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The Every Day Work of Studying the Law in Everyday Life

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Abstract

Susan Silbey began her academic training in political science and in the course of her studies became a sociologist of law, the last two decades as a member of the Massachusetts Institute of Technology's anthropology department and management school. The disciplinary transformations ground, in part, her attention to the ways in which the everyday life of scholarship has led her to study the everyday life of the law. In this article, she describes her scholarly life through seven chapters of relatively distinct challenges and themes. Across the arc of her life, she identifies the recurrent influence of both serendipity and theoretical inference acting within the immediate constraints of family and personal capacity. Reading across descriptions of her work on regulatory enforcement, dispute negotiation and mediation, and popular legal culture and consciousness, she points to the necessity of reconciling on-the-ground vicissitudes of doing legal work with the theories and narratives social scientists construct to make sense of institutions and history. She muses on theoretical attempts to align the particular and the general, the micro and macro forces working in legal cultures, and concludes by celebrating the ubiquity of social ordering whose own momentum both seduces and frustrates social scientists.

INTRODUCTION

I have a PhD in political science; was for 26 years a professor of sociology; and, for the last 18 years, have been a member of the Massachusetts Institute of Technology's (MIT's) Anthropology faculty, 9 as department head. I also teach in the Sloan School of Management in the Work and Organizational Studies Group. Across this time and these disciplines, my scholarship has been exactly the same: I study how law works. For these 40 years, my research has explored how law (as text, organizational practices, and historic institution) develops in response to the demands and contributions of ordinary citizens, how law is made from the bottom up. Although law has been a professional occupation and sphere of expert knowledge for thousands of years, my contribution to the social science of law has been to demonstrate how the long-lived durability of legal institutions derives directly from the multiplicity of citizen interests and narratives inscribed in the law. Rather than focus on what official texts claim to be the law, what sociolegal scholars colloquially call the law-on-the-books, I look at what those texts (statutes, regulations, cases) set into motion, the law-in-action. Although this dialectic animates sociolegal scholarship, the law-on-the-books and the law-in-action are not discrete and divergent spheres. They are tightly coupled and dynamically entwined in popular consciousness, and thus, as both text and action, both ideal and practice, the law is stronger, more durable, and longer-lasting.

An invitation to write a reflection on one's career prompts a good measure of anxiety as well as retrospection. Why do this? I have a pile of unread manuscripts waiting and deadlines fast approaching. Is it just ego? A desire to be remembered, to be personally archived? It cannot be. One cannot capture the future. The here and now is all and everything, and the work keeps piling up. So why meet this deadline before and possibly instead of the others?

Rather early in my career, I acknowledged the modesty of my ambitions. As an untenured assistant professor, I was pressed by a colleague one summer to forgo a vacation with my husband and daughters to collect more data for a collaborative project. No, I said, family came first. I justified my action by saying that I was not Weber and my colleague was not Durkheim; the world could wait a few months longer for another paper. Besides, I said, the more important work was the teaching. I just wanted to be a college professor, some version of Katharine Hepburn with an academic gown, a feminized rendition of the teachers who inspired me, but I lacked much appreciation of what that would actually entail. I remember another conversation some years later with a law and society colleague who was also surprised to learn that I thought teaching was our primary contribution and whatever value our professional lives might ultimately have. He thought so too but had not realized that we were such soul mates. As I sit down to write this article, these memories return with amazing vividness. Why meet this deadline? I cannot have been the only naïve scholarly initiate with little understanding of what I was doing as I was learning to do it. I needed to be shown the way. Surely, there are other young would-be scholars out there, thinking about what to do with their lives and wondering whether studying and teaching /about how the law works/ is a worthwhile life's labor. I imagine them as I write.

My husband was a voracious reader, someone from whom I learned much and with whom my life was entwined since high school. He often described life as a series of chapters, each with distinct struggles and themes. One of my former students recently told me that his metaphor had provided an important life lesson, keeping her patient, allowing her to enjoy the chapter she was currently living while generating aspirations for the next. Of course, authors develop their chapters within a carefully designed narrative arc; in life, the events come upon us perhaps more than we choose. The paths in life, and in academia despite its stable structures and conventions, are not always planned and executed. The themes, appearing to offer order if not consistency, emerge only upon looking back and imposing a narrative, as I am doing here.

Seven sections I shall call chapters comprise this article. Some themes repeat across chapters: first, the work I have done and how I work, the organization of time and my temporal pacing in the constant daily repetition, the ever-recurring annual cycles, and the longer *durée* of a single lifetime. I am preoccupied by the relations between the everyday and the *longue durée*, between the particularities of everyday life and their aggregation into a long-lived institution, what is sometimes described as links between the micro interpersonal interactions and the macro institutional order. I struggle constantly to understand the relations of the particular to the general. Have we social scientists of law built a solid body of truths about how the law works? How can we better represent what we think we know? Where does this lead? This is, after all, an annual review, and the genre demands suggested research agendas. I will try not to disappoint.

CHAPTER ONE: THE MAIN LEGAL THESIS

The central insight in my work derives from the observation that most people, most of the time, go along with the law, that law-abidingness rather than violation is the norm; that is why we can speak of the rule of law. Although the trial stands as an icon of the rule of law in popular culture, it is merely the tip of a giant iceberg of matters that come to legal agencies for resolution, reconstruction, and containment. Indeed, of the myriad activities that constitute modern life, this official, iconographic symbol of legality—the trial—is outpaced by the proliferation of signs, norms, and expectations in which the traces of professional and official legal work have been well hidden. So, when we speak of the rule of law, it is because most of legality lies submerged within the taken-for-granted expectations of ordinary daily life. Rather than contested and choreographed in spectacular but statistically rare trials, law is powerful, and rules or governs everyday life, because its expectations and ways of organizing affairs are habitual and uncontested. Law's constructions and mediations have been sedimented throughout the routines of daily living, helping to make things work in more or less clear ways, without having to invoke, display, or wield the law's elaborate processes, especially its ultimate, physical force or coercion. Of course, this sedimentation and normative regulation is never complete; we do not always stay within the boundaries of legally sanctioned expectations, and the reach of law is always disputed. As a consequence, much of the visible tip of legality is about what to do when norms are breached; some of those breaches lead to disputes and some disputes to litigation, and even fewer, less than one percent of all legal matters, end up in trials, of which many are appealed (Trubek et al. 1983). These visible, carefully choreographed legal battles, especially the trials and appeals, are the outliers of the law's more routine management of everyday life.

