CAMBRIDGE
UNIVERSITY PRESS

RESEARCH ARTICLE

Theorizing Subdisciplinary Exchange: Historical Sociology, Ethnography, and the Case of SSHA

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Abstract
What happens at the point of interchange between scholarly communities? We examine this question by investigating the case of growing ties between historical sociology and ethnography, two social scientific methods that once seemed to have little in common. Drawing on methodological writings by ethnographers and original interviews with practicing historical sociologists, we argue that these ties have been shaped by structural and methodological homologies between the two disciplines. Structurally, ethnography and historical sociology are similarly positioned in sociology more broadly, as enterprises with sometimes-tense relationships with dominant assumptions of the social sciences. Methodologically, both ethnographers and historical sociologists face the challenges of bounding the research process, navigating access to data, analyzing and retaining data while "in the field," and overcoming cultural distance between themselves and the worlds they are studying. Taken together, these findings extend work in the sociology of science and knowledge and suggest some key conditions for intellectual efflorescence.

Keywords: ethnography; historical sociology; homology; knowledge; trading zones

Introduction

The Social Science History Association (SSHA) is widely recognized as a premier organization for historical sociologists (Abbott 1991). In recent years, however, ethnographers have also begun to attend the conference in force. Conference programs are now peppered with high-profile author-meets-critics sessions on books by young ethnographers such as Claudio Benzecry, Marco Garrido, Alice Goffman, and Iddo Tavory. The names of countless other ethnographers can be found throughout the programs of recent meetings, and in 2018, the association even elected as president Frederick Wherry, a sociologist best known for his ethnographic investigations. More curious still, these ethnographers are not *only*

those who have a greater interest in macro-structural or comparative ethnography, but hail from across the contemporary field.[1]

That ethnographers attend SSHA might be surprising. In the popular scholarly imagination, historical sociologists address *past* events and processes, while many proponents of ethnography see it as defined by observations of the *present* – to "subjecting yourself... to the set of contingencies that play upon a set of individuals" (Goffman 1989:125; see also Garfinkel 1984; Jerolmack and Khan 2014). At first blush, moreover, the methods the two groups use seem to have very little in common: historical sociologists hunker down with archival documents, secondary histories, and historical reports; ethnographers explore the land of the living, engage in interviews and casual conversations, chronicle people's worlds and lifeways, and often engage in exciting – even salacious – acts of derring-do (e.g., Goffman 2014).

In terms of their subject matter and their methods, therefore, historical sociologists and ethnographers appear to occupy remarkably different parts of the academic sociological terrain. Yet at the same time, ethnographers attending SSHA plainly shows at least *some* engagement between ethnography and historical sociology. And this contact seems fruitful: recent years have seen the publication of award-winning books that merge ethnographic and historical methods (e.g., Kim 2016; Pacewicz 2016; Reyes 2019), and there is a burgeoning genre of work seeking to apply methodological insights from one field to another (Abramson and Gong 2020; Lara-Millán et al. 2020; Lichterman and Reed 2015; Pacewicz 2022; Reed and Lichterman 2022).

In this paper, we identify the conditions for the unlikely contact between historical sociology and ethnography and also describe some substantive features of that contact. Our evidence in this undertaking takes advantage of the fact that ethnographers write self-consciously about the *practice* of ethnography a great deal; we therefore draw on a survey of these reflections. Historical sociology, though, is different. While there is considerable methodological writing in the field, it is rare to find systematic reflections on the practical conduct of historical sociology. Accordingly, we draw upon an original set of interviews with historical sociologists about their practices and stances within the field.

Using these two different forms of evidence, which nonetheless represent equivalent levels of analysis (descriptions of research practice and orientations within their respective intellectual fields), we argue that the conditions of possibility for contact between ethnographers and historical sociologists come from their similar structural position within sociology. Intellectual orientations in both are partially structured by a tense relationship to the dominant epistemology and ontology of the social sciences. Yet if shared tension with the social scientific mainstream supplies the *possibility* for exchange, the *substance* of the contact is the

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[1]Ethnographers who frequent SSHA received their PhDs from most sociology departments that regularly train academic ethnographers. A full accounting of these ethnographers' intellectual commitments is beyond the scope of this article, but we note that many were not trained in styles of ethnography, like the extended case method, that emphasize history and macro-micro connections. On the contrary, many were trained in departments more commonly associated with the Chicago school and symbolic interactionism, which ethnographers see as especially focused on observing interaction in the present (Jerolmack and Khan 2014).

product of overlapping methodological challenges: how to bound the research process; navigate access to, and analyze and retain, data while "in the field"; and span the cultural distance between themselves and the worlds they study.

Our investigation proceeds as follows. We first develop an analytic vocabulary to describe the origins and nature of disciplinary contact; then we discuss our methodological approach in greater depth. After that, we describe the scholarly orientations (and tensions) within ethnography and historical sociology, before presenting the shared methodological challenges in the field. We close by reflecting on the implications of this investigation for historical sociology and ethnography, as well as the study of disciplinary contact generally.

Explaining contact among subdisciplines

Why would the SSHA, an organization oriented to social science *history*, increasingly attract ethnographers, whose methodology is generally not known for its engagement with archives or historical approaches? Aside from its intrinsically puzzling nature, the question is an instance of asking how and why certain disciplines and subdisciplines come into dialog with one another.

One starting point to think about such exchange comes from historians of science, who have argued that interdisciplinary exchanges should be understood as *trading zones* where *local* agreements over the meaning of scientific procedures or artifacts can be worked out between scholars who hail from (sub)disciplines with significantly different concerns or ways of conducting research (Galison 1997, 2010). These agreements need not detail at length what is being exchanged or why; indeed, it is the very thinness or partiality of these exchanges that allows scholars to coordinate their actions despite the differences that otherwise exist in their normal (sub)disciplinary ways of doing and valuing things. To understand the process of interdisciplinary exchange is therefore to understand the history of the thin local agreements in trading zones – and how they do and do not come to cohere over time.[2]

This somewhat narrow focus might imply that "anything goes" (Feyerabend 1975) in these trading zones so long as it facilitates exchange, but others have emphasized regular patterns within the apparent chaos of "interdisciplinarity." For example, Abbott (2001a) describes a complex process of differentiation whereby the ongoing repetition of familiar intellectual oppositions supplies a hidden order to disciplinary and sub-disciplinary debate. Likewise, Jacobs (2013) argues that, while the delimitations among and within disciplines may appear arbitrary, they in fact reflect those disciplines' extraordinary success at institutionalizing themselves, and that interdisciplinary contact usually represents battles over newly discovered intellectual territory. Finally, for Frickel and Gross (2005), the continuing dynamism of science is explicable not necessarily because of the "truth value" of

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[2]Perhaps because Galison (1997) introduced the notion of "trading zones" to resolve debates over the unity of science between logical positivists and some post-positivists, the strength and substance of the ties in a trading zone are deeply ambiguous. Galison sometimes says that these ties can account for the overall strength and resilience of science as a whole (e.g., p. 844) and accounts for the success of some trading zones by referring to macro-sociological events (such as World War II, pp. 816-827). Yet at other times, he stresses the historical fragility of trading zones (p. 805) or the need for a shared "story" spanning differences among participants to facilitate success (pp. 815-816)

its substantive theories, but rather because of the pathways carved out by "scientific and intellectual movements" – that is, relatively well-organized bodies of scholars fighting to establish paradigms in the face of intellectual and institutional resistance. From the standpoint of our investigation, all of these approaches point in the same direction: agonistic forces of intellectual politics drive *both* exchange *and* fragmentation at (sub)disciplinary boundaries.

