CHAPTER 23

CONCLUSION: "RESEARCH IS A MESSY BUSINESS" – AN ARCHEOLOGY OF THE CRAFT OF SOCIOLEGAL RESEARCH

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A common experience among those of us who talk to lawyers about their work is to hear the lawyers recite their war stories. In contrast, when you talk to an experienced social scientist about his or her work, you tend to get something of a sanitized version of the research process much like the “methods” section of a research article. However, if you start to dig, as the creators of this volume have done, you often start to see a much messier business, as Keith Hawkins ("Research is a messy business") and Stewart Macaulay ("[W]hen people write about research methods – it’s all so neat and pretty. The messiness of much of it just doesn’t come through in the books") so honestly tells us. The more vigorously you dig, the messier things often get. If, in addition to the wonderful interviews that compose this book, the editors had looked at early documents from the research projects – memos, research designs, grant applications, and the like – they would have often discovered that many of the authors started from very different points than the authors themselves now recall (I by no means exclude myself from this phenomenon, although I have not gone back to my own early research materials).¹

The ability to produce high-quality and influential social science research products is a craft that is acquired through a combination of

¹This is what I discovered in the course of my own archeology of research projects. See “Interpretation and Validity Assessment in Qualitative Research: The Case of H. W. Perry’s Deciding to Decide,” Law and Social Inquiry, 19:687–724 (1994); and “‘Data, Data, Data, Drowning in Data’: Crafting The Hollow Core,” Law and Social Inquiry, 21:761–804 (1996).
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instruction from those who have previously mastered the craft and one’s own practice doing that research.\textsuperscript{2} Dealing with the unexpected and being able to cope with messiness is a common feature of “craftwork.”\textsuperscript{3} The nature of the messiness depends on the specific craft involved. The interviews in this book show how skilled sociolegal scholars have confronted the dilemmas that have arisen in the course of their work. In this concluding essay, I highlight several themes that I found cutting across the interviews: problems in figuring out the right question to be asking and how this relates to designing research, the joys and complexities of original data collection, the challenge of analyzing data – whether qualitative or quantitative, the centrality of writing to the analysis process, and the challenges of working collaboratively. I also discuss two current and possibly growing problems for Law and Society researchers: problems in conducting surveys and the “problems” of increasing demands from Institutional Review Boards charged with protecting human subjects.

TYPE III ERROR

Anyone who has taken a course in statistics has heard about Type I and Type II errors. The former refers to “rejecting a null hypothesis that is in fact true” and the latter refers to “failing to reject a null hypothesis that is in fact false.” More generally, one can describe these as “thinking you see something that is in fact not there” (a false positive) and “failing to see something that is in fact there” (a false negative). In their brief monograph, Reliability and Validity in Qualitative Research, Jerome Kirk and Marc Miller describe a third error (“Type III error”): asking the wrong question.\textsuperscript{4} Although they describe this in the context of qualitative research, it is equally true of quantitative research.

I can identify a number of my own experiences where I discovered early in the data-collection or data-analysis process that I had asked the wrong question. For example, in the early 1980s I went to Toronto to spend a month interviewing large-firm lawyers and officers of the lawyers’ corporate clients about how the availability of fee reviews by a court official affected concerns about rising legal costs. Before I had

\textsuperscript{3} Ibid., p. 328.
completed even a half-dozen interviews, I realized that this was an irrelevant question, and the interesting question was that of understanding the broader structure of lawyer–client relations at this level.5

In the interviews you see many indications of this kind of evolution of research questions:

- Michael McCann (Chapter 16): “We tell ourselves that we have a research question and a set of hypotheses but in the process of developing [the] book, the questions were changing all the time.”
- David Engel (Chapter 8): “The “Oven Bird’s Song” article was not one that I had anticipated as a product of the research when starting it.”
- Sally Merry (Chapter 12): “[T]his was a book where the hypotheses I began with were all collapsing around me.”
- Doreen McBarnet (Chapter 14): “I think the breakthroughs come from coming up with the things that you wouldn’t have thought about . . . until you actually start doing the research.”
- William Felstiner (Chapter 17): “[I]f you could dig up the application we made to NSF I doubt if you would see very much other than, ‘here is the paper we wrote and now we want to try to see if these dispute transformations actually happen as we predict’.”6

All of the projects just referenced were primarily qualitative in nature. One might ask whether things are different for quantitative projects. If one looks at the interviews with Hazel Genn, with Tom Tyler, and with John Heinz and Edward Laumann, you see very different images of how research evolves. Genn captures this well when she observes that “[t]he difference between doing a quantitative survey and doing qualitative work is that with quantitative work you have to get it right before you start your fieldwork. Once you have got your questionnaire fixed you

