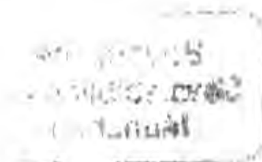
Beyond the Case

The Logics and Practices of Comparative Ethnography

Edited by

COREY M. ABRAMSON AND NEIL GONG

OXFORD
UNIVERSITY PRESS
2020



7

Sequential Comparisons and the Comparative Imagination

Iddo Tavory and Stefan Timmermans

Despite some protestations to the contrary, ethnographers necessarily work comparatively. Even a single site study entails important comparative dimensions and decisions, as observations and situations are compared for analytical leverage, and as the ethnographer thinks about it in relation to other texts. But what, exactly, is a comparison? What kinds of comparisons can we engage with in our work? This chapter outlines some of the ways in which ethnographers work comparatively as a way to enrich our comparative imagination, and makes a partisan case for one comparative research strategy, that of *sequential comparison*.

As ideal types, we begin by suggesting that there are at least three broad ways in which ethnography can be comparative. First, given that research takes its meaning in relation to a community of inquiry, *shadow comparisons* constantly lurk behind our work. On this basic level, any ethnography worth its salt is comparative. An ethnography of marginalized black urban men necessarily evokes comparisons to Liebow's (1967) *Tally's Corner* and Anderson's (1976) *A Place on the Corner*; an ethnography of working-class teenagers necessarily takes Paul Willis's (1977) "lads" as a point of reference. Working within a community of inquiry, our work resides in a larger historical matrix of those who came before us and who work alongside us, and the meaning of what we do is importantly defined by its location in a structure of similarities and differences to other research within this world. As with all signs, the meanings of ethnography emerge through difference and reference.

But this, perhaps, seems too general. If shadow comparisons are everywhere, is it even useful to highlight their importance as comparisons? As we proceed to show, explicit attention to such a comparative dimension is crucial, but insufficient to capitalize on the comparative potential of

Iddo Tavory and Stefan Timmermans, *Sequential Comparisons and the Comparative Imagination* In: *Beyond the Case*. Edited by: Corey M. Abramson and Neil Gong, Oxford University Press (2020). © Oxford University Press.
DOI: 10.1093/oso/9780190608484.003.0008

ethnography. Beyond shadow comparisons are more overt comparisons. But even here, what "comparison" means remains unclear. We suspect that for most people the words "comparative ethnography" evokes images of an ethnographer spending a year in one place, then a year in another; perhaps going to one group on Tuesday and Thursdays, another on Mondays and Wednesdays. Then we imagine her building her ethnographic narrative and claims around the similarities and differences that she finds between different—albeit carefully chosen—ethnographic sites. This second kind of comparison can be thought of as an *external comparison*, loosely organized around a set of expected similarities and differences.

Third, these kinds of comparisons can be productively juxtaposed to other kinds of comparisons—those that we constantly attend to as we conduct any "single site" ethnography. These *internal comparisons* are of a somewhat different order. Rather than thinking among sites or research projects, it focuses on the internal variation between what happens in one situation and the next; between how we see people act in one part of their social world, and another; between the lives of our interlocutors when we began our study, to their lives years later when we leave the field.[1]

Bringing these various forms of comparison into sharper focus, this chapter advances two arguments. First, and most basically, it aims to widen the scope of our comparative imagination. The relationship between shadow comparisons, internal comparisons, and external comparisons provides us with new ways to think about our work. There are multiple ways to compare, and each project needs to develop its own kind of comparison based on both the questions that motivate the research, and those that emerge in the research act. Widening the scope of our comparative imagination would thus help us avoid straightjacketing our own research into preset ways of conceptualizing, and moving among, cases.

Second, we make a partisan case. We argue that a neglected consideration—and resource—in thinking about comparative ethnography is requires us to re-evaluate how research is situated in time. Ethnographers think about time carefully when they trace objects as they move between situations, or when they trace people's careers. But they often neglect to think about the time of analysis. Those of us who ply the trade of external comparison often try to get their fieldwork in two places done as soon as possible, sometimes visiting two or more sites during the same period (especially when the sites are close by). The timeline of a PhD program, or the promises made on a grant application, exacerbate this tendency. We think, however, that this approach

doesn't play to the strengths of ethnographic comparison—the theoretically productive recursive relation between analysis and observation.

Instead, we argue that people would be better off practicing sequential comparisons in their fieldwork. That is, working through one field site carefully, attending to the internal comparisons that emerge there, and abductively figuring out the theoretical and empirical story we want to tell, and only then moving to another field, that (we have reason to hope) provides an interesting counterpoint to the analytic and empirical story we have constructed. We argue that although this sequential comparative strategy is time consuming, it will allow for more productive comparisons, and less straightjacketing of theoretical claims, a problem that haunts many external comparisons.

Deepening Shadows: Revisits, Reminiscence, and Comparing Notes

Ethnography takes place in a matrix of methodological conventions, tricks of the trade, and ongoing discussions about strengths, limitations, and variations. These writings, in turn, are embedded in iconic ethnographic exemplars. These ethnographies constitute the field's community of inquiry and its definition as something like a "field." Whether or not we agree with each other, we refer to the same discussions, take positions in regards to shared texts we assume we all read. In other words, this community forms a comparative shadow reference point for ongoing work. Especially in urban ethnography but also in the fields of healthcare, medicine, education, and immigration, researchers enter well-established traditions and new projects have to engage their predecessors: to build on their work, fill in gaps, extend their reach, update in light of new structural configurations, or take issue with the past.

Moreover, for each specific study, there are iconic works that the ethnographer needs to tackle, orienting points the ethnographer needs to orient herself in relation to. No study of dying is complete without careful thought about Glaser and Strauss's (1965) *Awareness of Dying*; no study of Orthodox Jews can sidestep Samuel Heilman's (1976) *Synagogue Life*.