More often than not, as we go about our daily lives, we rarely sense the presence of the law. Most of the time, law channels our behaviors without fanfare, without contest, without notice. We pay our bills because they are due; we respect our neighbors' property because it is theirs. We drive on the correct side of the road because it is prudent. We register our motor vehicles and stop at red lights. We rarely consider through what collective judgments and procedures we have defined "coming due," "their property," or "prudent driving," or why automobiles must be registered and red lights stop traffic. Our homes are suffused with legally prescribed power conduits, and the food we purchase comes in containers marked with legally prescribed nutritional labels (Frohlich 2012). If we trace the source of these material signs, expectations, and communications to some legal institution or practice, the origin is so far away in time and place that the circumstances of their invention have been long forgotten. As a result of this distance, sales contracts, property, food distribution networks, power grids, and traffic rules seem to be merely efficient, natural, and inevitable parts of contemporary life.

CHAPTER TWO: DISSERTATION AND DISTRACTION

My dissertation research and early publications tackled these questions head on as I explored the ways in which consumer complaints of misrepresentation were massaged by the Massachusetts Attorney General (AG) into new expectations for business practices. In the first 10 years of enforcement under a statute touted as revolutionary, the AG never took a single case to court (Silbey 1980–1981). The Commonwealth nonetheless secured annually many millions of dollars in refunds and restitution for consumers. The stream of complaints provided a picture of routine market failures that became data, eventually, for new laws that changed requirements and expectations for routine consumer transactions. Most importantly, I was able to identify the mechanisms—primarily negotiation and mediation—that transformed individual consumer complaints into new market practices. Although sympathetic to the law enforcement accomplishments, my analysis also exposed many limitations and problematic implications of this otherwise successful case-by-case negotiation of consumer complaints.

My dissertation research was informed quite directly by the jurisprudence of Benjamin Cardozo, Oliver Wendell Holmes, Norberto Bobbio, and Karl Llewellyn. I was trying to see how the law's fundamental regulation of force (Bobbio 1965) provided a tool—how the abstract worked in the particular—or how, in Llewellyn's phrase, the "law makes us go around in more or less clear ways" (Hoebel & Llewellyn 1943). It was shaped equally by the growing body of empirical studies of law in practice, studies of police, of regulatory enforcement, of histories of administrative practices. I was able to see how the piling up of complaints—as the AG sought to make a political career by inviting the public to provide the substance of his office's work—could slowly aggregate to ordinary market routines, for example, in notices of return policies at cash registers across the Commonwealth or in automatic rain checks for sold-out sales items. Yet, I also saw that this effort to provide remedies for the inability of market mechanisms to protect individual consumers became immediately available for businesses to mobilize in their intrabusiness disputes. I observed, as we wrote in an early paper, that although the law helped shape and constitute cultural norms and expectations, it was also an instrumental tool available for skillful users (Bittner & Silbey 1982).

The dissertation had a long gestation, which included two genuine human incubations—although those children do not provide the full account of why my degree was probably among the longest in the history of the University of Chicago Political Science department. Throughout my scholarly career, I have found it difficult to put aside what I experience as the immediate demands of everyday life—whether it was at the outset just taking care of household and children, becoming involved in neighborhood politics, or as the years accumulated the obligations of teaching and managing academic departments.

I have never been able to develop a habit of daily sacrosanct writing time as many novelists describe and highly productive colleagues follow. I work in fits and spurts as projects begin, thinking here and there about a problem or set of texts as I am driving, reading the newspapers, speaking with colleagues and students. I scribble notes to myself of associations and metaphors, as I struggle to make sense of a topic, concept, or pieces of data. I make noticeable progress on manuscripts, however, only when I do carve out longer stretches of unprogrammed time—full days and preferably sequences of days with no or very few competing obligations. The actual writing—putting words on the page as I am doing now—may occupy only a portion of the allocated time, but it is the sitting and thinking and organizing thoughts that shape that day.

In cycles of weeks and months, I manage to rotate data collection and analysis—which I do squeeze into the pieces of days without necessarily cabining extended periods—with punctuated writing times. The data collection, if it is interviewing, is scheduled and thus can be folded in

with other commitments; however, observing, like writing, needs extended time, as it too includes writing the notes along with the day's observations. Aspects of data analysis can be tedious, for example when coding and cleaning data, qualitative or quantitative; it can be broken up into smaller pieces without much loss of acumen or continuity. When getting to the synthesis and writing, however, I always try to discipline myself as I once read about how the younger Hemingway organized his writing. You can leave the work at the end of a day's labor, he said, if you know what you are going to do the next morning. You cannot stop before then. I usually write myself a note about what to do next before closing down for the day.

Why am I indulging in this self-report from the field? What is the point I am trying to make? I want to describe how life is made from disparate pieces and endless transactions. We social scientists (and other self-reflective actors) infer, or perhaps impose, pattern on this flux. In our accounts, the multiplicity of actions and transactions is simplified across the repetitions, and the durations themselves become recognizable rhythms or patterns.

For more than a decade as a graduate student, I studied in relative isolation, working at home when children were napping or could be assigned to others—there was no day care then—without a close community of other students. My only regular interaction was in bi-weekly meetings with Egon Bittner in the Brandeis sociology department, who took me under his wing, became my mentor, and led me from political science to ethnomethodology and sociology. The wife of a friend I met in a photography course introduced me to Egon. The photography was just another one of the distractions, along with workshops on tailoring and oriental carpets, delaying completion of the dissertation.

I conclude this particular chapter of this article with one of the most important observations I learned from Egon and that has grounded my sociology ever since: Plans rarely work out as begun or designed, and the ultimate product or act is constituted by its making (which can be ongoing, which is the point of the story I tell here). Forks in a road force decisions about which direction to take. Serendipity and diversion have shaped my career as much as or possibly more than carefully mapped programs. I first learned this when struggling to complete the dissertation and at the time elevated it to a sociological insight in some of my earliest papers about how the AG's office implemented the consumer protection statute.

