Open to Disruption

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Experience is not so much what happens to you as what you make of what happens to you.

*Aldous Huxley (paraphrased)*

For all the discussion about the research process in social science, few accounts open up that process to inspection. Rather, most published descriptions of the research process take a standard narrative form: an idea for study, followed by data collection, analysis, and writing. This narrative assumes a smooth, continuous arc that invariably ends with publication (otherwise we wouldn’t be reading it). Someone has finished a project, sent it off into the world, and is now ready to move on to the next item on the academic agenda. The trajectory of the arc appears set from the beginning. The dynamic way in which our craft actually evolves is rarely chronicled and the backstage stories remain hallway talk at professional meetings or uncomfortable silences we keep to ourselves. And, not surprisingly, given the current pressures on academics to publish—pressures experienced especially by those without tenure but increasingly by those with tenure as well—the arcs revealed in published accounts encompass ever shorter periods of time. We could take some of our own scholarship as examples; we could find many more among our colleagues.

The essays in this collection describe work that took longer than whatever might be considered usual, even if that “usual” is only in the mind of the author. These are essays about taking time. But they are *not* essays about planned longitudinal research. Although some of the studies described ended
up taking a very long time (even decades in some cases) and thus came to constitute longitudinal research, in no case was this the original intention.

These essays are about something unanticipated, something disruptive. They discuss the complexities of inventing concepts that, as yet, have no name and no scholarly history. They evoke the excitement of breaking disciplinary boundaries. They explore the consequences of becoming aware of and open to new possibilities for collecting data and to new ways of thinking about, analyzing, and writing up our findings. They acknowledge circling back to work we had considered “done” and allowing ourselves the possibility of thinking about it in a different way. They are about unfinished business that gnawed at us until curiosity or “chutzpah” led to a return to the field. In short, these essays are about what have been called “crucible moments,” those moments when individuals sense that they will continue to grow and their art, “whatever form it takes, will continue to evolve” (Thomas 2008, 17–18). What holds the essays together is that in each case, although sometimes grudgingly in the beginning, ultimately the author not only allowed disruption to occur, but even met it with openness.

Even so, almost to a person, the authors of these essays expressed uneasiness about some aspect of this disrupted process. Susan Bell, for example, speaks of blushing when her work was praised because it had taken so long for her to complete it: “For many years I would not name the starting date, hoping to avoid the stigma of being so slow and preferring to pass as a ‘normal’ scholar, one who managed to publish at least one article or book chapter per year.” Annette Lareau is equally explicit about how embarrassed she was about questions about her book, Unequal Childhoods, during the many years she was working on it. And Naomi Gerstel is surprised by, and still worries about how others read, “gaps” in her impressive résumé.

Writing about this disrupted process was difficult too. And it was not “just” the discomfort of taking longer than “normal,” or of noticing one’s own years of skimpy output, that made acknowledging disruptions publicly so emotionally complex. In some cases the emotional content came from writing about personal disruptions that were private—sometimes deeply painful; sometimes deeply pleasurable; sometimes both. In some cases, the emotional content came from writing about open disruptions to (or near misses of disruption to) significant relationships with people who began as the “objects” of study and ended up being friends. In some cases, the emotional content came from writing about the difficult lives of the people being studied. And, in still others, the prevailing standards of the academy,
combined with one’s own sense of professionalism, made the acknowledgment of oversights and inadequate technique feel like public humiliation. But in almost every case, whether in oral or written form, the authors expressed satisfaction with having had the opportunity to have a say. One author, as she struggled to get her words down on (virtual) paper, said in an e-mail, “This is a new adventurous type of writing for me.” Another commented on a range of issues, including emotional intensity and reflexivity and her fear that the writing might be insufficiently scholarly: “I want to tell you that this piece has been a bit emotionally difficult for me (I imagine this is fairly typical of such long-term involvements), and I found myself really needing to write this piece. But it is highly reflective, and not terribly academic.”

