getting a good fix on the social world, but it just isn’t that much fun—why go out and find what you know already?

6. Despite what you may have come to believe, if research is not fun a lot of the time, *you are doing it wrong*. Please note that fun and playfulness are not at all incompatible with discipline, rigor, and dead seriousness.

7. You might well ask yourself, following along with the previous paragraphs, the classic postmodern conundrum: if traditional methods and traditional assumptions about research are steeped in doxa, what about the methods and assumptions in this book? Well, it so happens that they are too, and your job is to figure out where (that’s the reflexivity thing again). I often tell my students to ignore half of what I tell them, but the cosmic joke is that I don’t know which half. But if you take the guidelines in this book seriously, at least you will know where to start.

8. It matters a lot to me for research to have integrity, and I’ve only become more passionate about this in an era of politicized think tanks that can come up with almost any idea-to-go that fits the political needs of the moment. These two dimensions, serendipity and persuasiveness, cover a great deal of ground. If you are honest about your scholarship, you can find and be surprised by what Max Weber calls “the inconvenient fact.” (Or as I tell my students, “There’s nothing like some data to mess up a perfectly good theory.” It turns out that Thomas Huxley said this before I did, about the tragedy of a fact killing a theory, but I was saying it long before I found out that he did.) And if you have done your scholarship carefully and attentively, then you will be protected against some of the logical and methodological errors which can undermine your findings. So it sounds simple—

being surprised and being persuasive—but the whole rest of the book will be dedicated to helping you get to those two goals.

9. Anne Lamott has a variation on this same theme, in which, quoting E. L. Doctorow, she reminds people that when you go driving on a dark night, your headlights only illuminate an area about twenty feet in front of you. Yet with only this minimal illumination you can drive across the entire country. Anne Lamott, *Bird by Bird: Some Instructions on Writing and Life* (New York: Anchor Books, 1995), p. 18.

10. AZT is one of the anti-retroviral medications that is at the center of the current treatment of HIV/AIDS, turning it in many cases into a chronic disease rather than a death sentence.

11. One of my deepest and most powerful political and intellectual commitments is to make scholarship accessible to everyone who cares about the social world. I know all too well what it's like to read something that I ought to be interested in, but find myself closed out of because of references to things that the author assumes that I should know, but I don't. I imagine these authors sitting in ancient rooms filled with books, dressed in tweed jackets with leather patches on the elbows, smoking pipes and dropping in-groupy names to each other. As you will see in the pages to come, I have been very influenced by the work of Pierre Bourdieu, who claims that in this day and age “cultural capital” is in many ways more important in social life than plain old capital, i.e., money. We will be talking more about this as we go along, but for now I just wanted you to know that if you don't know a term or a name, you are almost certainly in very good company. If anywhere in this book you run into a term or a concept or a name that is unfamiliar, such as Martin Buber (a noted theologian and philosopher who died in 1965) or Twelve Steps (a self-help formula devised in the 1930s by Bill W. and Doctor Bob that eventually came to be known as Alcoholics Anonymous, and has since given rise to other groups modeled on the “twelve steps” of AA), just Google it and keep notes on what you find. You will eventually discover, as I did much too late in life, that it's not that you are stupid, but that many of these authors simply don't know how to communicate with the outside world, and, more important to you as a salsa-dancing social scientist, when to assume that people know what you are writing about and when they need a little help. This is quite a challenging and tricky

social dilemma, entailing a good sense of what Clifford Geertz calls “tacit knowledge.” Googling is not the last step in learning about what you don’t know—you have to have a framework into which you put knowledge—but it’s a beginning.

12. I know, of course, that lots of what I am calling “Foucauldian” in this context is actually attributable to a great many other thinkers, most notably Jacques Derrida. However, going back to my Freud metaphor, the same thing happened there, too, in that many of the things ascribed to Freud were not actually his.

13. There’s probably a nice Ph.D. thesis waiting here for someone to explore, namely the spread of postmodern sensibilities from one disciplinary field to another over the twentieth century. By the way, for fun it would be well worth your while to read Michèle Lamont’s “How to Become a Dominant French Philosopher: The Case of Jacques Derrida,” *American Journal of Sociology* 93.3 (1987): 584–622.

14. See Kathy Charmaz, *Constructing Grounded Theory: A Practical Guide through Qualitative Analysis* (Thousand Oaks, Calif.: Sage Publications, 2006), p. 13.

15. The way things are supposed to work is that the teachers would have a better handle on this than the taught; but this is not the case, as I will argue shortly, because this is a generational thing, and most of those of us on the teaching side of the desk are *less* equipped than most students are.

16. You simply cannot survive in this world of info-glut unless you commit to memory one of my favorite sayings that I share with all my students, namely, “work smarter, not harder.” That’s what this book aims to teach you, but commit this saying to memory anyway.

17. Jack Goody and Ian Watt, “The Consequences of Literacy,” *Comparative Studies in Society and History* 5 (1963): 304–345. Goody and Watt argue that in an oral culture, you are always editing and adapting the “truth” about the past to reflect the present, such that there is no contradiction between the past and the present. Once “the past” is fixed in a written form, contradictions emerge, and with them the capacity to grapple with complex and abstract notions, capacities untapped in non-literate cultures.

18. There is such a huge literature on the philosophy of the social sciences that it would take an additional book to sketch out for you what the

conventional epistemology is. My point is one that those books don't usually make, namely that in a world dominated by print, most philosophers of social science (and most social scientists themselves) think reality is linear. Young people raised in a world dominated by the Web don't necessarily make that assumption. We will be talking about other philosophies of science as we go along, but I recommend George Steinmetz, *The Politics of Method in the Human Sciences: Positivism and Its Epistemological Others*, Politics, History, and Culture (Durham, N.C.: Duke University Press, 2005), as a place to begin.

19. Andrew Abbott has made a similar and far more sophisticated case for how a technique much used in the social sciences—general linear regression—tends to force sociologists and others to assume that the world is ordered in ways that are convenient for that methodology. Abbott makes clear that in some ideal sense, general linear regression is a “heuristic” (that is, a “what if” model of the world) that in principle need not make *any* assumptions about the world. But users of linear regression, precisely because it is so elegant and parsimonious, come to forget it's a heuristic, and come to think it's a realistic depiction of reality. Andrew Abbott, “Transcending General Linear Reality,” *Sociological Theory* 6, no. 7 (1988): 169–186.

20. Real-life social researchers of the traditional sort will tell you, often after a few glasses of wine, that traditional research is rarely as linear or as orderly as I have portrayed it. But the point I am making here is that order and linearity are *ideals* in traditional research, and are in some sense built into the methods themselves. After all, it makes no sense to analyze a survey before all the data are in, while in the method outlined in this book, we will begin analyzing data from the very first moment we begin to gather it. As you will see in the pages that follow, I have been deeply influenced by Barney Glaser and Anselm Strauss's Grounded Theory, a model that comes closest to what I mean by “salsa dancing” research. Even here, though, as their most articulate and sympathetic contemporary advocate demonstrates, the research project is still presumed to be largely linear. See Charmaz, *Constructing Grounded Theory*, p. 14.

21. Robert Berring, legal scholar and head of the library at Berkeley's Boalt Hall School of Law, once wrote an article about Whelans, a little newsstand next to the University of California. He argues in this article that

when you go to a newsstand, you can distinguish between reliable and unreliable stuff because of a set of social “filters” that you have grown up with and take for granted. Cheap paper, naked breasts, invading aliens—you know from experience not to take “journals” with these hallmarks too seriously. Such filters, however, are almost entirely missing on the Web. See Robert Berring, “Extra, Extra: World Wide Web Swallows Whelans!” *California Monthly*, November 1998, pp. 15–17. Berring makes much the same case in a more theoretical vein in Robert C. Berring, “Legal Information and the Search for Cognitive Authority,” *California Law Review* 88, no. 6 (2000): 1673–1708.

22. This is an exaggerated claim for rhetorical effect, but it points to some features of the social field I am describing here that I want to come back to later in the book. Of course, when I said that only a few other people “knew” more than I did, what I meant was that a few people publishing in the area in journals and books that I was likely to read knew more than I did. There may well have been and probably were people who knew much, much more, but were not in my “field.” And when I say “the planet,” I am again exaggerating—but in the 1970s, few American sociologists of my age read very many sources outside of the United States, and when they did, they typically read only those published in English. Nowadays it is becoming increasingly possible to talk about worldwide knowledge, as more and more social scientists routinely read articles in other languages. I think I realized that the jig was up one day in the late 1980s when one of my colleagues was waxing enthusiastic over a great article he had found on the Web in the *South African Journal of Criminology* or some such publication, and I was overwhelmed by the idea of figuring out how to assess a whole new realm of (international) scholarship.

23. Betty Fussell, *My Kitchen Wars* (New York: North Point Press, 1999). This wonderful memoir captures pitch-perfectly how life has changed for a generation of women (and men) since the 1950s.

24. The same Andrew Abbott who made the point that using linear regression often makes sociologists think the world is linear calls ideas like this “importable novelties.” I call it shameless borrowing. But in either case, it’s about taking an insight from one field to another. See Andrew Abbott, *Methods of Discovery: Heuristics for the Social Sciences* (New York: W. W.

Norton, 2004), p. 6. As a stellar example, Charles Ragin, whom we will be meeting later in this book, took a notion developed by engineers to map electrical switching statements and turned it into a really nice way of examining patterns in your data that are not visible to the naked eye.

25. Richard A. Peterson and Roger M. Kern, “Changing Highbrow Taste: From Snob to Omnivore,” *American Sociological Review* 61 (1996): 900–907. “Distinction” is what Pierre Bourdieu calls the practice of defining differences between and among people. In the world of salsa-dancing scholarship, “distinction” means that other people think you are very, very smart, and very, very talented—just the kind of person one would want to hire. Or promote. So think carefully about my sociological claim that “distinction” in the social sciences is likely to come from bridging boundaries. For more on distinction, see Pierre Bourdieu, *Distinction: A Social Critique of the Judgment of Taste* (Cambridge, Mass.: Harvard University Press, 1984).

26. There are several jokes built in here to make sure that you are paying attention. I know that Mozart did not have that many “later years,” and I picked the Carter family to make the widest reach I could think of between traditionally “high” and traditionally “low” art. Maybelle and A. P. Carter and clan recorded country and “old-timey” music between 1927 and 1943. They were the parents of June Carter Cash and Johnny Cash’s in-laws.