These conditions for cross-(sub)disciplinary exchange in turn are structured by relations to the centers of gravity in disciplines themselves as well as even broader sociocultural factors. Bourdieu (1975, 1985: esp. 736-739), for instance, argues that connections across (sub)disciplines result from similar structural positions of those (sub)disciplines vis-a-vis the social scientific mainstream – and even political-economic and cultural assumptions in society at large (Steinmetz 2005). At this widest angle, historical sociology and ethnography may be drawn together because of a homology between their positions in the social field of sociology, as practitioners in both subdisciplines realize that similar questions are being asked in each. In other words, given fragmentation, various forms of homophily (at various disciplinary costs) are possible (McPherson et al. 2001), and we would expect to find substantial overlap in the intellectual approaches undertaken by historical sociologist and ethnographers.

This is a necessarily multi-layered view of interdisciplinary exchange, and so in what follows, we argue that such exchange is facilitated by *both* structural *and* methodological homologies. This synthetic view extends each of the three streams above in important ways. With studies of social-intellectual movements and the disciplines, we incorporate the social and institutional bases of intellectual life. At the same time, we also emphasize how sometimes-general socio-intellectual processes are experienced at the "ground level" of practical research problems, and how any given research must therefore be seen as doubly embedded in both disciplinary and phenomenal domains. Like Bourdieu, we argue that studies of intellectual politics require attention to field-wide structural forces. Yet we also extend beyond Bourdieu's emphasis on *structural* homologies vis-à-vis the mainstream to the *methodological* homologies which form the content of exchange.

Finally, our synthesis also enriches Galison's concept of trading zones. Galison emphasizes the deeply practical necessities that shape subdisciplinary exchange, but we argue that structural and methodological homologies also affect trading zone activity. These homologies, moreover, do not only coordinate exchange; they also feed back into the subdisciplines themselves, sometimes productively altering their course. In sum, trading zones are more than *thin*, perhaps even fleeting ties across boundaries; they can also be relatively *thick* and enduring connections, motivated by lively struggles with concrete research challenges.

Data and methods

With this analytic perspective, we delve into a concrete case study of how ethnographers and historical sociologists think about methods and themselves as social scientists. This kind of case study is not oriented toward explicit causal inference by comparison; rather, its object is to trace the various processes through

which certain outcomes are possible (George and Bennett 2005). Since our argument is that similar structural conditions motivate exchange, and that this exchange is made up of the experience of grappling with common methodological challenges, the logic of our argument demands that we demonstrate a fundamental *similarity* between the structural positions and methodological challenges of the two fields.[3]

To do this, we draw on two distinct bodies of evidence that nonetheless speak to the same level of analysis. While both ethnographers and historical sociologists have published discussions of their structural positions vis-a-vis sociology's mainstream, one crucial *difference* between the two fields is the extent to which they discuss concrete research practices in print. Ethnography is blessed with a rich tradition of methodological appendices and other reflections, and we draw upon canonical statements about participant observation methods by leading ethnographers. Unfortunately, no comparable tradition of methodological reflection exists for historical sociology, where the methodological literature is dominated by stylized, normative reflections on the relationship between theory and evidence, with little attention paid to the nuts-and-bolts of historical research (Mayrl and Wilson 2020b).

Accordingly, to examine the methodological practices and challenges of historical sociologists, we rely upon original in-person interviews with practicing historical sociologists. Between September 2014 and December 2018, we conducted semi-structured interviews with 68 historical sociologists about their research practices. All participants held a Ph.D. and faculty position (or were recently retired) at the time of the interview. Our interviews inquired into the entirety of the research process from design to publication and were designed to tease out the practical factors that shaped historical sociological research. We did not, however, explicitly ask about parallels with ethnographic methods; the fact that so many of our respondents did draw such parallels (as will be seen below) was a spontaneous and unexpected finding.

Participants were recruited through a mixture of convenience and snowball sampling (Weiss 1994). To maximize reach across career stage and institution type, we randomly selected twenty names from the 2014 membership list of the American Sociological Association's Comparative-Historical Sociology section. To further maximize our sample's diversity, we supplemented these names by approaching fifteen historical sociologists known to us through personal networks, whose work dealt with a range of topics and themes using archival, synthetic, and quantitative strategies. At the end of each interview, we asked our interviewees to suggest additional historical sociologists we should speak with. Because the population of historical sociologists is relatively modest in size and features dense personal networks, however, our two samples rapidly converged. We thus sought out

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[3]We do *not* believe we need to show that these challenges are necessarily *experienced* in exactly the same way in both ethnography and historical sociology. Indeed, as we discuss in the conclusion, one fascinating challenge of this investigation itself is its embedding in the very homologies we describe. As such, we expect that part of the institutionalization of the connection between ethnography and historical sociology will be the articulation of similar methodological concerns in ways that facilitate the experience of them in increasingly similar ways.

additional interviewees representing a range of theoretical, methodological, and substantive approaches. Our final sample thus includes historical sociologists from a variety of career stages and kinds of institutions, with varied substantive and methodological foci (for further details, see Mayrl and Wilson 2020a).

Structural homologies between historical sociology and ethnography

Historical sociology and ethnography share a paradoxical structural position within the broader field of sociology: simultaneously dominated and elite. While both methods are probably somewhat overrepresented at elite departments,[4] the lion's share of symbolic capital within sociology accrues to quantitative and "scientific" methods associated with methodological positivism (Steinmetz 2005: 122). Despite their differences, both historical sociology and ethnography share an epistemological estrangement from this positivist mainstream.[5] In historical sociology, archival and content analysis methods predominate, while in ethnography, participant observation and interview methods are the norm. As a result, while both fields do indeed have more positivistic manifestations, in general historical sociologists and ethnographers have both long struggled with skepticism from the sociological mainstream.