As is the case for the scholar just quoted, these essays are all “highly reflective.” With almost surprising resonance, one to another, they consider how a disruption (e.g., taking more time than usual at one stage or another, circling back to issues they thought had been resolved, learning new methods, or deliberating about word choices) altered the process so that the narrative arc not only felt different but was different from what other scholars described (even if it merely looked longer). And they reflect on how that new arc, at the very least, helped them reach a different level of understanding of their project and, in its most provocative incarnations, helped them write something more serious, more profound, and more honest to themselves and those they studied. A few weeks before she submitted a first draft of the paper she was writing with Carol Stack, Linda Burton commented on her awareness of these issues: “The paper looks different than I thought it would look, but I love it. In it you see the real Carol and Linda and how we struggle with making sense of our ethnographies and doing justice to the lives of girls, women, boys, and men in our studies in the context of the politics of science.”

There are some common themes. From one perspective, what several write is really quite simple: they say that once the research process was disrupted, one thing led to another. That is, no matter what the initial reason for the disruption—and these reasons varied—the earth didn’t stand still either during the time it took a scholar to find her or his intellectual footing or during the hiatus before a scholar returned to an unfinished project.

Disruptions created an ever-moving research target. Albert Hunter wrote about the same place for many years, but that place underwent such major transformations that in a very real sense it no longer was the same place. The
Hare Krishna movement Burke Rochford joined in prayer in 1975 had transmuted into a major world religion with generations of congregants by the time he wrote about child abuse there. The women Bell interviewed as adolescent “DES daughters” faced their own problems of giving birth to healthy children as they matured. The maid’s daughter, a twenty-three-year-old college graduate when Mary Romero first met her, was a middle-aged adult with children of her own when Romero ended the project twenty years later.

The world around these “subjects” changed as well. Joanna Dreby comments movingly on the changing significance of being documented, as the immigrants she studies worry more now than they did half a dozen years before about being deported. Timothy Black captures how individual lives intersected with broader social systems and the changes that occur over time (such as changes in the labor force, the growing role of the criminal justice system for marginalized people, and the increasing social insecurity that characterizes the recent past). Marjorie DeVault wanted to “study—and bring into view—some of what women knew but didn’t have words for.” Pamela Stone starts out with a subject no one wants to fund, and ends up writing about a phenomenon that had central billing in the *New York Times Magazine* as well as on multiple blogs. Meanwhile, new technologies became both the means for keeping up to date with these changes and the subject of these changes themselves.

Especially intriguing are the discussions about the changing nature of relationships with those individuals who constitute the topic or subject, if you will, of the research. Lareau worried that “Mr. Williams,” grouchy and exhausted after a long day of work, would one day “come home, get mad at his wife, and end the study.” Karen Hansen’s experience of a long period of research was different: she found that her repeated visits to the North Dakota reservation where she was collecting data meant that, after some time, she was no longer a “stranger” to those she interviewed. Hunter writes, “Many of the people who would enter into my research as subjects were the same individuals I had known in my youth, yet they were now different people.”

As relationships stretched out in time, they also deepened, to varying effects. Some scholars, like Burton and Stack, felt they could get to the heart of certain issues only because of their long-term relationships with the people about whom they were writing. Some felt their responsibility more deeply and that sense of responsibility slowed them down or gave them pause. Some could find no easy way to finish a story; some found that their
research relationships morphed into a quite different set of ongoing friendships and responsibilities in everyday lives that had no endpoint. Of course, these changing relationships with respondents cannot be separated from the changes experienced by one’s self. Dreby, for example, writes, “Every return to this place is an exercise in self-reflection, of how I have changed as a mother and a woman, of how my relationships have changed, and of how my children have grown across these movements through time and place. Returns are highly personal. They involve influential characters in my formative years, tugging at my insides for reasons I cannot explain.” And while Dreby talks about her “returns” as being “scholarly returns,” it is clear that there is something more to it than that: each time she returns to the site in which she conducted her research, she finds herself reflecting upon the length of time she has known each family, recalling the life events she has shared that mark her relationship to the people, and considering both how she has aged and her children grown. These changes in subject matter, the world, and ongoing research relationships are the focus of Part I.

