

Cracking the Iron Ceiling: On Ethnography as Theory

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journals.sagepub.com/home/crs**Zachary Levenson** 

Florida International University, USA; University of Johannesburg, South Africa

Josh Seim 

Boston College, USA

Abstract

There is no alternative to theory-driven ethnography. Some ethnographers may refuse to acknowledge the epistemological bases of their respective methodological approaches, but this amounts to unreflexive rather than atheoretical fieldwork. This essay elaborates three theses along these lines with the aim of sketching out a self-consciously theory-driven approach to ethnography. First, all ethnography is necessarily theoretical whether researchers acknowledge it or not. Without acknowledging this point, ethnography remains a fundamentally unreflexive enterprise. Second, extending upward toward ‘structure’ from empirics illuminates the conditions under which observations tend to hold true. Third, theoretical reconstruction, rather than confirmation or discovery, is the primary goal of ethnography. Drawing on the works of Michael Burawoy, Imre Lakatos, and others, these three theses motivate a defense of what we term ‘hard-core fieldwork’. We distinguish this approach from both grounded theory and more contemporary iterations of empiricism in sociological ethnography.

Keywords

ethnography, theory, extended case method, reflexivity, philosophy of science

Introduction

When Barney Glaser and Anselm Strauss (1967: 10) first developed a program for grounded theory, they were writing polemically: ‘great men’ like Talcott Parsons and Robert K. Merton, they argued, ‘lacked methods for generating theory from data’ (p. 10). In advocating for the empirical

Corresponding authors:

Zachary Levenson, Department of Global and Sociocultural Studies, Florida International University, 11200 SW 8th Street, SIPA 327, Miami, FL 33199, USA.

Email: zlevenso@fiu.edu

Josh Seim, Department of Sociology, Boston College, McGuinn Hall 426, 140 Commonwealth Avenue, Chestnut Hill, MA 02467-3807, USA.

Email: seimj@bc.edu

'discovery' of theory, they accused the generation before them of playing the role of "'theoretical capitalist[s]" to the mass of "proletariat" testers, by training young sociologists to test their teachers' work but *not* to imitate it' (Glaser and Strauss, 1967: 10–11, emphasis in original). But in rejecting the logico-deductive method most characteristic of structural functionalism, they bent the stick all the way back toward data fetishism.¹

A line was drawn in the sand. Ethnographers, as Glaser and Strauss (1967: 32) argued, should only engage in the generation of 'grounded theory', which they were careful to define as necessarily 'middle range'. Structural determination, they insisted, is rooted in purely logical, speculative, and normative assumptions and has no place in social scientific inquiry (Glaser and Strauss, 1967: 35–36). An *iron ceiling* was thereby constructed, effectively trapping ethnographers in the micro- and meso-level domains, unable to reach, or even see, the macro-level.

Thankfully, there has been no shortage of theoretically informed challenges to the construction of the iron ceiling, even if these took a few decades to emerge. By the late 1990s, 'political ethnography' arrived on the scene (Auyero, 2006; Baiocchi and Connor, 2008; Benzecry and Baiocchi, 2017), including now classic contributions from Auyero (2001), Baiocchi (2005), Eliasoph (1998), Glaeser (2000), and Lichterman (1996), among many others. Overlapping with this ethnographic approach to the study of politics was a Bourdieusian-inflected approach to ethnography, building primarily on the Bachelard-inspired *The Craft of Sociology* (Bourdieu et al., 1991). This approach was executed and sharpened across a variety of regional contexts, from North America (Bourgeois, 1996; Wacquant, 2003, 2004) to Latin America (Auyero, 2001, 2003) to South Asia (Ray, 1999 [1998]). And when Willis and Trondman (2000) launched the journal *Ethnography* in 2000, they were explicit: 'Ethnography and theory should be conjoined to produce a concrete sense of the social as internally sprung and dialectically produced' (p. 6). This list is hardly exhaustive; it simply serves to illustrate the extent to which the late 1990s and first years of the new millennium were marked by an unmistakable break with the empiricist approach to ethnography of the preceding decades.