Every week or two, I would bring to Egon, at his Brandeis office or home, one after another chapter draft for him to read. He would write comments on the pages and sometimes would type up a note in summary form. I would rewrite and send him the revised chapter. He would write comments on the revision. Every text elicited comments, which I often took as helpful suggestions. At some point, perhaps in the mid-1970s, after five or six years of work, I asked him when I would be done. He said, "That is for you to decide. It is your job to write and my role to comment." I was stopped short. I often was by these little aphorisms. What did he mean that it was for me to decide when the work was done? He was the professor, I was the student. This prompted more conversation, during which he suggested that I might think about the cases in the AG's office in a similar manner. How did they decide when a case was done? How did any professional assess their work, know that they had done as much as they should or could, and, even if they could do more, whether what was done was sufficient in the circumstances?

It had been staring me in the face all this time. I had observed that an AG, empowered by this radical statute to litigate on behalf of consumers, had never actually taken anyone to court. Yet, the AG was raking in millions of dollars in refunds for consumers in the Commonwealth. If the AG negotiated settlements but never litigated, how did they know a case was ready to close? What if they did not get a refund? What if they could have gotten a larger refund? When did they decide that a case was finished when there were no criteria—such as a judicial decision or full refund plus compensatory damages—that the case was closed? How would I decide that the dissertation was

sufficiently done, if there is always more that could be researched or analyzed, more that is not yet completely understood? I actually spent an entire year writing a chapter looking in-depth at a specific industry to show how each of the steps of the processes I had observed worked in this particular market. The chapter never made it into the dissertation. I do not recall why. But this exchange (about when professional work is complete) did lead to a chapter in the dissertation, eventually to my first publication in the *Law & Society Review* (Silbey 1980–1981), and to a life lesson, which I also try to give in Egon's honor to my students. You do not get from here to there—whether it is a spatial or a temporal path, whether it is a life plan, an outline of a text, or a logical argument, he told me that day—by moving through a straight, resource-efficient channel. Things happen along the way from here to there, and the path and decisions are situationally structured, although not determined. We do not work in or move through carefully orchestrated and efficiently sequenced steps.

Some years later, when I was well on my way to tenure, I sent Egon a draft of a paper from the dissertation research that I was writing for a collection, *Enforcing Regulation* (Hawkins & Thomas 1984). In his response to the draft, Egon articulated in his idiosyncratic self-reflexive syntax the lesson that animated much of the dissertation analysis and the observation with which I conclude this chapter of this article:

> I went over the pages you propose the paper for Macmillan to build on. You say everything just as it should be said. People undertake to do something; then they wonder why it doesn't get done as they imagined. Well, it is because people think that to get something done one only needs to do it—as if they had never, themselves, done anything and didn't know that the doing of something must first be put in its place, which has its own demands, etc. I think those who do not understand this do not understand because their minds are choked with too much social science. Your obligation to put the idea in the form of an article is a fine example of what you are trying to explain. This is the way the saying of what needs to be said is itself put into its proper place, etc.

CHAPTER THREE: GETTING SERIOUS ABOUT BEING A SCHOLAR

Upon completing my degree, still from the University of Chicago, and attending my first Law & Society Association (LSA) meeting in 1980, I discovered—what I hope most students learn much earlier—that working within a community of lively exchange and jousting nourishes the scholar and drives the scientific enterprise. I realized that I thrived in highly interactive groups with constant back-and-forth conversation and argumentation. One can certainly do that by oneself, as I had basically done for most of my graduate education (with the exception of these visits with Egon), but it was a struggle and sort of intellectually lonely, certainly not as much fun or as productive as the engagement with others turned out to be. My work pace increased and my topics broadened. I was enticed by research opportunities and invitations to collaborate with others. The pleasure in the work escalated as these engagements deepened, and I very self-consciously reoriented my time and attention.

A second bit of serendipity shaped my sluggish career; the first was meeting Egon. My theoretical and methodological resources expanded dramatically when following that meeting I was invited to join what we later named the Amherst Seminar. We were political scientists (John Brigham, Christine Harrington, Austin Sarat, and Adelaide Villmoare), sociologists (Ron Pipkin and Susan Silbey[1]), and anthropologists (Sally Merry and Barbara Yngvesson) living in or within two hours'

[1]Working with Egon Bittner had moved me into sociology, although the degree was defended in and awarded by the Chicago Political Science department.

drive to Amherst, Massachusetts.[2] Having originally met each other at the Law & Society Association annual meetings in 1980 and 1981, we realized that we were all, in one way or another, studying dispute processing—a popular subject at the time. Beginning about 1985 and over the next few years, we invited Kristin Bumiller, Patricia Ewick, Alice Hearst, Brinkley Messick, and Martha Umphrey to join the group. Meeting at least once a month for 13 or 14 years, we developed a supportive environment in which to advance our research. We presented drafts of papers to each other; invited scholars from Europe and elsewhere to present their work; and together read through the newly translated French poststructuralist writers, the emerging canon of critical legal studies, and other works we thought important. The cultural turn was just arriving on the American shores; feminist and critical race scholarship were flourishing. We organized small conferences and panels at national and international conferences, developing the intersections of our work with these intellectual trends.

Although the seminar proved to be a rich and challenging intellectual environment, it was also functional in the most obvious, direct, and material ways. More important than the geographic proximity, which made the meetings possible, with the exception of Brigham and Pipkin, who taught at the University of Massachusetts, the others taught at small liberal arts colleges. Moreover, with the exception of Brigham, Pipkin, and Sarat, the others—all women—were untenured assistant professors at the very beginning of their careers. We were not working in large universities, with groups of graduate students and colleagues with whom to test ideas, share research, and collect early criticism on emerging work. Teaching primarily at liberal arts colleges, we were ignored for the most part by the very elite institutional strata of American research academia, except happily within the law and society community in which we wanted to participate. Although the students in the colleges in which we taught could be considered elite, as admissions are selective and lifelong career prospects generally much above average, the faculty could not claim similar status among research scholars. Nonetheless, we became mentors for each other and all were successfully tenured. The seminar provided collectively what we individually lacked. The work of seminar members became recognized, in part because of individual merit; in part because of association with the seminar, which itself became a recognized source of important sociolegal scholarship; and in part because of the reciprocal support members provided for each other. Members collaborated on projects, some of which lasted decades. Most important, we learned from each other, developing a kind of "pluridisciplinarity" that French sociolegal scholars describe and advocate (Commaille & Lacour 2018). The different disciplinary tools (theories, concepts, and methods) became available to us all—a focus on the state from political science, organizational processes from sociology, culture from anthropology. From persistent interaction, we began to see limits in what was taken for granted in our respective disciplines while forging a synthesis across them to encourage new ways to understand how law works. The study of legal consciousness and legal culture developed directly from the various contributions of the several disciplines. I described this in the first volume of the *Annual Review of Law and Social Science* (Silbey 2005).