The intellectual world in which scholars wrote altered as well. Analytic frames changed under scholarly feet. Gerstel notes that what was once “sex roles” became reconceptualized as “gender”; understanding the family increasingly involved looking beyond relations between spouse and partners or parents and children to examine kin connections. Bell describes how daughters exposed to DES in utero created new knowledges, institutions, and practices “rooted in their embodied experiences of having cancer, worrying about having cancer, becoming and not becoming mothers, and the lifelong effects of endocrine-disrupted, synthetic chemical–infused bodies.” Hunter’s understanding of the significance of Evanston’s becoming “wet” (after decades of being a “dry” town) relies on a student’s insight and the cultural symbolic turn in contemporary sociology. Burton and Stack indicate that the discourse on men in families, throughout the 1960s and beyond, never quite provided them with an audience that would listen, without a political ear, to what they were observing in African American families. More specifically, their chapter reveals that the scholarly literature on African American men locked those men into “public personas” that were difficult to challenge with ethnographic data. Just as Burton and Stack had to wait for a shift in politics before they could present their analyses of African American male youth, Hansen had to wait for the theoretical concept of settler colonialism before she could use it to frame her study of Scandinavians and Native Americans at Spirit Lake.
Changes in prevailing methodologies were equally, if not even more, common. A project begun with the notion that one could apply an almost positivist vision of methodology ended with an awareness of, and reliance on, a more self-reflexive stance, encouraging participants to become partners of sorts in the research process. For some, this meant throwing out the guidelines that had characterized their graduate training. When Romero’s interviewee, Olivia, took over the interview and steered it the way she wanted and for as long as she wanted, Romero sat back, initially stunned, and only later realized she had stumbled onto another approach to interviewing, one that in this case opened up rich narrative material. Will C. van den Hoonaard found that his old understanding of cartography was no longer sufficient and that he had to dive into the churning waters of interdisciplinary—and international—study. Stone, familiar as she was with a quantitative approach, had to retool to understand why women had “opted out” of their careers. DeVault discovered conversation analysis and learned to evaluate “talk” in a new way. And Bell moved from the standard use of the interview—decontextualizing the excerpted quote—to a narrative analysis of those interviews along with visual materials and performative evidence. These changes in analytic frames and methods constitute Part II of the collection.

In some cases, what we call “disruptions” were, in and of themselves, the direct cause of changes in perception and understanding. Emily Abel, for example, came to understand religion in a new way as she dealt with the aftermath of breast cancer; that new understanding seeped into her analysis of the nineteenth-century caregivers she was writing about. Hunter experienced his community in new ways as he raised a second family. And a number of others, including Gerstel, Hertz, and Bell, comment, albeit in different ways, about how becoming mothers altered their understanding of women’s lives and issues of care.

In still other cases, what we call “disruptions” had fewer direct but no fewer consequential effects. Delays helped produce confidence in an individual voice; as they matured, scholars became more certain that they wanted to tell the story that made sense to them rather than the one their respondents might want them to tell. Hansen bristled when a respondent answered her question “Is there anything [else] I should know?” with the sharp retort, “Ask me. You’re the one who wants to know. Ask me. If I know it I’ll tell you.” Over time, she came to an alternative understanding and embraced ownership: “Ultimately, the interviews were mine. I directed the
conversation; I asked the questions; I was motivated to publish. My challenge was to ask the right questions.” At the same time, some scholars also became more humble about their own abilities and aware that the story that makes its way into print is always going to be partial—the best anybody could do at a given time. And some became more frustrated with separating their activism from their academic activities; some allowed their political beliefs to inform their scholarship more openly as they became more fully engaged in what our students call the “real world.”