27. The notion of “cultural capital” comes up early in Bourdieu’s work, and it is much contested. To see how sociologists have elaborated on the concept, see Michèle Lamont and Annette Lareau, “Cultural Capital: Allusions, Gaps and Glissandos in Recent Theoretical Developments,” *Sociological Theory* 6.2 (1988): 153–168. (Loïc Wacquant, one of Bourdieu’s coauthors, disagrees with this article, so you’ll just have to read it—and Bourdieu—and decide for yourself.)

28. The term “social closure” comes from Max Weber; the recent development of the idea is usually attributed to Frank Parkin (Parkin, *Marxism and Class Theory* [London: Tavistock, 1979]).

29. This is one of those assertions that will strike some readers as painfully obvious, and others as simply preposterous. This view parallels some emerging work in the social studies of science, about how tacit social barriers include some groups and individuals and exclude others. I suggest a quick read of Jerome Karabel’s “Status-Group Struggle, Organizational In-

terests, and the Limits of Institutional Autonomy: The Transformation of Harvard, Yale, and Princeton, 1918–1940,” *Theory and Society* 13, no. 1 (1984): 1–40, and his recent book: *The Chosen: The Hidden History of Admissions and Exclusion at Harvard, Yale, and Princeton* (New York: Houghton Mifflin, 2005). (Full disclosure: this is a great book, with an enormous amount of documentation of my point, but I could well be biased, as I am married to its author.)

30. This is another way of saying that you have to have the kind of boundary-crossing mastery that is recognized as such by the relevant “community of knowers,” as Karl Popper calls them (Popper, *Conjectures and Refutations: The Growth of Scientific Knowledge* [New York: Basic Books, 1962]), and more relevantly, what Jean Lave calls communities of practice (Jean Lave and Etienne Wenger, *Situated Learning: Legitimate Peripheral Participation* [Cambridge: Cambridge University Press, 1991]).

31. In this context, check out an article by my former San Diego colleague, Murray Davis: Murray S. Davis, “That’s Interesting,” *Philosophy of the Social Sciences* 1 (1971): 309–344. (Thanks to Scott Harris for reminding me of this article.)

32. Malcolm Gladwell’s *The Tipping Point: How Little Things Can Make a Big Difference* (Boston: Back Bay Books, 2002) was on the *New York Times* best-seller list for 164 weeks as of October 2007 (Clark Hoyt, “Books for the Ages, If Not for the Best-Seller Lists,” *New York Times*, October 21, 2007), and his more recent *Blink: The Power of Thinking Without Thinking* (New York: Little, Brown, 2005) spent more than a year there.

33. The term “professionalization project” comes from Magali Sarfatti Larson’s *The Rise of Professionalism: A Sociological Analysis* (Berkeley: University of California Press, 1977) and has since generated a vibrant literature on how intellectual “fields” come to seek and win social acceptance as professions with the right to exclude outsiders.

34. See Kristin Luker, “Is Academic Sociology Politically Obsolete?” *Contemporary Sociology* 28 no. 1 (January 1999): 5–10. My favorite example of what I mean is that after the state of Wisconsin decided to implement a welfare-to-work plan, the right-leaning Bradley Foundation rushed out an attractive, well-written report showing that the program (W2) worked. The positive evaluation was often cited and was used as fodder for the debate on the 1996 welfare reform bill, the one that ended “welfare as we know it.”

Subsequent research undertaken by Wisconsin's well-respected Institute for Research on Poverty painted a much bleaker picture of the program's capacity to get welfare mothers off the rolls and into work. By the time this analysis was published, however, the welfare reform bill had already passed and been signed into law. See Sally Covington, "Moving a Public Policy Agenda: The Strategic Philanthropy of Conservative Foundations" (Washington, D.C.: National Committee for Responsive Philanthropy, 1998). (Since I called the Bradley Foundation rightward-leaning, I should also probably note that the NCRP is "liberal-leaning.") For more on this, see the Wisconsin Policy Research Institute, in particular its series of articles on welfare reform (www.wpri.org); for the Institute for Research on Poverty at the University of Wisconsin (www.irp.wisc.edu), see in particular Institute for Research on Poverty and University of Wisconsin–Madison, "Special Report 69: Evaluating Comprehensive State Welfare Reforms," Madison, Wisconsin, November 21–22, 1996.

35. My colleague Michael Burawoy has been at the forefront of putting "public sociology" on the map, and it was the centerpiece of the 2004 American Sociology Association Annual Meetings in San Francisco. For an introduction, see www.asanet.org/convention/2004/ as well as Michael Burawoy, "Public Sociologies: Contradictions, Dilemmas, and Possibilities," *Social Forces* 82, no. 4 (2004): 1603–1618, and the responses to that article. For a somewhat different take, see Robert C. Prus, *Symbolic Interaction and Ethnographic Research: Intersubjectivity and the Study of Human Lived Experience* (Albany: State University of New York Press, 1996). Strictly speaking, my approach is somewhere on the boundary of what Burawoy calls "critical" and "public" sociology.

36. You may recall that the technical term for that trapped-in-the-headlights feeling is "tharn" (Richard Adams, *Watership Down* [New York: Avon, 1975]). Tharn is a regular part of scholarly life, but it need not be. I have found regular practice of salsa-dancing skills to be a tried and true antidote.

2. What's It All About?

1. Much of the material that follows was inspired by the pathbreaking work of Mary Jo Deegan, *Jane Addams and the Men of the Chicago School, 1892–1918* (New Brunswick [U.S.A.]: Transaction Books, 1988). My story

takes up the methodological and institutional dimensions of the “gender project” that Deegan first brought to light, but no one could write about this topic without her very important contribution. Deegan’s work really is a case of a person putting an entirely new topic on the research agenda.

2. The University of Chicago was the first university in the country to grant both a Ph.D. and an undergraduate degree in sociology. However, the sociology that they taught in the early years of the twentieth century would hardly be recognizable to most modern sociologists, containing as it did material on “charities and corrections” and “Christian socialism.” (For a sense of this, you might want to browse through the first years of the *American Journal of Sociology*, founded in 1895, the same year the university and the sociology department were founded, to see what our forerunners were thinking about.) There is now a rich literature on this first department of sociology, some of it critical. For an early and largely boosterish view (written by the son of one of the original members of the department, who was himself—the son, that is—a sociologist), which covers a slightly later period than I am talking about, see Robert E. Lee Faris, *Chicago Sociology, 1920–1932* (San Francisco: Chandler, 1967). For a more modern (and hence reflexive) view, see Martin Bulmer, *The Chicago School of Sociology: Institutionalization, Diversity, and the Rise of Sociological Research* (Chicago: University of Chicago Press, 1984). For something in between, see Andrew Abbott, *Department & Discipline: Chicago Sociology at One Hundred* (Chicago: University of Chicago Press, 1999). Note for the record that while this is a history of the first hundred years of the department of sociology at the University of Chicago, there is no mention of Jane Addams, thereby proving my point.

3. Albion Small to Jane Addams, Addams papers, DG1, box 4, Swarthmore College, Peace Collection.

4. Mary Jo Deegan points out that when Addams published her books, her publisher asked to whom she wanted complimentary copies sent. Addams replied that she only wanted them sent to people she knew personally, and this was the list. Deegan, *Jane Addams and the Men of the Chicago School*.

5. The “canon” of our intellectual ancestors is not as straightforward as it would seem, and it, too, is the result of the ebb and flow of a variety of

social and intellectual currents. In this context, be sure to read R. W. Connell, "Why Is Classical Theory Classical?" *American Journal of Sociology* 102, no. 6 (1997): 1511–1557. Connell takes on the subject of how our forefathers came to be canonical, and while one could argue with his position (and Randall Collins has: "A Sociological Guilt Trip: Comment on Connell," *American Journal of Sociology* 102, no. 6 [May 1997]: 1558–1564), that discussion highlights how it is that we think of some of our predecessors as "founders" and others (like Jane Addams) not. Not surprisingly, inventing a canon (and intellectual ancestors) is itself a social process that cannot be easily reduced to the merit of the contributions of our various foremothers and -fathers.

6. Deegan, *Jane Addams and the Men of the Chicago School*.

7. Martin Bulmer, one of the foremost chroniclers of the Chicago School, cites Burgess and Newcomb, among the early founders of the Department of Sociology at Chicago, to the effect that Chicago had moved from a tiny settlement of only 4,400 souls in 1840 to a metropolis of almost 1.7 million in 1900. Half of those 1.7 million people had been born outside of the United States. Martin Bulmer, *The Chicago School of Sociology: Institutionalization, Diversity, and the Rise of Sociological Research*, *The Heritage of Sociology* (Chicago: University of Chicago Press, 1984), pp. 13–14; Ernest Watson Burgess and Charles Shelton Newcomb, *Census Data of the City of Chicago, 1920* (Chicago: University of Chicago Press, 1931).

8. We will be coming back to this theme a great deal in the next chapter, but for now, just bear in mind that "objectivity" as the sine qua non of what it means to be "scientific" is itself a historically situated notion, one that bears some close scrutiny. A good overview is Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (Cambridge: Cambridge University Press, 1988); more recent works include Lorraine Daston and Peter Galison, "The Image of Objectivity," *Representations* 40 (1992): 81–128, and Alan Megill's book *Rethinking Objectivity* (Durham, N.C.: Duke University Press, 1994). (Thanks to Steven Epstein for bringing these to my attention.) Jane Addams was also a pacifist who opposed World War I, an act that turned her into a political pariah. See Joan Tronto, *Moral Boundaries: A Political Argument for an Ethic of*

Care (New York: Routledge, 1993), pp. 5–8; see also Allen Freeman Davis, *American Heroine: The Life and Legend of Jane Addams* (London: Oxford University Press, 1975).

9. There is a large and expanding literature on the rise of quantification in the social sciences, and indeed in the sciences more generally. In an earlier era, scientific (and, for that matter, legal) arguments were judged on their internal coherence, and their fidelity to formal rules of rhetoric. See, for example, Barbara Shapiro, *Probability and Certainty in Seventeenth-Century England: A Study of the Relationships between Natural Science, Religion, History, Law, and Literature* (Princeton, N.J.: Princeton University Press, 1983). Since then, the work of people like Theodore Porter, in particular his *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton, N.J.: Princeton University Press, 1995) along with his earlier book *The Rise of Statistical Thinking, 1820–1900* (Princeton, N.J.: Princeton University Press, 1986), look at how numbers came to seem so much more reliable and “objective” than narrative accounts. In the same vein, I found Mary Poovey’s *A History of the Modern Fact: Problems of Knowledge in the Sciences of Wealth and Society* (Chicago: University of Chicago Press, 1998) enormously thought-provoking, along with the work of Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England*, *Science and Its Conceptual Foundations* (Chicago: University of Chicago Press, 1994). Closer to home, the work of Charles Camic and Yu Xie, “The Statistical Turn in American Social Science: Columbia University, 1890 to 1915,” *American Sociological Review* 59, no. 5 (1994): 773–805, looks at how that sociology department turned to statistics early in the twentieth century. (In terms of the story I am telling here, the “quantification” of the department at Chicago, that is, the acceptance of the numeric approach to data originally championed by Addams, is conventionally dated to 1928, with the arrival of William Ogburn from Columbia.)