My second major project following the dissertation, which began a year before attending that fateful Law & Society Association meeting, examined the consequences for the legitimacy and authority of law by using negotiation as a means of law enforcement. In this study, I worked with Sally Merry to compare more than 150 cases that were handled in local district courts to 150 similar cases that were sent to informal, community mediation programs. My interest developed directly from the dissertation observing how the AG had achieved remarkable changes not through litigation but through negotiation of complaints. This was a theory-driven project that was a logical,

[2] Richard Maiman (Southern Maine), Craig McEwen (Bowdoin), and Lynn Mather (Dartmouth) came to some early sessions.

almost textbook-like next step from my dissertation research. This developing expertise on law from the ground/bottom up, on citizens' participation in the legal system, resulted in my being asked by the US Department of Justice, Office for Improvements in the Administration of Justice, to complete a thorough review of the lower courts, which would be the first and only such official account of these more than 18,000 American courts. It was driven specifically by a Massachusetts District Court Judge, John Cratsley (1978), who was interested in our work comparing cases in court and in mediation. I learned what I should perhaps have known already, that networks really do matter. Because it was a comparative project of matters at the virtual boundaries and bottom of the legal system, I was provoked to reconceptualize the study of legal phenomena and processes, which ultimately occupied me for almost a decade.

From the mid-1980s through the 1990s, I wrote a series of papers about how to study and understand legal processes and the law as cultural phenomena (Silbey 1985, 1992; Silbey & Sarat 1987, 1989; Sarat & Silbey 1988). These works developed from collaboration in the Amherst Seminar, which some commentators suggest helped produce a transformation in the working paradigms in law and society research (Amherst Semin. 1988, de Sousa Santos 1989, García-Villegas 2003, Trubek & Esser 1989). We articulated what was known elsewhere in academia as the cultural turn, demonstrating that law cannot be understood solely in terms of what is in the law books or what is observed as behavioral and organizational processes. It is the relationship between the threads that constitutes the institutional durability, an insight that would emerge more sharply in my next major project and still animates my research, to which I return at the end of this article. For me, this theoretical move emerged with my conversion to sociology, training as an ethnomethodologist under Egon Bittner and then working as a sociology professor. I took seriously the empirically grounded work in social construction, for example, writings by Erving Goffman, Peter Berger, and W.I. Thomas, as well as Egon's studies of organizations and the police (Bittner 1965, 1967, 1973, 1974, 1990). These texts are regularly taught in undergraduate sociology classes but had not been, before the 1980s, well integrated across disciplinary fields. As a convert, perhaps, I took my sociology quite seriously and began to reinterpret how we had been, even as sociologists, (mis)conceiving legal processes.

I argued that it is not sufficient to show that legal institutions fail to function as they claim. It is crucial to examine and interrogate the very ideals and principles that law claims for itself. The standards that legal institutions announce, even though they fail to realize them completely, are part of how legal institutions create their own power and authority. The ideals of law, such as open and accessible processes, rule-governed decision making, or similar cases being decided similarly—despite their inaccuracy as a description of how the law works—are nonetheless circulating in popular consciousness, part of shared understandings of what law is. Although law's ideals are not accurate empirical descriptions, and consequently were not documented when scholars looked at the law in action, they nonetheless serve as circulating aspirations that help shape and mobilize support for legal institutions. The legal concepts also become part of the linguistic repertoire that names aspects of daily life, e.g., property or right. The paradigm shift we initiated, referred to sometimes as "critical empiricism" (Trubek & Esser 1989) or at other times as the "study of legal culture and consciousness" (Silbey 2003), became the central focus for a new generation of scholars, as well as the subject of review essays and books.

The 1980s in effect initiated my scholarly career, for which I had been in basic training for nearly two decades. It was an intellectually exciting time, filled with passion and joy, but also a nerve-racking time, as I tried to pull a central academic and research thread from what had become the tangle of my life. I often wish now that I had been a Pollyanna then, believing that all would work out well in the end, able to enjoy the adventure and discoveries along the way—learning as my student did about life having chapters. But I did not know the narrative arc through

which the chapters were moving, and this chapter had, at least until past its median, no clear theme.

In 1974, I had begun teaching part-time, as an adjunct faculty in the sociology department at Wellesley College. I was recommended for the position by a law student at Boston University, where I was also teaching part-time a course in the sociology of law (which Egon had secured for me). The Boston University law student's husband was in the Wellesley department; he was going on leave, and she recommended me to fill his slot. I was writing dissertation chapters, teaching part-time, raising two young daughters, and all the rest that comes with home ownership, etc. My husband was just reaching tenure at MIT. By the time I finally graduated in 1978, I had no confidence that a regular, long-term academic career was in my future. So, I gambled. I applied for a National Science Foundation (NSF) grant for the study comparing mediation and adjudication of lower court cases and decided to run for political office as a member of the Newton, Massachusetts School Committee. Perhaps there was a future as a researcher, if not my fantasized professorship, or perhaps I could do politics professionally. Unfortunately, or too fortunately, all three paths opened. We received the NSF grant; I won four elections to the School Committee; and because a new chair was hired to rebuild the sociology department, I was put on a tenure line at Wellesley. It was a difficult balancing act for sure. I eventually foreswore a fourth and legally limited final term on the school committee because I did not think it was good for the children. I was more available to any citizen in Newton than I felt I was for them. However, the experience as a labor negotiator for the committee, and as an overseer of a large suburban school district, fed my scholarly interests. I had a platform from which I could both teach about and enable more productive forms of negotiation, alternative approaches to race issues in elementary and high school curriculum, and strategies to manage legislative intrusions in the classrooms. I was observing how law was diffused in so many often unexpected ways across so many settings and was able to offer explanations and insights derived directly from my research and teaching. Eventually, I wrote a paper about a special education mediator (Silbey 1994), another about how children interpret the law as part of a seventh-grade social studies curriculum (Silbey 1991), and finally another about a six-year-old boy who was suspended from another school district for sexual harassment (Silbey 2006). Sociology colleagues kept pressing me to write about the politics I was living, but I had too much on my plate already.