While this is true for all ethnographic work—much as it is true for academic work more generally—some shadows are thus deeper than those we usually evoke. Ethnographers often treat their shadow comparisons

opportunistically, as these emerge, and as they help them think in interesting ways about their field. The deeper shadows, on the other hand, become more like what Michel Callon (1986) called "obligatory passage points"—points that actors *have to* move through in order to make their claims. And while many of these obligatory passage points are perfunctory, a performance of the relation rather than a true attempt to grapple with such shadows, others are more deliberate.

One such instance is the strategy of "revisiting." This approach to ethnographic work, made famous by Michael Burawoy's (2003) serendipitous discovery that the machine shop he was conducting his ethnography in was the same that Donald Roy studied years before (see also Duneier 2004), uses comparison as its motivating force. Previous work, conducted by a different ethnographer at a different time is the foil that the ethnographer uses. The tenor of such revisits is variable. Usually, revisits are ways to rethink and retheorize a substantive field along a linear temporal dimension. And yet a revisit is perhaps most powerful in allowing us to think about how history shapes our field—something that ethnographers yearn for, but that their methods cannot neatly capture.

Revisits thus use other people's work and theory as a comparative vantage point. However, unlike more shallow shadow comparisons, a revisit has the unique advantage of holding the place (and sometimes even some of the people) constant, thus allowing for a more focused leverage. As in all shadow comparisons, the cases leveraged are largely "silent"—they provide the background against which we work, and although we may question the conclusions of the earlier ethnographer, the original ethnography is where we take off from, not where we aim to get to at the end of our journey.

Another distinct way of deepening shadows consists of comparing notes. Thus, although ethnographers are (speaking in terms of doing the actual research) solitary creatures, they talk and think together. One outcome of such exchange is that ethnographers may come to realize that aspects of their work are productively seen in relation to each other—either because a shared framework or process can illuminate their observations in both fields, or precisely because the observations are so different that putting them side by side is theoretically and empirically productive.

This is the case with articles such as Nina Eliasoph's and Paul Lichterman's "Culture in Interaction" (2003) as well as other joint publications (e.g., Tavory and Winchester 2012). To take Eliasoph and Lichterman's work, as they compared notes from their ongoing ethnographies of civil society in churches

(Lichterman) and small groups that mostly avoided politics (Eliasoph) they came to see that in both cases the groups they observed crystallized general cultural tropes in ways that had a distinctive group style. This, then, became the basis of an important theoretical intervention about the way in which culture and interaction are intertwined. Although they could, perhaps, have developed this theoretical model using only one ethnographic project, the ongoing comparison of their projects allowed them to make more ambitious claims.

Lastly, some ethnographers use their own past work as their "significant other." This is rarer than one would expect. Most ethnographers do not produce more than one or two serious ethnographies in their lifetime (after which they usually begin writing endless methodological and theoretical tracts such as the one you are currently reading). But some ethnographers—such as Gary Alan Fine and Diane Vaughan—have used their own trajectory as a way to continue working on the same set of problems, deepening and expanding on a core set of concerns. Thus, Gary Fine has produced more than ten separate ethnographies—ranging from groups playing Dungeons and Dragons (Fine 1983), to kitchen workers (Fine 1996), meteorologists (Fine 2010), and mushroom collectors (Fine 1998). And, although the fields are quite different, Fine continuously works on the same set of concerns—the construction and inner working of little group culture, or "idiocultures" (Fine 1979). Similarly, in Diane Vaughan's work (Vaughan 1986, 1992, 1996, 2004), whether in NASA, in marriages or with flight controllers, the ethnographies build on each, constructing an increasingly sophisticated theory of organizational breakdown.

In short, then, revisits, reminiscence and comparing notes are modes of comparison that punch the logic of "shadow comparisons" to its extreme, deepening the shadows so that they almost become an external comparison.

Internal Comparisons in Grounded Theory and Beyond

A second analytic strategy, that of *internal comparison*, was most explicitly developed in the grounded theory literature. Grounded theory has a rather convoluted attitude toward comparison. The approach puts comparison front and central as a data analytical strategy, even as it largely downplays its potential as a research design tactic. Perhaps most tellingly, grounded theory also travels under the heading of the "constant comparative method," a term

that Barney Glaser, one of the two architects of grounded theory, introduced in 1965 in an article published in *Social Problems* (Glaser 1965). In the paper, Glaser proposed a three-stage set of comparisons.

Researchers, according to Glaser, should code every instance in their data in as many categories as possible. The added value of this strategy was that the researcher should compare the newly observed instance with the already present instances of the category. Comparisons done in this way should alert the researcher to salient theoretical properties, which, Glaser recommended, should be spun off as analytical memos. Next, the researcher should integrate different analytical categories and their properties, again by comparing the instances between and across categories. In the third stage, the researcher is concerned with parsimony and scope of the emerging theorization and works on pruning down the data categorization to maximize generalizability. The deciding criterion is what Glaser and Strauss famously called "saturation": if no new information is found in subsequent observations, the analytical construct can be considered saturated. Only then, after this threefold process of comparison, should the researcher write-up their emergent theory.

The comparative aspect of grounded theory thus consists of continuously comparing categories that emerge in the coding process with each other in order to delineate their theoretical properties. This was different from, for example, analytical induction where researchers develop theoretical properties from a deliberate search for negative cases as way to recursively construct the theoretical categories and scope conditions of the emergent theorization (Becker 1993; Katz 2001; see also Lakatos 1978).

Glaser and Strauss revolutionized qualitative data analysis with their emphasis on conceptualizing observations in ways that were not pregiven, but they did the field of qualitative research a dubious service by presenting grounded theory as an inductive research method. Glaser, in particular, remains adamant that existing theories and research would contaminate the researcher's engagement with her field. As a data analysis strategy, the constant comparisons of grounded theory were thus hobbled by the strong admonition to not engage existing literatures and research until the project's wrap-up.