10. Published in 1895, *Hull-House Maps and Papers, a Presentation of Nationalities and Wages in a Congested District of Chicago, Together with Comments and Essays on Problems Growing out of the Social Conditions* (New York: T. Y. Crowell, 1895) is a startlingly modern investigation of the life and conditions of a neighborhood. In terms of both content and graphic

representation (color-coded charts of blocks of Chicago by the ethnic origin of the residents of each individual building), it anticipates modern social science in ways that I believe have not been fully appreciated.

11. William Isaac Thomas and Florian Znaniecki, *The Polish Peasant in Europe and America: Monograph of an Immigrant Group*, 5 vols. (Boston: Richard G. Badger, 1918). In all fairness, the “statistics” that the Chicago sociology department resisted were not the statistics of modern times, namely the manipulation of data in order to test assumptions about the relations between and among variables, but closer to the usage we moderns have in mind when we talk about “vital statistics,” that is, the numerical *description* of data in contrast to the numerical *analysis* of it. Nonetheless, in terms of the kinds of arguments put forward by people like Theodore Porter, it is still relevant that the department argued for narrative methods over quantitative ones.

12. This is the note I always have to put in when I write about gender. I mean gender as a socially constructed category, a bundle of tendencies, rather than a physical sex marked by the possession of a penis or a vagina. Charles Zueblin, for example, was a member of the Chicago School in good standing, worked closely with Addams, wrote a chapter in *Hull-House Maps and Papers*, and seems even to have lived there for a time. Florence Kelley, on the other hand, was educated in Switzerland and in terms of cultural capital was quite the peer of the men of the Chicago School. (Sophonisba Breckenridge, another Hull House resident, obtained both her law degree and her Ph.D. from the University of Chicago.) Robyn Muncy, *Creating a Female Dominion in American Reform, 1890–1935* (New York: Oxford University Press, 1991). So “gender” as I am using it is a probabilistic cultural category that does not always map neatly over the physical bodies of those creatures we call “men” and “women.”

13. Albion W. Small, “Seminar Notes: The Methodology of the Social Problem. Division I: The Source and Uses of Material,” *American Journal of Sociology* 4, no. 3 (1898): 380–394.

14. This rejection by the men of the Chicago School was almost surely overdetermined: nineteenth-century German statistics eschewed the quantification of statecraft (the basis of the word “statistics”) that the British were so enamored of. Paul Lazarsfeld traces this difference to the fractured and

balkanized nature of the German “state” in contrast to the unification of Great Britain where the “statistical” approach triumphed. See Paul F. Lazarsfeld, “Notes on the History of Quantification in Sociology—Trends, Sources and Problems,” *Isis* 52 (1961): 277–333.

15. Even within these fields, physical anthropologists and archaeologists tend to think of themselves as more rigorous than social anthropologists, and there was a brief craze for “cliometrics” (quantitative history) some years back. In political science, the “dataset” political scientists often think they are more rigorous than the “process tracing” ones. “Dataset” and “process tracing” as terms for distinguishing political scientists come from Henry Brady and David Collier, and they map pretty well across what Charles Ragin calls “variable” and “case” social scientists. See Henry E. Brady and David Collier, *Rethinking Social Inquiry: Diverse Tools, Shared Standards* (Lanham, Md.: Rowman & Littlefield, 2004), and Charles C. Ragin, *The Comparative Method: Moving Beyond Qualitative and Quantitative Strategies* (Berkeley: University of California Press, 1987). (Both of these books are ones I urge my students to buy, and I would give you the same advice, too.)

16. Some scholars will disagree entirely with the account I am giving here; I’ve listed some of the classic references on the matter in these endnotes so you can decide for yourself. Obviously, in something as sprawling and contested as the disciplines of the social sciences were between the turn of the (last) century and World War II, this generalization should be taken as just that, a statement about the pattern I see when looking at the development of the social sciences in this period. There are exceptions, such as the case of Columbia University’s department, noted above, but I think the task of social science is that of *pattern recognition*, and this is the pattern I see. I would suggest looking at Charles Camic and Yu Xie, “The Statistical Turn in American Social Science”; Martin Bulmer, *The Chicago School of Sociology*; George Steinmetz, *The Politics of Method in the Human Sciences: Positivism and Its Epistemological Others*, Politics, History, and Culture (Durham, N.C.: Duke University Press, 2005); Jennifer Platt, “The Chicago School and Firsthand Data,” *History of Human Sciences* 7, no. 1 (1994): 57–80, and Platt’s *A History of Sociological Research Methods in America: 1920–1960* (Cambridge: Cambridge University Press, 1996). See also Anthony Oberschall, *The Establishment of Empirical Sociology*:

Studies in Continuity, Discontinuity, and Institutionalization (New York: Harper & Row, 1972).

17. On the rise of the standardized survey see Jean M. Converse, *Survey Research in the United States: Roots and Emergence, 1890–1960* (Berkeley: University of California Press, 1987), and Martin Bulmer, Kevin Bales, and Kathryn Kish Sklar, *The Social Survey in Historical Perspective, 1880–1940* (Cambridge: Cambridge University Press, 1991).

18. What I have in mind here includes such regularly repeated surveys as election polling, The General Social Survey (www.norc.org/projects/General+Social+Survey.htm), the Youth Risk Behavior Surveillance System (www.cdc.gov/HealthyYouth/yrbs/index.htm), the National Longitudinal Study of Adolescent Health (www.cpc.unc.edu/addhealth), and the National Longitudinal Survey of Youth (www.bls.gov/nls/), which represent just the merest tip of the iceberg of easily available national survey data on what Americans are up to.

19. Particularly relevant in this context is Steinmetz, *The Politics of Method in the Human Sciences*.

20. Stephen Skowronek, *Building a New American State: The Expansion of National Administrative Capacities, 1877–1920* (Cambridge: Cambridge University Press, 1982). See also Michel Foucault, Graham Burchell, Colin Gordon, and Peter Miller, *The Foucault Effect: Studies in Governmentality: With Two Lectures by and an Interview with Michel Foucault* (Chicago: University of Chicago Press, 1991).

21. James Scott, *Seeing Like a State: How Certain Schemes to Improve the Human Condition Have Failed* (New Haven: Yale University Press, 1998).

22. See (among others) Arthur L. Norberg, “High-Technology Calculation in the Early 20th Century: Punched Card Machinery in Business and Government,” *Technology and Culture* 31, no. 4 (1990): 753–779; Leon E. Truesdell, *The Development of Punch Card Tabulation in the Bureau of the Census* (Washington, D.C.: U.S. Dept. of Commerce Bureau of the Census, U.S. Government Printing Office, 1965); Geoffrey Austrian, *Herman Hollerith, Forgotten Giant of Information Processing* (New York: Columbia University Press, 1982).

23. Barry D. Karl, “Presidential Planning and Social Science Research: Mr. Hoover’s Experts,” *Perspectives in American History* 3 (1969): 347–409.

24. Martin Bulmer estimates that there were fifty to sixty sociologists

working at the Department of Agriculture before World War II under the aegis of Carl Taylor. Martin Bulmer, in Terence C. Halliday and Morris Janowitz, eds., *Sociology and Its Publics: The Forms and Fates of Disciplinary Organization* (Chicago: University of Chicago Press, 1992), p. 320. See also Jean M. Converse's magisterial *Survey Research in the United States*, cited in note 17 above.

25. Converse, *Survey Research*, pp. 5 and 160.

26. *Ibid.*, pp. 160–161.

27. L. J. Rhoades, *A History of the American Sociological Association, 1905–1980* (Washington, D.C.: American Sociological Association, 1981). See also Katherine J. Rosich, *A History of the American Sociological Association, 1981–2004*, Appendix 12, “Membership by Year,” p. 140.

28. See Irving Louis Horowitz, ed., *The Rise and Fall of Project Camelot: Studies in the Relationship Between Social Science and Practical Politics* (Cambridge, Mass.: MIT Press, 1967). (The project was eventually canceled after protests among social scientists, but see Michael Latham, *Modernization as Ideology: American Social Science and “Nation Building” in the Kennedy Era* [Chapel Hill: University of North Carolina Press, 2000]. That makes even a larger case for how social science and ideology were intertwined in this period.) On the New Jersey Income Maintenance plan, see David Kershaw et al., *The New Jersey Income-Maintenance Experiment* (New York: Academic Press, 1976).

29. What I am doing in this book, formally speaking, is containing a perspective on *methods* (how we ask questions) with a perspective on *methodology* (a critical reflection on how we ask questions).

30. Or conversely, depending on which history you read, first philanthropic foundations and then the government. Bulmer, in *The Chicago School of Sociology*, attends to the role of philanthropy in the 1920s in creating sociology; see also Edward Shils, *The Present State of American Sociology* (Glencoe, Ill.: Free Press, 1948), and Oberschall, *The Establishment of Empirical Sociology*.

31. This is the idea of social closure again. In a sense, this is the “professionalization project” cited in note 32 below.

32. I mentioned earlier the work of Magali Sarfatti Larson, *The Rise of Professionalism: A Sociological Analysis* (Berkeley: University of California

Press, 1977), and Stephan Fuchs, *The Professional Quest for Truth: A Social Theory of Science and Knowledge* (Albany: State University of New York Press, 1992).

33. The smartest thing I have read on this is the work by Burt Singer, who analyzed how it was that the women scientists in the National Academy of Sciences were on average less productive than male scientists. Keep in mind that both men and women were in the National Academy as a mark of significant and outstanding professional achievement. So why had women published less? The answer is that a series of very small setbacks, no one of which was definitive, eventually culminated in a situation where men were more productive. (When denied a grant, for example, women more often rethought their whole research project, and then more often re-submitted to another agency.) See J. Cole and B. Singer, “A Theory of Limited Differences: Explaining the Productivity Puzzle in Science,” in *The Outer Circle: Women in the Scientific Community*, ed. H. Zuckerman, J. R. Cole, and J. T. Bruer (New York: W. W. Norton, 1991), pp. 277–310. Theodore Porter, quoting some research by Liam Hudson, notes how mathematics can serve as a way of pushing people into and out of disciplines. Porter calls this the “self-vindicating laboratory,” whereby a social process (the difficulty of mathematics) squeezes out “weaker” students such that the process re-creates the hierarchy. “But social selection, including a gendered dimension in physics and biology at least as strong as in psychology, provides an important part of the explanation for the distinctive character of modern science as a form of knowledge and practice.” Theodore Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton, N.J.: Princeton University Press, 1995), p. 17.

