

Relational ethnography

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Abstract All matters related to ethnography flow from a decision that originates at the very beginning of the research process—the selection of the basic object of analysis—and yet fieldworkers pay scant attention to this crucial task. As a result, most take as their starting point bounded entities delimited by location or social classification and in so doing restrict the kinds of arguments available to them. This article presents the alternative of relational ethnography. Relational ethnography involves studying fields rather than places, boundaries rather than bounded groups, processes rather than processed people, and cultural conflict rather than group culture. While this approach comes with its own set of challenges, it offers an ethnographic method that works with the relational and processual nature of social reality.

Keywords Ethnography · Relational sociology · Philosophy of science · Object of analysis

Most sociological ethnographies are either of *places* (e.g., neighborhoods, workplaces) or *groups* (e.g., single mothers, political activists). This article presents an alternative to group- and place-based fieldwork: *relational ethnography*. Relational ethnography takes as its scientific object neither a bounded group defined by members' shared social attributes nor a location delimited by the boundaries of a particular neighborhood or the walls of an organization but rather processes involving configurations of relations among different actors or institutions.

Since the earliest days of the Chicago School (e.g., Zorbaugh 1929), ethnographers have shown that we cannot hope to understand the dynamics of the social world by comprehending delimited groups as individual cases with semi-autonomous histories and lifestyles. Anthropology, too, has a longstanding interest in studying social relations, going back to the Manchester School (e.g., Mitchell 1956), and has witnessed an efflorescence of relational approaches to fieldwork (Appadurai 1990; Marcus 1998). I am not the first to have discussed nor to have conducted relational ethnography.

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Drawing on previous research as well as my own fieldwork (Desmond 2012a, b, forthcoming), the modest aim of this essay is to render explicit what often remains unspoken in most ethnographic accounts—the ontological underpinnings of various ethnographic objects—and to articulate in detail what a genuinely relational ethnography might entail, recognizing its advantages and limitations. After critically examining group- and place-based approaches, this essay reviews four ways in which one can conduct relational ethnography—by studying fields rather than places, boundaries rather than bounded groups, processes rather than processed people, and cultural conflict rather than group culture. Challenges and tradeoffs specific to relational ethnography are discussed in the concluding pages.

This article does not seek to narrow the ethnographic enterprise to a single “right way” of conducting fieldwork but rather to *broaden and expand* its horizons beyond conventional approaches.¹ Its intent is not to issue some sweeping critique of group- and place-based fieldwork—indeed, these approaches come with nontrivial advantages—but to hold up the act of constructing a basic object of analysis as indispensable to ethnographic methodology. The first and arguably most important decision facing the ethnographer, one from which everything else flows, does not have to do with conducting an inductive or a deductive study or with gaining entrée but with constructing the scientific object. And that decision boils down to the choice between substantialist and relational units of analysis (Cassirer 1923 [1910]; Emirbayer 1997).

The ethnographic object

To break with everyday presuppositions and to construct a sound scientific object: these are the first and most fundamental steps of all analytic endeavors, together forming an essential prelude to social-scientific inquiry. In *The Rules of Sociological Method* (1982 [1895], p. 62), Durkheim warned of “prenotions” that, “resembling ghost-like creatures, distort the true appearance of things, but which we nevertheless mistake for the things themselves.” The social analyst, he believed, must discard all such prenotions and proceed from carefully developed categories, ones grounded in sociological theory and not imported uncritically from public debate or everyday conversation (e.g., Mauss 2003 [1909]). “The sociologist,” Durkheim advised, “either when he decides upon the object of his research or in the course of his investigations, must resolutely deny himself the use of those concepts formed outside science and for needs entirely unscientific. He must free himself from those fallacious notions which hold sway over the mind of the ordinary person, shaking off, once and for all, the yoke of those empirical categories that long habit often makes tyrannical” (pp. 72–73). In *The Craft of Sociology*, Bourdieu and colleagues (1991 [1968], pp. 23, 34) reinforced these injunctions, observing that “[j]ust as the physical sciences had to make a categorical break with animist representations of matter and action on matter, so the social sciences have to perform the epistemological break that can separate scientific interpretation

¹ These days, it seems every sociologist thinks herself or himself a relational thinker (Fourcade 2007; Swartz 2013). Perhaps this is especially true of ethnographers. But simply observing the social world, notebook in hand, does not automatically promote relational sociology. We can analyze ethnographically social interaction without adopting a relational perspective. We can study social relationships in an un-relational way.

from all artificialist or anthropomorphic interpretations of the function of society. ... The task of constructing the object cannot be avoided without abandoning research to preconstructed objects—social facts demarcated, perceived, and named by spontaneous sociology, or ‘social problems,’ whose claims to exist as sociological problems rises with the degree of social reality they have for the sociological community” (see, too, Douglas 1986, pp. 92–99; Mills 1959, p. 226).

Despite the importance of constructing scientific objects, ethnographers have paid far too little mind to this elemental practice.² This is not for want of methodological fastidiousness or lack of reflexivity. On the contrary, since the postmodern turn, ethnographers seem to have picked apart virtually every aspect of fieldwork. They have dissected the epistemological and ontological assumptions of qualitative research (Becker 1996; Shweder 1996), confronted legitimization “crises” (Clifford and Marcus 1986; Denzin 1996), engaged in pointless debates pitting qualitative research against quantitative studies (Schram and Caternio 2006), upheld a longstanding antimony dividing inductivism from deductivism (Burawoy et al. 1991; Glaser and Strauss 1967), devoted much attention to the ethical dimensions of fieldwork (Bosk 2004; Strathern 2000), debated the politics of representation in urban ethnography (Wacquant 2002), and considered the validity of ethnographic claims (Duneier 2006; Klinenberg 2006). They have developed strong (sometimes unbending) positions on gaining entrée, obtaining consent, institutional review boards, insider/outsider ethnography, jotting and coding fieldnotes, recording conversations, analyzing data with computer programs, engaging with sociological theory, voice and representation, reflexivity and standpoint epistemology, taking people at their word, case studies, studying up or down, undercover ethnography, ethnographic writing, the thickness of thick description, using pseudonyms, using photographs, using quotation marks, “member checking” with “informants,” and whether “informants” should be called “informants” in the first place (e.g., Duneier 1999; Emerson et al. 1995). Ethnographers have proven to be as critical and careful about *how* to study something as they have been cavalier about *what* to study. They have overturned every stone save the bedrock, admirably pursuing what appears to be every question related to fieldwork save the most basic: *What, exactly, should be my object?*

For social theorists and philosophers of science, the scientific object should be “won.” It should be “won against the illusion of immediate knowledge” and constitutes the result of much reflexive and analytic labor involving the construction of a theoretical model (Bourdieu et al. 1991 [1968], pp. 13, 57). For a good number of

² Burawoy (1991, pp. 273, 280) comes close—even mentioning the “orienting level of analysis” adopted by various ethnographic approaches and comparing extended case method’s “object of analysis” with grounded theory’s—while stopping short of calling into question the fundamental constitution of ethnographic objects. Burawoy’s eye is trained on the relation between the ethnographic case and theory, historical processes, and macro-structural forces. By “orienting level of analysis,” Burawoy means an argument’s relation to the micro or macro; by “object of analysis,” he means the stuff fieldworkers use to make arguments. The present essay, by contrast, deals with a level of analysis a few strata below those taken up by Burawoy. If Burawoy is concerned primarily with the *back end* of ethnographic methodology—how fieldworkers use data to make arguments and amend theory—I am concerned primarily with the *front end*: how fieldworkers construct their basic objects of analysis. For Burawoy, it is not important that one chooses to study a group of ironworkers, only that one selects a group that maximizes the potential for theoretical reconstruction and that one “seeks to uncover the macro foundations” of the workers’ lives (p. 282). For me, it is important that one chooses to study a group of ironworkers.

ethnographers, by contrast, the scientific object is “discovered”—or, better yet, “stumbled upon.” Venkatesh (2008) explains that he first met gang members operating around the Robert Taylor Homes when trying to administer a questionnaire on urban poverty and being told by the gang leader that if he really wanted to learn about their lives, he needed to embed himself in them.³ Aspiring ethnographers, for their part, rarely are instructed to consider carefully their ethnographic objects. Recycling Robert Park’s old quip about getting “the seat of your pants dirty in real research” (cited in Prus 1996, p. 119), instructors of ethnography seminars hurriedly push their students out the door with little direction. Some require students to select their field site by the first day of class. This approach is laudable, especially as it exposes students right away to the practice of fieldwork. But when students pick some object (any object will do), this usually entails that they bypass the difficult work of constructing a scientific object and instead rely on common categories of thought.

If categories of the common vernacular more often than not present themselves as tidy, atomistic, bounded things, then it is small wonder that most fieldworkers conduct ethnographies of static entities delimited by location or social classification. At first glance, the field of ethnographic research appears lush and variegated, covering all corners of the globe and contributing to a wide array of literatures. Yet if one digs through the topsoil of various topics and geographic settings and searches the ontological substratum in which ethnographic studies are rooted, one finds (by and large) two basic types: ethnographies of *groups* or *places*. Group-based studies primarily concern themselves with the habits, beliefs, behaviors, and interactions of a set of individuals culled together (by society *and* the fieldworker) on the basis of some shared social attribute, be it race, gender, class, sexuality, occupation, political identity, religion, or some other characteristic. There are ethnographies of poor black families (Stack 1974), wildland firefighters (Desmond 2007), meteorologists (Fine 2007), community organizers (Chen 2009). Place-based ethnographies primarily concern themselves with the organization, structure, and dynamics of specific types of social habitats, be they residential (e.g., neighborhoods), occupational (e.g., workplaces), or organizational (e.g., political associations). There are ethnographies of an Italian American slum (Whyte 1943), a Chicano community (Horowitz 1983), an English country village (Bell 1994), luxury hotels (Sherman 2007). Some ethnographies concern themselves with multiple places or groups, as is the case with those that have compared labor processes in the United States with those in other national contexts (e.g., Burawoy 1979; Sallaz 2009). If anything, comparative ethnography only further legitimates “groups” or “places” as basic objects of analysis, for these are the objects being juxtaposed across contexts. With some oversimplification, then, sociological ethnography can be divided by form or object (group or place) and by number of sites (single or multiple) to produce four bins into which most work can be slotted.

³ Who cannot help but wonder, after reading such a captivating account, if the field site found the ethnographer not the other way around? Anthropologists of the classical generation often reserved their most sentimental language for “the encounter.” Here is Malinowski (1984 [1922], p. 4): “Imagine yourself suddenly set down surrounded by all your gear, alone on a tropical beach close to a native village, while the launch or dingy which has brought you sails away out of sight.” Here is Powdermaker (1966, p. 51): “This way my first night in Lesu alone. As I sat on the veranda of my thatched-roofed, two-room house in the early evening ... I asked myself, ‘What on earth am I doing here, all alone and at the end of the world?’”