In the language developed here, the problem with grounded theory is that it rejected the shadow comparisons that must be a part of parcel of both the process of research design and the ongoing context of research. The difference between grounded theory and abductive analysis, which we have developed elsewhere (Tavory and Timmermans 2014b; Timmermans and Tavory

2012), is precisely there. In abductive analysis, shadow comparisons are evoked throughout the research context. A site is chosen because we have an inkling that it would be theoretically interesting, but we continue to compare our findings to various literatures throughout the research project, leveraging these shadow comparisons to identify analytic and empirical surprises.

Interestingly, although grounded theory was conceived as a form of internal comparison, the relation to *external* comparison haunted is from its very inception. As with most data analytical innovations, grounded theory originated from a specific research project and carries some of the specificity of this project along with it. The formative project was a study of hospital dying in the Bay Area (Glaser and Strauss 1965). This study had tremendous comparative potential: Glaser and Strauss observed dying in six different hospitals and on different hospital wards, differentiating, for example places where dying was expected from unexpected dying, dying of the young from dying from old age, dying in secrecy and dying in the open. Yet to contemporary readers, *Awareness of Dying* does not read as a comparative study because Glaser and Strauss downplay the specificity of dying in the different San Francisco hospitals. They spell this out:

> We might have organized our analysis in this book to highlight differences and similarities among the various medical services. Instead, we chose to offer our readers a more abstract—and so more powerful—explanatory theoretical scheme. This scheme arose from scrutiny of the data and should illuminate the data far more than a comparative analysis of the medical services. (Glaser and Strauss 1965, ix)

Indeed, even though the hospitals include such diverse institutions as a mental health hospital, a VA hospital, an academic center, and a general public hospital, none of the institutional specificities made it into the analysis. Instead, Glaser and Strauss develop the notion of awareness contexts to capture a general process of social isolation during dying in hospitals. They pay attention to institutional factors, but none of these factors is situated within any of the hospital settings. In fact, the hospitals themselves are not described in their book. The commitment to treat all observations in terms of an overarching internal comparison thus sacrifices specificity, history, and context for conceptual abstraction, consolidating what could have been considered different sites into one meta-site within which comparisons are made "internally."

Still, despite the problems and elisions of grounded theory, it has made a number of crucial contributions to theorizations of comparison in ethnography. First, alongside analytic induction, grounded theorists stressed that internal comparisons are the sine qua non of the research process. But second, the grounded theory notion of "theoretical sampling" as a basis for comparison is promising. Theoretical sampling takes advantage of findings in one site to determine the most appropriate second site or community to set-up a comparison. The key take-home message of theoretical sampling is less its role in opening up theory-construction following grounded theory's open-ended coding approach but as a means to think through the most plausible grounds for comparison. In other words, we need to find a way to link sites to compare, and this linking can only be done through the work conducted within a site.

External Comparisons

Within this landscape of shadow and internal comparisons, we can now think about the place that external comparisons play. In the typology developed here, what distinguishes external comparisons is that (a) the researcher actively engages two or more sites; so that (b) the comparison become an obligatory passage point for claims-making; and where (c) this engagement is recursive, either in the moment of data collection, or as the ethnographer tries to make sense of their materials. Even here, however, there is more than one way to think about external comparison. Generally speaking, we may think about two warrants for external comparison—*causal analysis* and *theoretical provocations*.

First, and despite some protestations to the contrary, many external comparisons are loosely based on Mill's methods of similarity and difference (Mill 2002 [1843]). Even as they know that no perfect controls exist in the social world, the specter of such neat causal inference haunts such comparisons. Very few ethnographies pull this off in a consistent way, in part because reducing rich historicity for a number of key characteristics, and turning the overdetermination of lives into specific variables that we control is usually anathema to the ethnographer's work ethic. Still, in specific cases, this Millian logic leads to incisive work.

One such research project is Katherine Kellogg's study of educational reform in three hospitals (Kellogg 2011). Concerned about sleep-deprived

medical residents harming patients, the federal government set a maximum number of hours residents could work in a shift. A lack of compliance could result in a loss of accreditation of the residency programs. Starting her project prior to the implementation of the reform, Kellogg conducted fifteen months of ethnographic observations in two hospitals and added a third hospital one year later. The three hospitals were not only similar in organizational characteristics and surgery programs but also subject to the same external regulatory pressures to reform their residency programs. All three hospitals initiated similar reform processes supported by the hospitals' medical directors. Yet in the first hospital, reformers succeeded in changing working hours, in the second hospital reform never took off, and in the third hospital reform initially was implemented but later defeated. Why these different outcomes?

As Kellogg shows, surgery is an extraordinarily hierarchical and masculinized specialty in which everyone is expected to remain responsible for "their" patients and "scutwork" is delegated to the lowest on the hierarchy. Surgeons demand total dedication for the seven years of residency. Limiting residency hours required interns to hand-off patients to senior residents working the night shift and these seniors refused to do to be what they considered demeaning tasks. The resulting "dropped balls" in patient-care came back to haunt the interns rather than the night shift residents: interns would rather capitulate than endanger their reputations. In the hospital that succeeded to reform resident hours, reformers were able to build alliances; work out scripts to redefine the derogatory, feminine language associated with reform efforts; and isolate defenders of the old policies. The leaders of reform were mostly transient residents, doing a one-year surgery residency as part of a different specialty. These transient residents' reputation was less vulnerable to defender attacks.

Kellogg's key theoretical contribution is that struggles for macro-political reform require equivalent processes of micro face-to-face "collective combat" at the institutional level for reforms to be implemented as intended. Kellogg's study then comes the closest to a traditional experimental comparative ethnography where the researchers controls for certain factors in case selection (made easier in her case by the standardization of hospitals and residency programs), looks at a similar external force (the educational reform), and uses ethnographic data grounded in various theories to tease out the key explanatory interactional processes.