Historical sociology

Historical sociologists have a long tradition of remarking upon their marginal position relative to the rest of the discipline. Reflections on the subdisciplinary enterprise from both the second and third "waves" (Adams et al. 2005) of historical sociology have repeatedly noted the epistemological and methodological tensions between themselves and the broader subdiscipline, and the agonistic dynamics that have often resulted from those tensions (Wilson and Mayrl Forthcoming). Sewell (2005: 347), for instance, has noted that "quantitative methods and positivist epistemology have long been dominant in American social science," and that on the whole, "scholars tend to be dismissive of interpretive research . . . as unworthy of the name of social science." As a result, there is a sense among many historical sociologists that "attempting to satisfy the requisites of positivistically minded sociological gatekeepers did not (and perhaps cannot) mix easily with attention to history" (Adams et al. 2005: 23). Historical sociology's "messages about contingency, conjuncture, figurational analysis, narrative, the historicity of concepts, temporal process, and the category-dependence of social practice" challenge "the deeply entrenched dominant epistemic habitus in sociology" (Steinmetz 2005: 133). In the context of contemporary empirical practice, therefore, historical sociology is nothing short of "revolutionary" (Abbott 2001b: 183).

This sense of domination is particularly visible in in-house critiques of attempts by some historical sociologists to develop formal methods akin to those in standard

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[4]The fear that historical sociology has been relegated to a boutique function within the discipline's top departments has famously been dubbed the "Prada bag" problem (Adams et al. 2005; Prasad 2006).

[5]Both historical sociologists and ethnographers vary in how deeply they feel this estrangement; some find aspects of positivism to be worthy goals to aspire toward. On balance, however, the center of gravity within each subdiscipline tends toward greater estrangement.

sociology (e.g., Skocpol 1979; Skocpol and Somers 1980; Mahoney and Rueschemeyer 2003). These formal methods have been sympathetically criticized as an unfortunate yet perhaps necessary nod to this dominated position. Calhoun (1996: 305) famously argued that these attempts to "domesticate" historical sociology were "thrust upon historical sociologists" by "leaders of the dominant quantitative, scientific branch of the discipline [who] dismissed their work as dangerously 'idiographic,' excessively political, and in any case somehow not quite 'real' sociology." For Steinmetz (2005: 153–154), the "inferiority complex vis-à-vis the more positivistic sectors of the discipline" has led to "identification with the aggressor" among historical sociologists, visible in attempts to create formal comparative frameworks for historical inquiry. In a parallel vein, Abbott (1991: 228) argued that "Locating islands of causal regularities within a sea of historical change was essential to justify historical sociology to a hostile discipline."[6]

This sense of marginality is visible in our contemporary interviews with historical sociologists as well. One said, "I'm super sad that it seems like historical-comparative work is kind of out of fashion . . . that it seems like it doesn't seem as sexy as some of the other methods."[7] Several reported encounters with colleagues that called the legitimacy of their methods into question:

> I was giving a presentation at [Harvard] on methods or something and [one attendee] said, "Well, what's the point of doing this? Because can't anyone just make it up and do comparative history?" I think her opinion actually is somewhat typical of the discipline as a whole, right? The attempts of everyone to do the SSHA notwithstanding, I don't think it actually has much acceptance.[8]

Some even recounted how undertaking a historical project in graduate school led to abandonment by their mentors:

> The project became deeply historical in a way that it didn't look like it was going in the beginning. And because of my decision to make that shift, I found myself without support . . . [Some of my mentors] didn't have the expertise or knowledge or interest to really support that kind of work. And then there were others who, quite frankly, were saying, "That's not real research, and I don't want to be a part of a project that's doing that." So, I found myself alone and having to figure it out.[9]

For many, this sense of marginality was visible in their stance toward the over-use of conventional statistical methods. One historical sociologist recalled encountering a

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[6]Although a minority view, some of our respondents did embrace these efforts to be more classically "scientific." For instance, one told us that "the methodological literature that has been produced in response to large-*n* methodology by political scientists . . . because they have to justify their existence in political science, that has absolutely been an eye opener for me. I wish comparative-historical sociologists were as reflective and as precise as these people are about their own practices." Interview #34.

[7]Interview #13.

[8]Interview #51.

[9]Interview #3.

disjuncture between her approach to social inquiry and mainstream assumptions during her graduate coursework: "[I remember taking one] class specifically [where] we were reading all this historical stuff, and I remember people saying, 'What's the dependent variable?' I remember saying, 'What are you talking about? History keeps on unfolding. It doesn't just end!' So it never made sense to me."[10]

At its extreme, this sense of marginality manifested itself in an agonistic stance toward mainstream sociology and its positivist underpinnings. One historical sociologist asserted that he made a point in his work of "try[ing] to take down, a notch or two, positivism and post-positivism and the hubris of quantitative social science."[11] Rejecting the classic positivist distinction between the context of justification and the context of discovery (Popper 2002 [1959]), another scholar told us:

> The idea that there is one thing which is the hypothesis or research question, [and] there's another thing which is the data, [and] there's another thing which is the analysis of these data, and this produces some results . . . I take all of this with a degree of skepticism. Even when I write the sections in the papers – data, methods – I feel I'm being slightly ironic about it. I mean I just feel like I don't believe in any of it: "You want stupid, mainstream sociology? You want this? Okay, I'll give it to you."[12]

As this quote suggests, even while rejecting many of the norms of the mainstream, historical sociologists feel compelled to work within it to survive. This yielded both resentment and a set of chronic problems. Many pointed to problems getting published in top journals. "It's always a struggle. I get a lot of reviews that say, 'This isn't a history journal,' or even harsher things: 'This has no business being in a sociology journal,'" said one.[13] Finding venues to publish long-form articles was another practical challenge: "A lot of journals that we would've loved to publish in don't want very long articles," said one. "I think that placing one's papers in some journals becomes a challenge, and again, I don't think necessarily because of the epistemological issues as much as the space that we need in order to tell the story."[14] Others pointed to funding challenges as an additional hurdle imposed by their marginal position. "I found that it's very challenging to write grant proposals because I am very inductive," said one. "I usually have a theoretically motivated question I'm starting with. Beyond that, I'm going to immerse myself in sources and figure it out. It's hard to write that in a grant proposal . . . I think in social science, you're expected to have much greater control over where things are going. That's always been a struggle for me, I think."[15]

In short, historical sociologists, both in their methodological literature and in contemporary conversations, evince a sense of distance and marginality from the

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[10]Interview #20.
[11]Interview #10.
[12]Interview #60.
[13]Interview #4.
[14]Interview #17.
[15]Interview #26.

mainstream. This distance is a product of their methods in part, but also in terms of how they conceive of sociological questions and social reality; and they experience this marginality both in terms of their disciplinary careers and in terms of practical challenges to their research practices.