Some disruptions were less the result of external events than created by scholars themselves. Scholars invented terms to describe what existed but as yet had no name, as did Gerstel when she wrote about “commuter marriages.” Scholars also expanded or stretched the meaning of current terms, as did DeVault as she gave substance and meaning to the everyday language of “feeding the family” and advanced feminist understandings of “invisible work.” And scholars breached disciplinary boundaries, as did Hansen when she navigated a route through the fields of sociology, history, and the study of Native peoples. As people moved beyond their comfort zones, these kinds of disruptions created deep internal turmoil, even as they reverberated out to alter the intellectual landscape. The reconsiderations occasioned by or introduced through disruptions are the central theme in Part III.

To be absolutely clear, little of this happened by design. For the most part, disruptions were “purely” serendipitous. Margaret Nelson is surprised to find that the same person has been caught three separate times in her research net; Rosanna Hertz is taken aback when the child of the family she lived with twenty years before in another country almost literally shows up on her doorstep. Dreby is astonished to realize that the child she has come to interview was not yet born when she attended that child’s mother’s baby shower, a moment memorialized in a photograph of Dreby “measuring Leticia’s round belly with sheets of toilet paper.” Stone drops a child off at a playdate and muses about why the mother is no longer employed. DeVault pats herself on her back when she is applauded by an audience of Taiwanese faculty and graduate students, only to discover a few days later that they had not understood a key phrase in her talk. Each of these serendipitous moments made authors reflect back on what they had done before, as well as propelling them forward into what they had to do next.

These reflections offer ironies to be savored—and ironies to be regretted. Gerstel explores inequality wherever it appears in the lives of the workers she interviewed and acknowledges that she can be attentive to this issue because
of the “cumulative advantage” of a paid semester off to fashion a major grant proposal and a sabbatical year at a posh research center with the luxury of uninterrupted time to focus on writing up her analysis. And she writes about disruptions in both her life and in the lives of the men and women she studied. The events of 9/11 would have disrupted the ability of Ari (the son of the family Hertz had lived with two decades before in Israel) to work in the U.S. underground economy; they also made Hertz pause before agreeing to sign legal documents saying that Ari was working for her family. The same events made Stone wait a year before conducting more interviews because she did not want an artificial emphasis on family. A decade later, as Stone arranged space in her busy life to write about the disruptions that afflicted her study of disrupted lives, Hurricane Sandy threw her careful arrangement to the (literal) winds.

These various themes combine to produce a deeper story about the comfort and protection of the “usual” narrative arc (whether or not it is ever fully enacted) and, by way of contrast, the risks and uncertainty of the disrupted one. In its own irony, the pressure to get work out—imposed by a dissertation committee, a promotion review, an editor’s deadline, an internal time clock—creates safe space: we each do the best we can before a project is due; the ticking clock creates a familiar structure. So, too, do the constraints of disciplinary boundaries and well-worn concepts. Disruptions do not just lengthen the research trajectory. They also bump us out of that safe space into a place without clear-cut norms. Does anyone know what rules apply when we stop asking, “How can I get this done?” and start asking an entirely new set of questions: How do I know when this is good enough? How do I decide when this story is finished? Now that my usual methods have been debunked, what approach do I take? Have I used the right words to describe the phenomena I observed? If mistakes were made, and could be made again, how could I possibly be secure in my judgment that this time I have it right?