CHAPTER FOUR: THE COMMON PLACE OF LAW

It would be nice to say that my next and most widely cited project studying legal consciousness among residents of New Jersey emerged directly and logically from the study of alternatives to litigation. Its empirical and conceptual framing certainly did derive from our theoretical evolution as Patty Ewick, my coauthor on this work, and I wrote in the preface to *The Common Place of Law: Stories from Everyday Life* (Ewick & Silbey 1998). However, the fact of the project was serendipitous rather than planned, and has been described in *Conducting Law and Society Research* (Halliday & Schmidt 2009).

In late 1989, I was asked to spend a day with Bill Chambliss (George Washington University) and Howard Taylor (Princeton), who were consulting for the New Jersey Supreme Court. They were trying to find out how minority and nonminority populations (the terms they used) experienced the state court system. They asked me to tell them about the body of law and society research on citizen interactions with the courts. I spent a day doing that. It was as if I took my 13-week law and society class and presented it over 8 continuous hours, after which they asked me to write up what I had told them. They would provide a consulting fee. When I sat down to write the literature review, I observed something I had not previously noted: that by using different

data collection and analysis methods, different disciplinary groups were producing alternative accounts of the meanings and uses of law. It was quite obvious but somehow had eluded me until then, that is, the alignment of methods, disciplines, and substantive findings. They were more often described as alternative theoretical paradigms, but the methods, disciplines, and findings seemed to be tightly coupled, each making strong claims for their generality. I offered to write the New Jersey team a research proposal with what I thought would be a corrective to the current literature, which I would of course also summarize. They agreed, and the rest—three major research grants and almost a decade of data collection and analysis and then a book—followed. I had not planned to do this research, merely to design it and perhaps consult as it progressed. Very soon, because the team we hired looked like it was screwing up the sampling strategy, I abandoned the work I was then doing studying children studying law (Silbey 1991) and began learning my way around both stratified random sampling and the state of New Jersey.

We laid out a model of how legal phenomena should be studied as cultural practices and went to work putting our theory to the test. Over the course of four years, we interviewed 430 residents of New Jersey to find out what role, if any, law played in their everyday lives. The design of the research responded to the inconsistencies we had noted in the existing literature. Telephone surveys completed with large, random samples of the population had regularly reported that Americans evaluate their legal experiences in terms of the processes and forms of interaction—the procedures—rather than the outcomes of those interactions. People cared most about having neutral, honest authorities that treat them with dignity and allow them to state their views. In contrast, researchers who spent long periods of time engaged in deep community ethnographies produced very different results. They described how people resent and resist the law, how they refuse to go along, and how processes are not a substitute for getting what you think you need. Placed side by side, these studies appeared contradictory. Perhaps they were focusing on different parts of the elephant: generalizing from national surveys versus looking closely in local communities. The distinct research communities, using different conceptual resources and research methods, were producing very different accounts of the place and use of law in the lives of ordinary people. In response to this inconsistency, we proposed to engage in long, semi-structured conversational interviews, thus achieving some of what the ethnographic fieldwork provided, and yet to do it with a large, random, and generalizable sample as the surveys provided. In the end, we were able to reproduce and validate the results of both methods and more still.

The Common Place of Law: Stories from Everyday Life describes three narratives of law composed of commonly circulating memes and tropes. One story of law is based on an interpretation of the law as magisterial and remote, a system of rules both enabling and confining decisions and processes. Here we have an ahistorical, generalized ideal, which the law routinely announces for itself. It is a special place, remote and different from the bustle of everyday life, housed in leather tomes and marble halls with rigidly demarcated spaces and roles. Another narrative views the law as a game with rules that can be manipulated to one's advantage. This is a pragmatic account of the law as a resource for handling the contests and disputes of everyday life. This second account brings the idealized aspiration to ground, as a useful tool that endows the skilled in pursuit of self-interest. Here, the haves come out ahead, and the adroit citizen is alert to the ever-present possibilities of legal entanglements. A third narrative describes the law as an arbitrary power that can be actively resisted by finding one's way around and through the ellipses and hierarchies of formally and legally structured organizations. Promising protection, the institutionalized power of law can be overwhelming and damaging as well as enabling and protecting. In these accounts, people do what they can to get what they need from an institution that they experience as more often stacked against them. Together, as a plait of cultural tropes and schemas, we argue that law is stronger and more durable than if it were experienced solely as a corpus of neutral principles,

or as merely skillful gamesmanship. Because the law is recognized and spoken about as both, our experience can be consistent with our ideals. The abstract ideal provides legitimacy, while the game makes it useful. Importantly, we showed that these schemas are not associated with types of people; any one person tells different stories of law. Legality, we argued, is constructed from cultural tropes and memes circulating within the events of everyday life. What might seem like contradiction in empirical observations turns out to be a not-uncommon protective institutional tension.

CHAPTER FIVE: LAW AND SCIENCE

My current work is an extension of the themes and theory laid out in *The Common Place of Law*. I have been studying the understandings and uses of law, not of ordinary Americans as they go about their daily lives, but of elite scientists as they design their experiments, manage their laboratories, and respond, or not, to demands to change their habits to produce safer, greener laboratories. This project took some retooling, as I had to learn an entirely new field: science studies and sociology of science. At the same time, it also took me back to my dissertation research on consumer protection as I reentered the community of scholars who study regulation and compliance, now reincarnated as regulation and governance. My current work asks questions raised but not explored in *The Common Place of Law*: Would the stories of law told by ordinary people doing routine, daily activities—e.g., paying their bills, buying their groceries, seeing doctors, renting or owning a home, driving cars, raising children, paying taxes, seeking government services—be echoed by people who are not "commonplace" (as that term was used in the book) but who are instead involved in highly specialized, expert technical work? Is the law experienced and used differently in the research world of elite scientists than it is in the daily lives of ordinary Americans? In some ways, scientific spaces are no different than most others, equally saturated with health and safety regulations, employment and financial regulations, susceptible to claims of loss and liability. Yet, I am discovering through my research that for scientists, who are authorized and insulated by layers of education, expertise, and consequent privilege, the law that infuses their work has been, until now, largely unnoticed, irrelevant, and inconsequential, not at all the colonizing, contradictory institution described by the people we interviewed for *The Common Place of Law*. I began to wonder: How do elite scientists respond to legal demands (newly passed laws and regulations) that disrupt their usual practice, i.e., by requiring them to change laboratory routines; complete new training and yearly retraining; and submit to periodic surveillance of laboratory practices in the name of environmental health, and safety? And what does this tell us about the universal aspirations of the rule of law?