34. However, as Adam Przeworski and Henry Teune have pointed out, even in quantitative research there is a version of the Heisenberg uncertainty principle, which formally states that you can know the location of a particle or its movement, but not both. Przeworski and Teune argue that in quantitative work, as in qualitative, there are always trade-offs between accuracy, generality, parsimony, and causality. Adam Przeworski and Henry Teune, *The Logic of Comparative Social Inquiry* (New York: Wiley-Interscience, 1970), pp. 20–23. (Another “Best Buy” book, if you are interested.)

35. Loïc Wacquant, “Scrutinizing the Street: Poverty, Morality, and the Pitfalls of Urban Ethnography,” *American Journal of Sociology* 107 (2002): 1468–1532. While I couldn’t agree more with Wacquant’s point about the need to build theory, I’m not convinced that the work he cites in this article makes his case.

36. A “sinusoidal” curve is one that looks like that slinky toy from your childhood, and “damped” means that each of the humps of the slinky gets smaller as it wends its way across the graph. If you were to graph the numbers of babies who died in the first year of life (“infant mortality”) across time after you instituted a program to reduce infant mortality, in more than one case I know of, it would look like a slowly deflating Slinky, or, to be precise, a damped sinusoidal curve.

37. I certainly tried. I drew up a list of all (non-Catholic) hospitals in the San Francisco Bay Area, developed a random sample from that list, got permission to interview all women having babies in those hospitals, and then did the same thing with the one and only hospital providing abortion services. I even “logged” interview times for the women having abortions so that conception dates would be broadly similar. Then I bumped up against the limits of survey research: what would I *really* know after doing hundreds of surveys? What I was really interested in was what Brady and Collier call “process tracing.”

38. Barney Glaser, whom I never met, was trained at Columbia, the place quantification came from when it came to Chicago. As Kathy Charmaz notes, this meant that grounded theory grew up in the same “sweet spot” that this book aims for, between the rigor of quantitative social science (Glaser) and the open, emergent, and pragmatic model—note the role of Chicago and Dewey again—of traditional field methods (Strauss). Kathy Charmaz, *Constructing Grounded Theory: A Practical Guide through Qualitative Analysis* (Thousand Oaks, Calif.: Sage Publications, 2006).

39. Barney Glaser and A. L. Strauss, *Awareness of Dying* (Chicago: Aldine, 1965); Fred Davis, *Passage Through Crisis: Polio Victims and Their Families* (Indianapolis: Bobbs-Merrill, 1963). Perhaps because of the historical accident that both Glaser and Strauss were located in a medical school, much of the best-known work in grounded theory is about medical processes, broadly speaking. The list is very long, but for a few examples,

see Kathy Charmaz, *The Social Reality of Death: Death in Contemporary America* (Reading, Mass.: Addison-Wesley, 1980); Charmaz, *Good Days, Bad Days: The Self in Chronic Illness and Time* (New Brunswick, N.J.: Rutgers University Press, 1991); Adele Clarke, *Disciplining Reproduction: Modernity, American Life Sciences, and “the Problems of Sex”* (Berkeley: University of California Press, 1998).

40. Or, to quote Michael Burawoy, “grounded theorists . . . [t]oo often . . . remained trapped in the contemporary, riveted to and contained in their sites, from where they bracket questions of historical change, social process, wider contexts, theoretical traditions as well as their own relationships to the people they study.” Michael Burawoy, “Revisits: An Outline of a Theory of Reflexive Ethnography,” *American Sociological Review* 68 (2003): 646.

41. And when Burawoy does participant observation, the emphasis is on the word *participant*—he’s worked in more factories than almost any other sociologist I could name, from Chicago to Siberia and in between. See Michael Burawoy, *Manufacturing Consent: Changes in the Labor Process under Monopoly Capitalism* (Chicago: University of Chicago Press, 1979), and Jeff Byles, “Tales of the Kefir Furnaceman,” *The Village Voice*, New York, April 10, 2001, p. 76.

42. Nina Eliasoph and Paul Lichterman, “We Begin with Our Favorite Theory . . . Reconstructing the Extended Case Method,” *Sociological Theory* 17, no. 2 (1999): 228.

43. Foundations have long had a complex and intricate relationship with social scientists. To the extent that foundations want to “do good,” they have tended to turn to social scientists to help define and evaluate social problems. For example, see Donald Fisher, *Fundamental Development of the Social Sciences: Rockefeller Philanthropy and the United States Social Science Research Council* (Ann Arbor: University of Michigan Press, 1993).

44. Qualitative methods are not immune to the lure of governmentality. As you will see later in this book, many of the methods we take for granted were developed during World War II by social scientists in order to help in the war effort, and there is an active debate at this very moment about the uses of ethnography in counter-insurgency. On the latter, see David Rohde, “Army Enlists Anthropology in War Zones,” in *New York Times*, October 5,

2007, and Richard A. Shweder, “A True Culture War,” *New York Times*, October 27, 2007.

45. I exempt historical-comparative sociologists from this generalization, because they, the heirs of the founders of sociology, tend to ask big questions with big answers. They don’t fit neatly into my schematization, even though they usually use qualitative methods, because building theory is what they’re about. I’ve dedicated a whole chapter to them later on, so if you’re feeling impatient, you should go straight there.

46. Frank L. Luntz, “Focus Group Research in American Politics,” *The Polling Report* 10, no. 10 (1994): 7.

47. In my training as a canonical sociologist, for example, I was taught that good variable construction (also known as good operationalization of a concept) meant coming up with a set of values—possible answers—that were both “mutually exclusive and exhaustive.” That is, the answer to the question could only fall in one of the possible answers offered, and those answers had to include all the possible answers to the question. Think about this requirement in the context of Frank Luntz’s quote above.

3. *An Ode to Canonical Social Science*

1. You might want to reread the article by Andrew Abbott mentioned in Chapter 1, note 19, to refresh your memory about the taken-for-granted assumptions in linear regression, namely that categories stay the same, that context doesn’t matter, that time is not a relevant actor in what you are studying. It may just be that you are the kind of person who finds it hard to relax your assumptions about social life that much.

2. Keep in mind that for rhetorical purposes, I am assuming that canonical sociology, at least since the Second World War, is quantitative, built on already-accumulated databases generated from survey research, and amenable to the application of linear models. We could think of exceptions to these overlapping categories, but for rhetorical purposes, I am collapsing what are in essence three overlapping sets into one. Michael Burawoy reminds us that “positive science” is the *model* here, and surveys are the *method*. (Michael Burawoy, “The Extended Case Method,” *Sociological Theory* 16, no. 1 [1998]: 4–33.)

3. The “zafu” is the little black pillow on which Zen meditators sit, and the “zabuton” is the somewhat larger black mat on which the little black pillow itself sits. Or vice versa; I told you that I don’t spend time on what I think are irrelevant details. But I must remind you, I’ve been meditating for many, many years and doing research for even more, so I feel reasonably confident that what I think is an irrelevant detail really is. Before you start cutting corners, however, I strongly urge you to check with other like-minded people. I don’t want to get into the “Where did Wordsworth go to school?” trivia test that I talked about in the first chapter, but some details, though small, really are crucial. Proceed with caution.

4. Rabbi Hillel, when asked to summarize the Torah while standing on one foot, said, “What is hateful to you, do not do to another. That is the whole of the Torah. All else is commentary.” Would that I could do as well.

5. On the discovery of probability, Ian Hacking’s *The Taming of Chance* is terrific (Cambridge: Cambridge University Press, 1990). For more on the social context of these developments in the social sciences, be sure to read Theodore M. Porter, *The Rise of Statistical Thinking, 1820–1900* (Princeton, N.J.: Princeton University Press, 1986). If you want to read a really devastating critique from deep within the belly of the beast (economics), take a close look at Deirdre N. McCloskey, *The Rhetoric of Economics* (Madison: University of Wisconsin Press, 1985).

6. Lisa Remez, “Oral Sex among Adolescents: Is It Sex or Is It Abstinence?” *Family Planning Perspectives* 32, no. 6 (2000): 298–304.

7. See, for example, Sharon Thompson, *Going All the Way: Teenage Girls’ Tales of Sex, Romance, and Pregnancy*, 1st ed. (New York: Hill and Wang, 1995).

8. William Blake, *Auguries of Innocence*: “To see a world in a grain of sand, / And a heaven in a wild flower, / Hold infinity in the palm of your hand, / And eternity in an hour.”

9. “Face” validity means that to the naked eye it looks like you really are measuring that which you think you are measuring.

10. In other words, you want to prepare for the eventuality that whatever relationship you are exploring, say the effects of education on later income, will hold in poor, white lesbians under forty, but not black, wealthy heterosexuals over thirty, and so on and so on.

11. Strictly speaking, you are not “dropping” categories, but you are not oversampling them such that you will have an adequate number to examine in light of other variables.

12. “Oversampling” means collecting more elements (“people”) than you would expect from your random sample. In a random sample of Americans, for example, you would expect to find about 12 percent of them to be African-American, because that is roughly the proportion of African-Americans in the population (assuming, of course, that there is a more or less stable category of “African-American” these days, which is increasingly problematic). If you had reason to believe, from previous research and/or theory, that A was related to B in very different ways for African-Americans and white Americans, you might want to “oversample” African-Americans such that they made up 25 percent of your sample rather than 12 percent, so that you would be sure that you had enough African-Americans of different classes, educational levels, genders, religions, and whatever else you thought was important. One of the big datasets that I mentioned earlier, the Panel Study of Income Dynamics (PSID), routinely oversamples people on welfare, since that is a group of particular interest to those who study “income dynamics.”

13. The worst possible reason being, of course, that we chose our observations, consciously or unconsciously, to prove our point.

14. This is, I think, a variation of what Howard Becker called the “hierarchy of credibility,” meaning that the closer your work hews to accepted, taken-for-granted cultural norms, the more “reasonable” and “intuitively obvious” your work and your findings are. The more you find yourself challenging these norms, the harder you have to work to have any kind of credibility. Howard S. Becker, “Whose Side Are We On?” *Social Problems* 14 (1967): 239–247.