6 Felstiner is referring to the well-known “Naming, Claiming, and Blaming” paper that he and Sarat coauthored with Richard Abel. W. L. F. Felstiner, R. L. Abel, and A. Sarat, “The Emergence and Transformation of Disputes: Naming, Blaming, Claiming,” Law & Society Review, 15:631–54 (1980–1981). Another of my archaeology of research endeavors was a study of Sarat and Felstiner’s divorce lawyer project; I have looked at the original grant application, and Felstiner is correct in stating that the original project proposal focused on dispute transformation, a topic that you cannot even find in the index of their book, Divorce Lawyers and Their Clients.
are stuck with it. With every single survey I have ever done, there have always been questions that I wished I had asked” (Chapter 20). She goes on to describe a second problem that one frequently encounters in survey research: including questions that you later discover are not of much use; in the case of the study she describes, this involved asking detailed questions about court experiences only to discover that too few respondents had such experience to allow for any meaningful analysis.7

In a sense the difference between qualitative and quantitative research is reflected in the ability to obtain data. If one is doing a survey or a series of social psychological experiments, the data you have in hand constrain your analysis, and the data-collection process often limits your ability to make adjustments. Moreover, in a lot of survey work, you do not know what you missed until you have finished collecting the data. For example, John Heinz and Edward Laumann describe how client type turned out to be a central dimension, but that their questions about specialization had not taken that into account; this limited the analysis they were able to do with the 1975 data, which they sought to correct when the study was replicated in 1995. In the context of experimental work, it may be the case that individual experiments are limited, but through a series of experiments you can hone in on something that is very different than what you originally set out to look at. The classic example of this is the working condition experiments done in the 1920s at a Western Electric’s Hawthorne Works in Cicero, Illinois; many of the apparent effects of modifying working conditions reflected not the impact of the conditions but the impact of the workers being singled out in a positive way – what is now known as the Hawthorne effect.8

Even in survey-based studies, it is not uncommon to discover that the main effects one is looking for are not the central story in the data. Early studies of voter behavior in American presidential elections had an image of how the voter made his or her choices at election time; what the researchers found was a very different kind of

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7 Genn’s experience mirrors my own experience with the data collected for the Civil Litigation Research Project (CLRP). We designed quite elaborate questionnaires to get at lawyer and litigant experiences in litigation. We discovered that for the vast majority of cases, we could have used a much simpler survey instrument.

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process. The researchers designed subsequent election studies to try to get at the kinds of things the first studies uncovered. The differences between qualitative and quantitative research in this regard do not reflect that quantitative research is designed more carefully than qualitative research. Rather, it reflects the fact that, in quantitative research, particularly survey-based quantitative research, analysis does not start until data collection is completed. In contrast, in qualitative research, whether it is based on observation or interviews, much or all of the data are usually collected directly by the primary researcher(s), and the initial analysis of those data starts simultaneously with the data collection.

Sometimes the question changes not because the researcher posed the wrong question to start with but because of changes that occurred during the research or data-collection process. When I taught graduate research methods I would routinely recount the following mythical tale.

Some years ago, a political science Ph.D. student arrived in a foreign country to undertake her field research on some aspect of how the country’s democratic system was operating. The day after she arrived there was a military coup, and her research plan was out the window. She sent a cable back to her advisor describing what had happened and pleading for advice about what to do. Her advisor sent back a two-word response: “Take notes.”

A college classmate of mine went directly to graduate school in political science. In 1971 or 1972 he began work on his doctoral dissertation focusing on the presidency, and by early 1973 he was largely finished. However, his work was overcome by events – the Watergate burglary by Republican operatives, the subsequent investigations, Supreme Court cases, and President Richard Nixon’s resignation in 1974 – and in the


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end he abandoned both his dissertation and graduate school. His topic: executive privilege.

Of course change need not be this dramatic, and it may clearly create an opportunity (see interview with Robert Kagan, Chapter 3) rather than undermine a planned or ongoing research project. In 2000 I had a survey in the field at the time of the U.S. Supreme Court’s decision effectively making George Bush the President-Elect. While my original research had nothing to do with the public’s view of the Supreme Court, I knew that one of the stock questions on the survey was a measure of confidence in the Court. I was able to swap out one of my original questions and substitute a question about agreement or disagreement with the decision.12

The fact that researchers often encounter Type III errors raises the question of whether one should engage in the work of preparing a research design. Yves Dezalay describes his project with Bryant Garth as not having started with a design or plan of any clear sort (“There was no plan”; Chapter 18). Similarly, Doreen McBarnet comments, “I don’t know that I had a plan as such. You have to make it up as you go along because you have to take the opportunities that you either create or that come your way” (Chapter 14). Although some readers might interpret this as Dezalay and McBarnet having begun their research with nothing more than an idea that it might be interesting to investigate their broad subjects, there clearly was some thought about what kinds of questions to ask and how to go about doing the research, with a more detailed plan evolving in the early stages of the research. Undoubtedly, some very good research is done without an initial plan, but having a plan provides a starting point to enter the field and it is easier to rethink a research project if you have something to rethink from. Michael McCann says this extremely well: “It is important to develop a good formal research design, while at the same time recognizing that, once you go out into the field, you have to be very willing to adjust, reconstruct, adapt, or maybe even throw it out. But you need to come in with an organisational framework of understanding and analysis and expectations” (Chapter 16).