All this is straightforward, but therein lies the rub. Groups and places present themselves as natural entities for ethnographic exploration because they are the dominant categories of common thought, the stuff of journalism, art, television, much of sociology, and everyday conversation. It is not that groups and places do not work well as ethnographic objects; it is that they work *too well*. Consider Whyte's (1943, p. 272) widely cited finding that a slum district is not, contrary to conventional wisdom of the time, a disorganized community. To understand this claim, one does not need to rewire one's mental map around new sociological concepts. Whyte's argument slides seamlessly into everyday mental categories; it follows the basic structure of popular discourse: *a* is *x*, not *y*. This enshrouds the objects of group- and place-based ethnographies in an essence of conceptual impartiality. But the seemingly innocent decision to carry out an ethnography of police officers, teenagers, Chinatown, or General Motors rests upon countless ontological assumptions, not the least of which is that what the world is made up of is a collection of isolated groups and places. In a phrase, group- and place-based ethnographies are steered by (and help to validate) what Cassirer called the *substantialist perspective*. This perspective accepts as the starting point of inquiry "internally homogeneous and externally distinctive and bounded objects" (Wolf 1982, p. 6), entities separated by economic class, racial or national identification, geographic boundaries, or a number of other such divisions.

Substantialism and arguments

The substantialist perspective has been subjected to three broad criticisms. The first is that it imposes static and atomistic categories onto a world made up of bunches of intertwining interconnections. The analyst operating under the substantialist perspective artificially severs relations between people, places, organizations, or ideas to study these entities in relative isolation, thereby separating what in reality is inseparable and "[positing] discrete, pre-given units such as the individual or society as ultimate starting points of sociological analysis." However, as Emirbayer (1997, pp. 287–288) goes on to say, "[actors] are inseparable from the transactional contexts within which they are embedded. ... By the same token, structures are empty abstractions apart from the several elements of which they are composed; societies themselves are nothing but pluralities of associated individuals." These individuals are connected in organized, patterned ways, and the replication of these relational patterns over time and space is what comes to be known as "structure." "The overall structure, one might say, is precisely the amount of macro-connectedness among micro-situations" (Collins 1988, p. 246)—as opposed to static macro units comprised of intrinsic attributes. Substantialism, in other words, propagates a vision of social reality far from, and in some ways fundamentally opposed to, the very nature of that reality, a vision of the world as a vast collection of isolated entities stacked side-by-side like so many jarred specimens on laboratory shelves (Mills 1959, p. 224). "If there are connections everywhere," asks Wolf (1982, pp. 7, 4), "why do we persist in turning dynamic, interconnected phenomena into static, disconnected things? ... The habit of treating named entities ... as fixed entities opposed to one another by stable internal architecture and external boundaries interferes with our ability to understand their mutual encounter and confrontation. ... Any account of Kru, Fanti, Asante, Ijaw, Igbo, Kongo, Luba,

Lunda, or Ngola that treats each group as a ‘tribe’ sufficient unto itself thus misreads the African past and the African present.”

The substantialist perspective not only snaps apart the totality of interconnected relations of which the social world is composed, but it also presupposes the existence of groups. Substantialism, in a word, promotes a kind of “groupism,” an intellectual habit that ostensibly recognizes the “social construction” of groups but nonetheless fails to document the process or the everyday maintenance of group construction. “*That ethnicity and nationhood are constructed is a commonplace; how they are constructed is seldom specified in detail. ... Despite a quarter-century of constructivist theorizing—or perhaps precisely because constructivism has lost its intellectual edge—ethnic groups continue to be understood as entities and cast as actors. ... Somehow, when we talk about ethnicity, and even more when we talk about ethnic conflict, we almost automatically find ourselves talking about ethnic groups*” (Brubaker et al. 2006, pp. 7–8). When ethnographers decide to study a group that piques their interest, they presuppose that group’s existence. It is not that groups do not exist in the world—this critique is not a renewed call for methodological individualism and is not motivated by an aversion to aggregation—it is that their existence should be treated as a curious something fit for empirical investigation instead of accepted as a given. To presuppose a group, moreover, is to assume that the attributes one uses to draw boundaries around the group—the “blackness” of the black middle class; the “gayness” of young gay men—are the most salient ones to group members. Yet we should exhibit caution when “[treating] as natural and automatic the move from the imposition of racial [or other groupist] categories to the existence of concrete groups that embody those categories. ... [I]t is not axiomatic that membership in a category will correspond directly to experienced group boundaries or social entities” (Loveman 1999, pp. 891–892). By assigning utmost importance to the categories that define a group, ethnographers may unintentionally bias their informants to filter the representation of everyday life through the prism of that category (Rabinow 1977, pp. 118–122, 152–153). They also may concentrate on the significance of, say, one’s racial, religious, or occupational identity at the expense of more understated, but no less important, aspects of the human condition. Lévi-Strauss pondered a similar point in *Tristes Tropiques* (1977 [1955], p. 13): “Instead of simply arranging meetings on a professional basis, doctors with doctors, teachers with teachers and industrialists with industrialists, we might perhaps be led to see that there are more subtle correspondences between individuals and the parts they play.”

A final criticism of the substantialist perspective has to do with “process-reduction.” The phrase is Elias’s and involves “the reduction of processes to static conditions,” resulting in “the changeless aspects of all phenomena being interpreted as most real and significant.” Process-reduction compels analysts “to speak and think as though all ‘objects’ of our thought—including people—were really not only static but uninvolved in relationships as well” (Elias 1978 [1970], pp. 112, 113). Process-reduction may seem like an ill-fitting criticism to level at ethnography. After all, what is fieldwork if not the act of observing people as they *proceed* through their daily lives? Then again, many ethnographic accounts seem to halt time’s passage, especially by employing the timeless present tense (e.g., Bell 1994; Stack 1974). Such accounts provide a snapshot of groups or communities as if frozen in time, an “ethnographic present” to use an old term (Burton 1988). Their objects are not set in motion; they simply “are” (Abbott

2007; Katz 2009). In other instances, ethnographers study, not processes themselves, but *groups or places in process*. These scholars pay mind to temporal processes and contexts but, because they often do not accept processes themselves as their fundamental units of analysis, they tend to end up presenting not accounts of processes per se as much as accounts of things-in-process. This is not processes-reduction as much as *process-circumscription*: the tendency to conform processes to the confines of bounded units of analysis.

Each of these criticisms has deep implications for ethnographic explanation. The analytical possibilities or “explanatory structures” available to any analyst depend on the kinds of questions asked (Somers 1998, p. 769). And the kinds of questions asked, in turn, depend entirely on the constitution of one’s scientific object. If ethnographers select a group or place as their primary unit of analysis, their explanations are limited to claims referencing either external/structural forces or internal/cognitive processes. Tilly (2005) would refer to these types of models as, respectively, “systemic” and “dispositional” accounts. *Systemic* accounts “posit a coherent, self-sustaining entity [e.g., a community, organization, polity] ... [and explain] events inside that entity by their location within the entity as a whole” by referencing *environmental mechanisms*, “externally generated influences on conditions affecting social life.” *Dispositional* accounts “similarly posit coherent entities—in this case more often individuals than any others—but explain the action of those entities by means of their orientation just before the point of action” by referencing *cognitive mechanisms*, “alternations of individual or collective perception” (pp. 14, 26). Ethnographers who favor systemic accounts tend to pitch their study as a “case” of some macro force, viewing groups and places as receptacles of large-scale processes. The aim of ethnography, then, is to “see the big through the small,” to allow for the “examination of large-scale social issues by means of investigation of small-scale social situations” (Stolte et al. 2001, p. 387). The micro field setting is “determined” by macro-historical forces (Burawoy 1991), and the ethnographer’s task is to observe how people “respond” to those forces: how workers respond to changes in the labor process (Burawoy 1979); how poor black families respond to the harsh conditions of urban poverty and racial segregation (Stack 1974). As history lunges forward, the ethnographer lags behind documenting what happens in its wake. Ethnographers who favor dispositional accounts, on the other hand, tend to view groups and places as relatively self-contained social worlds with their own local knowledge and practices. Thus Suttles (1968, p. 9) treats the Addams area as a world unto itself, speaking of a “provincial moral order.” Dispositional ethnographers often focus on group culture and norms and nurture an interest in “small group dynamics in the laboratory” (Fine 2003, p. 46). Some follow Goffman and Garfinkel in documenting the minutiae of social situations while ignoring how those situations are steered by historical and structural processes.

In addition to structural and dispositional accounts, Tilly (2005, pp. 14, 26) proposed (and championed) a third approach: transactional accounts. *Transactional* accounts “take interactions among social sites as their starting points” and explain social action by referencing *relational mechanisms*, which “alter connections among people, groups, and interpersonal networks.” To generate relational mechanisms for social action, one must first take “the basic units of social analysis to be neither individual entities (agent, actor, person, firm) nor structural wholes (society, order, social structure) but the relational processes of interaction between and among identities.” Explanation is found,

then, neither in reference to external forces deemed “macro” or “structural,” nor in the minds or dispositions of self-propelling agents, but in “contingent relational pathways” presented in narrative terms. “In place of a language of essences and inherent causal properties,” a relational approach “substitutes a language of networks and relationships that are not predetermined but made the indeterminate objects of investigation” (Somers 1998, pp. 766–767). This approach to explanation enriches and expands the analytical possibilities of ethnography. But for those possibilities to be unlocked fully, one must adopt a different approach to fieldwork, one that breaks with the substantialist perspective.

A relational approach

Relational ethnography gives ontological primacy, not to groups or places, but to configurations of relations. The point of fieldwork becomes to describe a system of relations, “to show how things hang together in a web of mutual influence or support or interdependence or what have you, to describe the connections between the specifics the ethnographer knows by virtue of being there” (Becker 1996, p. 56). The relational ethnographer designs “[s]trategies of quite literally following connections, associations, and putative relationships” (Marcus 1998, p. 81), its proper object being “chains of interdependence” (Weber 2001, p. 484; see also Beaud and Weber 2003). Relational ethnography is not propelled by the logic of comparison, as is the multisited ethnography of sociology. It does not seek to understand if a certain group or community is peculiar vis-à-vis their counterparts in other contexts. It often does not seek to understand if a certain group or community is anything at all. Rather, it is “designed around chains, paths, threads, conjunctions [that come to support] the argument of the ethnography” (Marcus 1998, p. 90). Most basically, *a relational approach incorporates fully into the ethnographic sample at least two types of actors or agencies occupying different positions within the social space and bound together in a relationship of mutual dependence or struggle*. Let me unpack this definition by reading it backward.