Gerstel writes, “So many of the synonyms the thesaurus gives for ‘slow’ indicate problems.” They also stigmatize. The meanings of “judgment” do not carry the same connotations, but they are almost contradictory as they range from sparkling capacity (“genius,” “grasp,” “incisiveness,” “ingenuity”) through reasonable skills (“rationality,” “sanity,” “soundness”) to a cautious approach to the unknown (“prudence,” “apprehension”). Because you cannot teach judgment, these essays are not advice manuals; they don’t tell us
how to become better at what we do. But they do tell us about occasions of learning to become better by deviating from whatever rules guided us in the past. They tell of risks taken—of working past a cautious approach to the unknown, through reasonableness, to achieve something newly incisive, newly ingenious. There is no moral. But there is open acknowledgment of the chaos of normlessness and of the emotional intensity experienced in the midst of that chaos. And taken as a whole, and even if the authors do not acknowledge it themselves, these essays are testimony of the solid, good judgment that ultimately emerged as these wise scholars drew on their own maturity and their own life experiences to find their way out of that chaos.

In the physical sciences and math, it is said, the best work is often done when one is in one’s physical prime. Lareau fears something similar could be true in the social sciences, not because of the benefits of youth per se, but because it is only early in one’s career (maybe even while collecting data for a dissertation) that one can find the uninterrupted time to engage in extensive fieldwork. Taken as a whole, both Lareau’s own scholarship—and the essays included here—suggest a more complex understanding of age, time, career, and achievement. And, we would add, we think that this understanding does not just rest on the fact that so much of the research described here relies on qualitative data. As Gerstel notes, “doing both qualitative and quantitative analyses in a focused way demands constant revision and rethinking” (emphasis added), a sentiment Stone seconds. Nor does it rely on the fact that these issues are raised by sociologists: scholars in other fields (some of which are represented here) might also experience disruptions in their lives, their understanding, and their methods; scholars in other fields might find that the ground they thought was novel or new had already been cultivated by someone else or that the burning question of one day appeared far less significant at another moment.

Time alone does not produce better scholarship. Scholars could take (and have taken) years without producing anything better than what they started out with. And not all disruptions have happy or positive outcomes. But what the authors of these chapters intuitively knew when they were invited to write (or were cajoled into writing) for this collection is that while the disruptions to their scholarship led to their taking an unusual amount of time to finish a given piece of work, it was their openness to those particular disruptions and their subsequent willingness to accept normlessness that opened up opportunities for them to develop their craft. In the end, then,
the projects in this collection are testaments to authorial mindfulness, self-reflection, humility, risk-taking, and honesty. They are the products of time and craft, disruption and openness.

In higher education today, much of this—the time, the craft, and the openness—is threatened by the emphasis on productivity and the reliance on external grants obtained by individual faculty members. We have seen the speedup ourselves, even in our own liberal arts colleges where what was once sufficient for achieving tenure (excellent teaching and an article or two) is now laughable. Of course, excellent teaching remains a criterion, but on top of that is the expectation of at least one book and a new research project well underway with published articles in peer-reviewed journals. Increasingly, both colleges and universities now toss aside the first book (mere dissertation output) and require a second. And in both types of institutions, the counting continues unabated for each year of one’s career: Is there enough here for a raise? Has this person met the standards for promotion to full professor? Is there sufficient new scholarship to help the department maintain its status in the world? This type of thinking has the potential to extinguish the kind of valuable scholarship described in these essays. Moreover, even as money is drying up in the social sciences, and the awarding of grants in those disciplines has become more competitive than ever before, faculty members are judged by (and rewarded according to) whether or not they have supported their institutions in just this way. But most grant applications assume more knowledge, and the ability to craft more specific hypotheses, than is possible in the initial phases of the kind of exploratory research showcased in these essays. Of course, even under the conditions of speedups and expectations about external funding, research trajectories will still be disrupted, but in the absence of the safety net the academy has provided in the past, these disruptions will be occasions of endurance alone, rather than fertile ground for innovative ideas.

Reading the chapters of this book, we have come to believe that the academy could even go beyond providing a “mere” safety net. We suggest changes that could be made by institutions—and by the individuals who work in those institutions—that might encourage the production of the kind of innovative scholarship described here. We acknowledge the utopian quality of some of these ideas; we believe that utopian thought can provide a framework for productive discussion and effective action.
As part of the speedup, most academic institutions (our own included) now require regular reviews of each faculty member’s scholarly accomplishments. We see major problems here: in the frequency of these reviews, in the criteria for what “counts” within them, and, conversely, in what gets overlooked by current counting practices.