It was in this context that Michael Burawoy (1998, 2009) revived the extended case method—an approach he would later describe as 'theory-driven ethnography' (Burawoy, 2019). If Glaser and Strauss broke explicitly with sociological structural functionalism, the extended case method was originally developed by the Manchester School to do the same with regard to anthropology. In both disciplines, structural functionalism was unrepentantly deductive in its approach, deploying case studies as straightforward instantiations of social structures. But if grounded theory simply stood this model on its head, swapping out a heavy-handed deductivism for an empiricist inductivism, Max Gluckman (1940, 1958), the key figure in the Manchester School, sought to locate social structure in everyday life. He therefore understood case studies not as chances to confirm the existence of social structures, but instead as 'social situations' representative of broader social phenomena (Schritt, 2022: 42). This is precisely what Burawoy sought to do with the extended case method. He was never interested in swapping out the inductivism of grounded theory for a return to the dogmatic deductivism of the functionalists. Rather, much like Gluckman in anthropology, he sought to provide 'micro-foundations' to overly abstract theoretical formulations (Burawoy, 1989). The idea was never to confirm preformulated theories in the wild, but rather to descend from the abstract to the concrete and back to the abstract again, modifying it in the process. Burawoy's (2009) revival of the extended case method, therefore, was never simply about critiquing inductivism, but equally about targeting a clumsy deductivism: 'In our fieldwork we do not look for confirmations but for theory's refutations' (p. 53).

This is what makes subsequent critiques of the extended case method, which we discuss below at length, so perplexing: Burawoy is consistently accused of deductivism. But theory is never identical with structure, as so many of his critics allege. If anything, theory is about a far more

subjective dimension: the reflexivity of the researcher. Reflexivity means the unapologetic embrace of the ethnographer's intervention into the field and a frontal critique of any fantasy of pure objectivity. But it equally means a self-conscious acknowledgment of the ethnographer's theoretical orientation, rendering epistemology and methodology as two sides of the same coin. This is crucial because everyone brings theory to the field, whether they want to or not. Burawoy (2009) argues that theory is not something to be discarded at the beginning of a study and then discovered anew in the field. Rather, '[t]heory is essential to each dimension of the extended case method. It guides interventions, it constitutes situated knowledges into social processes, and it locates those social processes in their wider context of determination' (p. 55). This 'wider context of determination' amounted to cracking the iron ceiling.

Over the years, the extended case method has faced significant resistance. Some have accused the method of overemphasizing 'structure' at the expense of 'culture' (Eliasoph and Lichterman, 1999). Others have accused it of relying on 'substantialist' categories and comparisons (Desmond, 2014). Another common critique emphasizes the method's supposed tendency to 'impute' macro-forces to one's field site (Gong, 2020: 177, 189).

The most significant challenge, however, has come in the form of 'abductive analysis' (Tavory and Timmermans, 2014; Timmermans and Tavory, 2012, 2022). Building on the pragmatism of Charles Sanders Peirce, Stefan Timmermans and Iddo Tavory offer a sort of radical centrist position. They suggest that abductive analysis, as a 'creative inferential process aimed at producing new hypotheses and theories based on surprising research evidence', offers a happy middle ground between the induction of grounded theory and the supposed deduction of the extended case method (Tavory and Timmermans, 2014: 5). They accuse Burawoy of offering a method that too firmly and hastily commits ethnographers to analytical theory, severing their opportunities for 'creativity' (Tavory and Timmermans, 2014: 19). As they put it, ethnographers exercising the extended case method 'tend to ignore their data or cut it up in little snippets, then focus on reiterating (or, at best, slightly modifying) an existing theory' (Tavory and Timmermans, 2014: 4). Much of this, we argue, is based on a misrecognition of theory-driven ethnography as an eternal return, a regression from grounded theory back to the logico-deductive method from which Glaser and Strauss broke in the first place.

Our primary goal in writing this essay, however, is not to defend the extended case method against this battery of criticisms, no matter how unconvincing we may find them. Instead, we aim to specify and develop a theory-driven approach to ethnography with *and beyond* Burawoy. It should come as no surprise that we need to turn Burawoy against himself: his theory too requires reconstruction. He conflates the distinction between folk and analytical theories with the opposition between micro- and macro-level theories. But, as we show, analytical theory can be oriented toward the micro- and meso-levels, just as folk theory can assume a structural guise. Burawoy also blurs an important distinction between positivism and empiricism. He accuses grounded theory of positivism, but that tradition actually emerged as an empiricist critique of a specific brand of positivism in the first place! While theoreticism and empiricism might be antipodal concepts, the same is not true for positivism, which can assume both theoretical and atheoretical iterations. And third, Burawoy grossly underspecifies his notion of theoretical favoritism, which is ultimately a diversion from the hard core of the extended case method.

We push—and reconstruct—theory-driven ethnography beyond these shortcomings. In what follows, we elaborate three theses toward a theory-driven ethnography. First, all ethnography is necessarily theoretical whether researchers acknowledge it or not; and without acknowledging this point, ethnography remains a fundamentally unreflexive enterprise. Second, extending upward toward 'structure' from empirics illuminates the conditions under which empirical observations tend to hold true. Third, theoretical reconstruction, rather than confirmation or discovery, is the

primary goal of ethnography. We conclude by considering the implications of these three theses for our understanding of ethnography as a reflexive science.