Occupying different positions within the social space and bound together

This is the bright line separating relational ethnography from all other approaches: its focus on dynamics that emerge between groups or agencies qualitatively different from, yet oriented toward and enmeshed with, one another. My position on the primacy of relations is a realist one. This position does not understand, say, the concept of “field” as a heuristic but as an objective space of relations between positions occupied by agents or institutions (Bourdieu and Wacquant 1992, p. 97). It follows that studies that do not investigate the transactions between at least two differently-positioned people or organizations—be they studies of multiple processes conducted by the same set of actors, those of the relations between a past and future self, those focused on interactions between members of the same “group,” or those that seek “the principle of agents’ actions in the interactions between them” instead of in objective relations (Bourdieu 2004, p. 20)—are not relational ethnographies, strictly speaking. To investigate social relations ethnographically one must, at minimum, study multiple actors and agencies who are *engaged with one another* (because they belong to the same field and are

participants in the struggles that define its stakes) *and dissimilar from one another* (because they occupy objectively different positions within that field).

Consider the deeply relational approach Mische (2008) adopted while conducting fieldwork on civic-partisan relations in Brazil. “I did not simply want to understand the characteristics of organizations, nor of the individuals who belonged to them,” she writes, “but rather to examine the intersection of multiple networks—student, religious, NGO, antidiscrimination, professional, and business, as well as partisan—in a changing field” (p. 8). “Characteristics of organizations or individuals” is the stuff of substantialist ethnography. “Intersections of multiple networks” is the stuff of relational ethnography. Notice, too, that Mische studies networks “in a field.” That is, the people she observed were “*field oriented*, directing much of their elaborative work toward the task of marking similarities and differences with other sets of mutually oriented actors” (p. 235). Her ethnographic object was the field of Brazilian youth politics consisting of activists enveloped in relations of conflict and cooperation yet in possession of different outlooks, aims, and resources. If Mische had limited her object to focus on dynamics within a single political organization (or multiple organizations selected for some theoretically-strategic comparative purpose, for that matter), she would have documented *relationships between people*—all ethnography does that—but she would not have documented *social relations across a political field*. The field biologist knows that, say, large animal abundance cannot be explained by reference only to the large animals themselves; rather, the entire ecology of the savanna—the relations between large and small animals, between predator and prey, the availability and geography of insects, grassland, and water—must be taken into account (Pringle et al. 2010). In the same way, the relational ethnographer holds that, say, the political strategies of Brazilian socialists cannot be explained by reference only to interactions between socialists; rather, the structural conditions to which the socialists are oriented—the architecture and internal dynamics of the political field—must be reconstructed.

Where in a field should a fieldworker position herself or himself? Which is to ask: how, precisely, can a set of social relations be studied ethnographically? Relational ethnographers can adopt at least three distinct approaches. The first concentrates on *points of contact and conflict*. The relational ethnographer can observe in real time fight and struggle, cooperation and compromise, misunderstanding and shared meaning-making between actors occupying different positions in a field. To understand some “mechanisms by which the poor come to experience the structural violence perpetuated by the state...[through] corruption, inscription, and governmentality,” Gupta (2012, pp. 33, 77) focuses on the interactions between low-level bureaucrats and poor citizens of Uttar Pradesh. In these confusing, frustrating, and complex interactions, the “workings of a translocal institution ... is made visible in localized practices.” The moment of an abortion clinic opening in Fargo, North Dakota brings together grassroots organizers on both sides of the debate engaged in a political and moral struggle captured in Ginsburg’s *Contested Lives* (1989). Similarly, in her ethnography of the relations of global capital responsible for the destruction of Indonesian rainforests, Tsing (2005, p. xi) sought out strategic settings where worlds collided, settings that threw the most light on international relations and competing interest: “zones of awkward engagement ...[,] of cultural friction.”

The second approach explores the *production of coordinated action* through relational mechanisms. Economic anthropologists and organizational ethnographers long have been concerned with how institutional actors manage to work together by creating routines and shared meaning scripts. This problem can be pursued relationally by, for

one, treating a single organization or market as a field unto itself. For example, Kellogg (2009, p. 657) treats hospitals as “relational spaces” and observes interactions between “middle-manager reformers and subordinate employees” to deepen our understanding of institutional change. Not only hospitals but universities, firms, and civic organizations also are relational spaces of organized striving, competition, and cooperation and can be studied as such. Although organizational ethnographers should be mindful that “the analysis of interactions alone (e.g., among individuals) can never ... suffice to reveal the larger framework of power relations that expresses itself within such interactions—and that helps to frame them in the first place,” they may pay mind to such power relations while studying the struggles that “typically take place within an organization’s own internal *space of struggle for organizational power*” (Emirbayer and Johnson 2008, pp. 22, 25). Alternatively, they may treat the bounds of organizations as porous, taking as their object relational processes involving multiple organizations and different actors within those organizations. To study the creation and consequences of “empowerment talk,” Eliasoph (2011) participated in a “galaxy of empowerment projects” involving multiple organizations (volunteer groups, clubs, summer camps) and differently positioned actors within those organizations (“at-risk youth,” paid organizers, corporate sponsors).

Similarly, an organizational ethnographer could follow Stark’s (2001, p. 77) argument that “the real unit of economic action is increasingly not the isolated firm but networks of firms” and construct an object of multiple firms or agencies in a configuration of relations. Again, Mische’s *Partisan Publics* (2008) is exemplary in this respect. Mische sought out “institutional intersections” to study processes such as political mediation or “communicative practices at the intersection of two or more (partially) disconnected groups, involving the (provisional) conciliation of the identities, projects, or practices associated with those different groups” (pp. 37, 50). A relational approach to organizational ethnography could encourage students to focus more on inter-organizational competition and less on “parallels in organizational development” (Abbott 1988). On the other hand, a study of institutional isomorphism—replication of standard operating procedures and workplace protocol within a network of curiously similar organizations—naturally lends itself to a relational approach, not only with projects that examine transactions between firms (organizations occupying different positions in a coherent field: e.g., the field of unions, universities) but also with those that theorize how organizational practices are informed by the dynamics of more encompassing social fields (e.g., the economic field, the field of intellectual production) that envelop specific organizations and their subfields (Douglas 1986). Organizational practices are the product of (a) dynamics generated within the organization-as-field, (b) inter-organizational copying and coordination (horizontal isomorphism), and (c) processes of “selective mimesis,” whereby organizations chose which aspects of the larger culture they wish to mimic, endorse, and reject (vertical isomorphism) (Desmond 2011, p. 73). A relational approach to organizational practices promotes a trifocal reading sensitive to these multiple levels.⁴

⁴ Yet another approach to the study of how organizational actors coordinate action involves tracing the trajectories of organizational actors across fields to increase understanding of organizational fit, misfires, and the social production of certain ways of thinking. Glaeser (2000) observes how outlooks and mannerisms cultivated by East and West Germans before the fall of the Berlin Wall continue to divide Germans postunification. Desmond (2007) shows how the upbringings of rural, working-class men imbibe them with certain skills and dispositions that prepare and precondition them for working dangerous and demanding jobs.

The third approach fieldworkers have applied to the study of social relations involves documenting the *ecology of a field*, primarily by focusing on the internal logics of distinct but interconnected social worlds. Consider, for example, Zorbaugh's *The Gold Coast and the Slum* (1929). Social disorganization, urban isolation, and political inaction were the problems that propelled Zorbaugh; and he plumbed these problems by taking as his object the (past and present) relations between immigrant groups and various neighborhoods found on Chicago's Near North Side. What emerged from his work was a portrait of the city consisting in fluctuating, morphing relations "not only between the old and the new, between the native and the foreign, but [also] between wealth and poverty, vice and respectability, the conventional and the bohemian, luxury and toil" (Zorbaugh 1929, p. 4). Thus changes among African Americans affected the Italians; the Italians reacted with changes of their own, these in turn affecting the Germans. These actions and reactions, migrations and counter migrations, constituted Zorbaugh's basic object of analysis, but he spent far less time recording interactions between different ethnic groups than documenting the unique subcultures and daily routines of multiple ethnic groups and their neighborhoods and placing them in a larger social and spatial ecology. By doing so, he recognized that various ethnic groups were so intertwined with one another that studying this group or that enclave in isolation made little sense. Regarding slum residents, he wrote, "Groups [accommodating] themselves to one another assimilate one another's folkways and mores. . . . Groups tend to lose their identity, and the social patterns of these groups tend to merge into a hybrid something that is neither Sicilian nor Persian nor Polish, but of the Slum" (p. 152). The analyst who would study Sicilians or Persians alone, then, would risk misidentifying (and therefore reifying) certain cultural patterns *as* Sicilian or *as* Persian—perhaps thinking them imports from the old world—instead of seeing them for what they truly were: cosmopolitan amalgamations resulting from intergroup contact.⁵

At least two types of actors or agencies

Pattillo's *Black on the Block* (2007) is a study of neighborhood renewal in Chicago that centers on conflicts between a wide array of actors: home owners and renters, poor and non-poor residents. Stark's *The Sense of Dissonance* (2009) is an ethnography of the relations among business strategists, interactive designers, programmers, information architects, and merchandizing specialists all involved in the creation of new media. Pine's *The Art of Making Do in Naples* (2012) is an ethnography of a "scene" involving musicians, fans, and criminal organizations. By pursuing relational and processual ethnographic objects—neighborhood renewal, organizational evaluation, the creation and distribution of neomelodica music—works like these follow multiple people who occupy qualitatively different positions in a social space.

Sometimes fieldworkers' commitment to a relational object requires them to follow a large number of actors across multiple social settings. Smith's (2006) ethnography of "transnational life" takes as its object political, cultural, economic, and familial

⁵ Deener (2012) is a contemporary example of a relational ethnography of the ecological variety. Citing Zorbaugh as a model, Deener describes his approach like this: "If I wanted to make sense of this Los Angeles community where immigration, homelessness, countercultural movements, gentrification, and African American segregation have converged in close proximity, I needed to study different neighborhoods, think more clearly about what is happening in each one, and examine the relationships between them" (pp. xii-xiii).

relations between a sending community (Ticuaní, Mexico) and a receiving community (New York City). (His commitment to documenting relational processes is evident in his avoidance of the static terms “transnationalism” or “transmigrants.”) For 15 years, Smith followed members of the Ticuanense community between Mexico and the United States, revealing how life in Ticuaní was intimately and inextricably bound with life in New York. We learn how “cooperation and the negotiation of conflict in New York and Ticuaní have institutionalized a transnational political community and how changes in Mexican national politics have affected local Ticuanense politics in Mexico and in New York” (p. 16). Or consider Tsing’s (2005, pp. xi, ix, emphasis mine) “ethnography of global connection,” which documents “the cultural processes in which certain kinds of predatory business practices, on the one hand, and local empowerment struggles, on the other, came to characterize the rainforests of Indonesia. Large pieces of [her] story draw on fieldwork in the mountains of South Kalimantan, *but this is not a story that can be confined in a village, a province, or a nation*. It is a story of North American investment practices and the stock market, Brazilian rubber trappers’ forest advocacy and United Nations environmental funding, international mountaineering and adventure sports, and democratic politics and the overthrow of the Suharto regime, among other things.” In pursuing this inquiry, Tsing effectively demonstrates that a truly comprehensive explanation of deforestation in Indonesia is not found in the forests of Indonesia alone but in a matrix of global connections, a matrix that *can* be studied ethnographically.