The first of these could be easily addressed. Reviews might be less frequent; they might also be initiated by an individual faculty member rather than occurring according to some preordained time frame. One factor that might be taken into account as institutions and individuals decide when the next evaluation will occur is whether or not a faculty member has experienced a type of disruption (e.g., divorce, the birth of a child, illness) discussed in some of these essays. Clearly, it is inappropriate to apply the same time frame in all cases.

The other questions raised by institutional reviews are even more complex and troubling. Institutions vary in their practices. But in some institutions (including both of ours), faculty members are asked to report only on those pieces of scholarship “published and appearing” at the moment of the review. This emphasis on publication means that a project in the works is ignored even though it represents considerable effort. Similarly, a project in the pipeline (e.g., accepted for publication in a journal that has a large backlog) cannot be mentioned even though it might represent the culmination of many years of work. Alternatively, assessment materials might ask for information about projects begun, progress made, and projects finished during a particular period. In that way, institutions would be acknowledging that no one collects data and writes a book or article of any merit in a short period of time. Such practices would allow scholars to find the best form for their material (i.e., whether it should be presented in a book or an article) and not just the one that could be produced (and then published) most quickly.

This current emphasis on outcome over process is especially problematic because, as so many of the authors represented here suggest, speed is particularly difficult for scholars who collect their own data. They write about how long it takes to discern the rudiments of different cultures, to form relationships with informants, to develop trust, and to “hang out” with respondents. They indicate also how long it takes to craft adequate research memos, to transcribe interviews, and to visit archives. These practices have no parallels in the scholarship of those relying on the secondary analysis of data. And for those who have not been able to garner the external funding to underwrite
research assistants and otherwise facilitate data collection and analysis, the time necessary to accomplish those tasks can stretch on indefinitely. Of course, whether one is molding qualitative or quantitative data, craft takes its own sweet time. None of this time is acknowledged when completed publications are all that count.

Academic institutions are also increasingly using criteria we find problematic. One of these is the special attention paid to the ordering of names, and the accounting of the unique contributions of each person, on a coauthored piece. The scholars writing here have suggested that coauthorship can be a source of intellectual enrichment; others note that coauthorship can help them overcome the loneliness of being the sole representative of a field at a given institution. (The three editors of this collection are all well aware of these benefits.) When we list others on our publication, we frankly acknowledge that no scholar works alone; when we put down our names alone, we neatly conceal that fact. Current institutional practices not only promote the latter practice of concealment, but also have the potential to turn collaboration into competition and to encourage individuals to make claims of ownership over ideas that have collective origins.

Most recently, at one of our institutions, as is already the case at so many, the “impact factor” of a journal is being used to assess the quality of our research. Our concern here stems directly from issues raised in the chapters in this volume. Particular topics included here (such as Stone’s analysis of the labor-force behavior of professional women, and Burton and Stack’s studies of the caretaking actions of young, African American men) did not initially receive the credit they deserved because they did not fit with prevailing political agendas. Similarly, what was once regarded by many as being marginal (such as DeVault’s investigation into what a family ate) or invisible (such as Romero’s forays into the experiences of the maid’s daughter) have now become central topics of inquiry, but they too were initially mocked. For some of the authors, this mockery led to destructive self-doubt. But in so many cases the initial judgments were particularly misleading. While we understand that those working in the “hard” sciences might also have their work poorly evaluated when they buck prevailing frameworks, we believe that biased assessments are probably issued more frequently in social science. Hence it is especially likely there that individual articles might in the long run turn out to be far superior—or far inferior, for that matter—to a journal’s current or future reputation. Not surprisingly, at all but the top-ranked institutions, administrators are eager to proclaim that the yearly rankings by
the *U.S. News and World Report* should not be viewed as definitive; we suggest that these administrators might be equally willing to regard “impact factor” assessments of journals with skepticism.