15. If you were reading carefully a few pages ago, you will recall that I mean generalize theoretically, not statistically. In other words, you can speculate that this is “a case of . . .” something bigger, *theoretically speaking*, than the few individuals or settings you studied, but you cannot prove it. That step is up to others, the folks who do theory-testing, or to your next project.

16. It's not really a *convenience* sample, as it happens, but a *theoretical* one. We'll get to that later.

4. What Is This a Case of, Anyway?

1. I found Frederick Crews's discussion of the difference between what I call a research interest and a research question very helpful; see Frederick C. Crews, *The Random House Handbook*, 2nd ed. (New York: Random House, 1977). This is nominally a book about rhetoric, but inasmuch as it tells you how to make claims in print, it is extremely useful.

2. This is what in my family we call a "lobster problem"—you have to go through a lot of shell to get to the good stuff.

3. Walter L. Wallace, *Sociological Theory: An Introduction* (Chicago: Aldine, 1969). This past year one of my students came up with a way of remembering the difference—the "explanans" explains.

4. This is a bit tricky, in that much of the work that salsa-dancing social scientists do is hypothesis (or theory) *generating*, not testing. As such, you typically don't have a set of possible answers at the beginning of the research project among which you can adjudicate. In fact, as I just said, and say elsewhere in this book, often enough the full research question in all its clarity is the last thing you discover, not the first. But what I have in mind here is a research question that is at least in principle *logically* falsifiable, in that you can show that your answer is better than the conventional wisdom, or that there are other logical explanations for the patterns you see, but that your account of them is more robust than other accounts.

5. As it happens, someone *did* write a book about cocaine dealers: Patricia A. Adler, *Wheeling and Dealing: An Ethnography of an Upper-Level Drug Dealing and Smuggling Community* (New York: Columbia University Press, 1985).

6. When scientists call an explanation "robust," they mean that it accounts for more of the data than do alternative explanations.

7. Gary King (himself the author of a key book on methodology that claims that qualitative social scientists would do better work if they just thought like quantitative social scientists) argues that the way to publish is

simply to replicate the findings of a published article, but tweak it. This is a classic—and indeed valuable—example of “normal science” in action. See Gary King, “Publication, Publication,” *PS: Political Science and Politics* 39, no. 1 (January 2006): 119–125. On the larger issue, see Gary King, Robert O. Keohane, and Sidney Verba, *Designing Social Inquiry: Scientific Inference in Qualitative Research* (Princeton, N.J.: Princeton University Press, 1994). You have already been introduced to the book that takes on King et al.’s assertion that qualitative research should be more like quantitative, namely the Brady and Collier book (Henry E. Brady and David Collier, *Rethinking Social Inquiry: Diverse Tools, Shared Standards* [Lanham, Md.: Rowman & Littlefield, 2004]). Be sure to read it carefully before signing on to the King et al. position, but there’s still no reason you can’t meditate on King’s ideas about how to get into print.

8. For the intellectual and epistemological problems inherent in using a technology (statistics) derived from real experiments to assess quasi-experimental designs, see William R. Shadish, Thomas D. Cook, and Donald Thomas Campbell, *Experimental and Quasi-Experimental Designs for Generalized Causal Inference* (Boston: Houghton Mifflin, 2002).

9. This may look like a medical question, but as the discussion will make clear, I think the problems were really *social* in nature—which permits me to use this as an example in a book about salsa dancing in the *social* sciences.

10. Canonical social scientists are not unaware of these problems, and getting a handle on them is something of a Holy Grail for them. Models that use matching effects are one thing that canonicals are trying these days, and matching algorithms are getting better all the time.

11. Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962; 3rd edition, 1996).

12. In this context, see Steven Shapin, *A Social History of Truth* (Chicago: University of Chicago Press, 1994), pp. xv–xxiv.

13. See Zygmunt Bauman, *Legislators and Interpreters: On Modernity, Post-modernity, and Intellectuals* (Cambridge: Polity Press in association with B. Blackwell, 1987).

14. Yes, I know that Gregory Bateson (Margaret Mead’s erstwhile hus-

band) was the inventor of the notion in 1972. But Goffman, a sociologist, made it work, at least in my opinion. See Erving Goffman, *Frame Analysis: An Essay on the Organization of Experience* (Boston: Northeastern University Press, 1986).

15. Daniel Simons and Christopher Chabris, “Gorillas in Our Midst: Sustained Inattentional Blindness for Dynamic Events,” *Perception* 28 (1998): 1059–1074. To see the video for yourself, go to <http://viscog.beckman.uiuc.edu/grafs/demos/15.html>.

16. Let me be absolutely clear: HIV/AIDS in Africa is a human and social tragedy of world-historic scope, and much more needs to be known and written about it. But in terms of getting your own views into print, precisely because of the overwhelming nature of the phenomenon, you have to have something new, the “hook,” to catch the attention of others. Again, I know very little about the subject, but I can think of one or two new ways of approaching the question. Here’s one example: it turns out that a lot of people in South Africa who are doing what you and I might think of as prostitution don’t think of themselves as CSWs (“commercial sex workers”). At least some of them find themselves involved with long-distance truckers. If I were looking for a “hook” (a.k.a. “frame”), I would pitch exploring the juxtaposition of women who don’t think of themselves as sex workers having sex with lots of different men who drive all over the continent of Africa. See how I’ve put in some elements and a proposed relationship? For a really smart piece on these women (and some men) involved in what the authors call “survival sex,” see Christine Varga, Eleanor Preston-Whyte, Herman Oosthuizen, Rachel Roberts, and Frederick Blose, “Survival Sex and HIV-AIDS in an African City,” in *Framing the Sexual Subject: The Politics of Gender, Sexuality, and Power*, ed. Regina Maria Barbosa, Richard Parker, and Peter Aggleton (Berkeley: University of California Press, 2000).

17. “In the room the women come and go / Talking of Michelangelo” (T. S. Eliot, “The Love Song of J. Alfred Prufrock”).

18. For a nice overview of the state of the debate, see Robert D. Benford and David A. Snow, “Framing Processes and Social Movements: An Overview and Assessment,” *Annual Review of Sociology* 26 (2000): 611–639.

19. Keep in mind here that you don’t actually have to choose only one of

these and ignore all the others. In fact, to the extent you can say that you're a sociologist of gender *and* a sociologist of work, you've just doubled the number of jobs you are eligible for. But at this stage of the research it's important to keep only one or two of these subspecialties in the foreground and let the others recede somewhat into the background. If you were to announce that you were a sociologist of all of the subspecialties I listed above, people would think you were either a maniac or a jack of all trades and a master of none.

20. I've quoted this for many, many years, and I'm not sure where I first read it. Erica Jong once wrote that while the Muse comes on her own schedule, she does not come to a messy or unprepared house. I've forgotten where I read that, and alas, Jong has forgotten where she *wrote* it (I e-mailed her and asked). But her most recent book (*Seducing the Demon: Writing for My Life* [New York: Jeremy P. Tarcher/Penguin, 2006]) shares a lifetime's thoughts on writing. The Muse, by the way, is just another name for inspiration or whatever else you want to call it. Remember Erica Jong's admonition.

21. Although Bourdieu has made this point many times in many places, I think he says it most succinctly in his *Pascalian Meditations*, when he observes, "Successful initiation . . . secures the essential privilege of all 'well-born' persons, an adaptation to the game so immediate and so total that it seems to be innate and gives its possessors the supreme advantage of not needing to calculate in order to win the rarest of the profits offered by the game." Pierre Bourdieu, *Pascalian Meditations* (Stanford, Calif.: Stanford University Press, 2000), p. 36.

22. Alison Schneider, quoting Harvard sociologist Barbara Reskin. See Alison Schneider, "Gender Gap in Scholarly Publishing: Why Don't Women Publish as Much as Men?" *Chronicle of Higher Education*, September 11, 1998, p. A14.

23. It has been so long since I've collected these two reviews that I have forgotten exactly who wrote them. If you recognize yourself, please let me know, and I promise to credit you in future editions.

24. Michael Hout, Clem Brooks, and Jeff Manza, "The Democratic Class Struggle in the United States, 1948–1992," *American Sociological Review* 60, no. 6 (December 1995): 805–828.

5. Reviewing the Literature

1. This part of the book draws heavily on the work of Thomas Mann, *Library Research Models: A Guide to Classification, Cataloging, and Computers* (New York: Oxford University Press, 1993). You may be thinking that I recommend far too many books, but trust me, all of them are what *Consumer Reports* would call “best buys.” This book by Thomas Mann (to the best of my knowledge, no relation to the Thomas Mann of *The Magic Mountain*) will make you so much smarter as an information user that you owe it to yourself to buy it.

2. The Dewey Decimal System was designed by one Melvil Dewey (no relation to John). In fact, the online library catalog here at Berkeley is called Melvyl.

3. Mann, *Library Research Models*.

4. Barbara Christian, *Black Women Novelists: The Development of a Tradition, 1892–1976* (Westport, Conn.: Greenwood Press, 1985).

5. Patricia Ianuzzi, quoted in the *Berkeley Daily Planet*, August 10, 2004.

6. www.annualreviews.com. This statement is from their website: “Annual Reviews is proud to publish authoritative, analytic reviews in 30 focused disciplines within the Biomedical, Physical, and Social Sciences. Annual Reviews publications are among the most highly cited in scientific literature.”

7. It was in 1979. Anthony Giddens, *Central Problems in Social Theory* (London: Macmillan, 1979). There’s a hint of it in his 1971 book, *Capitalism and Modern Social Theory*, but it gets more fully developed here. (And then, of course, later: go look in your dictionary of sociology to see his whole bibliography.)

8. I think the difference between scholarly books and articles is increasingly one of audiences. Articles are for the in-group, the “we really are a science” people of your field, while books are for larger audiences who are not necessarily narrowly interested in the bounded questions pursued in your field. My hunch is that you will be drawn toward books, being interested in juicy cases, *but there is no reason whatsoever that you should restrict yourself*

to books. I think the journals need the kind of work that salsa-dancing social scientists do.

9. Gene Burns, *The Moral Veto: Framing Contraception, Abortion, and Cultural Pluralism in the United States* (Cambridge: Cambridge University Press, 2005).

10. <http://wwwlib.umi.com/dissertations>.

11. “The Second Shift” was named by Arlie Hochschild in her book of the same name as that work that a family member—usually but not always a woman—does after she comes home from her regular (“first shift”) job. Arlie Russell Hochschild and Anne Machung, *The Second Shift: Working Parents and the Revolution at Home* (New York: Viking, 1989).