And yet the decision to turn to quasi-experimental logic comes at a high price. While an insightful study, and perhaps one that comes quite close to

a true Millian method, Kellogg's ethnography is focused on answering a narrow question: Why did reform work in one site but not in another? Her ethnographic appropriation of the experimental approach produces an added value: she discovers a social mechanism of joining forces collectively that we would not have obtained in its specificities from any other methodology but the trade-off is that she leaves much on the cutting floor. For a book of surgery residents, we learn almost nothing about how surgery is performed (Bosk 1979) and, importantly, the impetus for the reform (that sleep deprived residents endanger patient lives) remains also unexplored. In other words, the price of the Millian method is purifying ethnography from what people may see as its unique strengths—the abilities to capture different social worlds in their fullness.

While the pursuit of the Millian method is one possibility opened by external comparison, it is not the only one. Another possibility is that of *theoretical provocation* (see also Krause 2016). Here, rather than finding a specific difference, researchers proceed by showing how a shared theoretical framework helps bring the finding culled in the (usually) two or more sites to life. The origin of this approach to comparison can be found in the heuristics developed by Everett Hughes (1971 [1945]). Hughes encouraged students to draw far-out comparisons in order to nail the specificity of a case. For instance, he noted that scholars learn much about physicians from studying plumbers and from psychiatrists from studying prostitutes. The first pair practices esoteric techniques to help people in distress while the second pair cannot become too personally involved in intimate matters of their clients. They, then, share similar occupational challenges. Hughes proposed that theories can be strengthened by incorporating a wide range of seemingly dissimilar comparative cases into analytical view.

Two examples of this kind of ethnography are those of Ofer Sharone (2013), and of Jeff Guhin (forthcoming). First, Sharone (2013) shows how different understandings of the relationship between selfhood and employment structure the labor markets in Israel and the United States. In Israel, workers see the job openings through what Sharone calls a "specs" game. That is, they understand their hiring (or lack thereof) in terms of job descriptions, as well as through the network ties they have (or don't have, as the case may be). In the United States, in contrast, workers understand their ability to get a job through what Sharone calls a "chemistry game," that is, they internalize their successes and failure, and make them about the fit between the person him or herself and the job. As Sharone shows, playing these different games in the two national contexts have profound effect for how white-collar

workers understand both hiring and unemployment, thus giving rise to different experiences of work.

Taking a very different object of analysis, Guhin (forthcoming) compares four religious schools in New York—two Muslim Sunni Schools and two Christian Evangelical ones—in order to parse out when and how do specific practices and ideas become morally salient. The importance of evolution in the Evangelical schools or of gendered religious practices in the Sunni ones cannot be derived from abstracted beliefs, but they emerge in the intersection of core practices and histories with aspects of religious doctrine. Thus, as Evangelicalism practically focuses on the literal reading of the bible, and in relation to the history of the church (e.g., the Scopes "monkey trial"), evolution becomes a rallying cry and salient boundary; as Sunni Muslim families are dealing with their status as immigrants and their American kids, and in relation to the "veil debates" gendered practices emerge as salient boundaries.

These examples, like others (e.g., Lee 1998; Snyder 2016), may sometimes play loosely with the Millian method, but the power of the analysis is not derived from such comparative logic. Rather, the sites recursively work to focus the researcher's attention of a shared theoretical architecture as well as to defamiliarize aspects of each case that we may take for granted. So, in Sharone's case, the American and Israeli cases are not really comparable, but they show how different national games can refract the precarity of employment in late capitalism; in Guhin's case, the institutional context of the school, and the specific differences between the two Evangelical and the two Sunni schools take second fiddle to the larger theoretical point he develops about the salience of specific boundary practices.

In short, external comparisons are often motivated by a Millian hope to identify particular differences or similarities, or by a mode of thinking that uses the different ethnographic cases more to shed light on each other than to strictly compare them. Indeed, the best ethnographies that leverage external comparison manage to artfully blend these two warrants, so that at one moment they imply a *differentia specifica* between the field sites, and at the next they relax the comparison to gain a wider appreciation of the way worlds are structured.

Toward a Sequential Comparative Ethnography

Elsewhere we argued that delineating social mechanisms plays to the particular strengths of ethnography (Tavory and Timmermans 2014a). Social

mechanisms refer to processes the ethnographer traces to link an explanandum to an explanans. We proposed that the relevant building blocks of such an attempt to trace processes are the meaning-making sequences that people engage in in the unfolding moments of action. Second, we proposed that the explanatory value of these building blocks can be assessed by examining variation across cases. Lastly, we argued that a necessary step in the research process was to engage the proposed mechanism within the various plausible alternatives offered by a broader intellectual community. In other words, we spent most our effort on thinking through the relationship between shadow comparisons and internal comparisons.

Here, we want to push our argument further by looking for comparative history for inspiration. In making the case for comparative historical research, Haydu (1998) notes that comparative research either sacrifices thick understanding of historical particularities for causal generality, or offers distinctive in-depth understanding of a limited number of cases. Generalizing following an experimental model runs in the problem of independence and equivalence of cases. As Sewell (1996) noted, the more independent cases are, the less they tend to be similar. Comparative historians therefore often move to study societies or communities that are closely related. However, that raises the opposite problem. The more equivalent, the more likely they are connected through explicit links. In Skocpol's comparative study of revolutions, the Chinese Revolutionaries did not only directly model their revolution on the Bolsheviks revolution in Russia but received direct support from them—something of a problem given that Skocpol's causal model analyzed the revolutions as independent events.