Thesis I: Ethnography Must Be Self-Consciously Driven by Analytical (Rather Than Folk) Theory

All ethnography is theoretical. This is not to suggest that ethnography is not rooted in the accumulation of data. Of course, it is: all ethnographers assemble and organize ‘facts’. But, as Burawoy (2019) reminds us, ‘there can be no facts without theory, that is without a lens to select from the infinite manifold that is the world we study’ (p. 48). Indeed, entering the field is impossible without theory. It is not a question of whether or not we enter as theorists, but if we enter as reflexive scientists aware of our various leanings and predispositions. Just as it is a mistake to assume we can enter as objective and impartial observers detached from our experiences as classed, raced, and gendered creatures, it is a mistake to assume we can enter as atheoretical or even theory agnostics.

Burawoy (2009: xiii) draws a line of demarcation between ‘folk theory’ (or common sense) and ‘analytical theory’ (or social science). If grounded theorists do not carry analytical theory into the field, they most certainly bring in folk theory, and this shapes the questions they ask, the events they record, and the categories they ‘discover’. Conscious of this fact or not, this folk theoretical orientation is a worldview: the way they see the empirical world. And while folk theory may be a worthy point of departure, analytical theory must be the central driving force. The point of sociology is not to record and reproduce folk theory but to develop it into—or break from it by way of—analytical theory, transcending the limitations of pure immediacy.

Burawoy (2009) clarifies this distinction:

On the one hand there is the theory of the people we study, namely, folk theory, buried in common sense and sometimes elaborated into ideology. On the other hand, there is the theory of the philosophers and social scientists, that is to say, of intellectuals, what I call analytical theory, which we can also call science. I assume that folk theory, while it has to contain some truth, a practical truth, is not as adequate as the truth of analytical theory, scientific truth. This is an act of faith, perhaps, but also the *raison d'être* of our scholarly existence. As sociologists, therefore, we may think of ourselves as breaking with or elaborating folk theory, but in either case we are moving from folk theory to analytical theory. (p. 270)

As social scientists, we are partial to analytical theory by default. It must be said that analytical theory is frequently misrecognized as an ‘act of faith’. Even grounded theorists, with their supposed commitments to data-driven ethnography, must conjure some kind of analytical theory in the ‘uninterrupted quiet, away from the field’ (Glaser and Strauss, 1967: 72). The difference is that self-consciously and intentionally theory-driven ethnographers recognize the significance of analytical theory during the conception and execution of their fieldwork; not just in its conclusion. They may seek to cultivate folk theory into analytical theory à la Antonio Gramsci, or else break from it entirely à la Pierre Bourdieu, but they always move toward analytical theory eventually.

But if sociologists all end up with analytical theory in the end, then why should it matter if fieldwork is data-driven or theory-driven? Why not just embrace a methodological pluralism if we all end up at the same destination with abstractions of the empirical world?

The problem is that in adopting a data-driven approach, the ethnographer risks misrecognizing common sense as analytical theory. Folk theory layered with a veneer of sociological abstraction is still folk theory at its core. And while ethnographers must no doubt learn, empathize, and perhaps even embody the taken-for-granted assumptions of those they study, they must be careful not to

essentialize the subcultures they examine. This is the trap of empiricism. At the opposite extreme, we must be careful not to distort folk theory into analytical theory—what Glaser and Strauss (1967) described as the “forcing of “round data” into “square categories”” (p. 37). This is the trap of theoreticism. And we avoid both of these traps through our commitments to *reflexive science*: the self-conscious recognition of our interventions, influence, and positioning in the world not only as social agents but also as social analysts.

This does not mean that all analytical theory is equally valuable. As social scientists, Burawoy (2009: 89–91) argues that we should be partial to theory that helps us draw links between internal processes and external forces. Some, like Kathy Charmaz (2014), have attempted to weld the data-driven method to the microanalytical theories of symbolic interactionism, constructivism, ethnomethodology, and the like, but such approaches nonetheless risk reproducing folk theories under an iron ceiling (see also Berthelsen et al., 2017). In other words, they abandon C. Wright Mills’ (1959) sociological imagination: the articulation of personal troubles and public issues.