12. I assume that everyone who is doing scholarly work does have Internet access, but this is not an unproblematic assumption. Anita Schiller pointed out over twenty years ago that the increasing digitization of information, especially by for-profit firms, meant that once-freely-available information (often gathered or catalogued at public expense) had become a commodity and now could be charged for. She worried about the day when a modern Karl Marx would sit down in the British Library and be stymied because he could not afford the fees needed to look at the equivalent of what his namesake had examined for free. See Anita Schiller, “Shifting Boundaries in Information,” *Library Journal* 106 (1981): 705.

13. www.library.ucla.edu/bruinsuccess/.

14. Mortimer Adler and Charles Van Doren, *How to Read a Book* (New York: Simon and Schuster, 1972).

6. On Sampling, Operationalization, and Generalization

1. Honesty compels me to say that although we trained together and were part of a volunteer team on my campus, we never did put in the intensive kind of time—roughly several hours a day—that would have made us eligible to become a *real* (FEMA-certified) dog and handler team. Since at the time there was only one officially-certified K-9 SAR dog in the entire city of Berkeley, the other members of my volunteer SAR team on campus figured that in the event of an earthquake causing massive damage, our help, though limited, could prove useful.

2. This is a direct “importable novelty” (Andrew Abbott’s term) or shameless borrowing (my term) of the Glaser and Strauss grounded theory model of sampling.

3. Michael Burawoy, *The Colour of Class on the Copper Mines, from African Advancement to Zambianization* (Manchester: Manchester University Press for the Institute for African Studies University of Zambia, 1972).

4. Reva B. Siegel, “‘The Rule of Love’: Wife Beating as Prerogative and Privacy,” *Yale Law Journal* 105 (1996): 2117–2207. An article to be read by all aspiring social scientists, not just sociolegal ones.

5. Seymour Martin Lipset, *Union Democracy: The Internal Politics of the International Typographical Union* (Glencoe, Ill.: Free Press, 1956); Kim Voss and Rachel Sherman, “Breaking the Iron Law of Oligarchy: Union Revitalization in the American Union Movement,” *American Journal of Sociology* 106, no. 2 (September 2000): 303–349.

6. Annette Lareau, *Unequal Childhoods: Class, Race, and Family Life* (Berkeley: University of California Press, 2003). (It was Andrew Carnegie who called schools “ladders of ascent.”)

7. On home schooling, see Mitchell L. Stevens, *Kingdom of Children: Culture and Controversy in the Homeschooling Movement*, Princeton Studies in Cultural Sociology (Princeton: Princeton University Press, 2001.)

8. This particular mistake is called “sampling on the dependent variable.”

9. It’s pronounced “Sin EK doak ee,” and confusingly enough, it also means where the whole stands in for a part. The part standing in for the whole would be when you say “we have a roof over our heads” when what you mean is that you are housed; the whole standing in for a part would be when you speak of “the law” (as in “scram, here comes the law!”) when what you really mean is an individual police officer. I first learned about this in Hayden White’s *Metahistory*.

10. What do we mean by “more or less similar”? Again, this is theoretically determined. If you think your Silicon Valley workplace has a lot of flirting because it’s organizationally “flat” (i.e., not a lot of hierarchy), you would want to sample another such group, unless you chose a *contrasting*

case where the organization was a pyramid. But note that if you choose a contrasting case, the research question has changed from something like “How do people think about flirting in the workplace?” to “What effect does organizational structure have on flirting?” This is fine, of course, but because sampling is *theoretical*, the choice of a certain kind of sample (in this case, a contrasting organizational style) moves you into a new theoretical dimension.

11. See AnnaLee Saxenian, *Regional Advantage: Culture and Competition in Silicon Valley and Route 128* (Cambridge, Mass.: Harvard University Press, 1994).

12. This is a variation of an argument that Alain Touraine once made. Touraine, *The Post-Industrial Society; Tomorrow's Social History: Classes, Conflicts and Culture in the Programmed Society* (New York: Random House, 1971).

13. Stephen Jay Gould points out that three is often a magic number in narratives, and the cognitive psychologists think that they can tell us why. See Gould, “Carrie Buck's Daughter,” *Natural History*, July 1993, pp. 14–18.

14. Strictly speaking, these were data for *all* abortions, not just second and later ones. Although I did not know for a long time that there *were* second and subsequent abortions, I did know from the literature that there had been a vastly expanded access to publicly subsidized contraception in the 1960s, and that contraceptive use was becoming more and more common all the time. For fun, see Jane Mauldon and Kristin Luker, “Does Liberalism Cause Sex?” *American Prospect* 24 (1996): 80.

15. For human-subjects reasons, I interviewed people when they came back for a follow-up visit after the abortion. (I was worried that if I asked beforehand, women might think they *had* to talk to me in order to get an abortion.) Not all women came back for their follow-up visit, so there was sample selection bias in that regard, but my hunch is that had I been able to find all these women, my hypothesis would have been strengthened, not weakened. I did try to follow up, by the way, but a surprising number of women who did not come back for a checkup after the abortion used false names and/or addresses.

16. I have it on very good authority that two distinguished faculty members (not at Yale) had a bet with each other in the early 1960s as to how many women they could seduce. In order to make the challenge more meaningful, however, they agreed that neither of them would count graduate students in the final tally.

17. Catharine A. MacKinnon, *Sexual Harassment of Working Women: A Case of Sex Discrimination* (New Haven: Yale University Press, 1979). I just reread MacKinnon's "Feminism, Marxism, and the State: An Agenda for Theory" (*Signs* 7 [1982]: 515–544) and am struck by the power of her analysis. A variation on this point is the idea that in order to have a right, you have to engage in a process of "naming, blaming, and claiming." See William L. F. Felstiner, Richard L. Abel, and Austin Sarat, "The Emergence and Transformation of Disputes: Naming, Blaming, Claiming . . .," *Law & Society Review* 15 (1980): 631–654.

18. OK, I'll come clean. I'm using data from 1990, because they make the point more clearly, and permit me to use Hoff Sommers as my "straw woman." As a point of comparison, in 2004, the FBI reported (in the Uniform Crime Reports) 93,934 forcible rapes reported to law enforcement, while the Department of Justice's National Criminal Victim Survey (which now asks more directly about rape) discovered 209,880 rapes and sexual assaults committed against persons age 12 or older in the United States that same year.

19. FBI, Uniform Crime Reports (Washington, D.C., 1990); Department of Justice, Bureau of Justice Statistics, Criminal Victimization (Washington, D.C., U.S. Department of Justice); Christina Hoff Sommers, *Who Stole Feminism? How Women Have Betrayed Women* (New York: Simon & Schuster, 1994), pp. 209–226.

20. Katie Roiphe, *The Morning After: Sex, Fear, and Feminism*, 1st paperback ed. (Boston: Little, Brown, 1994); Neil Gilbert, "Realities and Mythologies of Rape," *Society* 29 (4) (May–June 1992): 4–10; Sommers, *Who Stole Feminism?*; Camille Paglia, *Sex, Art, and American Culture* (New York: Vintage Books, 1991); Heather MacDonald, "What Campus Rape Crisis?" *Los Angeles Times*, February 24, 2008.

21. Not surprisingly, this points to the role of institutions in creating and

validating data. In more cases than we like to think, data have gone through a sieve of human activity before they get to us.

22. Jane Gross, “203 Rape Cases Reopened in Oakland as the Police Chief Admits Mistakes,” *New York Times*, September 20, 1990, p. 14. Gross reports that Oakland police routinely dropped rape cases involving prostitutes and drug abusers, failing to even minimally investigate them.

23. This argument will either make perfect sense to you, or will seem just one more example of feminist “PC.” But for the purposes of my argument, you don’t have to accept that this is the way things really were; you just have to accept that in the old days rape was a binary event, and the boundaries between raped and not raped were high.

24. Siegel, “The Rule of Love.”

25. For more on this point in the case of sex education, see Kristin Luker, *When Sex Goes to School: Warring Views on Sex—and Sex Education—Since the Sixties* (New York: W. W. Norton, 2006).

26. Mary Koss, “Hidden Rape: Sexual Aggression and Victimization in a National Sample of Students in Higher Education,” in Ann Wolbert Burgess, ed., *Rape and Sexual Assault* (New York: Garland, 1985). See also Mary Koss, Christine A. Gidycz, and Nadine Wisniewski, “The Scope of Rape: Incidence and Prevalence of Sexual Aggression and Victimization in a National Sample of Higher Education Students,” *Journal of Consulting and Clinical Psychology* 55, no. 2 (1987): 62–170.

27. Koss, “Hidden Rape.”

28. Edward O. Laumann et al., *The Social Organization of Sexuality: Sexual Practices in the United States* (Chicago: University of Chicago Press, 1994), pp. 333–338. Laumann’s survey was incredibly controversial (see Edward O. Laumann, Robert T. Michael, and John H. Gagnon, “A Political History of the National Sex Survey of Adults,” *Family Planning Perspectives* 26 [1994]: 34–38) and, unlike the Kinsey studies of the late 1940s and early 1950s, was based on a random sample of adults.

29. *Ibid.*, p. 333.

30. Pierre Bourdieu and Jean Claude Passeron, *The Inheritors: French Students and Their Relation to Culture* (Chicago: University of Chicago Press: 1979).

31. Or, put another way, what is this a case of? How could we, as readers, “bump up” Lareau’s study?

7. *Getting Down to the Nitty-Gritty*

1. Don’t believe me? Check out this article: Joshua Guetzkow, Michèle Lamont, and Gregoire Mallard, “What Is Originality in the Humanities and the Social Sciences?” *American Sociological Review* 69, no. 2 (2004): 190–212.

2. Eleanor Rosch et al., “Basic Objects in Natural Categories,” *Cognitive Psychology* 8 (1976): 382.

3. There is so much new research on the neurology of everyday life that I don’t even know where to begin. What is fascinating about neurotransmitters is that it is at this level where the boundaries between “nature” and “nurture” begin to dissolve, because our brains (“nature”) get rewired all the time in response to external events (“nurture”)—what could be cooler for a sociologist? You might want to take a look at Candace B. Pert, *Molecules of Emotion: Why You Feel the Way You Feel* (New York: Scribner, 1997).

4. I really believe that at some level all research is autobiographical, even though the connections may be indirect. I have a friend, for example, who studies bioluminescence, but I know that at heart he’s still an eight-year-old boy fascinated by fireflies. What I need to know in this step is why other people *besides yourself* should and will care about your research problem.