But studying crisscrossing transactions between differently positioned actors need not always require the relational ethnographer to traverse the globe. This is particularly true if a field or market largely is *placed* in a single location. Tsukiji is a single fish market in Tokyo. But it is the largest fish market in the world, and Bestor’s (2004) comprehensive ethnography of Tsukiji’s organization and complex rhythms incorporates truckers and shippers, buyers, loading dock agents, auctioneers, tourists, wholesale and retail customers, government inspectors, sushi chefs, food processors, and fishmongers. Bestor investigates “networks of both vertical and horizontal relationships. . . . Vertically, relatively stable alignments among suppliers, auctioneers, and intermediate wholesalers revolve around the exchange of products and the information flows that such transactions require. Horizontally, trading communities coalesce around specific commodities. These groups become significant institutional actors themselves that create, constrain, and channel particular forms of cooperation and competition among traders operating at equivalent levels within the market system” (p. 182). Tsukiji is a market with transnational influence, but it also is a market with a specific address. In this case, “place matters because sense of space and place is connected with the creation of boundaries, identities, and affiliations. . . . The operations of the marketplace cannot be understood apart from its placement” (p. 17). Deterritorialization need not always be the outcome of a relational approach to ethnography (Smith 2006, pp. 54–55), but locality must be ancillary to relationality.

This raises another issue: namely, that the setting of an ethnography is rarely the subject of it. “The locus of study is not the object of study,” Geertz (1973, p. 22) memorably wrote. “Anthropologists don’t study villages . . . they study *in* villages.” Geertz was right that many ethnographers’ ideas, questions, and contributions are not parochial though their settings may be. Look how much we learn about the underground economy, violence, and masculine honor from a few crack dealers in Spanish Harlem (Bourgeois 1995). Look how much we learn about globalization, popular resistance, and identity from a couple

women in Argentina (Auyero 2003). In the drab visiting room of San Quentin State Prison, we witness emotional-charged dynamics that affect poor families and their imprisoned men throughout the nation (Comfort 2008). All these studies shed light on important problems. But as Geertz himself would point out, confining oneself to a specific locality means viewing the thing you are trying to understand from a single vantage point. “There is a certain value, if you are going to run on about the exploitation of the masses in having seen a Javanese sharecropper turning earth in a tropical downpour or a Moroccan tailor embroidering kaftans by the light of a twenty-watt bulb. But the notion that this gives you the thing entire ... is an idea which only someone too long in the bush could possibly entertain” (p. 22). The purpose of relational ethnography is to get a little closer to the thing entire, to view processes from multiple and even opposing perspectives, to follow—and not just theorize—broader relations of power (Jerolmack 2013, p. 19); to witness the clash first-hand.

The thing entire is beyond the reach of any single ethnographic study. But the goal of our explanations, as Tilly (1992, p. 36, emphasis mine) recognized, is “not to give a ‘complete’ account (whatever that might be) but to *get the main connections right*.” But how do we know which connections are the main ones? When it comes to deciding which relations to study, rather than getting bogged down in theoretical debates ethnographers can adopt a pragmatic approach: certain relationships can be accentuated and others minimized depending on the relevance to a specific research question. This means that *before fully entering the field* the relational ethnographer should spend a considerable amount of time articulating a set of research questions and constructing a scientific object molded around them. What I have in mind is much more demanding than searching for ways to fill gaps in a literature or extending a theory of which one is particularly fond.⁶ Breaking with commonsense categories, constructing a (relational)

⁶ In Burawoy’s (1998, pp. 16, 2) approach, ethnographic inquiry is steered by “our favorite theory ... [which] can span the range from the folk theory of participants to an abstract law. We require only that scientists consider it worth developing. ... The *extended case method* ... [aims] to extract the general from the unique, to move from the ‘micro’ to the ‘macro’, and to connect the present to the past in anticipation of the future, all by building on preexisting theory.” Nothing could be more straightforward than suggesting ethnography should be informed from the start by something that is known about the social world (also known as “theory”) and that it seek to deepen and extend that knowledge. What is of more concern from the perspective of this essay is Burawoy’s (1991, p. 282) charging fieldworkers to discover the “macro foundations” of the “micro” world. This position privileges the macro over the micro, seeing the macro as that which is global, causal, and historical and the micro as that which is local, descriptive, and current (Katz 2002; Tavory and Timmermans 2009). Accordingly, it encourages ethnographers to modify *systemic accounts* of social life that revolve around macro constructions (e.g., nation-state) and explain human interaction by reference to external forces generated by those constructions. The field site is always a “case” or “instance” of something larger, and the ethnographer can observe effects but cannot document causes. Yet, as Collins (1988, pp. 245–246) has argued, “the macro-social world ... consists of human beings interacting; what is macro is their patterns across time and space. ... To speak of a ‘world system’ or anything else is just a gloss, a verbal category we use for convenience in summarizing such patterns.” The structure is in the social relations; connections between micro situations are the building blocks of macro patterns. This perspective is not reductionist—Collins repeatedly points out the irreducibility of some macro features—nor does it lead us to the “interactionist” or “ethnographic” fallacy, whereby you “define reality [only] by what you see” (Duneier 1999, p. 343); history, theory, and social-scientific knowledge are crucial to constructing the ethnographic object. But it does privilege *transactional accounts* built on relational mechanisms. “In a relational view, inequality[, for example,] emerges from asymmetrical social interactions in which advantages accumulate on one side of the other, fortified by construction of social categories that justify and sustain unequal advantage. ... Explanation of inequality and its changes must therefore concentrate on identifying combinations and sequences of causal mechanisms—notably exploitation, opportunity hoarding, emulation, and adaptation—within episodes of social interaction” (Tilly 2005, pp. 100, 107).

object, and developing a corresponding set of research questions—these are the time-consuming but necessary preliminaries that can help a fieldworker identify a core set of relations that can guide the beginning stages of fieldwork.

When beginning to construct our object, it can be clarifying to restrict it to a relationship between two points in a field. Relational ethnography need not entail observing a great many parties, a sprawling and unwieldy set of connections. Several relational ethnographies primarily revolve around two parties: local bureaucrats and poor people (Gupta 2012), East and West Germans (Glaeser 2000), hospital managers and their employees (Kellogg 2009), tenants and landlords (Desmond *forthcoming*). Another effective entry point into relational ethnography is to begin by observing brokers or mediators. This was Auyero's (2000, p. 91) approach to investigating the "Peronist problem-solving network" in Argentina, that network consisting of "a series of wheels of irregular shape, pivoting around the different brokers." By following a class of people whose job and identity depends on understanding and leveraging social relations, connecting the local community to the larger political system, Auyero's fieldwork necessarily encompassed a relational object. A relational space can seem limitless, but what is most important is not the number of relations one studies but that relations themselves—not relations artificially bounded within groups or places—come to be the building blocks of ethnographic explanation.

There is only so much you can know before entering the field. The work of constructing a scientific object is not relegated only to the beginning of the research endeavor but is "a protracted and exacting task that is accomplished little by little, through a whole series of small rectifications and amendments" that take place over the course of the fieldwork (Bourdieu and Wacquant 1992, p. 228). Constructing a relational object, then, entails breaking not only with commonsense categories and unexamined assumptions about social reality but also with out-of-touch scholasticism that negates the complexity of social practice by interpreting it through theoretical categories considered to be superior to the "folk" categories of everyday life. People's own interpretations of relational configurations might turn out to be more consequential to understanding their beliefs and actions than the relations the fieldworker initially thought most pivotal. On second consideration, then, constructing the ethnographic object requires a double break: a break with commonsense and its substantialism as well as with theoretical interpretation and its scholasticism (Emirbayer and Desmond *forthcoming*). The end result is an ethnographic posture that "consists of a highly unlikely combination of definite ambition, which leads one to take a broad [and relational] view... and the great modesty indispensable in burying oneself in the fullest detail of the object" (Bourdieu and Wacquant 1992, p. 252).⁷

⁷ More than anyone else, ethnomethodologists and conversation analysts have "buried themselves in the fullest detail of the object"—by refusing, e.g., "to dismiss what persons said or how they said it as 'just as manner of speaking'" (Schegloff 2007, p. 465)—and in so doing have shown that the categories people deploy in everyday conversation can illuminate the broader social relations around which they organize their lives. As Zimmerman and Wieder (1977, p. 199) learned in their insightful ethnography of marijuana smoking, for example, "treating informants' statements as quasi-social-scientific observations diverts attention from a research task prior to the determination of the truth of a given account, namely, the specification of how to *hear* remarks like, 'You can't help but get stoned,' or 'smoking is spontaneous,' that is to locate their sense within the context of the social arrangements of which they are a part and which they describe. Such statements are not so much propositions advancing truth-claims as they are *instructions* informing the initiate or the outsider how to 'see' or 'understand' events from the point of the native."

Full incorporation into the ethnographic sample

The final element of relational ethnography is the easiest to explain in principle and the hardest to implement in practice. The above two sections dealt with what I meant by “relational.” This section has to do with what I mean by “ethnography.” Ethnography is what you do when you try to understand people by allowing their lives to mold your own as fully and genuinely as possible. This is accomplished by building rapport with the people you want to know better and following them over a long stretch of time, observing and experiencing what they do, living and working and playing where they do, and recording as much action and interaction as you can until you begin to move like they move, talk like they talk, think like they think, and feel something like they feel. Relational ethnography simply entails this be done with people occupying qualitatively different positions in a field. In my experience, this meant getting just as deep with landlords as with tenants (Desmond 2012b, *forthcoming*). For now, let us set aside the pragmatic challenges of this approach, which are discussed later, and confront a question raised by this issue, a question about an argument that up to this point has been implied but neither specified nor defended. Does relational thinking always follow from constructing a scientific object? And if so, does relational thinking always require fieldworkers to conduct relational ethnography?