While Burawoy’s formulation of the extended case method has done much to advance these principles of theory-driven ethnography, there is still work to be done to strengthen and clarify the method. Among other things, he offers a one-dimensional distinction between folk and analytical theory. Burawoy essentially projects the folk/analytical distinction onto a micro/macro opposition. Instead of a straightforward antithesis then, we conceive of theory as a set of perpendicular axes. Both varieties of theory, folk and analytical, can be oriented to the micro-level, just as both can describe the macro-level.

At one extreme, we find a terrain for symbolic interactionism, ethnomethodology, certain strands of social psychology, and the like. At the other, we find the kind of ‘grand theory’ targeted by Glaser and Strauss, as well as macro-focused analytical theories of capitalism, racism, patriarchy, and so forth. Between the micro and macro poles lies a theory of the ‘middle range’. The exercise of Mills’ sociological imagination—our vocation according to Burawoy (2009: xiv)—necessitates that we link microprocesses to macroforces. This then calls into question those analytical theories that are limitedly focused on just the macro or just the micro or even just the ‘middle range’. And since ethnographers study the world at ground level, they can be easily seduced by the convenience of theories only, or at least primarily, focused on microprocesses.

To put our concern more bluntly: not all analytical theory is equally valuable for ethnographers committed to the sociological promise. We need theories that help us link personal troubles to public issues. A distinction between folk and analytical theory is necessary, but insufficient. We must be wary of analytical theories that trap us beneath the iron ceiling.

Thesis 2: Extending Upward Toward ‘Structure’ From Empirics Illuminates the Conditions Under Which Observations Hold True

Interestingly, like Glaser and Strauss, many vocal critics of the extended case method understand the ‘macro’ as something located beyond the field in a domain of theory. Dorothy Smith (2005), for example, argues, ‘While at the “micro” level, the extended case method is ethnography, using participant observation, at the macro it is theory that is operative’ (p. 35). From this perspective, much of analytical social theory is framed as purely speculative. Accordingly, theory is said to operate as a borderline metaphysical approach to imagining macro-level determinations—determinations that can never actually be observed empirically. This suggests that theory and social structure go hand in hand, neither of them immediately accessible to ethnographers. Since ethnography is rooted in the power of direct observation, they all insist, theory must come after the fact and with an empirically delimited scope. They have come to fill the cracks in the iron ceiling.

Despite a professed hostility toward extension—as in the *extended* case—empiricists actually engage in a bit of extension themselves. And, we would argue, they tend to *overextend*, refusing to particularize their findings as a case. This is precisely what extensions allow ethnographers to do, that is, to particularize one's empirical observations through contextualization. This is but another way of asking under which conditions these observations would tend to hold true. By locating traces of social structure in seemingly micro-level fieldwork, we can understand how certain external forces—structural, organizational, institutional, cultural, and so forth—constrain and enable social action. This is true for the case being observed, but it also allows us to understand the conditions under which such forces might obtain more generally—hence the centrality of comparative research to the extended case method. This is not to insist that all ethnography must be comparative, but rather to point out that all ethnography—like all sociology—is implicitly comparative.² The problem with the reigning empiricist approach is that its practitioners tend to overextend without specifying the structural conditions under which they made their observations.

But without extending to social structure—and from social structure back to one's field site, for extensions are always multidirectional—ethnographers wind up reproducing the common sense of subjects at the field site without simultaneously situating these observations in relation to broader social forces. We are not calling for the prioritization of any specific theoretical tradition here, but we do agree with Mills (1959) that the sociological imagination requires an extension from personal troubles to public issues, which is but another way of saying that it necessitates a concern for structure, history, and power. Again, Burawoy's (2009) defense of theory-driven ethnography consistently engages Mills on these points, particularly in his call to extend 'out from process to force' (pp. 49–52).

This is the chief limit of grounded theory. This approach to fieldwork is ostensibly driven by data, but this is a sleight of hand. Data do not exist in the wild to simply be harvested by the devoted fieldworker. Rather, each researcher's respective theory of the social shapes what they conceptualize as data in the first place: what is worth observing and why? And in remaining resistant to broader theories of social structure, an iron ceiling is erected at the meso-level.