To partly solve that problem, Haydu suggests that historical comparative analysis would get analytical mileage out of connecting events over time as repeated attempts to solve similar problems. The advantage of putting human problem-solving front and central is that it focuses the comparative historian on how to carry forward the explanatory weight of the past to shape later turning points and critical choices. Dependence, rather than independence, is the operating assumption to debunk before one can speak of different cases or different periods. Solutions in one period create problems in subsequent periods that require new solutions, looking for both contrast and continuity. Previous actions not only close off future courses of action (as in path dependency), but open up new historical pathways due to contradictory or unintended consequences. Periods are distinguished based on contrasting solutions for recurring problems and the goal of the analysis is to explain why

people pursued different solutions to enduring problems. Problems qualify as organizing principles of historical analysis on empirical observable or theoretical grounds, but, as Haydu requires, they cannot be imposed by social scientists. There needs to be some correspondence to how actors themselves experience the world.

Translating an iterative problem-solving approach from comparative history to contemporary ethnography is obviously not going to be a panacea for all forms of comparative ethnography. And yet it has some advantages. The key insight, following Haydu, is that sites are linked by a common issue—by a similar problem that requires a solution. The comparison consists in examining both the interdependency and the solutions to similar problems.

There are two ways that such an iterative problem-solving approach can inspire ethnographic work. One, well established, is the idea of a multisited ethnography which—in its pure form—is by definition *not* comparative but rather follows an object from one site to the next. Here, interdependence becomes the central linchpin of the analytical project. The other, which we refer to as "sequential" comparative ethnography, starts from an abductive identification of a phenomenon in one site, before the ethnographer begins tacking back and forth with other sites where a similar structure of meaning making is apparent. Here, while the problem-solving instances may or may not be interdependent in terms of their genesis in the field, they are interdependent in the biography of their analytical development by the ethnographer. We review each in turn.

Multisited Ethnography

Millian comparison presumes that dissimilarities and similarities are analytically explored while holding other things constant. In ethnography, this approach inevitably implies that we can access a common structure of human experience, something that has become contested in certain corners of anthropology. At the end of the twentieth century, anthropologists increasingly questioned representation and the ethnographer's scientific authority as part of a humanist, postmodern critique and postcolonial awareness. The idea of going to a far-away tribe and describing it in light of the reader's own society was deeply problematized as the relationship between anthropology and colonialism was interrogated. Consequently, the notion of *comparison* itself became problematized in a globalizing world, where the assumption that places

and peoples can be treated in isolation became increasingly indefensible. The late modern world seemed to call for multisited, rather than comparative, ethnography.

Multisited ethnography, as advanced by George Marcus (1995), follows an issue across different sites focusing on movement and geographical transformation. The method arose out of the realization that people are globally interconnected through goods, political systems, conflicts, and ideas. Ingredients for a commodity may be gathered in one place, processed in a different part of the world, packaged in another place, and then sold in yet a different global market. Some topics, such as migration, organ trafficking, or commodity chains, immediately suggest movement, and a nimble ethnographic method not stuck to one site allows to capture this movement. The focus of the analysis is on connections and associations, mapping both sites of resistance and accommodation, as well as action-at-a-distance in turn. In an exemplary study, Anna Tsing (2004) examines globalization in an Indonesian rain forest industry not as a homogenizing force but as a set of actions that only receive universality through resistance with local specificity. Such resistance is messy but still influential because it greatly transforms the lives of the people at the bottom rungs of society. Methodologically, Tsing's analysis involves a changing cadre of actors building contingent global alliances based on cultural fantasies about frontiers.

Similarly, within sociology, Dan Menchik (2017) made the case for studying tethered venues, persistent cross-venue linkages that link different sites together across distances and institutions. This approach takes issue with ethnographies that analytically privilege one place and resulting set of similar situations and instead aims to analytically capture the distributed nature of activities. Thus, if one wants to study a teacher's understanding of the curriculum a more traditional ethnography may focus on the classroom work but a multisited ethnography may instead examine the teacher's contacts with their local union, school board meetings, feedback from principal, fellow teachers, and parents, and attendance at professional development meetings. Menchik's point is that we need to study these venues in their own right for the resources they provide and the geographical constraints they put on getting work done.

More generally, any time an ethnographer attends carefully to the complex ways in which actors move among situations, they necessarily construct an intersituational ethnography (Trouille and Tavory 2019). However, these connections are not explicitly comparative. Instead, they break the

ethnographic mold of single site ethnography for following phenomena across different geographic spaces. Instead of studying people on a street corner only, we follow them into their jobs, relationships, leisure, and travels. Instead of a unified self, we study the multiple roles of a distributed self.

Sequential Comparative Ethnography

Whereas intersituational and multisited ethnography focuses on the ways in which different moments are interconnected, there are other ways in which ethnographers can keep their comparative impulse alive. To do so, however, would require us to change some of the ways in which we practically organize external comparisons. With sequential comparative ethnography, we refer to a deliberate form of comparative ethnography where ethnographers explore emerging themes and theoretical puzzles in one site, and only then explore a second site for analytical payoff—only to then tack back and forth between the fields again.

This approach differs from the external comparison modeled (explicitly or implicitly) after experiments in important ways. For many external comparisons, it is critical that the sites are as similar as possible except for one explanatory variable and that the sites are considered independent of each other. The aim of sequential comparison, in contrast, is to strengthen a set of analytical claims by exploring a different site. Sequential comparisons hinge on an abductively emergent theoretical insight as the ground for comparison.

The key practical difference between sequential comparison and most comparative ethnographies is that the decision to expand to a second site is not made in the research design stage but is a decision made halfway through the research project based on accumulated analytical and theoretical insights. The standard for initiating a comparison should be high: the study should lose critical analytical scope if the comparison is not pursued. And, while the difference between such an approach and the more experimental external comparisons seems small—a matter of when we decide to proceed—both the epistemology grounding it, and the potential analytic payoff is large.