Given the criticisms of substantialism discussed above, it is difficult to imagine a scenario in which a researcher who dedicated considerable time and effort to constructing a scientific object would not adopt a relational point of view. The work of constructing the object contains within it the act of rejecting “‘everyday essentialism,’ which naturally reifies what are really relations into substances. ... If common sense spontaneously adheres to a substantialist philosophy, science reflexively and methodically deconstructs phenomenal substances in order to reconstruct the phenomenon as a tangled tissue of relations” (Vandenberghe 1999, pp. 42–43). “It is easier to think in terms of realities that can be ‘touched with the finger,’” Bourdieu wrote (Bourdieu and Wacquant 1992, pp. 228, 232), “...than in terms of relations. ... Most of the time, researchers take as objects of research ... more or less arbitrarily defined populations, produced through the successive partitioning of an initial category that is itself pre-constructed: the ‘elderly,’ the ‘young,’ ‘immigrants,’ ‘semi-professions,’ the ‘poverty population,’ and so on. ... [But] the real is the relational.” Wrestling a reflexive ethnographic object from the pre-constructed involves recognizing the primacy of relations in everyday life: that “unfolding transactions, and not preconstructed attributes, are ... what most effectively explain” social reality (Emirbayer 1997, p. 293). Accordingly, if fieldworkers’ attempts to break with everyday modes of thought, which are themselves products of the substantialist perspective, result only in their arriving back at substantialism, then they either have not fully carried through an epistemological break or they subscribe to a very peculiar metaphysics not in fashion since the thirteenth century.⁸

Constructing an ethnographic object should always lead to relational epistemology, but it will not always lead to relational ethnography. A fieldworker can be a relational

⁸ One can begin with a group or place but not end there. A fieldworker may start by setting herself or himself down among a particular group or within a particular neighborhood, but as the fieldwork progresses, she or he may branch out to other groups, settings, and contexts, exploring other positions in the field. What begins as a group- or place-ethnography may transform into a relational inquiry.

thinker but for any number of reasons not conduct a relational ethnography. Carrying out an ethnography of a group does not make one “groupist.” One can study how that group comes to be constructed as such, place that group in a larger system of relations, and increase the magnification to capture the internal logic, style, and practices of that particular group. “The truth of these small preconstructed objects, of this given which offers itself, is in the relation with a whole ensemble of preconstructed objects which does not give itself at all, which is hidden from indigenous perception, and even from learned enquiry.” After making this observation and arguing that this kind of relational realization can only be accomplished when “the scientific fact is *constructed*,” Bourdieu (1992, pp. 44–45, 43) admits that “the work of construction is difficult. Often, one has to be content with a small isolated subject; the logically valid construction of an object is not always attainable for a single researcher, or even for a group of them.”

Access often prevents researchers from expanding their fieldwork beyond a single group or place. Auyero and Swistun’s (2009) investigation into environmental suffering in an Argentine shantytown adopts a relational perspective: it is about the how the lives of poor families are damaged and cut short by the multinational corporations responsible for polluting their soil, air, and water. But the ethnographers were not allowed behind the doors of those multinational corporations. So they filled in the holes best they could, interviewing company personnel, activists, and lawyers. On other occasions, relational thinkers may chose purposefully to limit their ethnographic sample depending on the questions that keep them up and night. Wacquant’s (2004, p. vii) questions about how one becomes a boxer—corporeally, mentally, spiritually—meant that his object would be men of “flesh, nerves, and senses,” not a configuration of social relations. Halle’s (1984, pp. xi, 292) questions about factory workers lead him to follow them through multiple fields as he aimed “to present a total picture of workers’ lives—their jobs, family relations and leisure activities, values and ideology, and their views on religion, ethnicity, politics, and social class” as opposed to “viewing part of the picture as the whole.” Each person is a deep well and a repository of historical, familial, cultural, and social forces. And even individuals can be viewed relationally by, for example, seeing them as a “field made flesh,” considering their cognitive structures as reflective of the structure of the broader field (Bourdieu 2004, p. 41; see also Emirbayer 1997, p. 296; Lichterman 2012).

Fields, boundaries, processes, conflict

What are some guidelines ethnographers can draw upon when attempting to construct a relational object of analysis? I offer four here (by no means an exhaustive list). One can study: (1) *fields rather than places*; (2) *boundaries rather than bounded groups*; (3) *processes rather than processed people*; and (4) *cultural conflict rather than group culture*. To expand on these approaches, I draw on an ethnography I conducted of the process of eviction in Milwaukee’s low-income housing market (Desmond [forthcoming](#)). By taking this process as my scientific object, I was able to explore the interactions and transactions between various actors involved in the process. While living for over a year in two very poor neighborhoods, I met landlords conducting evictions, tenants experiencing eviction, and spent time as well with many other people involved in the process. I would follow multiple parties both through the eviction—which could stretch on for months—and, during that time, would be introduced to

people in their network: kin, ex-boyfriends, pastors, caseworkers, building managers, lawyers, dope suppliers, and so forth. The goal of this project was to study urban poverty without taking the urban poor or a poor community as my basic object of analysis.

Study fields rather than places

Ethnographers undertaking community studies can overestimate the importance of their chosen analytic category, such as, for example, when they view relationships between people in close proximity to one another as more important than those between people separated by a considerable distance. As Robert Park observed in his introduction to *The Gold Coast and the Slum* (Zorbaugh 1929, p. xix), “Our political system is founded upon the conviction that people who live in the same locality have common interests, and that they can therefore be relied upon to act together for their common welfare. This assumption, as it turns out, is not valid for large cities.” While living in inner-city Milwaukee, many of my neighbors hardly knew one another, but inexpensive prepay wireless plans allowed them to stay in touch with friends, family members, and new acquaintances living throughout the city, state, and nation. And the women with whom I lived who sometimes worked as prostitutes drew clients from outside their neighborhoods (and outside their state) by advertising on the Internet. The Milwaukee ghetto was not the “city within a city” of which Drake and Cayton (1945, p. 12) spoke; it did not resemble Suttles’s slum comprising “territorial groups” governed by a local moral order. The social ties of its residents, rather, extended far beyond the confines of their neighborhoods. In contrast to place-based studies, relational ethnography can follow Zorbaugh (1929, p. 16) in treating the word “community” as “little more than a geographical expression” and analyze relations and transactions that transcend the borders of a single “neighborhood,” artificial borders usually erected by the sociologist in the first place as a matter of analytic convenience (Burawoy 2000). If people’s lives are not bounded by places—by this neighborhood or that organization—then why are our accounts of them? The relational ethnographer has no place to lay her head, so to speak; she does not amputate social relations through the imposition of categories based on bounded places. Rather, she attempts to reconstruct a network of relations that guide everyday life, studying fields rather than places.

Ethnographers may be tempted to leave field theory to historical sociologists and network analysts. Those working in the Bourdieusian tradition much prefer the Bourdieu of the *habitus* to the Bourdieu of fields. But in its commitment to documenting everyday life in fine detail, ethnography can contribute significantly to field theory. As Martin (2003, p. 35) describes it, “field theory implies a confident pursuit of what Lewin ... called ‘the full concreteness of the particular situation.’ ... Concreteness, or attention to the particularities of this case, far from being assumed to lead to a ‘small picture’ is inseparable from the field theorists’ emphasis on totality and synthesis.” In documenting ground-level dynamics and particularities of social situations, the relational ethnographer can contribute to field theory’s pursuit of the whole picture. And in addition to mapping out the terrain and complex dynamics of social spaces, field-theoretic fieldworkers also can work to reconstruct the perspectives of various actors situated at different points in the field. Guided by the internal structure of a field, one can “endeavor to situate oneself at the *geometric vantage point* in the various perspectives from which one can see, at the same time, both what can and what cannot be perceived from each of these separate points of view. ... The profit is less the ‘extraordinary’ man of

whom Weber spoke than the man of extraordinary situations” (Bourdieu 1991, pp. 2, 35, emphasis mine). The people the ethnographer follows, then, come to be viewed not so much as “ready-made individuals” but as “outcomes, the evolving product of bundles of relationships with other persons” (Weber 2001, p. 490; see also Mills 1959, p. 225).

Study boundaries rather than bounded groups

Once analysts accept a group’s existence as given, they may issue claims about the group’s actions and beliefs as if they were unique to the group; or they may come to view individuals’ actions only in relation to a single vector (e.g., “Mormon”), instead of viewing them as oriented toward multiple vectors (e.g., familial, social, religious, occupational) at different points in time (Waters 1990; Weber 2001). This often is problematic. Consider Whyte’s (1943) finding that slum dwellers were “organized,” a finding that was (and still is) considered a significant and surprising contribution. From the beginning, Whyte presupposed the existence of a thing called a “slum,” and in applying descriptions to that thing helped to reify its existence. “I began with a vague idea that I wanted to study a slum district,” he wrote. “... I made my choice on very unscientific grounds: Cornerville best fitted my picture of what a slum district should look like. Somehow I had developed a picture of run-down three- to five-story buildings crowded together” (Whyte 1943, p. 283). Whyte decided to take as his object of study a “slum” and did so by identifying an area of Boston that fit his preconceptions of “slumness”: overcrowded, ethnic, poor. If, however, he viewed a “slum” as Jane Jacobs (1961) did—as nothing more than a category of city planning arbitrarily affixed to immigrant and poor neighborhoods—then his finding would have appeared trivial and pedestrian: the neighborhood he studied was organized, like all others. Whyte relied on the categories of “the pseudoscience of city planning” (Jacobs 1961, p. 13) to challenged conventional wisdom only by flipping the fundamental description of the slum. Perhaps he could have issued a much more profound challenge by rejecting the very concepts and ontological propositions on which conventional wisdom relied, questioning not the description of the slum itself but the widespread and unquestioned belief in its existence. Or he could have taken as his object of study, not the slum itself, but the classification process of dividing slum from non-slum, the defining bounds of “slumness.”⁹

⁹ In other words, if Whyte was surprised it was because his conception of what constituted a “slum” was the same as that of city planners. Constructing one’s object of analysis offers the researcher an opportunity to break with such common preconceptions and to increase one’s mindfulness of the assumptions that undergird research questions. But this does not mean that we should always avoid addressing directly questions initially formed by policymakers or vocal segments of the public. Doing that would risk undermining sociology’s contribution to the public conversation and one of ethnography’s primary warrants: to challenge stereotypes about stigmatized groups and humanize the degraded (Katz 1997). And it would be wrong to think that questions that emerged from social-scientific research and theory were not themselves products of cultural presuppositions created within one’s own discipline. This is why Bourdieu (2000 [1997]) observes that common sense assumptions preconstruct our objects of inquiry at three distinct levels. The first level concerns our *social unconscious*, our race, class, gender, education, religion, and so on—our position in the social space. The second level concerns our *disciplinary unconscious*, how our training as sociologists or anthropologists profoundly shapes what we see and fail to see in the field. The third level concerns the *scholastic unconscious*: the collection of unspoken presuppositions that accompany the intellectual’s privilege “to withdraw from the world so as to think it” (Emirbayer and Desmond 2012, forthcoming). Breaking with the first level only to overlook the second and third just means choosing one type of presupposition (e.g., intellectual) over another (e.g., social). The goal is to attempt to master one’s presuppositions as much as possible; to be critical about the terms we employ; and sometimes to pose different questions from those prepackaged for us.