Ethnography

Like historical sociologists, ethnographers have long recognized that their position, relative to the positivist mainstream of American sociology, is a marginal one. Indeed, one of the primary factors driving the development of ethnographic methods in the United States has been ethnographers' desire to justify the value of their work to skeptical disciplinary others. As Chapoulie (1987) notes, this was especially true for ethnographers in the years following World War II who, following the ascension of survey methods in sociology, increasingly saw their work called into question. In the face of such challenges, ethnographers of this era began to place greater importance on the evidentiary basis of their explanatory claims, and they also began to routinely include in their manuscripts detailed accounts of the specific practices by which they had collected their data (Chapoulie 1987: 270).[16]

Subsequent "waves" of ethnographic researchers have likewise taken up the task of defending their craft from concerns about its purportedly subjectivist, unsystematic, and exploratory character. In response to such criticisms, many have articulated what they see as the distinctive methodological logic undergirding field work (Glaser and Strauss 1967; Becker 1970, 1996; Katz 1997, 2001, 2002, 2015; Burawoy 1998; Small 2009; Fine and Hallett 2014), while others have turned their analytical fire back at standard assumptions of positivistic rigor, arguing that that the inherently contextual and interpretive character of social action makes conventional survey-based quantitative methods ill-suited for examining social life (Cicourel 1964; Douglass 1967; Geertz 1973; Jerolmack and Khan 2014).[17]

Ethnographers have responded to this state of affairs in ways that range from attempts to reconcile with the mainstream to fiery denunciation thereof. At one extreme, a genre of methodological advice exemplified by Small (2009) provides advice for translating ethnographic findings into propositions legible to quantitative sociologists. Other ethnographers have responded by becoming "more royalist than the king" (Abend et al. 2013: 616) and pointing to ethnography's ability to make causal claims, the *sine qua non* of quantitative social science, as a reason for the necessity or even superiority of ethnographic methods. Others, like micro-interactionists or extended case theorists, celebrate instead the unique contributions of ethnography and maintain, if not a hostile orientation toward mainstream sociology, then one which recognizes that the two are strongly in tension with one another (e.g., Cicourel 1964; Burawoy 1998).

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[16]See, for example, the methodological appendix in the second edition of *Street Corner Society* (Whyte 1955). More recently, postmodern and postcolonial critiques have further encouraged ethnographers to reflect on their methods, particularly in terms of how they accounted for their own positionality in the research process (e.g. Clifford and Marcus 1986)

[17]While Blumer himself was not a practitioner of fieldwork as such, his writings, including his attacks on the standard assumptions supporting most quantitative research (Blumer 1956, 1986 [1969]), provide the theoretical foundation for much Chicago school work.

Ethnographers have not been entirely unsuccessful in pushing back at the boundaries of the mainstream discipline: like historical comparativists, ethnographers are reasonably well represented (and perhaps even overrepresented) within elite sociology departments. Some ethnographic studies have received widespread attention within the discipline in recent years (e.g. Duneier 1999; Wacquant 2004; Khan 2010; Rios 2011; Goffman 2014; Hoang 2015; Vargas 2016; Van Cleve 2016), and are perhaps overrepresented among ASA award winners. But ethnography nevertheless lacks a single, dominant paradigm, and each style of the methodology defines itself largely in opposition to the disciplinary mainstream – whether by seeking to conform to it, improve upon it, or reject it outright. And because ethnographers are less numerous than quantitative social scientists, they frequently face article and grant reviewers who, following Becker (2009: 2), insist that they should, "Quit whining and learn to do real science by stating theoretically derived, testable hypotheses, with methods of data gathering and analysis specified before entering the field."

Methodological homologies between historical sociology and ethnography

In addition to their similar structural positions, historical sociologists and ethnographers are brought together by a number of common methodological challenges. Both groups face a universe of data which is potentially limitless; both face difficulties gaining initial access to research sites; both struggle with the demands of retaining information and analyzing data during the process of gathering it; and both struggle with the problem of alterity – that is, of being different or "Other" to the individuals they encounter in the field or in the archive. Interestingly, historical sociologists and ethnographers have evolved very similar strategies for dealing with these methodological challenges.

Bounding the research process

Ethnographers have long noted the difficulty of determining when one is (or should be) done with the research process. The collection of observational and interview data in a given fieldsite is potentially endless – that is, the observational process can continue indefinitely. Ethnographers have typically solved this problem by attempting to reach a state of "saturation," or the point where "gathering more data about a theoretical category reveals no new properties nor yields any further theoretical insights" (Charmaz 2006: 189). The importance of reaching saturation has long been a principle of grounded theory (e.g., Glaser and Strauss 1967; Glaser 2001), but the basic logic of seeking saturation is common among many, if not most, contemporary ethnographers, whether explicitly engaged in grounded theory or not.

At the same time, the process of observation itself is never comprehensive. The ethnographer can never notice every detail of her surroundings, and as a result, her telling may miss important detail. As Fine (1993: 280) notes, "the ability to be totally aware is imperfect. We mishear, we do not recognize what we see, and we might be poorly positioned to recognize the happenings around us." Thus, ethnographers must constantly weave between knowing when to say "enough!", while

simultaneously acknowledging and accounting for the fact that the collection of data in the moment may have been incomplete.

Like ethnographers, historical sociologists face the problem of bounding their research process. It is difficult, if not impossible, to ever be "done" collecting historical data, because the historical record is just too large. "Maybe one of the hardest things about historical work is that you never really feel like it's really finished," recalled one historical sociologist. "There are a lot of things you could work on forever."[18] In our interviews, we repeatedly heard stories of being overwhelmed in the archives or by the sheer scope of primary data. "I initially went into my dissertation project with unrealistic goals about how much I was going to cover," recalled one historical sociologist. "I went into the archive . . . thinking, 'Hey, I'm going to get everything. I'm going to get it all and go through all of it. I'm going to tell the whole damn story from [beginning to end].' I was going to tell every part of it all the way. After a while, I was like, 'This is crazy.'"[19] Another recalled, of collecting government reports, "there's so much of it and you're swimming in it . . . there's so much to attend to, and so many actors involved."[20]

This sense that the potential data sources were limitless extended to secondary sources as well. One historical sociologist confessed to feeling "continually attacked" by the secondary literature: "Every time I read something, every time I went through one citation, I came up with a dozen more citations. So even though I was continually taking steps forward, I always felt like, with every step forward . . . I was twelve steps behind."[21] This problem was particularly acute for scholars studying transformative and highly studied events or topics. "These are cases where you could quite literally read forever in the secondary literature. There is no way to exhaust the secondary literature," said one.[22] "I don't think [you can] read enough. There's just so much that has been written, and is still being written, and different aspects of it to think about and see and all that," said another.[23]

Like ethnographers, many historical sociologists solve the problem of bounding the research process through a version of saturation.[24] Some even explicitly compared their process to ethnography. "You never quite read enough, but that is actually pretty similar to ethnography," said one. "Ethnography is all about saturation and you do interviews until you are just hearing the same old story, and then you know you have enough. I kind of do the same thing with the literature."[25]

Historical sociologists used this strategy to bound research in both primary and secondary sources. Of primary sources, one scholar told us, "I think there's a historical

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[18]Interview #20.