This project developed from a combination of theoretically driven deductions, opportunity, and serendipity. Two critiques of *The Common Place of Law* intrigued and challenged us more than others. One set of criticisms claimed to admire our detail and perseverance but nonetheless thought we wasted our time studying ordinary people. We ought to study elites, we were told; they make the world. This upset me, not simply for its political implications for scholarly integrity and democracy but because it seemed to miss central parts of our thesis: that law was powerful because it saturated everyday life and that most people—elites and masses—go along willingly. It so aggravated me that I thought, "I will study elites but not what he thinks are elites (e.g., corporate lawyers). I will study cognitive elites, scientists." I imagined, wrongly, that by selecting cognitive elites, I might not be preoccupied with the privileged classes as much as those studying corporate lawyers. Scientists actually enjoy remarkable privilege and authority. A second set of criticisms seemed much more theoretically grounded. Colleagues among organizational sociologists had worried that we had interviewed individuals, and in their homes rather than workplaces, and had

insufficient ways of observing the power of competing normative orders. If we studied people in formally organized settings where other or additional norms would be more immediately salient, our friends advised us, we might hear different accounts of legality. We needed to trace, they suggested, the ways in which law competes with other normative orders (Heimer 1999). We thought this quite right and began formulating a next project.

Following publication of *The Common Place of Law*, we proposed to the NSF just such a study to look longer term and in depth within complex organizations to see where and when legal considerations arose and how the organizational members interpreted those situations. We called the study "Truth, Profit, and Justice." We planned to study a scientific laboratory (whose goal was truth), a financial corporation (in the pursuit of profit), and a social movement (in search of justice). The NSF officers told us that we just wanted to repeat what we had done, and they would not fund that. This was disappointing in multiple ways. Why can't social scientists repeat studies—hasn't that been one of the major limitations to the advance of our science? Not good judgment on their part, we thought, but perhaps we did not explain it well enough. We wanted to test the results in different settings with distinct and strong normative commitments while holding methods and framing constant. Would the law be experienced differently in the face of these strong, competing normative orders?

If "truth, profit, and justice" did not seem worth funding, the program director told us, they would love to see research directly on law and science. So, I submitted another proposal describing a project in which we would do ethnographic research for 18 months in two different labs with significant variations in subjects or organizational form (I cannot quite remember and cannot find the proposal), after which we would interview a nationally representative sample of scientists to see if the hypotheses generated by the ethnography would generalize. It, too, was rejected. (Of course, approximately 80% of proposals are rejected.) The reviewers said that we would never find law in the laboratories, "it was like looking for a needle in a haystack," and that we "ought to watch the scientists as they spoke with their lawyers." Here again, the theoretical import and empirical findings of *The Common Place of Law* were misunderstood. Following scientists into their lawyers' offices would not tell us what place law had in the routine everyday work of science. Despite the hype about translation research flooding out of science labs, making intellectual property is not the ordinary work of most scientists, nor is the preparation of litigation evidence. These are the outliers of legal penetration into scientific practices. We did not want to look at the tails of the distribution of engagements between law and science. More to the point, we wanted to know what role law had in the house of science, the laboratory, not what role science had in the house of law, courtrooms and law offices.

As it turned out, while I was writing and revising research plans to explore the everyday relations of law and science, my husband was asked to join a small faculty committee at MIT, primarily women but joined by two men, to investigate the status of women faculty in the School of Science. My husband, a theoretical physical chemist and quantum mechanician, asked if I could tell him what the sociologists knew about women in science. So, I went off to the library to read a range of feminist and science studies literature. Some mainstream sociology, primarily the work of Harriet Zuckerman and the group working with Robert Merton at Columbia, analyzed a range of empirical data showing how women scientists were systematically disadvantaged in educational attainment, productivity, funding, lab space, and recognition (Cole & Zuckerman 1984; Zuckerman & Cole 1975, 1991). Another line of research claimed that women scientists displayed a distinct kind of cognition, more holistic and relational rather than what the authors described as positivistic and reductionist mainstream science. Women were disadvantaged, this work argued, because they worked in "a different voice" (Fox Keller 1985, Gilligan 1993). It seemed to be the same situation again in another field, here women and science, as we had observed in literature on citizens'

interactions with and interpretations of the law: Different data collection and analysis methods produced different accounts. Were they looking at different parts of the elephant again? Although new to the field, I was skeptical this time because I had direct and extensive experience if not systematically collected and analyzed results. I was married to a chemist, lived alongside if not within the field, knew dozens of women scientists at MIT and elsewhere, and had listened to their talk for decades. I had read and typed research manuscripts and had also studied quantum mechanics. None of the women scientists did their science very differently, used distinct modes of analysis, or made inferences in a radically alternative fashion. They did, however, often organize their labs differently and had fewer opportunities for collaboration, consulting, and business ventures.