For some time now, scholars have promoted the study of boundaries. They have noted that studying boundaries involves investigating “relational processes at work across a wide range of social phenomena, institutions, and locations” (Lamont and Molnár 2002, p. 169) as well as shifting attention from “groups” to “‘group-making’ and ‘grouping’ activities such as classification, categorization, and identification” (Brubaker et al. 2004, p. 45). Reflecting on Heidegger’s observations on boundaries, one philosopher (Klaver 2004, p. 46) has observed: “[Boundaries] are mediators, they trigger further developments, new trains of thought, new events. The boundary of a river is not just its banks, not even its floodplain, but its whole watershed: the nitrogen-rich agriculture in the Midwest creates a dead zone in the Gulf of Mexico.” Thinking critically about boundaries, then, requires thinking relationally. It would require, for example, carrying out an ethnography of the classification struggles within the religious field over the designation of “cult” (rather than an ethnography of a cult) or conducting an ethnography of the racialized dynamics of residential segregation (rather than an ethnography of a segregated neighborhood). Ethnographies of boundary formation, perseverance, and dissolution construct claims around what Tilly (2004) called “social boundary mechanisms,” processes that both cause and constitute boundary change.

Study processes rather than processed people

Above I argued that group- and place-based ethnographies often render dynamic processes in a state of rest (process-reduction) or contort processes to conform to the bounds of imposed analytic objects (process-circumscription). Ethnographers have yet to realize their full potential in the construction of processual and causal models of social reality (Glaeser 2006). The importance of macro-historical forces to sociological thought and ethnographic explanation is indisputable, but ethnographers need not relegate the study of social processes to comparative-historical sociologists alone; need not be the humble servant to a “macro explanation,” patching holes here and there; need not cede expertise to theorists of the social world who hardly have walked its streets. Consider, for example, Snow’s classic, *On the Mode of Communication of Cholera* (1965 [1855]). Snow set himself the task of understanding how cholera spread and employed systematic fieldwork to do so. His approach, moreover, was thoroughly relational. He did not study cholera patients alone, but the connections between the infected and the healthy as well as between the infected and her or his environment: comparing tenement buildings and poorhouses with high infection rates to those with low rates and mapping where cholera cases occurred in the city. As a result, Snow discovered the mechanism by which cholera spread—contaminated water—and recommended solutions: shut down infected pumps, boil drinking water, isolate the sick, and disinfect their discharges.

If processes live in relations, in intergroup competition and collaboration, in flows between institutions and their inhabitants—as social theorists long have held (Elias 2000 [1939]; Tilly 1998)—then studying them requires abandoning the group or place as the *locus classicus* of ethnographic exploration and taking processes themselves as the fundamental units of analysis. This is why I took as my object the process of eviction, not evicted tenants, evicting landlords, eviction court, or a poor neighborhood with high eviction rates. Eviction is a process involving a multiplicity of actors (e.g., tenants, landlords, lawyers, family members) and settings (e.g., tenants’ poor neighborhoods, landlords’ suburban homes, eviction court, jail, homeless shelters). “[H]as there ever been

a time,” Wolf (1982, pp. 19, 17) has asked, “when human populations have existed in independence of larger encompassing relationships, unaffected by larger fields of force? ... [T]he concept of the autonomous, self-regulating and self-justifying society and culture has trapped inside the bounds of its own definitions. ... [But what if] we take cognizance of *processes* that transcend separable cases, moving through and beyond them and transforming them as they proceed?” What if we were to study ethnographically the dynamics of modern-day slavery (rather than slaves) or the process of migration (rather than migrants)? Social facts, the ethnographer of processes well knows, should be treated as “*ings*.”¹⁰ At its best, then, relational ethnography “sees relations between terms of units as preeminently dynamic in nature, as unfolding, ongoing processes rather than as static ties among inert substances” (Emirbayer 1997, p. 289). This promotes an approach to social explanation that involves documenting sequences of connected events that are spatially and temporally contingent, what has been called “causal narrativity” (Somers 1998) or “narrative positivism” (Abbott 1992).

Study cultural conflict rather than group culture

A final way fieldworkers might carry out a relational ethnography involves the study of culture, the original ethnographic object. I limit my comments here mainly to the study of culture and poverty, but my more general points are not for poverty researchers alone. Recently, sociologists have called for a return to the study of culture in the inner city (e.g., Lamont and Small 2008; Wilson 2009), and urban ethnographers have rediscovered their longstanding interest in the role of cultural dynamics in the persistence of poverty (e.g., Sánchez-Jankowski 2008). Beginning with Liebow (1967) and Stack (1974), urban ethnographers incorporating cultural explanations into their framework were quick to distance themselves from Lewis’s (1959) “culture of poverty” thesis, which they found blames the victim. Overlooked, however, was Lewis’s more fundamental error: that of attributing to a bounded, homogeneous group (“the poor”) a clear set of behaviors, attitudes, and practices.¹¹ The many critics of Lewis went on to

¹⁰ This saying—a twist on Durkheim’s (1982 [1895], p. 35) well-known injunction that “social facts must be treated as things”—has been attributed to Melvin Pollner (Douglas Maynard, personal communication, May 24, 2010).

¹¹ People respond to structural conditions, like food scarcity or concentrated disadvantage, with patterned actions and beliefs. Lewis (1959) thought those patterned actions and beliefs themselves become sentient and help to reinforce the conditions that originally produced them. It is in this moment of sentience that those actions and beliefs are thought to constitute something like a “culture”—enduring and shared views and practices—instead of something more fleeting and circumstantially necessary. Omitted from this model are *institutions* that occupy a space between people and structural conditions and that encode disadvantage in peoples’ language, habits, belief systems, and practices. Resource-poor schools in low-income neighborhoods often leave children with subpar language and critical thinking skills (Paulle 2013). Those deficits will remain even if those children relocate to safe and wealthy neighborhoods later in life. To think of those school-conditioned speech patterns and belief systems as evidence of a “culture of poverty,” the invention of poor families themselves, is to overlook the mark left by broken cultural institutions through which low-income families pass. We do not think that the rich are rich because they invented a “culture of affluence” but because they pass through elite institutions that modify their behavior, habits, and worldviews, and this constellation of skills and ways of being, in turn, eases their entrance into other elite institutions (Khan 2012). What might be viewed as a “culture of affluence” is, simply, affluence; and “many of the features alleged to characterize the culture of poverty...are simply definitions of poverty itself” (Stack 1974, p. 23). Students seeking to understand the cultural dimensions of economic deprivation, stability, or luxury would gain more from mapping the linked ecology of institutions that help determine one’s social trajectory than presupposing the existence of bounded economic groups with shared values and practices.

commit this error themselves, by asserting that poor inner-city blacks have developed an “oppositional culture” or by viewing the urban poor as themselves “middle-class people without money,” as Sánchez-Jankowski (2008, p. 48) has observed. Hoping to avoid charges of essentialism, ethnographers now take pains to emphasize variation; theirs is the language of plurality. Hays (2004) speaks not of a “culture of poverty” but of “cultures of poverty.” This is a welcomed development, but the convention of adding an “s” does not address the deeper issue: that in their tethering to the urban poor a collection of discernable cultural attributes—whether positive or negative, singular or plural—ethnographic accounts of the cultural dimensions of poverty resemble, at their most basic level, the culture of poverty thesis itself, the very idea they most hoped to supplant.

Urban ethnographers thus rearrange the descriptors affixed to the “urban poor,” but the decision to accept the “urban poor” as the basic unit of analysis often goes unquestioned (Gans 1996). In so doing, ethnographers can act like “ethnopolitical entrepreneurs,” “unwittingly *doubling* or *reinforcing* the reification of [the urban poor] in ethnopolitical practice with a reification of [this group] in social analysis” (Brubaker 2003, p. 554). Perhaps the group most thoroughly reified by urban ethnographers, however, is not in fact the “urban poor” but the “middle class.” Just as black letters of text are accentuated against the white backdrop of the page, in many ethnographic accounts the culture and values of the urban poor are constantly, if implicitly, compared to “mainstream culture” and “middle-class values” (e.g., Liebow 1967, p. 8; Stack 1974, p. 43). Such analysts’ claims about the cultural traits of the urban poor—which are never about those traits themselves but those traits *in relation* to those of other groups—must be treated as suspect if for no other reason than because they study poor people in isolation. How, after all, can one marshal claims about the “oppositional culture” of one group without embedding that group in a configuration of relations comprised of other groups to which the former’s cultural traits supposedly are formed in opposition? One can only do so by imagining the existence of a “mainstream,” a group numbering in the millions that manages to possess a widely shared, unquestionably stable, internally consistent set of opinions, values, and behaviors that can be deployed like plumbines against which non-middle-class communities are measured. Here is one occasion when ethnographers do not add an “s.”

The relational perspective breaks with the convention of viewing culture as the stuff of “individual ‘attitudes’ or ‘values’” and instead locates culture in “bundles of communications, relations, and transactions” (Emirbayer 1997, p. 300). There are two prominent relational perspectives to the study of culture. The first maps a structural network of cultural forms. Here, culture is understood to be a linked constellation of narratives, concepts, and practices: a “conceptual network” that reveals the shared meaning of a single idea or ritual by locating it within a web of other ideas and rituals (Somers 1995). The second perspective documents the cultural dynamics of relational transactions. This view does not view “cultural forms as autonomous from networks” but focuses instead on “networks themselves as composed of cultural processes of communicative interaction” (Mische 2011, p. 88). If we combine these perspectives, we are left with an image of a historically-constituted conceptual network on which linked people and organizations draw to coordinate action, create shared meaning, and ultimately to reconstitute the conceptual network they have inherited. Ethnographers long have observed that “group styles filter collective representations,” as “actors make

meaning with collective representations, and they do so in a way that usually complements the meaningful, shared ground [group style] for interaction” (Eliasoph and Lichterman 2003, pp. 782, 737). What comes to be known as “group culture” is observed in *harmonizing* interactions: stable and shared beliefs and practices held by a collection of like people who come to think of themselves as a group (Fine 1979).