This is not to say grounded theory is immune from analyses of hierarchy, but hierarchy is no doubt limited by what Glaser and Strauss call the 'constant comparative method'. A library of grounded theory studies may give us insights into how student–teacher, employer–employee, doctor–patient, and related power asymmetries are similar or different, but such a method will provide minimal insights into how capitalism, racism, sexism, and related macroforces shape, and are shaped by, their existence. And while some grounded studies may 'situate' or 'contextualize' their micro-case in their turn to extant theory in the final moments of analysis (or opening chapters), the method generally prevents them from seeing the macro-foundations of microprocesses. As Burawoy (2009: 49) puts it, grounded theory relies on a 'segregative or horizontal' comparison at the microlevel, while the extended case method 'deploys a different comparative strategy, tracing the sources of small differences to external forces' under an 'integrative or vertical approach'. Where data-driven ethnography invites a horizontal abstraction (e.g. from a narrow 'substantive theory' to broader 'formal theory' under the iron ceiling), theory-driven ethnography invites a vertical abstraction (e.g. from 'personal troubles' to 'public issues').³

Analytical theory sensitive to macroforces is what allows ethnographers to extend, or rather integrate, the immediate microprocesses they study to the larger social structures they are embedded in. Why study doctor–patient relations without understanding each actor's location in social space, not to mention their relationship to the forces and relations defining the medical field? What does it mean to compare teacher–student relations to employer–employee relations without understanding each as structured in relation to the job market?

Our point is not that every ethnographer needs to do everything, but rather that severing the meso-level on down from its structural context artificially precludes the possibility of understanding how broader power relations play out in everyday life. If structure by definition structures social life, we should be able to discern traces of structure in our field sites. Relegating 'structure' to the realm of the empirically inaccessible reduces ethnography to a largely descriptive enterprise that has no basis for generalizing from particular cases.

And again, this brings us back to the problem of latent empiricism. There is a widespread assumption that ethnographic data are straightforwardly 'collected'. But data are never really collected, as if they are just sitting there waiting to be gathered; data are actively produced by the ethnographer. There are infinite possibilities as to what an ethnographer could potentially construct as relevant pieces of information. This empirical overabundance, as much as it might overwhelm the researcher, is not only the result of there being too many things in the world; it is equally a consequence of there being so many different ways to *see* these things *qua* things. Ethnographic research requires an orientation to the world that conceptualizes social events, processes, forces, and relations as convertible into data. We therefore do not simply 'collect' raw materials. Trees bear fruit, and generally, we know what fruit looks like in advance; once the tree bears fruit, we can collect it. But the social world bears no resemblance to trees. The gaze through which the ethnographer transforms a part of that world into a field site is also the gaze through which they construct certain data, to the exclusion of other possible data, as worth recording in the first place.

This is why ethnography is always theoretically driven, despite the consistent disavowals of the empiricists. Theory, be it analytical or folk, frames what we sense and assemble *as* data. Theory is not something that appears abruptly in some penultimate 'analysis' stage, nor is it limited to 'memos' (as distinct from 'notes'). This is because the very process of data construction itself requires analysis, whether the ethnographer realizes this or not. Our contention is that being reflexive about one's own theoretical approach in the assembly of data is far superior to the empiricist tendency to pretend such an approach does not exist. Certainly, theory can be retroactively separated from data and relegated to a separate section during the writing phase. But if cognitive schemes are in place that allow us to imagine and interpret some social processes as 'theoretical' and others as 'empirical', then we are operating through the lens of a social theory.

That said, Burawoy does not do enough to distinguish between the twin pitfalls of positivism and empiricism. He argues that a 'naive empiricism is often combined with an equally naive positivism', where the former 'assumes social theory grows *tabula rasa*' and the latter assumes 'we can and must stand outside the world we study' (Burawoy, 2009: xii). But this supposed combination is more myth than reality. Grounded theory's rupture from grand theory was itself an empiricist break from positivism. The point of grounded theory was to reject theory testing and to 'discover' theory in the field. Glaser and Strauss called for a kind of post-positivist hyperempiricism that pulled the ethnographic craft further away from analytical theory. Where positivists are deductivist, driven by hypothesis testing and perpetually guarding against alleged contamination by ethnographic intervention, empiricists are inductivist, driven by hypothesis discovery and guarding against alleged theoretical contamination.

Classifying grounded theorists as positivists, as Burawoy does, is generally inaccurate. He states, 'Participant observation, conducted according to positive principles, becomes grounded theory, which brackets involvement as bias and concentrates on deriving decontextualized generalizations from systematic analysis of data' (Burawoy, 2009: 64). Glaser and Strauss (1967: 34) may assume some objectivity, but more contemporary grounded theorists are at least partially 'reflexive' to the extent that they recognize the inevitable and informative interventions that ethnographers make in their field sites (e.g. Gentles et al., 2014). The problem is that empiricism, which is far more common in sociological ethnography today than positivism, lacks any

theoretical reflexivity. Whereas positivism at least demands an explicit recognition of analytical theory at the inception of research endeavors, empiricism assumes theory is irrelevant at best and dangerous at worst. It is empiricism, first in the radical form of grounded theory and now in the more moderate form of abductive analysis and other post-extended case methodologies, that erects and maintains the iron ceiling.