This prerequisite takes care of the recurring problem in comparative ethnography—that comparisons do not pan out. Many, probably most, studies start off in the design stage as comparative ethnographies where the explicit goal is to compare two sites. This is often done because of the

conviction that a comparison constitutes some kind of research insurance, in the sense that if one site is good, two sites should be even better, and in the sense that a comparison will guarantee stronger lines of analysis. However, because the comparison has been decided based on what the researcher's hopes and literature review, but prior to the observations, the ethnographer may discover that the sites are either very similar (which renders the comparison moot) or too different to facilitate a meaningful comparison. In other words, these comparisons often falter because the preconceived notions or theoretical expectations remain unfulfilled, either forcing the ethnographer to shave the most interesting aspects of each case to achieve meaningful comparison, or to position the cases side by side with an embarrassed admission that the connection between them is unclear. Instead of a comparative study, the project is often repackaged as a study that took place over two sites without any pretense of comparison. Or alternatively, the researcher stubbornly plays the circus contortionist, pursuing the initial comparison even while the differences in both explanans and explanandum makes such an exercise increasingly tenuous. In the latter case, the researcher is subjected to much criticism related to unwarranted causal statements that probably would not have occurred if they had opted for more modest causal claims limited to one site.

The reason why comparisons often fail in ethnography is no mystery. One of the exciting analytical tasks of conducting ethnographies is figuring out what the site is good for. Most research methodologies presume that the researcher first asks a question and then looks for an answer. Doing ethnography is more akin to solving a Jeopardy puzzle: the accumulated observations come in the form of an answer and the trick is to figure out what the question is. Of course, researchers enter the site with potential scientific and theoretical rationales of what they hope or could find based on their reading of the literature. But these rationales are often only loosely coupled to the actual analytical payoff of the site. Much of qualitative method data analysis tries to answer the reverse engineering question of "What is this set of observations in this site a case of?" Surprising findings, surprising in light of prior expectations and literature or prevailing theories, constitute the portal for abductively puzzling out what is happening from a sociological perspective (Tavory and Timmermans 2014b).

Yet exactly because there is some readjustment of what the study is about from the initial design and theoretical expectations may a comparison be a game changer in broadening the study's analytical scope. Sequential

comparative ethnography requires the researcher to first figure out precisely what case they have in their original site (and this does not need to be one geographical singular site but can be multisited or tethered as we discussed here) and then decide whether it would help to add a second comparative site for analytical purposes, and which kind of site it should be. Adding sites for comparison is not done because it is expected or as a form of analytical insurance in the design stage of the project, but because without adding the second site, the explanation would fall short and the findings stand isolated.

An example: Neil Gong started with a study of mental health provision in Los Angeles' Skid Row (Gong 2019). The problem he saw was that at a time that institutions for the mentally ill had closed down, public resources were scarce, and patients were empowered to refuse treatment, a growing urban mentally ill homeless population posed challenges for city living. Police or mental health authorities could not force the mentally ill into therapy or lock them up just for being on the street and talking to themselves, unless they were a danger to themselves or others. Many found shelters or board-and-care homes undesirable or unworkable, and thus lived on the streets. But the presence of mentally ill homeless people still remained disruptive for businesses and city dwellers. How did city officials address this problem?

With a strong commitment to comparative ethnography at the design stage, Gong initially planned to contrast the United States and Scandinavia, but was unsure whether the axis of variation was different welfare systems, patient rights laws, culture, or something else. He decided to start on Skid Row and hold off until he knew his first case better. Once he delved deeper into his research, Gong observed a highly tolerant approach to engaging mad people who resisted standard treatment placements but could not be legally coerced. Here workers utilized the "housing first" logic—subsidized apartments without disciplinary demands for medication compliance, sobriety, or therapeutic engagement. Given the limited capacity to treat and monitor people, the providers also accepted deviant behavior as largely inevitable. Thus, in stark contrast to theoretical expectations of controlling or paternalistic state services for the poor, these impoverished mad people received considerable autonomy.

Here the combination of civil liberties and the public sector's limited resources for treatment led to a "patient choice" that could be seen as either respectful or neglectful. This then raised an interesting comparative vantage point. What would treatment and choice look like for Los Angeles' wealthy? They also faced a changed institutional landscape of restricted legal means to

compel treatment outside of dangerousness, but were far better resourced. Gong added elite private clinics as a comparison site and found a completely different approach to managing mental health. Here the clinics worked with families to dangle financial support as carrot and stick for treatment compliance, and mobilized intensive therapeutic regimes to try and actually transform privileged patients. This greater capacity for care and surveillance, as well as classed expectations for behavior, also meant a different vision of "choice." Far from tolerating idleness, nonadherence, or substance use, these clinics disciplined clients in the name of health. In isolation, treatment of the poor mentally ill seemed an aberration, but in contrast to how the same problem was dealt with among the wealthy, the significance of these counterintuitive approaches became clearer: the choice for the poor was in part a lack of care capacity, and the constraint of the wealthy was precisely seen as care. The key point was that the grounds for an SES comparison emerged as a result of the initial research.

With Haydu, we may assume that an ethnographic site is good for solving a particular problem but what the exact problems, its contours, resources, consequences, and limits on future aspirations are, is the outcome of the ethnographic inquiry. These results may take the shape of a social mechanism by which various elements change over time. Thus, in a study of the return of newborn screening results, the problem requiring solving was that babies born without any visible symptoms may still be suffering from potential fatal diseases, but the result of the test may also be false (Timmermans and Buchbinder 2010, 2013). Adjudicating the results took time and in the meantime, parents and clinicians wondered whether the infant was sick or should be treated as healthy unless problems manifest. Ethnographically following the journey of parents over various testing regimens showed how the possibility of a diagnosis anchored itself in the lives of the families. Lying out the social mechanism of dealing with fundamental uncertainty in the life course opens up various comparative possibilities. Some of these are shadow comparisons as when connections are drawn with other literatures such as the plight of people facing contested illnesses (Timmermans and Buchbinder 2010) or internal when we compare asymptomatic infants with infants admitted to intensive care units with multiple health issues (Timmermans and Buchbinder 2013).