Relational ethnographers can approach the problem of meaning making from another angle, focusing not on the harmony but the *collision*: competition between people structurally *dissimilar* to one another. Studying eviction allowed me to examine how meaning was made through relations between landlords and tenants, tenants and family members, and so forth. It also allowed me to investigate the cultural foundations of (often consequential) misunderstandings between individuals possessing varying amounts of economic and cultural capital: the friction created between a soft-spoken, well-educated white judge and a black tenant with a sixth grade education who articulates her frustrations by yelling; or the different meanings landlords and tenants assign to rent, the former viewing it in relation to the housing market (and thus finding it low), the latter as a proportion of her income (and thus finding it high). A relational approach to the study of culture would not seek to determine if x group holds y value on z issue, a ubiquitous formula in urban ethnography and other qualitative studies of low-income populations. Rather, it would follow the social anthropologist Fredrik Barth (1969) and the literary critic Homi Bhabha (1994) in exploring the dynamics of meaning making, the cultivation of belief systems and value orientations, and the deployment of habits and skills as they occur in interstitial, friction-filled realms between people or organizations occupying different positions in a social space. Not, then, an ethnography of the “culture (or cultures) of the poor,” but a study of the cultural dimensions of relations between actors and agencies implicated in the reproduction of urban poverty, these together constituting a thick web of poor and non-poor people alike.

There is quite a bit more at stake here than research design or the philosophy of social science. As Katz (1997, p. 416) has observed, ethnography “plays a role of unappreciated significance as a vigorous form of *policy research*” because the method is well suited “not simply to dismantle bad stereotypes but rather to construct good stereotypes by producing a more holistic and satisfying summary view of the subject.” Ethnographic fieldwork, when done with commitment and humility, can offer a powerful corrective to thin and uncharitable beliefs about marginalized people or institutions. Relational ethnography goes further, not only challenging the stereotypes affixed to stigmatized people or places (e.g., lazy, violent) but also by rejecting the basic conceptual formula on which those stereotypes rely: that is, a is x , where a is a bounded group or place and x its defining attributes. In taking as their objects disadvantaged people and their neighborhoods, many urban ethnographers *accept poverty as a given*, as opposed to treating it as an active project involving people far removed from the gritty street corner where the fieldworker has chosen to plant himself or herself. Poverty is not a thing; it is a relation. And it is not just a relation between past and present, nor only between “macro structures” and “micro settings”; it is a relation between winners and losers, extractors and the extracted, discipliners and the disciplined—all bound together in real time. The slum, if we are to call it that, has never been a byproduct of the modern city, a sad accident of industrialization and urbanization. The slum has always been a main feature of landed capital, a prime moneymaker

for those who saw ripe opportunity in land scarcity, housing dilapidation, and racial segregation (Mumford 1961; Satter 2009). If exploitation long has helped to create the slum and its inhabitants, if it long has been a clear, direct, and systematic cause of scarcity and social suffering, why has this ugly word been erased from the poverty debate? Surely one reason is that researchers in general, and ethnographers in particular, have ignored poverty's profiteers, an absence that is a direct result of a substantialist perspective on poverty. The epistemological is the political.

Challenges and tradeoffs

Up to this point, I have been concerned only with the possible advantages of relational ethnography and have said nothing of its limitations. This section reviews the challenges and tradeoffs inherent in a relational approach to fieldwork. Specifically, it focuses on five issues: *entrée*, *depth*, *boundaries*, *writing*, and *generalizability*. Group- and place-based ethnography possess some nontrivial advantages over relational ethnography, especially with respect to practical matters of fieldwork.

Gaining entrée is among the most difficult and frustrating aspects of fieldwork. The ethnographer often is rebuffed, avoided, exploited, hoodwinked, provided only limited access, or taken down paths that lead nowhere. Gaining *entrée* in one community or group is hard enough; doing so in multiple communities and groups is that much more trying. Harder still is that which is demanded by relational ethnography: gaining access to an interconnected web of people, many of whom are bound in relationships of antagonism. During my fieldwork, getting in with both tenants and landlords proved challenging, especially when one was being evicted and the other was doing the evicting. Some tenants suspected I was working for the landlord, whom they sometimes referred to as "your friend." Some landlords, for their part, refused to discuss the details of a tenant's case. It took some time and some failures before I learned how to navigate (as best I could) these delicate relationships. With relational ethnography, access to one person may cut off or compromise access to another, just as access to two persons with competing interests may affect the kind of information both parties share (Simmel 1950, p. 136). In all cases, it is important to remember that no ethnographer gains *entrée* to "a group" or "a community" but only (and to varying degrees) *select members* of that group or community. Well-known ethnographies have their protagonists—Doc (Whyte 1943), Tally (Liebow 1967)—and it is a rare thing for ethnographers to mention all the people who said no or to tally all the doors shut in their face. Selection bias is an issue that besets all fieldworkers (even if discussions of this problem are largely absent from their accounts), but relational ethnographers need to pay special attention to this problem because their time with people occupying any given position in a field is necessarily limited by the relational imperative to which they have submitted.

This brings up the issue of *depth*. Depth is the ethnographic sine qua non, the method's unique and prized contribution to social science. The ethnographer is instructed to "get deep," to "submit himself to the fire of action in situ" (Wacquant 2004, p. viii). With group- and place-based ethnography, the idea is that, as time passes, the fieldworker lowers herself or himself deeper and deeper into the group or place, sinking. As Geertz (1988, pp. 4–5) knew, the ability of ethnographers to gain the reader's trust and respect has everything to do with convincing her or him (the reader)

of “having actually penetrated (or, if you prefer, been penetrated by) another form of life, of having, one way or another, truly ‘been there.’” Relational ethnography, however, requires fieldworkers not only to deepen relationships (to sink) but also to form many new ones (to expand). As the fieldwork progresses, the ethnographic sample broadens to include new ties in the network or people occupying new positions in the field. Simultaneously, the relationship between ethnographer and informant grows more intimate. Yet every minute spent with one person is a minute not spent with another. Mische (2008, p. 9) “made some sacrifices of depth in favor of breadth, although [she thinks] that those sacrifices were necessary in order to understand Brazilian activism as a field and not just as a collection of isolated groups. ... [She] explicitly sought out participants from a wide range of groups and attempted to understand the accounts of contending factions.” For me, spending time with landlords meant missing some events in tenants’ lives; and going to church with a tenant, say, meant missing service at her landlord’s place of worship. Each day, I decided where to go and with whom to spend time based on the intensity or importance of the day’s action, giving more weight to events I expected to yield the biggest analytic payout (e.g., eviction court, moving out) and tending to spend more time with people experiencing periods of instability (e.g., stretches of homelessness). The point is that relational ethnography necessarily sacrifices some ethnographic depth in order to investigate connections, transactions, and processes shooting through multiple contexts. Or, to put the matter more generously, relational ethnography replaces substantive depth (intimacy with a single group or place) with relational depth (intimacy with the dynamics of a network of relations).

Yet another challenge facing the relational ethnographer has to do with identifying the *boundaries* of the relational object. The boundaries of a relational space cannot be established a priori or in a single stroke; they are discovered over the course of the fieldwork (if they are discovered at all). Others have provided guidelines for establishing the boundaries of a relational object (Bourdieu 1996 [1989], pp. 232–244; Emirbayer and Johnson 2008, pp. 6–8), but network analysts and field theorists continue to grapple with the problem of designating boundaries to configurations of connections that are seemingly boundless. The boundary problem, however, is not one for relational ethnographers alone. Whereas the relational ethnographer must decide where to lay down boundaries across an infinite array of intercrossing connections, the substantialist ethnographer must justify the groupist or geographic boundaries she or he has accepted or created. Usually, the former views the boundary problem *as a problem*, while the latter takes the bounded thing as a natural unit of analysis. The lesser evil may be to draw an artificial parameter around a matrix of relations and admit to its artificiality rather than accept an artificial parameter as uncomplicated and real. When studying eviction, I limited my analyses by attempting to identify the *core actors* in the process. Some could be identified before setting foot in the field (e.g., tenants, landlords); but I became aware of other, less obvious players (e.g., building inspectors, disposable ties) only during the fieldwork. Yet more ties were identified than sufficiently plumbed. I did not follow paths that trailed deep into the inner corridors of Milwaukee’s public housing system or inner-city schools; my analyses of the housing market were limited to low-income dwellings; I spent much more time studying tenants’ networks than landlords’.

However, future ethnographers could pick up threads left dangling in my analysis, fusing together more connections and analyzing other sections of our relational world.

And yet another cadre of fieldworkers, in turn, could pick up their loose threads. I am speaking not only of ethnographies of connections but also of connected ethnographies, works that build upon one another in a purposeful, motivated fashion. What is needed is a kind of *collaborative ethnography*, one that replaces the outdated model of the lone-wolf fieldworker—a model based on the fetishism of distinct cultural differences—with that of a workforce of interconnected ethnographers, each working on her or his own piece of the puzzle. In the old model—Malinowski alone on a tropical island—each anthropologist painted on their own canvas, their completed works sitting silently next to one another in the literature, lonely portraits hung on a museum wall. In the proposed model here, multiple fieldworkers address different yet interwoven pieces of a vast patchwork quilt. I do not have in mind “group ethnographies” of the selfsame object but a collaborative ethnographic enterprise (a kind of “open source” ethnography, to switch metaphors) in which linked yet independent fieldworkers do their part to analyze different sectors of a sprawling network of relations that transcends the scope of any single endeavor. This resembles the model advanced by Park’s Chicago School of the 1920s: “Each [field study] piece added to a mosaic adds a little to our understanding of the total picture” of the ecology of the city (Becker 1966, p. viii). For relational ethnographers, the only real solution to the boundary problem is to confront it through collective enterprise.

Then there is the matter of *writing it all up*. “Whatever else ethnography may be,” Geertz (1988, p. 143) observed, “... it is above all a rendering of the actual, a vitality phrased.” Much thought has been devoted to ethnographic writing, to representation, ethnocentrism, voice, tense, and many other matters of the pen (Clifford and Marcus 1986; Van Maanen 1996). In addition to all the burdens of authorship borne by other fieldworkers, the relational ethnographer carries an additional burden: the difficulty of writing relationally in a language of substances. Everyday thought and language, as Elias (1978 [1970], pp. 112–113) noted, tends to “place at the forefront of our attention substantives, or conceptualizing what we really observe. ... [O]ur language compels us to speak and think as though all ‘objects’ of our thought—including people—were really not only static but [also] uninvolved in relationships as well.” This is why scholars have developed analytic terms, together constituting an altogether new conceptual language, that guard against substantialist tendencies and promote a relational ontology. Elias (2000 [1939], pp. 481–482) developed the concept of “figuration.” Bourdieu developed the concept of a “field” or “social space.” Somers (1994, p. 626) has advocated “[s]ubstituting the metaphor of a relational setting for ‘society,’” the former being “a pattern of relationships among institutions, public narratives, and social practices.” To break with substantialist modes of thought, relational ethnographers should make use of terms such as these and should work to contribute to the lexicon of relational sociology, lest they allow the dominant vocabulary to lull them into speaking in terms of things and essences.