Thesis 3: Neither Confirmation nor Discovery but Reconstruction

There is a tendency to grossly oversimplify the distinction between grounded theory and the extended case method as one between ‘induction’ and ‘deduction’. But that distinction only really makes sense when contrasting grounded theory with the tradition from which it broke: the logico-deductive method. Despite what Timmermans and Tavory (2012: 168) suggest in their call for a more moderate ‘abductive analysis’, the extended case method pushed us beyond this simplification long ago. If the logico-deductive method called on ethnographers to confirm theory and grounded theory called on them to discover it, the extended case method called on them to engage in a practice of *theoretical reconstruction*.

It is important to remember that Glaser and Strauss advanced *data-driven* ethnography. Theory for them is the destination and goal, but not the starting point or the motivation. Indeed, they call for ethnographers ‘literally to ignore the literature of theory’ at the beginning of their research ‘in order to assure that the emergence of categories will not be contaminated by concepts more suited to different areas’ (Glaser and Strauss, 1967: 37). While they admit that ‘no sociologist can possibly erase from his mind all the theory he knows before he begins his research’, they are clear that sociologists should still actively work to minimize the contamination of their field sites (Glaser and Strauss, 1967: 253). The point of ethnography, they insist, is not to confirm but to discover theory.

And it is not just hypotheses, frameworks, or models they worry about but the building blocks: *categories*. Again, ethnographers must beware of fitting ‘round data’ into ‘square categories’ (Glaser and Strauss, 1967: 37). They should instead let the categories *emerge* from the data (Glaser and Strauss, 1967: 37). The relations between these categories, detected through comparisons under the iron ceiling, ‘form an integrated central theoretical framework—the *core of the emerging theory*’ (Glaser and Strauss, 1967: 40, emphasis in original).

Grounded theory imagines field sites as natural landscapes that can themselves yield theoretical fruit. Only after an ‘analytic core of categories has emerged’ within the data should the ethnographer consider any similarities or convergences with extant theory (Glaser and Strauss, 1967: 37). And this apparently occurs through inductive coding followed by analytical memo writing and then finally the generation of theory, which ‘should be done in the uninterrupted quiet, away from the field’ (Glaser and Strauss, 1967: 72). It is here, in the quiet, that grounded theorists code, write memos, and eventually engage preexisting theory relative to the theory ‘discovered’ in the field.

Burawoy’s extended case method was a rejection of the inductivism of grounded theory, but it was not a return to the deductivism of the logico-deductive method. He may not have relied on the pragmatism of Charles Peirce—Timmermans and Tavory’s (2012) ‘favorite theory’—but he articulated a method for mixing predictions and surprises.⁴ Where abductive analysts search for a middle ground between discovery and confirmation, theory-driven ethnographers have already moved beyond ‘induction’ and ‘deduction’.

It is through a deep engagement with analytical theory sensitive to macroforces that ethnographers are best equipped to extend upward via the sociological imagination rather than just extending laterally out via the constant comparative method. This does not, however, imply an ethnographic mission to reproduce, reject, or replace analytical theory. Instead, it is a call, as Burawoy puts it, to reconstruct theory. On this point, it is worth citing Burawoy (2009: 43) at length:

Following Karl Popper (1963, chap. 10) and Imre Lakatos (1978), we seek reconstructions that leave core postulates intact, that do as well as the preexisting theory upon which they are built, and that absorb anomalies with parsimony, offering novel angles of vision. Finally, reconstructions should lead to surprising predictions, some of which are corroborated. These are heavy demands that are rarely realized but ones that should guide progressive reconstruction of theory.

The nod to Lakatos is particularly important but often overlooked. In *The Methodology of Scientific Research Programmes* (Lakatos, 1978), the philosopher of science specifies analytical theoretical traditions as diverse and differentiated ‘research programmes’. Each program is constituted by a ‘hard core’ of fundamental and unfalsifiable worldviews surrounded by a ‘protective belt’ made of testable, and therefore adjustable and replaceable, auxiliary hypotheses. Marxism, for example, is fundamentally premised on the claim that class struggle drives historical development. This is the hard core of Marxist theory. This core is surrounded by protective belts vulnerable to empirical scrutiny and therefore rejection, modification, and reconstruction. Staying with the example of Marxism, these auxiliary hypotheses include the tendency of the rate of profit to fall, the labor theory of value, the inevitability of working-class formation, the nature of the state as a class instrument, the tendency for being to generate consciousness, and so forth. Theory-driven ethnographers conceptualize and execute their studies within, and often across, theoretical traditions or ‘research programmes’. The point is to target the protective belts through empirical investigation—but investigation guided by hypotheses as methodological orientations. The idea is to continually modify the protective belts with novel predictions uncovered through theoretically guided research. This is what Burawoy means by the ‘progressive reconstruction of theory’. The hard core, meanwhile, cannot be refuted—at least not by any single study. This is not about dogmatic assertion but rather the fact that the hard core is a way of seeing the world rather than a falsifiable hypothesis as such.