When I counsel students or colleagues who are designing an empirical research project, whether the project is qualitative or quantitative in nature, one of the first things I tell them is to ask themselves how the

material they plan to collect will help them answer the question they are posing. It is very easy to ask questions or to collect some data, but frequently we fail to explicitly ask ourselves what we might find in the responses or in the data that will help us answer our own question. Hazel Genn captures this well from the quantitative perspective when she talks about “frontloading” your thinking (Chapter 20), by which she means thinking very explicitly and systematically about the analysis you will do as you design the data collection.

THE MESSIEST MESS IN THE RESEARCH PROCESS: COLLECTING ORIGINAL DATA

One of the features of all of the projects described in the interviews is that they involve original data collection. Although several of the interviewees mention both the time-intensive nature of their data collection and the large quantity of materials that they found themselves with, what is less clear is how messy data often (usually?) turn out to be. Imagine that you want to study something about trials in federal court. You could turn to the statistical reports published by the Administrative Office (AO) of the U.S. Courts and extract information from the Reports’ well-digested tables. Or, you could obtain from the Interuniversity Consortium for Political and Social Research (ICPSR) the case-level data reported to the AO and deposited with the ICPSR (these data form the basis for the published tables); you could then process these data to create whatever summaries you need. Or, if you have adequate resources, you could access the raw case files through the federal court’s Public Access to Court Electronic Records (PACER) system; you would then extract and code the information you want from the raw case records.13 Clearly, the work involved increases as you move from the reports through the archived case-level data to the raw case file data. However, you will also discover that things are a lot messier, and you have to make a lot of decisions and judgment calls as you move away from the predigested information in the reports, or away from the preprocessed and precleaned data stored at ICPSR.

13 PACER provides access to materials in the federal courts’ Case Management/Electronic Case Filing (CM/ECF) system. Before Pacer, collecting data from the case records required going to individual courthouses, something that is still necessary for many state courts (and probably most courts outside the United States, assuming that you can even get access to such records in other countries).
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Consider a comparison to a decision to have fried chicken for dinner. One option is to go to your nearest fried chicken emporium (e.g., Kentucky Fried) or a neighborhood grocery store that sells prepared foods, buy the ready-to-eat chicken, go home, and enjoy your dinner. That’s comparable to going to the published reports and pulling together some information. A second option is to go to the grocery store’s meat department and purchase raw chicken, take that chicken home, cut it up if you saved money by buying a whole chicken, fry it, and then sit down to dinner; anyone who has ever fried chicken knows that this is not only more work than buying ready-to-eat chicken but also a lot messier (grease splatters, grease to dispose of, greasy pots to wash). This is comparable to getting the data from ICPSR in a relatively ready-to-use form. A third possibility is to go out to a farm and buy a live chicken (or, perhaps there is a market not far away that sells live poultry). You take the chicken home, chop off its head, watch it dance around your yard for a while (imagine your neighbors observing this scene), pluck it, gut it, cut it up, fry it, and eat it – much, much messier than going to the store and buying a chicken that is ready to cook.

The messiness of data shows up even in what would seem to be a straightforward context. For example, for a current research project I am compiling data on state supreme court elections back to 1946. One thing I want to look at is winning margins. How do I treat elections where candidates do not run for specific seats but the top candidates win (i.e., if there are three seats, the top three vote-getters win). How do I compute the margin of victory for each of the three seats? Or, in a partisan election where there are two seats up for election and there are two candidates from one party but only one from the other, should I designate one of the candidates as unopposed? Or, what should I do when the official county-by-county totals do not sum to the state-level total reported? Or, how should I handle counties where the official reported vote shows that no one voted, or that one candidate got all the votes, or that one candidate’s name was left off the ballot? 15

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14 As someone who has done this many times, I will also note that it is more dangerous. You risk burns from hot grease and cuts from the slip of the knife while you are cutting the chicken if you bought it whole.