As we discuss the issues of what counts, and how it is counted, we need also to consider the issue of what gets obscured in assessment practices. Here we draw attention first to two areas of concern drawn from our own experiences over the past few years (choice and collaboration) and then to two distinctive areas of concern (skill acquisition and activism) raised by other authors in this volume.

Our own experience first. Over the past several years, as we worked on this manuscript, as well as on other projects while fulfilling our teaching obligations, we have had to make careful choices about resources of both time and money. Both have limits: we did not submit papers to conferences that conflicted with important moments in our teaching schedules, we withdrew conference submissions because we had already run out of money, and we failed to take advantage of opportunities to conduct interviews with respondents because we had neither the time nor the money to travel easily to their homes. When we are not asked about opportunities foregone, but only about those “accepted,” we look less productive (and less successful) than we know ourselves to be. Even more important for our intellectual development is our participation in collective endeavors that play no role in reviews. We have already discussed coauthorship and its benefits. Other such occasions (e.g., study groups) allow individual scholars to keep abreast of changes in the discipline and, in a democratic fashion, bring together junior and senior faculty on a topic of common concern. In fact, some of those represented in this collection initially met through such opportunities and one of us recently spent considerable effort in re-creating this opportunity for a new group of scholars. But whereas postdocs are (now) available at the early stages of one’s career, the efforts to create and participate in less formal networks have no place on one’s CV.

The authors in this volume highlight two other issues. First, adequate assessment of scholarship might also ask whether a particular project required—or led to—the acquisition of skills such as learning a new methodology or language, embracing a new analytic framework, or crossing into new disciplinary territory. Ultimately, of course, an institution profits enormously when a faculty member engages in this kind of professional development: new skills can enhance a faculty member’s teaching; the institution can bask in the reflected glory of a scholar’s pathbreaking work. At present,
however, the initial costs of these self-improvements are borne by the individual faculty member alone and they are totally ignored when the counting begins. The same is true of shifts in direction of interest: a scholar hired to teach a particular area might develop into one who can teach and publish in a far broader range of intellectual activity. Yet, because current practices of assessment discourage these shifts, faculty tend to become more and more narrowly focused. Curiously, for those of us at liberal arts colleges, incentives are available for the development of new courses. However, what we—and those elsewhere—need are equal incentives for the development of new areas of intellectual inquiry.

A couple of the authors in this volume make an additional, important point about activism. Bell’s chapters for *Our Bodies, Ourselves* and Hunter’s civic engagement in his local community have helped educate the broader public; while other contributors to this book do not explicitly mention this kind of activism in their essays, we know of them providing expert testimony and writing op-eds. But these activities are considered free-time hobbies or the pursuit of personal aggrandizement rather than essential uses of professional knowledge.

In short, we are suggesting that institutions do more to encourage and support risk-taking and innovation. As we have indicated, we believe that many current policies do just the reverse, even though some institutional policies encourage students to take risks as they make the transition from high school. At one of our institutions, for example, during the first semester of college, students are given shadow grades in a “pass-fail” system so that they can exercise new intellectual muscles in a stress-free environment. A parallel kind of support might be available to the faculty who are finding their way in new fields or embarking on particularly complex projects.

To be sure, the proposals outlined here cannot ensure positive results for every new endeavor: not all new projects will be fruitful; some projects will never see the light of day. Moreover, although we have emphasized the gains to be realized through supporting creative intellectual activity and responding well to the kinds of disruptions scholars experience in their real lives, we recognize that some “disruptions” are just that: caretaking, illness, depression, and bereavement do not necessarily bear intellectual fruit. Institutional openness to disruption must embrace these realities as well.