So, I decided to take up this question. If the NSF would not support my fieldwork in the laboratories, I would try another way. I would do in-depth interviews with a matched sample of male and female academic scientists at the most select institutions (i.e., elites again), pairing them by date of degree, specific field, and experimental methods. I would conduct three long interviews separated by several weeks or months, during which I would analyze each interview in preparation for the next because I might have to learn a little more about the science as I went along, and I would want subsequent interviews to build on the previous. I planned to begin with biography: childhood socialization, the decision to become a scientist, education, mentoring, and career to the present. The second session would focus on the scientific work: how projects were selected, organization of the projects over time, management of the labs, funding, and collegial relations. The third would focus on work outside the lab and university in the wider profession, in government, public service, and business. Following the method we used in *The Common Place of Law*, I would not focus on law or gender. I would note if any references to regulations, property, or disputes appeared and how they were handled. I would obviously take note of any litigation, although I did not expect much. I would also note if law was relevant but not mentioned. Even if I did not find the needles of law in the haystack of science, as the NSF had described my last proposal, I would have a gender study. It is always good to have a fallback question that could be addressed even if it is not your motivating question. I knew that if the stories my interviewees told did not vary by gender in the first and second session, in biography or the doing of science (which they might), I knew the stories would likely vary in the third session about outside activities. Existing literature suggested as much. Few women scientists consulted as often as did the men. And, at that time, among the scientists I knew, no woman had a start-up, whereas a good many of the men did, especially in some specific fields related to the emerging biotech industry. Today, many women are deeply involved in translational science in both academic labs and private firms.

By my eleventh or twelfth interview, I had found my research site, right there in plain sight. I did not have to sift through the haystack. As I would begin the interview, respondents would sometimes say, "I looked you up. You know there is no law here…Of course, we have rules about funding and hiring, but it has nothing to do with the science." I would reply that it was just fine, that my research was shifting topics from the organization of law to the organization of science, and we would continue with the interview. At the end, I would ask if I could visit the lab. As I did, I noticed the doors papered with signs: *BL2* (biohazard level 2), *Radiation Protection Required*, *Eye Protection Required at All Times*. The scientists had said there was no law here, but the laboratories were heavily regulated spaces; not only the physical construction of the labs and handling of materials but also the attire and behavior of the inhabitants were often fully prescribed. I abandoned the gender study.

I returned to the topic of my dissertation: regulatory enforcement and compliance, focusing on the environmental, safety, and health regulation of laboratory science. It has been exciting and productive, well supported by the NSF, and the source of dozens of dissertations in several disciplines (e.g., sociology, management, anthropology, science technology and society, engineering,

urban planning). Our first paper was called "The Architecture of Authority: The Place of Law in the Space of Science" (Silbey & Ewick 2003), describing how the scientists did not see the regulation of the labs as a matter of law.

CHAPTER SIX: MICRO–MACRO LINKS, THE EVERYDAY, AND THE *LONGUE DURÉE*

How does the everyday aggregate to constitute the *longue durée*? In one's life, the small projects, diversions, opportunities, and obligations cumulate. We do not have to identify a mechanism of aggregation. Time passes so that the tacking back and forth along paths anticipated or unplanned ends up being a lifetime, and a rewarding career and good life if one is lucky. We can write a story that turns the accidents into a well-plotted moral tale, or we can be agnostic, offering an account of just one thing after another, life as rolls of the dice. The tangents can be characterized as distractions or as textures enriching the tapestry. So many metaphors all to the same point: How do the pieces make up a whole? Does the law work the same way? Is the rule of law just one case after another? What are the stable, recurrent, and consequential patterns of action if not the law-on-the-books, written in the cases, statutes, and regulations? Is this not also the question animating much, if not all, of social science? How do particular events and specific transactions not merely illustrate but comprise the general? Our social science tends to divide the labor on this question, as on most. Some researchers focus on the individual actors and interactions at the micro scale, seeking patterns and causal mechanisms for one course of action or another among individuals. Others focus on the macro scale, noting persistent patterns as well as shifts over time and space. Is there a parallel between micro–macro connections as between the everyday cycles and the *longue durée*? How do the innumerable everyday activities come to be seen and interpreted as a unit, a practice, a routine, an institution, the law? Do we have a standard model similar to that which organizes the observations of electrons, atoms, and molecules into the theory of matter for the physicists, the periodic table for the chemists? How does the law work, after all, if this is what I have been studying all my life?

Over the years, I have tried several times to map what could be the pieces of a general theory of legal action, offering synthetic propositions that seemed to generalize across the empirical studies, without reaching a simple coherent model (Silbey 1985, 1989; cf. Macaulay 1984). I have been thinking about this a lot recently, often preoccupied with the tensions we produce from ambitions to generalize along with demands for well-identified findings. Perhaps in our twentieth-century efforts to "*consider* social facts as things" (Durkheim 1982, emphasis in original), we have deluded ourselves into thinking that social facts are things; we reified our own inventions and thus were captured by our particular golem (cf. Collins & Pinch 2012). We often talk about social life in terms that first abstract and then synthesize particular aspects of the innumerable activities of life. This is part of our method, after all. We then give a name, e.g., role, status, family, motherhood, law, to these abstractions as if the named phenomena existed independently from the living embodied persons doing, talking, interacting with others and with things, as if the abstractions existed as such and not as aspects of a complex. We talk about the organization as an actor, a single entity, although organizations are patterned networks of humans and technologies, distributed authority, and variable performances. Through our language we suggest homogeneity, generality, and perhaps independence, where upon closer examination we observe heterogeneity and perhaps concatenation. We focus on the central tendencies in our data, the medians, the size of the R-squared, and the statistically significant results. On a Gaussian distribution, we seek to characterize the hump in the curve, where we expect most of the examples or enactments of the phenomena, examples of the generalized abstraction, to lie. We know, however, that the curve

represents variation within the designated phenomenal category. When do we sort the distributed variation into separate categories?

How Do We Create the Factual Finding and the Abstraction If Not by the Central Tendency?

The literature about organizational, professional, and other institutionally recognized practices describes commonly circulating norms showing how persons share particular memes, tropes, ideas, interests, and sentiments. In this way, and in different projects, we identify populations, groups, organizations, or institutions with recognizable features. And so, we apprehend a whole composed of its abundant local actions. We sometimes call it a culture, e.g., legal culture, as we did in *The Common Place of Law*. Certainly, we also find instead of normative consistency examples of trial and error, bricolage, and satisficing—whether looking at various occupations and workplaces, religious and family practices, or even rational market behavior. More often than not, however, these scrupulously textured descriptions are read as variations from normative ordering and institutional coherence, of decoupling from some expectation of more common and thus the putatively legitimate performance. In sociolegal scholarship, we repeatedly show, for example, how the implementations of public policies, bureaucracies, and work organizations display less rigid divisions of labor and unwavering hierarchical control than didactic readings of classical organizational and institutional theory might suggest. Are the richly textured empirical accounts to be considered variations of the same phenomena or of alternative models of action or phenomena? Are they the tails of a Gaussian distribution or the hump of a different curve?