15 Another element of “messiness” here is locating the data. I have been compiling county-level election returns for state supreme court general elections (partisan and nonpartisan) and primaries back to 1946. For some states, all the information I need is in published reports or online. For other states, the materials have to be requested from a state archive. For still others, the only way to get the information appears to
Another issue regarding data collection is *how* you collect the data. Of course the biggest divide here is between qualitative and quantitative data. I have found that usually you are better off combining qualitative and quantitative data when possible because you learn different things from different types of data. An observation from Sally Merry (Chapter 12) reflects a somewhat traditional view on this: “It seemed clear to me from that study that the best approach was to do an ethnographic observation first so you know what the categories are and you know what people think about. Then you do the survey so that your survey includes their categories, not yours”; that is, use qualitative work in a kind of exploratory way and then use quantitative data for confirmation or generalization. Hazel Genn (Chapter 20) describes a different way of thinking about combining qualitative and quantitative work, the “quant sandwich,” which she describes as doing “qualitative work first – discussion groups – then the survey, and then qualitative interviews.” This is closer to how I see the combination of qualitative and quantitative work. In particular, I find that doing qualitative work after I have quantitative results in hand helps me understand the mechanisms underlying the quantitative patterns.

A separate issue arises in doing qualitative work, and that is the choice between data collected through interviews and data collected through observation. Several of the interviews touch on this point:

- Malcolm Feeley (Chapter 4): “Psychologists tell us all the time that our map of what we do is colored by the more problematic things that make it rather than the boring, ordinary routine. At any rate, as I hung around the courthouse, I became suspicious about the abstract things people would say. That’s why I liked to hang around the court and to ask people to comment on things they were doing or had just done – explain particular actions.”
- Stewart Macaulay (Chapter 2): “One of the problems of these interviews is that people like to entertain you. So what gets presented as the way things are is really the best story that they’ve got.”

be to have someone go to the archive. Interestingly, for one state I have not been able to find anyone who has information on the specific terms served by justices (information is available on start and end dates of service, but not when justices were subject to a potential election to retain their position).

16 Merry is not entirely convinced of this because she goes on to say, “But how valuable that survey data is, I’m not really persuaded on it.”
• Sally Merry (Chapter 12): “I believe that observation is the best way because then you can actually see what’s happening. If you rely on interviewing people about events and experiences you clearly have to get multiple perspectives on what happened and triangulate them as much as possible.”

Sally Merry makes another statement that captures some of this issue very nicely: “Fieldwork is a process where you become the research instrument” (Chapter 12). Of course many kinds of situations exist where observation is not possible (e.g., trying to reconstruct past events). Still, in understanding processes, the richness that you obtain from observation cannot be readily matched from semistructured interviews.17 Ways exist of getting better and worse information from interviews, and one of the best ways is to focus as much as possible on concrete situations the selection of which is in some way controlled by the interviewer. For example, instead of asking a respondent to describe a recent case, which will typically result in the respondent choosing something interesting and nontypical, you can ask the respondent to describe the most recent case defined by some characteristic (opened, concluded, etc.); this provides something akin to random selection.18

DATA ANALYSIS

Several of the interviews described the respondents’ process of analyzing qualitative materials. Often this involved extracting or copying quotes from interviews or sections from field notes, and engaging in some sorting process. Hazel Genn describes this kind of process well: “I went through the transcript and would mark it up in different colors for different themes. I even sometimes cut the transcripts up and put bits of quotes in piles on the floor” (Chapter 20). Doreen McBarnet describes a similar process (Chapter 14): “Going through the notes, annotating with numbers and categories, marking a quote as interesting for raising


18 While there are many books available on how to do interviews, the best I have found for the kind of interviewing I have done is James Spradley’s The Ethnographic Interview (New York: Holt, Rinehart and Winston, 1979).
some point in my mind” (see also Silbey and Ewick, Chapter 19).19 This kind of sorting and categorizing involves lots of judgment calls because language is often very ambiguous. One can develop “coding rules” or “sorting rules,” but inevitably there will be lots of things that do not fit. We see this often in some category that is reported as “other”; seldom do we get a good sense of what is in “other” or what might have been in “other” but a coder or sorter judged was “close enough” to go into a named category.

I have written elsewhere about the role of interpretation in the analysis of both quantitative and qualitative data.20 Simply put, that process is not well understood. It draws on a combination of the data themselves, side information possessed by the analyst, and the creativity of the analyst. One often hears something akin to, “What do the data say?” or “This is what the data tell me.” These kinds of questions and statements assume that data speak, and the analyst just has to listen to what the data say. Lawrence Friedman (Chapter 5) suggests that when he observes, “Somehow you look at the data, you see what you have, and somehow a plan forms in your mind about how to write it up and you just do it. There was a great political scientist who once said that you gather your data, you look at it and you say, ‘Speak to me!’ And somehow, it speaks to you and says, ‘This is how we should organize it.’ Somehow an organization suggests itself.”