But just as our first thesis complicates Burawoy’s distinction between analytical and folk theory and our second thesis rejects his conflation of empiricism and positivism, our third thesis amends the claim that ethnographers bring their ‘favorite theory’ in the field. In many respects, this is less a critique of Burawoy and more of a much-needed clarification and expansion of his argument.

Burawoy (2009) writes,

We begin with our favorite theory but seek not confirmations but refutations that inspire us to deepen that theory. Instead of discovering grounded theory, we elaborate existing theory. We do not worry about the uniqueness of our case since we are not as interested in its representativeness as its contribution to reconstructing theory. (p. 43)

Admittedly, the phrase ‘favorite theory’ risks quite a bit of confusion. This is not a matter of taste, but rather epistemological leaning. Having a favorite theory is not like having a favorite movie, artist, or sports team—it is not simply a matter of taste. It concerns each researcher’s outlook and worldview. Favoritism in this sense concerns bias in terms of which frameworks are most convincing and structuring—Lakatos’ hard core.

‘Favorite’ might best be interpreted as commitment. Whether folk or analytical, restricted by the iron ceiling or not, the theories we carry structure our social vision and hence our assembly of ‘facts’. We may pretend to ‘discover’ theory, but when we do so we are elaborating an existing folk theory under the iron ceiling. Folk theories are rooted in experience and immediacy, whereas analytical theories require a self-conscious analysis of mediation, which is to say how our experiences are themselves shaped and structured at multiple possible scales. Our case may be particular, as all cases tend to be, but theory-driven ethnographers fret little over this so long as the case can offer a

meaningful contribution to the reconstruction of theory. This is not a simple ‘tweaking’ but rather a targeted focus on the protective belts described by Lakatos. The hard core—the unfalsifiable claims that shape our worldview—remains intact, but the auxiliary hypotheses are modified, tightened, and replaced. Over time, the hard core may be replaced, for nothing is static. But this occurs through a protracted focus on the protective belts realized not by individual scientists but through a complex division of analytical labor.

Tavory and Timmermans (2014), in focusing on this phrase ‘favorite theory’, accuse Burawoy of disallowing a plurality of theories, thereby ‘limit[ing] the creativity of researchers’ (pp. 19, 49). This strikes us as a criticism made in bad faith. Burawoy is quite clear that theory includes a multitude of analytical frameworks situated within (and sometimes across) intellectual traditions. Think, for example, of the Black radical tradition, fourth-wave feminist theory, or actor-network theory. These are all theoretical traditions with multiple and often competing and contradictory theorizations. But even if we were to singularize these traditions into monolithic theories, any careful reader of an extended case method study would immediately notice that it is typical for such ethnographers to engage multiple and often competing orientations. For example, Jeff Sallaz (2009), in his theory-driven ethnography of casino capitalism, frequently draws on the distinct theories of labor process (e.g. Braverman), symbolic interactionism (e.g. Goffman), and field theory (e.g. Bourdieu).

And, despite what some might assume, the kind of ethnography advocated by Burawoy and advanced in this essay is explicitly anti-dogmatic. The objective of theory-driven ethnography is not to seek confirmation of our ‘favorite theory’, but to look for refutations so that we may *reconstruct* that theory.

Hard-Core Fieldwork

In his plea for an interpretive sociology, Richard Biernacki (2012) insists,

The premise of coding is that meanings are entities about which there can be facts. But we all know that novel questions and contexts elicit fresh meanings from sources, which is enough to intimate that meaning is neither an encapsulated thing to be found nor a constructed fact of the matter. It is categorically absurd to treat a coding datum as a discrete observation of meaning in an object-text. (p. 131)

While Biernacki aimed his withering critique at the text coding practices of cultural sociologists, it is equally applicable to ethnographers attempting to shield precious observations from being polluted by a ‘favorite theory’. As we have argued here, this theory is itself a worldview, akin to Lakatos’ unfalsifiable ‘hard core’. The point is never to purify the data of this contaminating worldview, for it is the theoretical lens that makes certain data appear *as* data in the first place. Rather, a rigorous approach to ethnography necessitates that the ethnographer lay bare their own worldview, which is but another way of saying that ethnography can only function as a science if it is premised upon the reflexivity of the ethnographer. We refuse the false choice between theory and empirical rigor not because we happen to like reading theory, but because ethnography can never be rigorous without being reflexive.