[19]Interview #23.

[20]Interview #25.

[21]Interview #35.

[22]Interview #7.

[23]Interview #9.

[24]Because of the diversity of approaches to historical sociology (Mayrl and Wilson 2020b), saturation is not the only heuristic historical sociologists use to bound their research. Those working in smaller archives may bound the process by exhausting the materials in an archive (e.g., Interview #49), while others gather data until they feel confident their story is correct or that they have gathered enough evidence to substantiate their claims (e.g., Interview #39, Interview #56).

[25]Interview #13.

equivalent to saturation: at a certain point, I'd start reading the same arguments being articulated and . . . I stopped reading stuff that was new or doing something new."[26] Similarly, of secondary sources, another said the key was to note "when you're not learning anything anymore, or it's repetitive, or you're going into details that are too arcane to really matter, right? Just like interviews in a way. At one point people are just repeating the same thing and then you're not learning much from them anymore."[27] We repeatedly heard variations of this strategy in our interviews, such as this commentary:

> I mean, in some ways it's almost a grounded theory approach of when you keep getting the same information and you're not finding anything new. That, to me, says, "Okay, you've gotten to the end of it." Or in the way when you're reading any literature, and you pick up a new thing, and . . . you already know everything they're citing. It's like, "Okay, I've actually hit the ends of the tendrils of this literature."[28]

For historical sociologists, then, much like ethnographers, bounding the research process is largely accomplished through an assessment of "diminishing returns."[29]

Gaining Entrée

The challenge of gaining *entrée*, or "getting in" with some persons or groups, has long been synonymous with ethnography. Without it, one simply cannot conduct research. While there are famous examples in which ethnographers have serendipitously stumbled into ideal relationships with "key informants" (e.g. Whyte 1955; Anderson 1976), gaining access is typically no simple matter. Not only must ethnographers be ready and willing to learn any variety of new things about the persons or groups they are studying – things which otherwise they may care little about or, worse yet, find rather distasteful – they must also be willing to open themselves up to ridicule and embarrassment when, inevitably, they show just how little they really understand about those they are studying.

As such, in seeking to navigate some "foreign" locale, the ethnographer must acquire the instincts of an exile of sorts, arriving "at the place of study without much of an introduction and knowing few people, if any . . . . They must then learn to move among strangers – while holding themselves in readiness for episodes of embarrassment, affection, misfortune, partial or vague revelation, deceit, confusion, isolation, warmth, adventure, fear, concealment, pleasure, surprise, insult, and always possible deportation" (Van Maanen (2011) [1988]). Consequently, "gaining continued access to the proposed research subjects, and *entrée* within their life-worlds, may be the most difficult part of a participant observation study" (Giulianotti 1995: 3). It is thus hardly surprising that narratives of *entrée* continue to

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[26]Interview #68.

[27]Interview #61.

[28]Interview #51.

[29]Interview #47. Other major constraints bounding the research process, of course, are time and money. We also repeatedly heard how the constraints of fellowship support affected archival strategies – though this is a topic for another paper.

be such a prominent feature of many ethnographic studies today (e.g., Goffman 2009: 341–342).

Most generally, when facing the challenge of access, ethnographers use a variety of means to construct interpersonal relationships. For some, access is a "matter of chance"; for others, it occurs by the careful managing of roles and behavioral expectations; still others obtain access through mechanisms of exchange, in which researchers give something to those who can grant them access (Harrington 2003). In all cases, however, access is largely contingent upon interactions with individuals who can facilitate *entrée* and the beginning of the research process.

Like ethnographers, historical sociologists must navigate their own version of *entrée*. While a great deal of historical data can be accessed fairly straightforwardly, historical sociologists can face issues gaining access to archives or other sets of primary data sources. Several kinds of access issues arose in our interviews. First, some documents were subject to access restrictions. Recalling his time in a government archive, one respondent recalled, "They did have a separate set of documents which . . . had some restrictions on them. Most of them—at least in that archive—had a 30 year window where . . . there was an embargo, and you couldn't even access the documents."[30] Others recounted having to navigate "different regimes of authorization . . . People making a donation to an archive can have the right to screen applications and to simply veto research."[31] "[A particular set of papers] which were very important to me [required] permission from a living relative to access," recounted one scholar, "which was weird because he's been dead quite some time. Not a long, long time, but long enough that it's not a close relative at this point; I think I [had to get] access from a great granddaughter or something."[32]

In addition to having difficulties accessing documents, historical sociologists sometimes have difficulty accessing archives themselves. Many faced serious administrative hurdles in trying to access the archives. "I had to get a countersigned archival certificate from the US Consulate, which was not at all a self-evident process," recalled one; "I was there for two weeks, and it took four days of those two weeks to even get access to the archive."[33] Others recounted multi-stage vetting processes:

> What I underestimated was access to this archive, how complicated that was . . . You had to talk to somebody and they will want to know about your project. They will want to check your references. They will want original documents. You can't just say, 'I'm a Ph.D. student at [Famous] University.' They want records that you are a Ph.D. student at [Famous] University. And then, for whatever reason, they wanted my [undergraduate transcripts]. So I had to dig those up. And then . . . [there] was a formal interview. Pretty intimidating.[34]

[30]Interview #6.
[31]Interview #42.
[32]Interview #45.
[33]Interview #7.
[34]Interview #22.

Where access was possible, some historical sociologists recalled additional difficulties surrounding the unpredictable nature of access to the archives. One historical sociologist who had done work in sub-Saharan Africa recalled, "It was very slow. Usually, depending on the archive, you would be assigned one person . . . who could get books and stuff for you. So if she or he were not there when you were done with something, then you had to wait until they turned up, maybe the next day, or after lunch, or when they were done with someone else."[35]

Like ethnographers, historical sociologists often overcame these obstacles by developing and leveraging personal relationships to gain access to data. At times, historical sociologists rely on preexisting contacts to gain access to data. "The connection that you have to a key player, or sometimes to a librarian . . . are absolutely essential," reported one. "I called up the [office I needed data from]. People wouldn't really answer. So, I called up a friend who was—at the time—in the [government], and she helped me identify a person in the [office] who could answer my questions because they were not particularly friendly over there."[36] Others drew on their collegiate networks to access restricted archives: "I used professors or old friends or classmates. They introduced me . . . Some people said, 'You're a [Princeton] graduate working this place!' Some had become politicians or bureaucrats at the local level. So, they were powerful [and] sent me a car and a driver to help me."[37]

More commonly, however, historical sociologists recalled gaining access to data by assiduously building new networks. One told us, "You had to have permission of the [agent] who's the overseer of the archives. And so . . . I made friends with [a] historian in the US who is one of the foremost experts on [my topic]. And through that contact, he wrote me a letter of presentation to his friend, the [man] who runs this archive, and then I was able to go with that letter."[38] Another recalled:

> Just trying to figure out where an archive is . . . took a long time – there was another family archive that took forever. You know, just asking on the street, like 'Where is this house?' Then they'd tell you to go somewhere else . . . [Ultimately] I got the name from someone and then I called the daughter [of the man who] had put the archive together . . . Then her telling me where to go, getting me another contact number of someone else, so I could ask to get into it.[39]

If in many cases, the work of making the necessary contacts to gain access to an archive is one that requires lots of shoe leather work, in others, it can happen unexpectedly. "There's a lot of serendipity in getting access to archives," said one interviewee. "People have told me stories about how they got access to archives by talking about football games with the person in charge of the archives. So you just never know."[40]

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[35]Interview #9.
[36]Interview #66.
[37]Interview #58.
[38]Interview #40.
[39]Interview #19.
[40]Interview #10.