Finally, we believe that a more “open” institutional stance will need to be balanced by a more “open” individual stance. We acknowledge that the kind of scholarship described in these essays is unlikely to be a possibility
before tenure. Quite simply, the risks are too high. But difficult as openness might be even after tenure, we do encourage it. The scholars represented here came from many different types of institutions; they also had varying levels of prominence before (and after) the disruptions they describe. As these essays show, they all paid a price for bucking the system: for some, the price was the invisible one of shame and embarrassment when their individual timetables did not conform to institutional expectations; for some, it was the equally invisible one of experiencing a normless chaos. We have all internalized the academy’s demands, and this internalization keeps us in line and unwilling to take some potentially worthwhile leaps into the unknown. “Take your time” and “trust yourself” are the easy mantras of advice manuals. Needless to say, they are far more difficult to enact. They are also impossible to dictate.

In the last essay in this volume, Lareau suggests that this collection of essays itself might become the “cheering team” for scholars stumbling along their way through complex research projects. It is in this spirit that we initially embarked on this project. We will be gratified if, in its final form, scholars rely on it for their own encouragement. We will be gratified as well if scholars use it as evidence that institutional change is necessary. We cannot forget that because institutions merely “rent” our reputations, but do not own them, our individual enhancement serves their purposes as much as (if not more than) our own.5

When we embarked on this project, we called it simply “slow sociology” and we made reference to Stacey (2007) and to the slow movements in other areas of life (e.g., slow cooking, slow journalism). We thought of the positive impacts on our lives when we slowed down—when we allowed ourselves to take the time to “get it right”; we also believed that others could profit from our experiences. We discussed these ideas at panel presentations at two recent Eastern Sociological Society meetings and the word spread. Other sociologists also wanted to write about their own experiences with “slowness,” as did a historian (Emily Abel) and an anthropologist (Carol Stack).

We initially organized the chapters of this volume into three sections, each referring to a way that the normal arc of the research process could be disrupted: at the initial stage of data collection (resulting in a long time in the field); during the period of writing and reflection (resulting in a long gestation); and at the point of reflection on, or reassessment of, initial ideas (resulting in reconsiderations). We solicited essays from scholars to “fit” into
one of these designations. As the essays came in (and maybe even before then), we came to realize that these were not clear delineations: as our discussion above suggests, one thing led to another, and a disruption for any reason might lead to another form of disruption.

We didn’t entirely throw out that initial framework, but have reconceptualized its elements to work in conjunction with the themes we found in these essays, even as we acknowledge that no individual essay fits squarely in one—or rather only in one—section of the collection. Even more significantly, as we read the authors’ chapters for this volume, we recognized that slow sociology was not a broad enough focus. Various essays (as well as our discussions of those essays) continue to use the concept and language of our initial formulations. However, taken as a whole, these essays go well beyond slowing down.

Notes

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Epigraph: The exact quote reads, “Experience is not what happens to a man. It is what a man does with what happens to him” (Huxley 1990). We chose gender-neutral language.

1. Other collections that open up the research process in different ways include Hammond 1964; Glassner and Hertz 2003; Deflam 2007; and Goetting and Fenstermaker 1995. See also the “Backstage” column that Rosanna Hertz and Naomi Gerstel edited for Contexts (2004–2007).

2. See, for example, Callaway (1992, 33), who described the process as “a continuing mode of self-analysis and political awareness.” For a more theoretical piece on feminist reflexivity, see Hemmings 2012, which addresses the “affective turn” that feminist theorizing has taken over past years.

3. We are grateful to an external reviewer for reminding us of this point.

4. In both of our institutions, assessments are made by college-wide committees that include representatives from a broad range of disciplines. This seemingly democratic practice can also cause particular problems for social scientists. The fact that everyone thinks they can assess that scholarship—because “good” social science should be readable—makes us particularly vulnerable to the evaluation of being trivial (mere common sense) and epistemologically problematic.

References