But the study also opened up more explicit grounds for comparison in our sequential comparative mode, such as with the return of genomic testing results to patients. Here, the ground of comparison is the inherent

indeterminacy of genomic information. Technologically, it is relatively easy to sequence thousands of genes at once, but interpreting the clinical relevance of variants of which little is often known in the biomedical literature becomes an overwhelming, resource-intensive problem that both commercial and academic labs will need to solve if they want to commercialize genomic testing. The comparative hinge linking both projects then is the problem of interpreting an explosion of genetic test results that may have far-reaching consequences for patients. Newborn screening forms a small-scale but high-stakes manifestation of a much larger issue.

This kind of comparison relaxes independency of the external comparison in an additional important way. Sequential comparisons inevitably take place one after the other, and therefore events observed in the second site will take place after the events in the second site. This adds a possible alternative explanation for observed differences; maybe it is not the different sites but the different time points that explain the observed differences. Rather than seeing this confounding as a drawback, it actually opens up analytical opportunities to explore the iterative nature of problem-solving. Gong, for instance, can revisit the Skid Row homeless outreach people to see whether anything in their approach has changed. And in the case of genomic testing, the learning curve of dealing with ambiguous test results across different applications can itself become a topic of inquiry. We can then examine how solutions to similar problems may accrue different meanings, like boundary objects (Star and Griesemer 1989), across sites, undergoing transformations, local adaptations, attempts at standardization, and so forth. Comparing such processes over time and across sites will reveal the power of various stakeholders to impact situations in recursive ways.

Conclusion

Like alpacas ogling grass to graze on, ethnographic observations look always more alluringly green in a next site or in more far away fields, because this promises more food to chew on. However, once we prance to that comparative site, we may find that what looked from a distance to be a thick verdant tapestry of greenery actually is a completely different flora. It is, to push the metaphor to its unsavory limits, much more complicated and less digestible than anticipated. Adding an external comparative component does not create an analytical shortcut. While it opens up opportunities for strengthening an

explanation, it more often results in impoverishing the work we have done in an attempt to create false Millian equivalence.

And yet there is no real question about making comparisons in ethnography. We all make comparisons. The question is, instead, what the relationship between shadow, internal and external comparisons is. Different projects call for different modes of comparisons, and different moments within which external, internal, and shadow comparisons are evoked. The generative question is thus never "Should I compare?" but rather "What kinds of comparisons should I focus on?" As we argued here, to the degree that ethnographer want to throw themselves into external comparisons, a fruitful way to do so is sequentially, after the analytical and theoretical contours of the first site have abductively emerged and a second site promises an incontrovertible payoff.

Note

1. As ideal types, these three kinds of comparisons are simplifications. Especially between external and internal comparisons lies a vast grey zone. Thus, for example, Niewöhner and Scheffer (2010) make the case for comparison as a process of analytical thickening, in which a straightforward comparison between sites is inadvisable because it requires some basic discursive and hermeneutic commonalities that are often lacking but where the comparative ethnographer aims to examine how and when layers of comparison shift, overlap, and respond. This works best at an intermediate level of abstraction. Here, the theoretical construct forms the comparative hinge that connects multiple sites. Because legal terms are often highly context specific, Niewöhner and Scheffer, for example, focus on the indexical meanings of legal discourse across European jurisdictions, following similar problem situations over time in different legal settings. When devoid of local specificity, it is unclear how such theoretical constructs are useful for understanding any of the legal systems.

References

Anderson, Elijah. 1976. *A Place on the Corner*. Chicago: University of Chicago Press.

Becker, Howard S. 1993. "Theory: The Necessary Evil." In *Theory and Concepts in Qualitative Research: Perspectives from the Field*, edited by D. J. Flinders and G. E. Mills, 218–229. New York: Teachers College Press.

Bosk, Charles L. 1979. *Forgive and Remember: Managing Medical Failure*. Chicago: University of Chicago Press.

Burawoy, Michael. 2003. "Revisits: An Outline of a Theory of Reflexive Ethnography." *American Sociological Review* 68: 645–679.

Callon, Michel. 1986. "Some Elements of a Sociology of Translation: Domestication of the Scallops and the Fishermen of St Brieuc Bay." In *Power, Action, and Belief: A New Sociology of Knowledge?*, edited by J. Law, 196–229. London: Routledge.

Duneier, Mitchell. 2004. "Scrutinizing the Heat: On Ethnic Myths and the Importance of Shoe Leather." *Contemporary Sociology* 33, no. 2:139–150.

Eliasoph, Nina, and Paul Lichterman. 2003. "Culture in Interaction." *American Journal of Sociology* 108, no. 4:735–794. https://doi.org/10.1086/367920.

Fine, Gary Alan. 1979. "Small Groups and Culture Creation: The Idioculture of Little League Baseball Teams." *American Sociological Review* 44, no. 5: 733–745.

Fine, Gary Alan. 1983. *Shared Fantasy: Role-Playing Games as Social Worlds*. Chicago: University of Chicago Press.

Fine, Gary Alan. 1996. *Kitchens: The Culture of Restaurant Work*. Berkeley: California University Press.

Fine, Gary Alan. 1998. *Morel Tales: The Culture of Mushrooming*. Boston: Harvard University Press.

Fine, Gary Alan. 2010. *Authors of the Storm: Meteorology and the Culture of Prediction*. Chicago: University of Chicago Press.