For many historical sociologists, archivists play the role of key informants, both as guardians of archival access and as troves of knowledge that can orient the researcher to the data collection process. According to one of our interviewees, "it can be great if you have developed a good relationship with an archivist, and they're sympathetic, and they like your work, and they think you're smart, and they like you as a person, then they can lead you to all sorts of interesting things that you wouldn't have thought to go to."[41] Indeed, in our interviews, we repeatedly heard of the important role that archivists and other specialists could play in orienting historical sociologists to data sources and archives in ways that expedite and improve the research process (for further details, see Mayrl and Wilson 2020a).

Much like ethnographers, therefore, historical sociologists negotiate access to data – either specific materials or the archives – largely by drawing upon and constructing personal relationships that facilitate their own access to the data. And, just like ethnographers, the relationships that they form structure data collection in important ways (Mayrl and Wilson 2020a).

Retention and analysis

A third challenge faced by ethnographers has to do with making sense of the massive amounts of data that they encounter in the field. This poses two concrete challenges. The first is, simply, a problem of retention, or how to keep track of all of the data one encounters. Second, however, is the problem of analyzing the data. Data gathered in the field is always subject to analysis and reanalysis. According to Howard Becker (2004: 45), "A distinctive feature of qualitative work is that analysis of data goes on continuously. It starts with the first item of information the researcher takes in...continues throughout the data gathering process, and of course is what happens in the last phase of the work, as you write up the results." Fine (2004: 82) similarly notes, "Theoretical analysis is not something that occurs only before entering the field or after one has been in the field but is a continuing recursive process...Researchers should always be engaged in theory building—before, during, and after the gathering of ethnographic data."

The main solution ethnographers have developed to these dual problems of retention and analysis is the taking of fieldnotes. Fieldnotes can act both as a "mnemonic device" that assists in the retention of data (Tavory and Timmermans 2009: 252), and as a useful tool for prompting analysis while in the field. Delamont (2007: 213) captures the dual function of fieldnotes well in her description: "Essentially an ethnographer observes everything she can, writes the most detailed fieldnotes she can, takes time to expand, elaborate, and reflect upon them outside the field and/or as soon as time permits...and sweeps up any documents, pictures, and ephemera available." As a result, ethnographers "keep fieldnotes and other kinds of more reflexive records such as an 'out-of-the-field diary' in which theoretical ideas can be rehearsed." In short, regular writing practices, both descriptive and reflexive, while in the field help ethnographers both retain and analyze the data they encounter during the active course of the research project.

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[41]Interview #31.

Like ethnographers, historical sociologists face the dual challenge of retaining mastery of the data they encounter and of making sense of the materials they encounter as they come across them. Retention is a challenge, in part because working in the archives enables an intuitive sense-making process that is not reproducible:

> I think this is related to the ethnographic sensibility, but I'm a big believer in when you're in the archive for the first time, and you're seeing things for the first time, you're evaluating historical documents as primary sources for the first time, there's something about the way your brain works that is not reproducible, so it's not the same the second time. You only get one shot at it . . . [A] lot of my most valuable ideas have come the first time I've seen something in the archival setting.[42]

Yet the volume of data the researcher encounters in the archive attenuates this sense-making process. "I noticed that you forget everything," said one. "You can be completely startled for a moment by how bright a thing something is and—a week later—I had forgotten it because I had since discovered five more really startling things."[43]

And so, like ethnographers, historical sociologists make use of regular writing practices while they are in the archives or otherwise collecting data that help them remember details and organize their thoughts – a recursive cycle of research and written reflection that many present as central to successful archival projects (Abbott 2014; Mayrl Forthcoming). "Memo-writing I did the entire time," said one. "It was often notes, lab book-type notes, on what I was thinking, what I was learning, interesting ideas I had about the comparison."[44] In our interviews, many scholars emphasized the ubiquity of these writing practices, even at times comparing them to the practices of ethnographers:

> I'm basically cataloging everything as I see it, and then afterward, when the Collections library was over, it was almost like ethnography: I would sit down for about an hour or two hours, force myself to do it for at least – the minimum was an hour; sometimes it would go into two hours – and just sort of shake my head out with everything I found, almost like field notes. Basic field notes about, "This is what I found, this is how I think it might relate to this theory, this is what makes me think of this, I don't know what else to write here, but this is what I'm thinking, this is why I'm excited."[45]

Many of the scholars we interviewed used these writing practices as mnemonic devices to help them retain the intensity and logic of the ideas generated while immersed in the data. "When I get back to my hotel room [from an archive] I try and write a couple of disjointed beginnings of paragraphs," said one. "So, 'Oh, what

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[42]Interview #52.
[43]Interview #48.
[44]Interview #41.
[45]Interview #50.

did I notice today? These things came together.' So that when I then come back to it, I have a memory of what really struck me on the days when I was deeply, deeply immersed."[46] These notes from the field paid dividends during subsequent analysis, as this historical sociologist shows:

> I also would keep notes of why I was copying the file. Like, "Why did I decide to copy this report?" And those turn out to be really useful notes to myself . . . Because the memory thing is, of course, critical: you think you're going to remember why all this was interesting to you, because how could you not? But when you're going back even years later, sometimes you're like, "Why did I copy this report?" I didn't always do this faithfully, but often I did write a little note at the beginning – you know, "This is what I'm looking at right now, and I'm following this trail, and this is so-and-so's letter to so-and-so, and this is why it's interesting." And it's so helpful to have . . . the context, what you're looking at and why you're looking at it at that moment . . . It's just a map to your own thinking.[47]

While this mnemonic function was mentioned most frequently by scholars collecting original primary data, historical sociologists conducting large macro-sociological comparisons using secondary sources also reported taking notes as a mnemonic device: "I'll take notes on notes, basically . . . piling things together that I think fit into a certain part of the paper as I'm seeing the story emerge. So, it's whole bunches of pieces of paper. I mean, I have a terrible memory, so it's not like this is all happening in my head anyway. It would not happen if we relied on that!"[48]

 Other historical sociologists described some form of memo-writing as a central technique for thinking through and making sense of what they were observing. For some, this writing process was discrete from their time in the archives. "I actually ended up taking a month-long break where I stopped going to archives and I just sat and processed stuff and wrote," said one historical sociologist. "I probably had some mentoring advice coming down, too, from my different faculty members I was working with that said . . . 'You could basically just sit in archives all day, and just continually be an archive rat and never write anything.' So they encouraged me also to step back and put it together and do some writing . . . The writing that I was doing in the field was more just for my own self, just making sense of what I had."[49] Another described a similar process:

> Over the course of my time doing the archival research, I would – about once a month, maybe once every two months – I would write an analytical memo to myself about the state of the research . . . I can't say that I'm anywhere near as systematic with the memo-writing as I would aspire to be because it's so time-consuming, but with the new project that I'm working on now, that's one of the first things that I have . . . There's a folder with 20 or 30 analytical memos that

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[46]Interview #33.
[47]Interview #40.
[48]Interview #56.
[49]Interview #19.