Data do speak, but only when asked. What Friedman does not say is that having the data speak involves looking at the data, whether that involves reading qualitative materials or producing statistical summaries, through some set of expectations. Sometimes those expectations are confirmed in whole or in part, but often they are not confirmed. One does not look at a set of data with the mind in a tabula rasa mode. What the data say depends on what they are asked, and how well the

19 Central to this sorting process is grasping the content of the data. Keith Hawkins (Chapter 9) describes his process as follows: “The data analysis was a fairly banal thing. There was nothing romantic or adventurous or intriguing about it. It’s just very banal reading the data so that you are utterly familiar with it, so that the patterns and regularities become really very clear. I knew of no other way except a constant reading and re-reading of the stuff.” Although he does not describe this as a sorting process, it is a mental sorting process, and I suspect that in practice he did some physical sorting as well.

asker listens to the data. You can see this quite clearly in the Heinz and Laumann interview. John Heinz (Chapter 6) speaks of “a lot of staring at the numbers and saying ‘what are these numbers telling us?’” Yet, Heinz and Laumann went into their analysis with a specific interest in networks and interrelationships among lawyers, and the analyses they did (i.e., the “numbers” they generated in the analysis) focused heavily around their questions about these kinds of relationships.

Just as in human conversation one must be attentive not just to what is said, but also to what is not said and the nonverbal forms of communication that come along with the verbal. The analyst interrogating her data must be mindful of what is not to be found and for indications that might be analogous to nonverbal communication. Just as the ability to interpret conversation comes with experience, the ability to interpret the results of data analysis draws heavily on experience. We can teach particular techniques of analysis; we can teach the formal meaning of things like regression coefficients; and we can teach the need to look beyond the simple results. Still, drawing conclusions often requires thinking beyond the explicit materials in hand.

Analyzing data, whether they be qualitative or quantitative, is directed toward arriving at conclusions. The path from data to conclusions is often a difficult and tortuous one. Central to finding one’s way down that path is the process of writing and showing to your audience how you get from the data to the conclusions. The centrality of the writing process, and the view that writing is essentially analysis, comes up in several of the essays:

- Michael McCann (Chapter 16): “[Writing is] really about organizing, about trying to put it all together.”
- David Engel (Chapter 8): “The first draft would be almost unrecognizable now because I added layer after layer as I understood my own materials better and better. This little article took a long time to write, probably over two years.”
- Carol Greenhouse (Chapter 10): “[T]hat’s when the thesis began to emerge – very much in the midst of writing.”
- Sally Merry (Chapter 12): “When I wrote this book I had a lot of data from a lot of different programs. I see the problem as a kaleidoscope where you try different ways of foregrounding something

and backgrounder something else, deciding what’s your core problem and what’s the framework within which you think about that core problem. It’s a matter of experimenting to see which one works.”

Although it may sound specific to qualitative research, it is also very true of quantitative research. When I sit down and start writing a paper based on quantitative analysis, I will have produced the statistical results that I think I will need to report in the piece. Very frequently, perhaps even most of the time, as I write I discover questions or issues that take me back to the data for additional analyses. Sometimes these additional analyses lead the writing in directions I did not originally anticipate.

WORKING COLLABORATIVELY

Several of the studies covered by the interviews reflect very successful collaborations. Collaboration can be wonderful when it works! To use a clichéd idea, a good collaboration is a bit like a marriage, although perhaps we should replace “until death do us part” with “until publication do us part.” Hazel Genn (Chapter 20) provides one cautionary note: “One of the dangers of working in teams is that as you get toward the end of the project people may start to disappear because their contracts are coming to an end and they want to take up other jobs. My experience with almost every project that I’ve managed is that, by the end, the only person holding the baby at three o’clock in the morning was me.” Another problem, changing priorities or demands, is alluded to by William Felstiner (Chapter 17) when he mentions Austin Sarat’s decision to enter law school rather than spending the coming year in Oxford writing (for which they had obtained a second NSF grant). Priorities can and do change among collaborators; fortunately for Law and Society scholarship, the result of Austin Sarat’s change in priorities (and William Felstiner’s appointment as director of the American Bar Foundation) was a delay and not the demise of a project.

Crucial to successful collaboration are a balance of skills and an understanding about how those skills play out in the course of the project and the overall work involved in the project. One person may have stronger theoretical skills and the other may have experience and skills related to data collection and analysis; these points are briefly mentioned in the interviews with William Felstiner and Austin Sarat (Chapter 17) and with Bryant Garth and Yves Dezalay (Chapter 18).
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This can work if the balance across the project involves both sets of skills. The collaboration can break down if it turns out that the project is very heavily tied to one of the skills and less to the other. For example, in one very large project I was involved in, the demands of the data analysis phase were much greater than anyone anticipated, and the majority of the collaborators either lacked the requisite skills or found other things to do that were more interesting to them. The collaboration disintegrated, the individuals involved went their separate ways, and only one continued to work with the data that were collected. My own experience is that collaboration has worked best when, rather than taking on specialized tasks, all of the collaborators were able to work on the various aspects of the project. This is not to say that who took the lead on various tasks was not related to skills, but simply that no collaborator felt that he or she was left “holding the bag” at any one point.