Relational ethnographers as writers confront not only substantialist but also scholastic temptations. When presenting their ethnographic observations and experiences in a way that advances a relational perspective, analysts might choose to employ staid, cold prose somewhat resembling the abstract language of structural-functionalist anthropology (Radcliffe-Brown 1922; Lévi-Strauss 1966 [1962]). In their dedication to the relational, that is, they might write of connections and ties—or resort to the white-coated words of social network analysis: “ego-centric,” “structural equivalence,”

“centrality,” “brokerage”—saying nothing all the while of personalities or biographies, the texture of grease on a stove top, the polish of one’s shoes, the rhythm of one’s breathing, and all the other miniature subtleties of life. “I find the usual account of field-research so boring,” Evans-Pritchard (1976, p. 254) once complained, “as often to be unreadable—kinship systems, political systems, ritual systems, every sort of system, structure and function, but little flesh and blood.” The same could be said of much sociological ethnography, too, especially emotionally barren community studies that somehow manage to catalogue everything in a neighborhood except its essence. Such studies are what led Ellison (1986, p. 276) to admit, “I don’t deny that these sociological formulas are drawn from life, but I do deny that they define the complexity of Harlem. They only abstract it and reduce it to proportions which the sociologists can manage. I simply don’t recognize Harlem in them.” Since Ellison wrote these lines, sociological ethnographers have moved from the abstract to the concrete, and many works today brim with human complexities and contradictions as well as with emotionally touching (and therefore sociologically instructive) moments: Bess slapping Tally for forgetting his son’s name (Liebow 1967); Bart dying alone, one leg of his pajamas pulled up (Duneier 1994). It would be a shame if relational ethnographers were to reverse this trend by favoring abstraction over details, sociograms over stories, and “nodes in a network” over names.

A final consideration has to do with *generalizability*. A unifying (if often unspoken) logic undergirds both group- and place-based ethnographies, a logic that allows their findings to decouple themselves from the limited confines of specific field sites. Although the authors of, say, group-based ethnographies often issue disclaimers about their claims pertaining only to the dozen or so people they studied and call for “future research” to evaluate their findings in other settings, the very fact that those people, their informants, present themselves to the ethnographer as members of certain groups—they are not just people but women, not just women but immigrant women, not just immigrant women but immigrant women from Mexico—allows the ethnographer to argue (or at least to imply) that observations gleaned during fieldwork will most likely be applicable to similar kinds of people. The logic is simple enough: Society is made up of a collection of individuals with different attributes; individuals who share certain attributes can be understood as a group; studying a small sample of group members with attributes *a*, *b*, and *c* can provide insight into the lives of all group members with attributes *a*, *b*, and *c*. The introduction of *Tally’s Corner*, for example, follows this logic. The study was conducted “with the aim of gaining a clear, firsthand picture of lower-class Negro men. ... The great bulk of the material is drawn from two dozen Negro men who share a corner in Washington’s Second Precinct as a base of operations. ... There is no attempt ... to describe any Negro men other than those with whom I was in direct, immediate association. To what extent my descriptive and interpretive material is applicable to Negro streetcorner men elsewhere in the city or in other cities, or to lower-class men generally in this or any other society, is a matter for further and later study. ... This is not to suggest, however, that we are here dealing with unique or even distinctive persons and relationships. Indeed, the weight of the evidence is in the other direction” (Liebow 1967, pp. 10, 11, 14).

In other words, group- and place-based ethnographers tend to think of generalizability through a logic somewhat resembling that of representative sampling. What matters is not the degree to which one is embedded in crisscrossing networks but only

one's attributes (e.g., age, race, sex) in relation to those of the broader population. On account of their ontological convictions, however, relational ethnographers cannot rely on such substantialist and groupist reasoning. Notions of generalizability rooted in representative sampling techniques rely on assumptions of the independence of observations and so discard the importance of connectedness. "In other words, present forms of sociological analysis make possible the separation of interrelated things into individual components—'variables' or 'factors'—without any need to consider how such separate and isolated aspects of a comprehensive context are related to each other" (Elias 1978 [1970], p. 116). But connected bundles of relations are *what are most important* to the relational ethnographer. How, then, should she or he think about generalizability?

Although a satisfactory answer to this question far exceeds the ambitions of this article, a preliminary response might suggest shifting the focus from the proportionality of certain attributes in relation to those of the broader population to the accuracy and exceptionality of certain connections. The question then becomes not, "Is this group I studied different from similar groups in other settings?" but, "Have I adequately connected all the key parts of the system, and is this system unique to the specific contexts of the fieldwork?" The relational ethnographer not only has to consider the uniqueness of certain individuals in the network ("Is this landlord typical of others who operate in poor neighborhoods?") but also the uniqueness of their *connectivity* ("Is this landlord's relationship with his tenants typical of others?") as well as particular aspects of the cultural, political, or legal context that affect all parties in the network ("Are Milwaukee's eviction laws like those in other cities?"). It is the difference between studying a single part of the engine and studying how all the engine parts work together. But just as the mechanic who cut his teeth under the hood of a 1967 Mustang will not know everything about the inner workings of a 2007 Volvo sedan but will know something about it, owing to generalities of automechanics, so too will the ethnographer who studies eviction in Milwaukee, deforestation in the Amazon, or immigration along the Texas-Mexico border be able to generate findings about each of these dynamic processes that are simultaneously informed by and transcendent of the particular locales in which they were discovered.

The extent to which insights from any ethnographic study can be applied to times and places beyond those of the particular field site depends above all on those insights being right. As Katz (1983) has observed, when it comes to ethnography the external validity of a study hinges on its internal validity. Its internal validity, in turn, improves with each additional "negative case," which in disconfirming the ethnographer's explanation forces her or him to amend it. Accordingly, the fieldworker "is enjoined to seek negative cases and the resulting opportunity to modify explanation" because more negative cases expand an explanation's range and precision, and by extension its representativeness. Thus, qualitative variation serves as the "basis for generalizability" in ethnographic research (pp. 130, 135). How can these principles be applied to relational ethnography? A negative case is determined by an ethnographer's working explanation for some social action; that explanation is determined by a pursued research question; and that research question is determined by the ethnographer's object of analysis. If the object of analysis is relational, so too will be the questions that flow from it, the preliminary answers to those questions, and the negative cases that function as "rival hypotheses" to those preliminary answers. Negative cases in relational

ethnography, then, have less to do with persons, organizations, or settings unique in their specific attributes and more to do with transactions between two or more parties that disconfirm one's explanation of the structure or dynamics of a particular set of relations.

Conclusion

All aspects of ethnography—from conducting fieldwork proper and analyzing data to building an argument and communicating the results—are influenced by a crucial decision that originates at the very beginning of the research process: the selection of a basic object of analysis. From the founding days of the discipline, sociologists have placed considerable weight on the act of constructing a scientific object. Ethnographers, however, have paid scant attention to this crucial exercise. As a result, they all too often have accepted as their scientific categories “concepts formed outside science and for needs entirely unscientific,” taking as their starting point bounded entities delimited by location or social classification. Although group- and place-based studies have done much to push sociology forward, they have adopted (and have helped to perpetuate) a substantialist perspective on social reality. This perspective is guilty of at least three shortcomings: (1) *it is un-relational*, disassembling a social world consisting of “a totality of interconnected processes” and replacing it with a collection of atomistic substances or essences; (2) *it is un-constructivist and groupist*, taking “sharply bounded, putatively homogenous groups as basic constituents of social life, chief protagonists of social conflicts, and fundamental units of social analysis” (Brubaker 2003, p. 553); and (3) *it promotes process-reduction or process-circumscription*, the former tendency freezing dynamic processes in a state of rest, the latter bending processes around the mold of the group- or place-cum-object. An alternative to place- and group-based approaches is relational ethnography, an approach that takes as its object configurations of connections, transactions, and unfolding relations. Relational ethnography involves studying fields rather than places, boundaries rather than bounded groups, processes rather than processed people, and cultural conflict rather than group culture. Doing so allows the fieldworker to construct sociological explanations around relational mechanisms. Although relational ethnography comes with its own set of challenges and drawbacks, it holds the potential to transcend the limitations of place- and group-based fieldwork by analyzing how social actors exist in a state of mutual dependence and struggle.

Although an emphasis on relational thinking goes back to the earliest days of sociology, the last two decades have witnessed a “relational turn.” Network analysis has blossomed throughout the social and biological sciences, producing some of the most exciting methodological and theoretical developments in recent years (Emirbayer and Goodwin 1994). Historical sociologists have broken ranks with conventional studies of nation-states or people groups and, instead, have developed influential accounts of social change based on relational units of analysis (Gorski 2013; Padgett and Ansell 1993). Quantitative analysts have ushered in new methods to study social relations, including advanced spatial statistics (Ripley 2004) and network analysis (Marsden 1990). Contemporary social theorists have championed the relational perspective, subjecting it to sustained review and critique, expanding it to various subfields

of sociology, generating concepts that promote its use, and devoting attention to field theory (Martin 2003). All this has amounted to a “quiet revolution ... underway in social science, [marked by analysts] turning from units to context, from attributes to connections, from causes to events,” and, one might add, from substances to networks, from essences to relations (Abbott 1995, p. 93).

Yet sociological ethnographers—and qualitative sociologists more generally—seem stuck in substantialism.¹² As the social sciences bend toward relational theories of action, as network analysis and other relational methods move from margin to center, ethnographers seem left behind. This need not be the case. Indeed, if ethnography hopes to keep apace with the rest of the field—if it aspires to help steer the discipline instead of being looked upon as intellectual garnish to “more scientific” methods—it cannot be. We can recognize that when it comes to our ethnographic settings or objects, “the truth of these small universes is in their relation with the other universes, in the structure of the relations between the universes” (Bourdieu 1992, p. 44). Only then will we be able to develop arguments that work with—not against—the relational, transactional, and processual nature of social reality.

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¹² It is telling that Abbott's (1997) essay championing a kind of relational sociology that values spatial and temporal contexts begins by reviewing Chicago School ethnographies that pioneered such an approach but ends by promoting only historical and quantitative methods, as if contemporary ethnography has nothing to contribute to this perspective.

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