I've thrown in there over the course of the last four years that I can go back to and kind of think with.[50]

In addition to their role as mnemonics and as a means of making sense of what they were uncovering in the archives, in-site memo writing served a variety of other functions for historical sociologists. For some, memos were useful means of retaining the nuances of arguments in the secondary literature. Said one, "What I have started doing is: I read [a] book, I take notes in the book, and then I write a pretty exhaustive Word document. I just go line by line, and write everything that is important. And I will also put what I'm thinking, too, in little parentheses: 'Oh, this relates to this.' . . . The thing that always trips me up is, you start to caricature the argument in your head later, but if you have that record, then you can read through it and remember the nuances."[51] For others, memos helped them to identify gaps in the empirical record that they could then focus their data collection efforts around: "The memos to myself that I was writing in the archive were little attempts to actually construct a full argument and say, if somebody put a gun to my head and said I could never go back to the library, what would I write right now based on what I know? And over the course of that I would find these little things that I don't know and that I needed to know."[52] Through the use of analytic memos throughout the research process, historical sociologists were thus able to overcome problems relating to the retention and processing of data in the fieldsite.

Alterity

For ethnographers, overcoming a perceived sense of distance with the communities they study is a central problematic of their method, one which is sometimes described as central to the entire methodological enterprise. As Katz (1997: 393) writes, "Perhaps the single most common warrant for sociological ethnography is that what is obvious to the subjects [of research] has been kept systematically beyond the cognitive reach of the ethnographer's audience because . . . social forces systematically maintain social distance between the ethnographer's subjects and his or her audience." In reality, ethnographers differ over exactly how to deal with the problem of alterity. While some double down on positivism and say that ethnography is like an experiment because you can see exactly why something happens (e.g., Jerolmack and Khan 2014), others insist that data are meaningless without theory (Burawoy 2003), and still others take intermediate stances. Nevertheless, at the heart of the methodological enterprise of ethnography is the recognition that the actor's understanding of his or her own actions is often different from an outsider's understanding of those same actions.

Ethnographers solve this problem of distance through deep immersion in the communities under study. Participant observation, according to Goffman (1989: 125), entails "subjecting yourself, your own body and your own personality, and your own social situation, to the set of contingencies that play upon a set of

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[50]Interview #31.
[51]Interview #13.
[52]Interview #7.

individuals, so that you can physically and ecologically penetrate their circle of response to their social situation." Wacquant (2004: viii) concurs:

> It is imperative that the sociologist submit himself to the fire of action *in situ*; that to the greatest extent possible he puts his own organism, sensibility, and incarnate intelligence at the epicenter of the array of material and symbolic forces that he intends to dissect; [and] that he strive to acquire the appetites and the competencies that make the diligent agent in the universe under consideration.

Immersion thus permits the researcher "to enter into the matrix of meanings of the researched, to participate in their system of organized activities, and to feel subject to their code of moral regulation" (Wax 1980: 272–273). In other words, overcoming the problem of "social distance" (or, as we term it here, alterity), for ethnographers, requires complete physical and psychic immersion in the fieldsite.

Just as ethnographers immerse themselves in their fieldsites to reduce their "otherness" and develop a deeper understanding of their subjects' social world, so too do historical sociologists. "To reconstruct past meanings means . . . to try and not impose your present-day categories on [the historical record]," said one of our interviewees. "It's a problem of alterity . . . Often you're looking at a culture that you think is your own culture, but in the past."[53] Again, some historical sociologists consciously liken this process to that of ethnographers. "I think some kind of historical work is like ethnography . . . like you're trying to crawl into somebody's mind at a different time or a different space and understanding the world from their point of view . . . We're just doing it over time when they're doing it across social space."[54]

Historical sociologists overcome this problem of alterity in a parallel fashion to ethnographers: by immersing themselves deep in historical data. Indeed, gaining a deep and immediate sense of the social and cultural world they are writing about is an obsession and a source of joy for many historical sociologists, allowing them to understand better what they are actually seeing in the primary data. Because immersion is central to understanding, it requires time to be done properly:

> Like any serious systematic fieldwork, doing primary historical work takes time . . . There is, nowadays, I think, some inclination to believe that you go to the archive, you stay there a few days, you stay there a few weeks, you take a lot of pictures, you come back and then you work on it – or at least you can pretend, you can claim. I think this is a mistake . . . It's like doing ethnography and staying for a few days or even a few weeks, and then claiming that you have acquired the ethnographic knowledge.[55]

Historical sociologists regularly referred to the deep immersion in sources they went through in the course of undertaking their research. For some, deep immersion enabled them to identify aspects of social life that were not immediately evident

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[53]Interview #28.
[54]Interview #35.
[55]Interview #42.

from more cursory scans of the data. One historical sociologist discussed the difference in terms of the internal dynamics of an organization he was studying:

> It's two completely different things to look at the abstract organizational chart . . . and to actually see the records, how they wrote letters to one another, what those letters tended to say, which organizational arms really mattered . . . If people are really fighting to be appointed [to a particular post], you know that that's a key organizational position. You may not know why, but you can infer that there's something important about it.[56]

Another emphasized how the very materiality of archival materials helped him appreciate the period better: "The physicality of things was also – You know, I felt like I lived with these people."[57]

More commonly, historical sociologists talked about how immersing themselves in documents enabled them to get a sense of the broader culture that informed their research questions. "In a lot of ways, I used [primary] sources not for factual information, but just to get a sense of how people talked about issues and problems in the past that was really revealing . . . In some ways, the more loosy-goosy feel-of-the-times, I feel, was actually more revealing," said one historical sociologist.[58] Another concurred:

> I think reading, going to archives and stuff, really help you to slowly get into the local semantics, local meanings, that people were acting upon. And I think you have to do a lot of work just to develop this sense . . . [Y]ou reconstruct a world that's very different from what you expect in the beginning, but it's not like [snaps fingers], you suddenly have a huge discovery. It's rather a slow process, a cumulative process of reconstructing this world that's quite foreign to our presentist understanding.[59]