THINKING ABOUT THE FUTURE

In reading these interviews two issues loomed out as issues for the future of Law and Society scholarship, at least in the immediate future: increasing problems with conducting surveys and institutional review boards (IRBs). Hazel Genn makes explicit reference to the problem of “survey fatigue” (Chapter 20): “I think nowadays it’s harder to keep response rates up because there is a kind of survey fatigue going on.” This fatigue arises from a variety of sources: the use of “fake” surveys that are in fact either sales calls or efforts to influence voters (“push polls”); the explosion of the customer satisfaction surveys that one seems to receive after just about any purchase of $100 or more; and the development of Internet surveys or automated telephone response systems (often used in the customer satisfaction surveys), which cost relatively little compared to traditional telephone surveys or mail surveys. Survey fatigue decreases response rates as does another issue: the increasing proportion of the population who have only cell phones.22

In the United States, where cell phone users typically pay for incoming

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calls, most survey organizations now offer to pay respondents contacted via a cell phone, but this is only one of many issues raised by contacting respondents via cell phone.\(^{23}\) These problems threaten the ability of legitimate researchers to use surveys, either of the general population or targeted individuals, as a data collection tool. In fact, the last survey-based study I tried to do was an utter failure. That study, which focused on a particular group of lawyers, sought to use email as a contact method and a web-based survey tool as the data collection method. Between the time the study was designed and the survey was fielded email spam went from a minor annoyance to a major problem requiring heavy filtering of incoming email, resulting in an abysmal response rate even though I sent the contact email out through an organization to which most of the lawyers belonged. I concluded that there was no way I could even begin to argue that the responses were representative, and I ended up abandoning the study even though substantial money had been invested in it.

General public opinion survey operations and market research firms are beginning to rely increasingly on Internet surveys, but they do this by forming panels of potential respondents who agree to participate in such surveys, often providing incentives of some sort. For example, the day before I wrote this paragraph, I received an email from Northwest Airlines inviting me to become part of a consumer panel; Northwest offered me frequent flyer miles as an incentive to participate (I deleted the email). The enduring concern about Internet-based surveys is representativeness.\(^{24}\) Moreover, the panel model is not a solution if the population of interest is a very specific group that is a relatively small portion of the population or is a group with some very specific


experience. Still, it does suggest that finding ways of motivating participation in surveys will be a key to solving the problem. Motivation can be created through compensation or through framing of the purpose of a survey to make it look appealing to the target population, which might involve sponsorship.

This does not get around the problem of establishing initial contact. I suspect that for targeted surveys traditional mail may prove to be better than either telephone or Internet methods. The RAND Corporation’s Institute for Civil Justice did a mail survey of members of the American Association for Justice (AAJ – formerly ATLA, the Association of Trial Lawyers of America) and obtained a reasonable response rate (about twenty-five percent); the mailing of the survey included an endorsement letter from the then president of the AAJ. The survey process involved two mailings of the complete package plus telephone follow-ups (up to five attempts to make contact) to nonrespondents for whom telephone numbers could be located. What this experience suggests is that researchers planning targeted surveys need to factor in significant costs in terms of both time and money if they want to obtain a useful level of response.

The second issue concerns IRBs and the increasing intrusiveness of the process originally intended to protect human subjects from the risk of substantial harm. It is virtually unimaginable today that Robert Kagan could have done his study of the price control bureaucracy in the

25 However, one study of an Internet survey found that adding a mail contact did not increase the response rate (Porter and Whitcomb, 2007).

26 It is worth noting that the researchers gave the potential respondents the option of completing the questionnaire online using a web-based survey; the web alternative was utilized by only 20 of the 965 respondents (Steven Garber, Michael Greenberg, Hilary Rhodes, Xiaohui Zhua, and John Adams, “Do Non-Economic Damages Caps and Attorney Fee Limits Reduce Access to Justice for Victims of Medical Negligence?” Working Paper, Rand Institute for Civil Justice (2008), 6n14).

early 1970s. He seized upon an opportunity that came up unexpectedly and that required immediate action. As I write this (mid-September 2008), the financial crisis is in full swing. I could imagine a variety of research projects that one might want to launch immediately if access were possible and the researcher’s time were available. However, under current regulations it would take three to six months to get the requisite approvals from an IRB.