5. William K. (“Sandy”) Muir, in his smart and beautifully written book about how a community did and didn’t respond to the school prayer decisions of the early 1960s, says that the difference between political scientists and journalists is that journalists tell what happened, while political scientists tell what didn’t happen. This is just an elegant way of saying that journalists look at the surface, while social scientists look at the (theoretical) depths. William K. Muir, *Prayer in the Public Schools: Law and Attitude Change* (Chicago: University of Chicago Press, 1967).

6. Susan Moller Okin, *Women in Western Political Thought* (Princeton, N.J.: Princeton University Press, 1979).

7. I argue now that much the same thing has happened to marriage. See Luker, *When Sex Goes to School*.

8. *Roe v. Wade*, 410 U.S. 113 (1973).

9. And sometimes even with historical comparative data you still need to get in. Someone owns key documents, or can give you permission to use a certain archive, or can set impossible hurdles in your way. (I once heard of a scholar doing work in archives where the archivist demanded releases from all the people whose letters were being cited, or if they were dead, from their heirs.)

10. Burawoy, “Extended Case Method,” p. 22.

11. Tom W. Smith, “Developing Nonresponse Standards,” presented at the National Opinion Research Center, University of Chicago International Conference on Survey Nonresponse, 1999.

12. In fact, the California Supreme Court has said that we may not misrepresent ourselves to those whom we research. We can dissimulate, or shade our motivations, but we cannot actively misrepresent who we are. Thus volunteering, temping, or working somewhere where you are also actively gathering research data is sailing pretty close to the wind for my taste. It’s not *active* misrepresentation, but it certainly is misrepresentation by omission. See *Taus v. Loftus*, California Supreme Court, 40 Cal. 4th 683, 2007.

13. Mark Granovetter, “The Strength of Weak Ties: A Network Theory Revisited,” *Sociological Theory* 1 (1983): 201–233.

14. Claude Lévi-Strauss was a French anthropologist, much read by sociologists of my era. Lévi-Strauss pointed out the theme of reciprocity, borrowed from Marcel Mauss, another French anthropologist. The argument is that a deep principle of human life is that “fair’s fair”—that if you share your time and insights with me, I need to give you something back.

15. If you want to know, you should read my book, *Abortion and the Politics of Motherhood*. In the end, it’s not the “facts” but how people evaluate them, and I guess the same could be said to be true of most of the books I write.

8. *Field (and Other) Methods*

1. Janet Lever, “Sex Differences in the Complexity of Children’s Play and Games,” *American Sociological Review* 43, no. 4 (1978): 471–483. For a

more modern version, see Barrie Thorne, *Gender Play: Girls and Boys in School* (New Brunswick, N.J.: Rutgers University Press, 1993).

2. Unfortunately, many practitioners use “ethnography” and “participant observation” or “P/O” interchangeably. But I want to make a clear distinction here as a guide to helping you think about what kinds of problems you are likely to face in the field, depending on how deeply immersed in it you are.

3. In this context, see Robert M. Emerson, Rachel I. Fretz, and Linda L. Shaw, *Writing Ethnographic Fieldnotes* (Chicago: University of Chicago Press, 1995), especially chapter 2.

4. I know I told you not to do this—that doing sociology while nominally doing something else is a form of covert research that can leave people feeling tricked. However, lots of sociologists do this all the time, and often they don’t share their thoughts on the “human subjects” dimensions of doing “undercover” research. (Although, to be fair, some do think about these issues a lot. For example, see Judith Rollins, *Between Women* [Philadelphia: Temple University Press, 1985].) One could come up with a very long list of other sociologists who have written about their “day job”; here are two: Jennifer L. Pierce, *Gender Trials: Emotional Lives in Contemporary Law Firms* (Berkeley: University of California Press, 1995); Fred Davis, “The Cabdriver and His Fare: Facets of a Fleeting Relationship,” *American Journal of Sociology* 65, no. 2 (1959): 158–165.

5. I have always thought that this is the genius of ethnomethodology, which asks people to formally break the rules of the game in order to notice how deeply rule-bound the situation itself is.

6. Lynne Haney, “Homeboys, Babies, Men in Suits: The State and the Reproduction of Male Dominance,” *American Sociological Review* 61, no. 5 (1996): 759–778.

7. Paul Willis, in his classic book *Learning to Labor* (New York: Columbia University Press, 1981), argues that an “oppositional culture” among some young working-class males in England permitted them to maintain their self-images and to oppose the dominant culture of the school, although this survival strategy in the end disadvantaged them, closing them off from whatever limited mobility opportunities the school offered them.

8. Elijah Anderson, *A Place on the Corner* (Chicago: University of Chicago Press, 2003 [1978]), p. 14.

9. Interestingly, the teachers read these two sets of signals quite differently, although they do get the bottom line, that “las chicas” do not hold them in high respect. The teachers take for granted that “las chicas” are more sexually active than the college prep girls, although Bettie tells us that in fact there are few differences between the two groups of girls. Julie Bettie, *Women Without Class: Girls, Race, and Identity* (Berkeley: University of California Press, 2003).

10. Howard S. Becker, “Problems of Inference and Proof in Participant Observation,” *American Sociological Review* 23, no. 6 (1958): 652–666. This is another one of the “best buys” for social scientists, and you should make it a permanent part of your collection.

11. *Ibid.*, pp. 656–657. Strictly speaking, Becker asks you to assess in the second step how *likely* the events are, but given the larger argument of his article, I think he might accept assessing the theoretical centrality of an event rather than its statistical frequency. See p. 656 for more details.

12. Because the state has such an interest in children and their schooling, it is also relatively easy to specify the ways in which children in public schools differ from the entire set of children in the age range, since data about how many kids are in private schools, or religious schools, or are being home-schooled, can usually be obtained with some effort. On the other hand, schools can drive researchers crazy when it comes to the question of *entrée*.

13. Renée R. Anspach, *Deciding Who Lives: Fateful Choices in the Intensive-Care Nursery* (Berkeley: University of California Press, 1993). For another view, see Carol Anne Heimer and Lisa R. Staffen, *For the Sake of the Children: The Social Organization of Responsibility in the Hospital and the Home, Morality and Society* (Chicago: University of Chicago Press, 1998).

14. You might, if you wanted to be picky, argue that Anspach’s book is not specifically about theory building, but I would argue that it is. Before Anspach’s book was published, parents and doctors alike assumed that decisions made in the neonatal care unit were made strictly on medical grounds. Anspach showed that, on the contrary, the way medical “facts” were constructed and then used to make decisions was a deeply social process.

15. I mentioned this valuable resource earlier, but let me remind you again of another one of the salsa-dancing social scientist's "best buys": Robert Emerson et al., *Writing Ethnographic Field Notes* (Chicago: University of Chicago Press, 1985). The authors tell you all these kinds of secrets, normally only passed on from professor to apprentice.

16. As the name suggests, "femocrats" are Australian bureaucrats who sought to change the very structure of the state apparatus. See Hester Eisenstein, *Inside Agitators: Australian Femocrats and the State*, Women in the Political Economy (Philadelphia: Temple University Press, 1996).

17. Survey researchers often use the term "interview" for an interviewer-administered questionnaire that uses fixed questions and close-ended responses. ("How would you rate the job President Bush is doing—very good, good, not very good, not at all good, or don't you have an opinion on this?") In this context, however, I will use the term to denote what is sometimes known as the "long" or "unstructured" interview. Both terms are, of course, misnomers, as many interviews are not long, and they are always structured, although the structure may be a loose one.

18. I recently heard Terry Gross ("Fresh Air") interview the director Werner Herzog, who had just finished a documentary on a young man who had spent many summers living with grizzly bears, becoming their advocate and eventually losing his life to them. Gross asked Herzog whether it was a switch to make a documentary since he had traditionally made fictional movies. Herzog said something very wise—that it's important not to confuse fact with truth. The same is true in interviewing. We don't look so much for facts in interviewing as for social truth.

19. Ann Swidler, "Culture in Action: Symbols and Strategies," *American Sociological Review* 51, no. 2 (1986): 273–286; Elisabeth S. Clemens, "Organizational Repertoires and Institutional Change: Women's Groups and the Transformation of U.S. Politics, 1890–1920," *American Journal of Sociology* 98, no. 4 (1993): 755–798.

20. For an overview see Robert D. Benford and David A. Snow, "Framing Processes and Social Movements: An Overview and Assessment," *Annual Review of Sociology* 26 (2000): 611–639.

21. I learned this technique in terms of data analysis from a great field-worker and colleague of mine at UC San Diego, Jacqueline Wiseman. She

used this technique to analyze data (more about that later), but I backed it up in the life history of a project and use it for generating data. For some of Wiseman's work, see Jacqueline P. Wiseman, *Stations of the Lost: The Treatment of Skid Row Alcoholics* (Englewood Cliffs, N.J.: Prentice-Hall, 1970).

22. This particular technique, like many of the others in this book, was also taught to me by Jacqueline Wiseman when I was a young assistant professor and she was my senior colleague. She is one of the most skilled fieldworkers I know of, and it would profit almost any working sociologist to go and take a look at her award-winning book, *Stations of the Lost*.

23. Alfred Hitchcock made a chilling 1951 movie of this same name, so memorable that you often hear people refer to "strangers on a train" when they really mean *Strangers on a Train*, the great film noir by Hitchcock. In it, two men discuss how they have family members in their lives they would like to be rid of (in one case a father, in the other a wife), and they muse about how if each of them "took care of" the other's problem, each would be in the clear since they would have ironclad alibis. One man thinks the other is joking, but imagine his surprise when . . .

24. See Barrie Thorne, "You Still Takin' Notes'? Fieldwork and Problems of Informed Consent," *Social Problems* 27 (1980): 284–297.

25. For a clear and comprehensive discussion of this, see Robert M. Groves, *Survey Methodology*, Wiley Series in Survey Methodology (Hoboken, N.J.: J. Wiley, 2004), pp. 226–236.

26. Frank L. Luntz, "Focus Group Research in American Politics," *The Polling Report* 10, no. 10 (1994): 7.

27. My lowercase "m" in marxist is meant to signal that this was an intellectual, not necessarily a political, identification.

28. Stanley B. Greenberg, *Middle Class Dreams: The Politics and Power of the New American Majority* (New York: Times Books, 1995), reporting on focus groups he ran in 1985 and 1989; Edward G. Carmines and James A. Stimson, *Issue Evolution: Race and the Transformation of American Politics* (Princeton, N.J.: Princeton University Press, 1989).

29. You might wonder, as I have, whether a particularly outspoken person or persons can create "slots" (in this case, categories that others resonate with) that aren't really slots. In other words, is it possible that one or two

people will create a vibrant discussion about something that the other people in the group don't really care about, but will go along with just to be polite? The short answer is yes. But just as with our interviews, unless our hypothetical X is something that people really *do* care about, the odds are slim that the vast majority of your focus groups will have outspoken people who care about X and will seduce the other people into agreeing with them. And if they do (that is, if lots of your groups have people who talk other people into caring about X), then that is social data as well.