This inchoate understanding can then, many scholars explained, pay off in a richer ability to understand historical action. As one junior scholar told us:

> I think there's really something – it's ineffable, but I think it's deeply, profoundly true – about what historians call immersion, immersion in a body of materials that gives you a kind of intuition for the period. It's a kind of tacit knowledge. And it's very hard to dip into and dip out of . . . When I was in grad school, I felt like I was living in the late 19th century for two months. And I just knew what was going on. I had intuition of what the actors cared about.[60]

Because of its importance in sensitizing the scholar to the lifeworld they are studying, some said they liked to immerse themselves in period materials at the

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[56]Interview #7.
[57]Interview #59.
[58]Interview #13.
[59]Interview #14.
[60]Interview #4.

outset of their research. Comparing archival work to ethnography, one historical sociologist told us to "be ready from the beginning to take more time to be immersed and to listen and to read promiscuously . . . Just think of it the way you might think about doing preliminary fieldwork for participant observation, that you're [new to] that area or that culture, and you're just trying to listen to that historical period and understand what it has to say on its own terms."[61]

For historical sociologists, this process of immersion can be both intensely pleasurable and profoundly disorienting. One recounted her experience in almost mystical terms:

> There is something amazing about being transported, the immersion factor, when you are working in these documents and you become so familiar with the characters and so involved in the decisions that they're making and the struggles they're facing . . . I feel like there's something really amazing about the intimacy with the sources and then the layering of time. It's as though you're in a meeting room, and it's [1890], and you're breathing that dusty air. You're just partaking of a different time . . . It's like the rest of your life falls away when you're doing this. When I'm at an archive, I am so present in whatever situation I'm investigating – it's very therapeutic, actually.[62]

At the same time, the pleasures of losing oneself in the historical data can lead to issues reacclimating to normal life upon return from the archives. One interviewee alluded to "months of anxiety and despair" that accompanied his return from the archives, drawing an explicit comparison to ethnography: "Part of it is just this process of returning from the tropics, that you just realize that you've been out wandering around the little paths deep in the forest, and now you have to . . . tell them what the island looks like, and that's a different enterprise."[63] Like ethnographers, historical sociologists, thus, transcend cultural distance through deep and total immersion in their data sources, in ways that can be profoundly reorienting and transformative.[64]

Discussion and conclusion

Ethnography and historical sociology are increasingly in conversation. While it may be surprising that such contact is happening between a subdiscipline of sociology which emphasizes the present-day observation of people and another which

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[61]Interview #32.

[62]Interview #26.

[63]Interview #15.

[64]There is also an important element of reflexivity to this immersion, particularly insofar as the ethnographer must consider how her presence in the fieldsite might affect her observations (Burawoy 2003). Historical sociologists must also be reflexive about their data, not because their presence alters the historical record, but because they must account for how that record itself has been selectively constructed and preserved (Luft 2020). While such reflexivity might serve as another methodological homology between historical sociologists and ethnographers, we think it to be less central, since even mainstream quantitative social scientists must be reflexive about how social factors shape data collection, most notably in principles of sampling and survey construction (Lohr 2022; McFarland 1981; Vannette and Krosnick 2018).

increasingly turns to archival sources to illuminate the past, we have shown there are good reasons for it. Structurally speaking, ethnographers and historical sociologists share a similar sense of marginality from the positivist core of sociology, even as they both grapple with how to best engage with it while maintaining their unique substantive perspectives. Likewise, even though ethnography and historical sociology appear to employ quite different methods on radically divergent types of data, we have also demonstrated that in fact the methodological challenges both subdisciplines face in bounding research projects, gaining *entrée* to archives and fieldsites, collecting and analyzing data, and even establishing an interpretive understanding of the people and events under study, are quite similar. That these convergences obtain over such a wide domain of "research in practice," moreover, seems to suggest a convergence in the underlying epistemological and ontological postures of historical sociology and ethnography. Given that, in turn, it becomes less surprising that ethnographers would attend SSHA, or that conversations would increasingly span this subdisciplinary boundary.

This example of subdisciplinary exchange offers some lessons for other forms of exchange across intellectual boundaries and fields. The first is that *deep homologies can act as structuring principles that provide opportunities and incentives for subdisciplinary exchange.* The two forms of homology we highlight here – structural and methodological – act as deeper logics that organize the trading zone between historical sociology and ethnography. Subdisciplinary trading zones can, and often do, arise out of the need to coordinate across difference. But, we argue, they may be particularly likely to flourish when groups similarly situated, either relative to some mainstream or to some shared set of problems, come together.

For this reason, second, *deep homologies may produce not the thin exchanges emphasized by Galison (1997), but instead thick exchanges that can be richly generative for both parties.* Because of their shared orientation or sets of challenges, conversations within subdisciplinary trading zones that feature such homologies can promote further substantive and methodological reflection. Methodological homologies, overlooked in much of the literature on interdisciplinary exchange, may be particularly important for fostering generative encounters. In such instances, exchange may help to surface hidden issues within each tradition that have not previously been explicitly theorized. Thus, ethnographers may have been surprised to learn that the relationship between a historical sociologist and an archivist is like that between an ethnographer and a skilled informant; while historical sociologists' new emphasis on the epistemology of archival methods draws not only from historians' work on the subject, but also reflects a transposition of ethnographers' long-standing engagement with what it means to be embedded in a fieldsite.

These conclusions suggest that scholars interested in subdisciplinary exchange – and interdisciplinarity more broadly – should be alert for the deeper homologies that may structure and facilitate those exchanges. And in particular, they should look for methodological homologies that may lie beneath the surface, which can promote thick as well as thin forms of exchange.

Finally, the above analysis suggests the promise of using social science history to unpack the dynamics of "trading zones" between and within disciplines. Because the concept of "trading zone" was developed for and has largely tended to stay within

the bounds of STEM fields (Galison 1997), more conceptual and empirical work needs to be done on moments of encounter beyond the so-called "hard sciences," and especially within and between the humanities and social sciences. While this article has examined subdisciplinary exchange within sociology, it would be worth examining how historical sociology intersects with subdisciplines in other social sciences – such as American political development and comparative politics in political science, economic history, or historical anthropology. In each of these cases, a full analysis would require a careful reconstruction of the positions of each subdiscipline versus their mainstreams, an analysis of their organizational opportunities for contact, and the transformations that result from those contacts.

More broadly, social science history is an ideal site to examine the dynamics of exchange between the social sciences and history itself. While parts of this inquiry would resemble the analyses of social structures relevant for other disciplines – how is history internally structured, and which subdisciplines have been in generative contact with the social sciences? – there are special epistemological stakes to an investigation of history: above all, the way that "the historical record," in all its multiplicity and contested meanings *within* history, acts as a crucial means to stabilize exchange in the trading zone between history and the social sciences.

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