Carol Greenhouse describes how it was not until she got into the field that her focus became the church community; as discussed earlier, it is very common to discover that, once one has started a project, the focus changes. Under current regulations, Greenhouse would have been expected to go back to her IRB and have the revised project approved. Under these circumstances, a field researcher is confronted with the choice of observing the expectations of the IRB, which means bringing the research process to a grinding halt while the revised project is reviewed, or proceeding with the research without informing the IRB of the changes that have occurred. This is only one of many problems raised by the IRB process as it is applied to the kind of field research that is common among Law and Society scholars.28

Law and Society research can raise significant human subjects issues. A good example is in Sarat and Felstiner’s Divorce Lawyers and Their Clients and the issue of attorney–client privilege raised by their observations.29 Specifically, the presence of an observer technically waives attorney–client privilege. To deal with this, Sarat and Felstiner had planned to ask the lawyers to go to the lawyer on the opposing side and ask those opposing lawyers to sign a waiver of the waiver of privilege. As it turned out, the lawyers did not want to do this and expressed the view that none of the local judges, who had approved or endorsed the project, would treat the observers’ presence as having waived attorney–client privilege. Imagine how this would be treated by an IRB. If the

28See D. Yanow and P. Schwart-Shea, “Reforming Institutional Review Board Policy: Issues in Implementation and Field Research,” PS: Political Science & Politics, 41:483–94 (2008). The problem of changing research on the fly is not necessarily limited to fieldwork situations. Some IRBs have required that survey instruments be reviewed and approved as part of their process. Presumably, they would also insist that any changes to a survey instrument be reviewed. Under these circumstances, it would not have been possible for me to change the survey instrument I was using at the time of Bush v. Gore.

IRB had required a signed waiver of the waiver, what would they have said when the researchers came back and told them that the lawyers would not agree to such a process?\textsuperscript{30}

In a number of the projects covered in this book, respondents and contacts were identified through a snowball process. I recently learned that at least some IRBs will not generally allow a researcher to use a snowball sample.\textsuperscript{31} The rationale is that a snowball sample puts pressure on the respondents to participate because they were recommended by someone who knows them. If one takes this to its logical conclusion, I could have never done either of my research projects involving observing in law firms because the only way I could gain access to some of the settings was through referrals by people I knew who in turn knew relevant people in the law firms. It also raises the question of whether one could use “letters of introduction” in interview projects more generally. For example, when I did interviewing of corporate lawyers and corporate officers in Toronto,\textsuperscript{32} I relied very heavily on a letter of introduction from a person in the community who was known by most of the people I wanted to interview. This letter of introduction was crucial to my ability to get the interviews I was seeking.

Institutional review boards have been moving to procedures that try to ease some concerns that have been expressed by social scientists, creating different modalities for “exempt” research, research that can receive an “expedited review,” and research that needs a “full review.” Undoubtedly this tracking has helped, but it does not go far enough. The process is built around a model that sees “subjects” as possessing little or no power and researchers as possessing significant power. Certainly this is true for some groups of subjects (e.g., prisoners) or for some researchers (e.g., medical providers who can control access to treatment). However, in a lot of research done by sociolegal scholars, the power relationship is reversed: it is the subject/respondent who sits in the position of power. The review process needs to consider where the power lies in the researcher–subject relationship and whether a group of subjects/respondents is in a position to make a considered judgment about their participation without being confronted with a formal consent form.

\textsuperscript{30} In my own working observing in lawyers' practices I have dealt with the attorney–client privilege issue by assuming the role of a paralegal in the firm.


\textsuperscript{32} Kritzer (1984).
Often IRBs insist that subjects be told that their participation is voluntary, and in some situations there may be some element of coercion. However, in other situations, such as many (most?) types of interviews and telephone interviews in particular, the participation is by its very nature voluntary because the respondent can terminate participation at any time and the researcher has no power over the respondent. When I was arranging interviews with lawyers, I was told that I had to tell the lawyers that their participation was voluntary; nothing I said to the IRB would convince them that including such a statement was superfluous and made the letter I was sending as my initial contact sound stupid (fortunately, the IRB did not insist that I have the lawyers sign a consent form – probably something I could not get away with from many institutions’ IRBs). Eventually I was able to convince the IRB to let me say something along the lines of “I would like to ask you to volunteer an hour of your time . . .,” rather than the more legalistic, “Your participation in this interview is voluntary.”