Glaser, Barney G. 1965. "The Constant Comparative Method of Qualitative-Analysis." *Social Problems* 12, no. 4: 436–445. https://doi.org/10.1525/sp.1965.12.4.03a00070.

Glaser, Barney, and Anselm Strauss. 1965. *Awareness of Dying*. Chicago: Aldine.

Gong, Neil. 2019. "Between Tolerant Containment and Concerted Constraint: Managing Madness for the City and the Privileged Family." *American Sociological Review* 84, no. 4. 664–689.

Guhin, Jeffrey. Forthcoming. *Make Belief: A Study of Muslim and Christian Schools*. Oxford: Oxford University Press.

Haydu, Jeffrey. 1998. "Making Use of the Past: Time Periods as Cases to Compare and as Sequences of Problem Solving." *American Journal of Sociology* 104, no. 2: 339–371. https://doi.org/10.1086/210041.

Heilman, Samuel. 1976. *Synagogue Life: A Study in Symbolic Interaction*. Chicago: University of Chicago Press.

Hughes, Everett. 1971 [1945]. *The Sociological Eye: Selected Papers*. Chicago: Aldine-Atherton.

Katz, Jack. 2001. "Analytical Induction." In *International Encyclopedia of the Social and Behavioral Sciences*, Vol. 1, edited by N. J. Smelser and P. B. Baltes, 480–484. Oxford: Elsevier.

Kellogg, Katherine C. 2011. *Challenging Operations: Medical Reform and Resistance in Surgery*. Chicago: University of Chicago Press.

Krause, Monika. 2016. "Comparative Research: Beyond Linear-Causal Explanation." In *Practising Comparison. Logics. Relations, Collaborations*, edited J. Deville, M. Guggenheim, and Z. Hrldckova, 480–484. Manchester: Mattering Press.

Lakatos, Imre. 1978. *The Methodology of Scientific Research Programmes*. Cambridge: Cambridge University Press.

Lee, Ching Kwan. 1998. *Gender and the South China Miracle: Two Worlds of Factory Women*. Berkeley: University of California Press.

Liebow, Elliot. 1967. *Tally's Corner: A Study of Negro Streetcorner Men*. Lanham, MD: Rowman & Littlefield.

Marcus, George E. 1995. "Ethnography In/Of the World-System: The Emergence of Multi-Sited Ethnography." *Annual Review of Anthropology* 24: 95–117. https://doi.org/10.1146/annurev.an.24.100195.000523.

Menchik, Daniel A. 2017. "Tethered Venues: Discerning Distant Influences on a Field Site." *Sociological Methods and Research* 48, no. 4: 850–876. https://doi.org/10.1177/0049124117729695.

Mill, John S. 2002 [1843]. *A System of Logic: Ratiocinative and Inductive*. Honolulu: University Press of the Pacific.

Niewöhner, Jörg, and Thomas Scheffer. 2010. *Thick Comparison: Reviving the Ethnographic Aspiration*. Leiden: Brill.

Sewell, William Jr. 1996. "Historical Events as Transformations of Structures: Inventing Revolution at the Bastille." *Theory and Society* 25, no. 6: 841–881.

Sharone, Ofer. 2013. *Flawed System/Flawed Self: Job Searching and Unemployment Experience*. Chicago: University of Chicago Press.

Snyder, Benjamin. 2016. *The Disrupted Workplace: Time and the Moral Order of Flexible Capitalism*. Oxford: Oxford University Press.

Star, Susan Leigh and James R. Griesemer. 1989. "Institutional Ecology, 'Translations' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907–1939." *Social Studies of Science* 19, no 3: 387–420.

Tavory, Iddo, and Stefan Timmermans. 2014a. "A Pragmatic Approach to Causality in Ethnography." *American Journal of Sociology* 119, no. 3: 682–714.

Tavory, Iddo, and Stefan Timmermans. 2014b. *Abductive Analysis: Theorizing Qualitative Research*. Chicago: University of Chicago Press.

Tavory, Iddo, and Daniel Winchester. 2012. "Experiential Careers: The Routinization and De-Routinization of Religious Life." *Theory and Society* 41, no. 2: 351–373.

Timmermans, Stefan, and Mara Buchbinder. 2010. "Patients-in-Waiting: Living Between Sickness and Health in the Genomics Era." *Journal of Health and Social Behavior* 51, no. 4: 408–423.

Timmermans, Stefan, and Mara Buchbinder. 2013. *Saving Babies? The Consequences of Newborn Genetic Screening* Chicago: University of Chicago Press.

Timmermans, Stefan, and Iddo Tavory. 2012. "Theory Construction in Qualitative Research: From Grounded Theory to Abductive Analysis." *Sociological Theory* 30, no. 3: 167–186.

Trouille, David, and Iddo Tavory. 2019. "Shadowing: Warrants for Intersituational Variation in Ethnography." *Sociological Methods and Research* 48, no. 3: 534–560.

Tsing, Anna Lowenhaupt. 2004. *Friction: An Ethnography of Global Connection*. Princeton, NJ: Princeton University Press.

Vaughan, Diane. 1986. *Uncoupling: Turning Points in Intimate Relationships*. New York: Vintage.

Vaughan, Diane. 1992. "Theory Elaboration: The Heuristics of Case Analysis." In *What Is a Case? Exploring the Foundations of Social Inquiry*, edited by C. C. Ragin and H. S. Becker, 173–203. Cambridge: Cambridge University Press.

Vaughan, Diane. 1996. *The Challenger Launch Decision: Risky Technology, Culture and Deviance at NASA*. Chicago: University of Chicago Press.

Vaughan, Diane. 2004. "Theorizing Disaster: Analogy, Historical Ethnography, and the Challenger Accident." *Ethnography* 5, no. 3: 315–347.

Willis, Paul. 1977. *Learning to Labor: How Working Class Kids Get Working Class Jobs*. New York: Columbia University Press.