Here I have a sense of déjà vu, recalling a conversation my husband and I used to have when the girls were growing up. It was a repeated theme in our household: What degree of micromanagement was good parenting or too controlling? Using a metaphor from television technology of the 1960s and 1970s, he would say that he just wanted to get them on the right channel and that I was always fiddling with the fine tuning. To this, I would reply, if you turn the fine-tuning knob enough, you get to the next channel. This is the conceptual and theoretical dilemma for social science, I think, difference in degree or category.

Lately, I have been trying to understand how regulatory compliance in scientific laboratories displays a particular pattern that for me enacts this more comprehensive tension between the particular and the general and how to synthesize across varied cases. What constitutes regulatory compliance? Are we talking about the organization or its members? How much variation signals regulatory failure? In the research we have been doing, most of the laboratories comply most of the time with the currently required practices for safety, health, and environmental sustainability. The labs are inspected twice a year. If an infraction is observed during an inspection, it is likely that the next few inspections will show none. For most of the labs, this pattern has persisted for nearly a decade. If, however, we look at the labs for which there were many infractions in year one of our observations, we find a different pattern. The number of infractions increases over the years, although the rules and inspectors remain constant. The bumpy relatively horizontal line—infractions followed by none, then an infraction followed by none—characterizes inspections in most of the labs, but not all. A small set of labs, perhaps 10% of the population, accounts for the majority of recorded infractions. If we focus on this tail of the distribution—the deviants, so to speak—we ignore the fact that most of the time, most of the actors are in compliance. If we focus on the fact that most of the labs are doing rather well, we ignore the danger that persists in a subset of the labs. To the extent that we write laws and regulations to respond to the dangers, the infractions, as illustrated in the increasing volume and detail of our regulatory regimes, we are engaged in a Sisyphean struggle. As Durkheim explained more than a century ago and thousands of studies have

documented, no matter where we put the norm of any behavior, not merely laboratory safety, there will always be variation. Approximately 5–10% of actors account for approximately 95% of the deviant variation for any behavior: organizational disputes, bureaucratic efficiency, divorce, crime, use of emergency services. Yet, we write rules for the tail of the distribution failing to comply, perhaps over-burdening the population on the hump of the distribution. If we suggest handling the tail differently than the hump, we face criticism for profiling, i.e., categorizing variations as different phenomena.[3] At what point do we treat these violating labs as a different kind? Yet, there surely is a relationship between what happens on the tails and what happens in the normal middle. This is what has been troubling me: a version of linking the particular and the general, the micro and the macro.

CHAPTER SEVEN: CONSTRUCTING THE NARRATIVE ARC

I am convinced that we need to rethink some of the assumptions and conventions that have animated our research over the twentieth century. We need to think more relationally, with modes of inquiry offering simultaneously less reductionist, less partial, less mechanistic, and yet also less general and normalized understandings of social action. We need to get beyond conceptions of compartmentalized social action by depicting the relational interdependence that may be elided in efforts to produce reliable and valid depictions of "social facts as things." Across so many studies, researchers show how relatively stable social practices and institutions—legality (Ewick & Silbey 1998), criminal justice (Packer 1964, 1968), medicine and sports (Ewick & Silbey 2003), scientific authority (Hilgartner 2000; Knorr Cetina 1999, p. 24), research excellence (Lamont 2009)—are produced through discrete, often fleeting, and sometimes contradictory transactions and events. What had been seen in particular studies as deviations from expected models can be reinterpreted as normal, everyday routines that demand explanation, and perhaps even normative legitimacy by virtue of their persistence and ubiquity. We argued in *The Common Place of Law* and elsewhere that there is a pattern to these variations, too often described as decoupled or contradictory experiences and accounts of social institutions: a general, ahistorical, idealized norm that sustains legitimacy alongside a narrative of pragmatic, grounded, and instrumental action. I remain comfortable with this understanding yet worry that it is a too-general understanding of how these schemas and cultural repertoires are produced, circulate, and change over time, and that it is insufficiently connected to the details of formal legal processes.

How to bring the narrative arc to its end point? What connects the beginning, middle, and end? I have thought about this, struggling unsuccessfully to conclude within the editorially imposed limit. Here is how I see it. In graduate school, in the 1960s, I often wrote essays arguing that it was more important to know what police were doing than to concentrate so exclusively on what the Supreme Court said. Where did I get this idea that the everyday, on-the-ground activities were where the action is, where life is lived? I am a city girl, lived my whole life in the crush of strangers, moving in every which way, but somehow managing most of the time without disturbing each other. I knew perhaps a dozen people among the hundreds who lived on my block. As I grew up, I navigated my way further and further from this Brooklyn street to larger avenues, eventually across bridges to the yet-bigger city in Manhattan. But this is not just my story, it is the story of all of us. We pass strangers on the streets, we stand and sit—sometimes touching—on the subways

[3]The literature on regulatory implementation has offered much advice over the years about how to achieve more reliable compliance. Here, I am not attacking the practical question as much as the methodological challenge concerning abstraction and generalization and the units of analysis that support the production of an empirically sustainable model of how law works.

and buses, we enter unfamiliar stores and buildings and find our ways to destinations completely unknown before we get there. How do these millions of people know what to do next? I see it all over the world, fascinated by the familiarity within the variation down to such banalities as food trucks, street vendors, and public toilets. We walk without thought, relying on unspoken expectations that people will step aside, not bump into one another. I realize too that this varies around the world; in some places people walk closer to each other than in other places. The local maneuverings are familiar nonetheless. Although most of the stories we tell—scholars, journalists, novelists—are about the disruptions, the tears in the social fabric, I never fail to be amazed. This is magnificent, this social ordering. This is my greatest pleasure, keeps me going to the office every day with enthusiasm, to sit at my desk, a student on the other side, showing them the puzzle: how much of the world is given to us, so predictable, yet so unknown and uncertain. Maybe one of them will tell the story better, help build our periodic table, a more complete model of how the law works. The task persists, even as the future recedes.

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