30. I said this earlier in the chapter, but it bears repeating: of course participants know that you are observing them. No covert research from us!

31. My comment that content analysis has fallen somewhat by the wayside outside of the linguistic analysis people was based on a not-very-systematic count of articles that included the term “content analysis” in JStor over the last thirty years. Much to my surprise, political scientists are still using content analysis, but sociologists are not.

32. Deanna L. Pagnini and S. Philip Morgan, “Racial Differences in Marriage and Childbearing: Oral History Evidence from the South in the Early Twentieth Century,” *American Journal of Sociology* 101 (1996): 1694–1718. I lay out the evidence for convergence in Kristin Luker, *Dubious Conceptions: The Politics of Teenage Pregnancy* (Cambridge, Mass.: Harvard University Press, 1996).

9. Historical-Comparative Methods

1. Wacquant actually made this comment about ethnography, but I am adapting it to qualitative methods more generally to make my point.

2. *Tsuris* is an all-purpose Yiddish word meaning sorrow or trouble or stress. Leo Rosten, the great expert on Yiddish, says the word means “troubles, woe, worries, suffering” and notes that it is the plural of *tsorah* or *tsurah*, “but trouble is rarely singular.” Leo Rosten, *The Joys of Yiddish* (New York: Pocket Books, 1968), p. 415.

3. Justice Potter Stewart (1915–1985) is famous for his concurring opinion in a case about pornography, where he noted that although he could not formulate a definition of pornography, “I know it when I see it.” (The case was *Jacobellis v. Ohio*, 378 U.S. 184 [1964].)

4. Barrington Moore, *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston: Beacon Press, 1993); Theda Skocpol, *States and Social Revolutions: A Comparative Analysis of France, Russia, and China* (Cambridge: Cambridge University Press, 1979).

5. We *all* incline to theory—or should. To be precise, what I mean here is that historical and comparative types usually—but not always—incline to theory *testing* models, rather than those more common in salsa-dancing social science, namely theory *generating*. There is something about historical and comparative methods that drives people to show that existing theories of something are wrong or inadequate—another reason these methods straddle canonical methods and salsa-dancing ones.

6. Karl Popper, a very influential philosopher of science, is the one who singled out the notion of “falsifiability” as the key to inquiry. His point was that you could see hundreds of white swans (thousands even) without being able to conclude that “all swans are white.” But just one black swan of course proves that all swans are not white. Your particular case is, in this sense, a black swan, in that it shows—if all goes well—that certain things thought to be causes of revolutions are necessary but not sufficient for the production of revolutions.

10. Data Reduction and Analysis

1. Furthermore, the fabulous people who run the CAQDAS site at the University of Surrey—Ann Lewins and Christina Silver—have a free, downloadable, and regularly updated article on how to choose a CAQDAS package. They have also published an extremely useful book: Ann Lewins and Christina Silver, *Using Software in Qualitative Research: A Step-by-Step Guide* (Los Angeles: Sage Publications, 2007). Yes, one more “best buy.”

2. Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago: University of Chicago Press, 1994).

3. www.u.arizona.edu/~cragin/ragin.htm.

4. “Mensuration” is the study of measuring things. I am using it here instead of the more straightforward noun “measurement” to indicate that

what we have here is a social practice, not necessarily an “objective” stock taking of something.

5. For the technically minded, what I mean to say is that B should be a function of A.

6. Barney G. Glaser and Anselm L. Strauss, *The Discovery of Grounded Theory: Strategies for Qualitative Research* (Chicago: Aldine, 1967).

7. For example, Barney G. Glaser and Anselm L. Strauss, *Awareness of Dying* (Chicago: Aldine, 1965); Barney G. Glaser and Anselm L. Strauss, *Status Passage* (Chicago: Aldine Atherton, 1971); Barney G. Glaser and Anselm L. Strauss, *Time for Dying* (Chicago: Aldine, 1968); Anselm L. Strauss and Barney G. Glaser, *Anguish: A Case History of a Dying Trajectory* (Mill Valley, Calif.: Sociology Press, 1970).

8. Charles C. Ragin, *The Comparative Method: Moving Beyond Qualitative and Quantitative Strategies* (Berkeley: University of California Press, 1987); Charles C. Ragin, *Fuzzy-Set Social Science* (Chicago: University of Chicago Press, 2000).

9. The software and manual can be downloaded from www.nwu.edu/sociology/tools/qca/qca.html.

10. Ragin, *The Comparative Method*.

11. The term “moral shock” is from James Jasper, *The Art of Moral Protest: Culture, Biography, and Creativity in Social Movements* (Chicago: University of Chicago Press, 1997).

12. I actually did a fast move there, and you would have to read the entire book (*When Sex Goes to School*) to get the whole set of steps in between. Essentially I argue that “social conservatives” are greatly concerned with sex, and that a new cleavage has emerged in American society having to do with sex. In terms of the way we have been talking about structuring social inquiry in this book, sexual conservatives are social conservatives, that is, in Venn diagram terms, the set of sexual conservatives is a subset of social conservatives. It’s not clear to me from my study whether sexual conservatives are social conservatives plain and simple; that will require further research.

13. Social studies of science people will probably quibble with this metaphor, arguing that even physical objects are shaped and chosen by the social context in which they find themselves. No matter, the point is clear:

our methods are even more deeply social than a physical object can ever be, in my opinion.

11. *Living Your Life as a Salsa-Dancing Social Scientist*

1. Anne Lamott, *Bird by Bird: Some Instructions on Writing and Life* (New York: Anchor Books, 1995).

2. Here's the best account (and the best remedy) I know of, from Anne Lamott's *Bird by Bird*, p. 16: "The first useful concept is the idea of short assignments. Often when you sit down to write, what you have in mind is an autobiographical novel about your childhood, or a play about the immigrant experience, or a history of—oh, say—say women. But this is like trying to scale a glacier. It's hard to get your footing, and your fingertips get all red and frozen and torn up. Then your mental illnesses arrive at your desk like your sickest, most secretive relatives. And they pull up chairs in a semi-circle around the computer, and they try to be quiet but you know they are there with their weird, coppery breath, leering at you behind your back." I hope this passage convinces you, if I haven't done so elsewhere in this book, that this is a "best buy" for writers of all descriptions.

3. Jane Anne Staw, *Unstuck: A Supportive and Practical Guide to Working through Writer's Block*, 1st ed. (New York: St. Martin's Press, 2003).

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Author's Note

You have surely noticed that I like stories quite a lot. This book is filled with anecdotes and morality tales about social scientists, salsa-dancing and otherwise. I have consciously changed some of the names and details to protect the innocent (or guilty, as the case may be). Moreover, I'm the kind of person who always remembers the emotional punchline of a joke, but not necessarily the details. Finally, some of the details might have gotten in the way of a point I wanted to make—reality is so messy that way—so I changed them.

What all this adds up to is that if you think a story is about someone you know, or a situation you have heard about . . . it most assuredly isn't.

Acknowledgments

I wish that the writing of acknowledgments were not such an unfor-
giving art form. For the reader, this last, obligatory section often
reads like a cross between the phone book of a mid-size and perhaps
boring city and a caricature of a bad Academy Awards speech (“I’d
like to thank my agent, and the director, and the producer, and the
writers . . .”).

For the writer, though, this section is born of sheer gratitude. Ev-
ery book, most especially a scholarly one, is the product of the labor
and generosity of so many people that to fully acknowledge them all
would read like a phone book, only of Manhattan. Or Tokyo. Or
Beijing. And to let you, the reader, really know how indebted the
writer is to friends and colleagues, this very long list (the one the size
of the Manhattan phone book) would also need to have narratives—
accounts of all the wonderful things, ideas, suggestions, comments
each person brought to the book.

The most terrifying thing is that while you try to thank all of those
people who have made the book possible, you know in your heart of
hearts that you are bound to forget someone, the more so when a
book has been brewing for a while, as this one has.

So let me just thank a few people who went above and beyond the call of duty in so many different ways. First come those who were kind enough to read and comment on the manuscript. The usual caveats apply here with a vengeance: these committed souls did their best to show me the errors of my ways, and any that remain are entirely due to my own pig-headedness. Steven Brint, Steven Epstein, Lynne Haney, Scott Harrison, Mike Hout, David Kirp, Jerome Karabel, Rebecca Klatch, Michèle Lamont, Kelly Luker, Chandra Mukerji, David Nasatir, Allison Pugh, Charles Ragin, and Loïc Wacquant put more TLC into reading this book and giving me good feedback than any author has a right to expect.

Elsewhere in this book I urge people to honor and cultivate librarians, and here I have the pleasure of following my own advice. Pat Maughan is not only a librarian's librarian, she is a dedicated and fearless teacher as well, and she is only the most visible part of a very large and ridiculously efficient and generous group of professionals whose work makes academia a much better place than it would be otherwise.

Margo Rodriguez is a generous friend and colleague who has rescued me from more late-night disasters and computer foul-ups than one can imagine, and nothing I can ever say will begin to convey how indebted I am.

My agent, Victoria Pryor, had faith in this book at its earliest stages, and served as editor, confidante, and cheerleader. She knew from the beginning where my heart wanted to go, and she gave me permission to go there.

Speaking of editors, I hope I get lots more chances to work with Elizabeth Knoll, because she reads deeply and sees the forest, not to mention the woodland ecology, at the same time as she appreciates both trees and bark with a keen and appreciative eye.

For a copy editor I got Mary Ellen Geer, a poet who cherishes words as they are meant to be cherished. Elizabeth Knoll told me to revere and enjoy Ms. Geer, because she is a vanishing breed in these

days when books have become mere “units” in corporate accounting. So I deem myself very lucky to have worked with one (well, two, if you count Elizabeth) of the best.

My friend and mentor Jane Anne Staw has given me the gift of her thoughts on writing, and has written a book (*Unstuck*) that every writer should own. Every writer should have a coach and mentor like Jane Anne.

My families—Jerome, Alexander, and Sonya Karabel and Jim, Laurie, and Kelly Luker—remind me why families matter so much, and what a gift of joy good ones are. Edit and Yuri Tulchinsky, too, are also partners in this adventure known as family life, and I am indebted to them in ways they will never know.

And of course the dogs—Misha and Mo—lightened my heart and, as the poet Mark Doty says, “unsnared time’s warp.” I thank you all, from the bottom of my heart.

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