The issues do not end with the question of power and consent. As suggested by the Kagan example, there should be procedures to allow projects to get quickly into the field when some unique situation arises. There should be a category of “exempt-exempt” research that does not require any submission or review by an IRB (e.g., routine surveys that do not touch on a predefined list of subjects such as criminal behavior or sexual behavior, or research within a governmental agency that does not involve access to confidential case files of the type that would be protected under the Freedom of Information Act).33

Unfortunately, I am not overly optimistic that these kinds of changes will occur. The IRBs have become increasingly institutionalized. There are now what amount to professionals in the IRB process. Most important, however, is that institutions’ big fears here relate to the loss of large amounts of research funding. The research funding involved is not Law and Society–related, nor is it social science more broadly. The

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33 The web page of the University of Minnesota IRB does list several types of research that “do not need IRB review,” and among these are “Interviews of individuals where questions focus on things not people (e.g., questions about policies),” http://www.research.umn.edu/irb/applying/whichstudies.cfm (last visited September 25, 2008). I suspect that in practice the IRB still insists on reviewing projects involving interviews “about policies” because any such interview will almost always include some information about the respondent’s views or participation in the policy process or implementation.
big bucks are in the biomedical arena; social science is the tail and Law and Society is the very tip of the tail, if even that.

CONCLUSIONS: WHAT DOES THIS MEAN FOR TRAINING AND FOR CONDUCTING RESEARCH

What broad lessons can we draw about the research process from these interviews that might inform how we train the next generation of Law and Society scholars? During my thirty-year tenure in the political science department at the University of Wisconsin we had many debates about how to train our graduate students in methods. In the early 1990s we adopted a program that offered students the option of a research design course focused on quantitative approaches or a design course focused on qualitative approaches. I would argue that our best-trained students took both courses. Too many scholars see themselves as doing either quantitative research or qualitative research. Too often scholars on one side of this divide attack research on the other side. Research design needs to fit the question, and researchers should be able to choose questions based on their theoretical or practical importance rather than based on a researcher’s limited methodological repertoire. This does not mean that every researcher should be able to do the most sophisticated statistical analysis, nor does it mean that every researcher should be trained in conversation analysis.

The challenge in training students how to do research is that it is not something that can be learned by just sitting in a classroom. As suggested at the very beginning of this essay, I see research as a “craft”; the skills of a craft are acquired by practice, by doing. Research projects, particularly projects involving the collection of original data, do not fit nicely into the semester (to say nothing of the quarter) framework. Students’ participation as research assistants is one important way of integrating research practice with research instruction. Designing curriculum to allow students to work on multisemester projects is another way to provide the opportunity for realistic research experiences, although there is the ever-present problem of students putting off working on something until a due date looms on the horizon (not necessarily a problem limited just to students!). One of the lessons of the interviews in this book is the need to be able to adjust when the unexpected happens, and if that unexpected problem does not happen until the week before a project is due because the student has put off
working on the project, making an adjustment or refocusing the work is not really possible. The design of research requirements needs to include ways to force students to get started early and to plan (and hold to) a realistic work schedule.

Finally, what might these interviews tell us about how we should describe our methods in our own reporting of our research? In Tales of the Field, John Van Maanen describes three genres of ethnographic writing: realist, confessional, and impressionistic. One of the central elements distinguished among these genres is how the research process itself is treated in the presentation. In the confessional genre the research process is central. We see reviews of the output of individual scholars, sometimes in award citations, sometimes in festschriften, sometimes as part of field histories; however, these tend to focus on the intellectual contributions. It would be interesting to have what might be called “research biographies” (or autobiographies) in which accomplished scholars are asked to think back over how they carried out research over their careers. Many of the scholars interviewed for this book would be great candidates for such a biography. The problem is whether any publisher would see a market for such biographies. One could imagine a professional association taking on such a project, possibly as a kind of oral history of the research careers of major scholars. Rather than producing traditional books, the oral histories or biographies or autobiographies could be made available electronically. A possible place to start on such a project would be with recipients of the Law and Society Association’s Harry Kalven Prize, which is given for a body of work that has contributed to the advancement of research in Law and Society.

But what about our reporting of our research results? One of the challenges is that journal editors are typically under pressure to limit the length of articles. Detailed descriptions of how a project evolved take up space that could go to other articles. Even so, it would be interesting if articles regularly included a paragraph or two about where the project started and how the project ended up with the particular focus of the article. Books often include a methodological appendix, but often those appendices do not trace the evolution of a research

project. Why not include a project biography as an appendix? A good example of such an appendix is in the late Elliot Liebow’s *Tell Them Who I Am: The Lives of Homeless Women*.

36 Liebow describes how the project came to be, how it evolved, and how he dealt with writing the book. Alternatively, perhaps we could ask some of the key editors at major university presses to include as appendices to books something similar to the interviews in this volume. Regardless of whether a project biography is written by the author or is in the form of an interview, students, other scholars, and “lay” readers would all have a better grasp of the research process if such material were readily available.

In his book on Supreme Court advocates, Kevin McGuire describes how the “regulars” of the Supreme Court Bar will frequently go watch oral arguments both to see what might be currently motivating the justices and to see other top advocates practicing their craft. 37 Our craft is producing research products. It is generally not possible for us to watch others practicing that craft. The interviews in this book provide our community with a view of how the masters of our craft confront the struggles and apply the techniques of social science research in the legal arena.