We hesitate to describe theoretical outlooks as ‘biases’ because that term suggests the possibility of an ‘unbiased’ outlook, as if theories are refracting lenses warping the sociologist’s gaze rather than the retina itself. A Lakatosian approach to social science requires transparency about our research program, appearing in the form of a guiding problematic, which amounts to being reflexive about one’s own theoretical approach. This ‘hard core’ constitutes the very language the author uses to articulate hypotheses, but calling this language into question does not get us very far. Yes,

the ethnographer can learn new languages, but ultimately one language is never preferable to any other so long as they are reflexive about the fact that they *are* using a language in the first place, with all of its potential limitations.

That is the irony of the hostility to theory-driven ethnography: in effecting the empiricist separation between theory and data, this hostility *refuses* reflexivity. In making this move, willfully or otherwise, the ethnographer still has a favorite theory, but now they lack the ability to be transparent about this fact. Instead, in imagining fieldwork as the collection of facts in the wild, the ethnographer is unable to specify why this or that ‘fact’ appeared as a datum in the first place. What appear as worthwhile data are the direct product of the hypotheses the ethnographer develops, which are in turn shaped by the ethnographer’s outlook: theory. What we assemble as data is always already formed by the theoretical language of the ‘hard core’ of our research program. There is no data ripening on the branch, just waiting to be ‘collected’ by the social scientist.

By the same token, ‘protective belts’ *can* be penetrated. Or more precisely, when empirical observation challenges the basis of an auxiliary hypothesis, we rarely toss the hypothesis in a trash can. More typically, researchers *reconstruct* that hypothesis—which is to say, theory—on the basis of novel empirical findings.

Ethnography then requires a constant back and forth movement between the abstract and the concrete. But where grounded theory invites a horizontal abstraction from substantive to formal theory under the iron ceiling, theory-driven ethnography invites a vertical abstraction between microprocesses and macroforces. We depart from the abstract, but reflexively, acknowledging both the enabling conditions and limitations of our given theoretical outlook. We move down to the concrete, an overwhelming mess of empirics not yet assembled into discrete data. And then we move back up again not to reproduce but to *reconstruct* such an abstraction.

This is a roundabout way of driving home our first and third theses: the need for analytical rather than folk theory, and reconstruction rather than confirmation or discovery. But we also argued, as in our second thesis, for *extension*: the upward movement toward ‘structure’. We must beware of analytical theories that dismiss or reject the sociological imagination—our very vocation according to Burawoy.

Every field site is located in a sociological context. To acknowledge this is not to abandon processes internal to the field site *in favor of* external forces—this would be an egregious example of what Desmond (2014) calls ‘substantialism’. In fact, structural forces are never simply external to the field site. The point is to recover traces of the macro in the micro, understanding how sociological processes play out and are articulated at multiple theoretical scales and levels. And in the unlikely case that a site *is* disarticulated from its larger context, this would itself constitute an aberration requiring explanation.

Herein lies the chief problem with the iron ceiling. In severing the field site from its broader context, we no longer know the conjunctural conditions under which certain observations might hold true. We are left in a state of pure contingency and reduced to the level of ad hoc explanations that do little to specify the conditions of generalization for an ethnographically derived conclusion—or ‘substantive’ theory.

This is why calls for ‘less theory’ and ‘more description’, like that recently advocated by Max Besbris and Shamus Khan (2017) are so absurd. With straight faces, they encourage ‘empirical description for its own sake’ (Besbris and Khan, 2017: 147). But as we have argued here, there are no ‘findings’ devoid of theory. Their argument is not so much a polemic against theory as an admission of refusing reflexivity—a moment that is crucial to any science. While we might argue that all atheoretical ethnography is necessarily empiricist, a more precise formulation would maintain that there is no atheoretical ethnography, but rather reflexive and unreflexive ethnography. The empiricist trap is not a pit that envelops ethnographers, pulling them away from theory in general; it is a

foot snare that keeps them in the domain of folk theory. Only theory-driven ethnography—what we might also call *hard-core fieldwork*—can help us avoid this springe.

Author's note

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ORCID iDs

Zachary Levenson  <https://orcid.org/0000-0001-9077-4664>

Josh Seim  <https://orcid.org/0000-0002-4503-1228>

Notes

1. The obvious parallel with Marx's theory of commodity fetishism is hardly overstated. As Tad Skotnicki (2020: 363) has recently argued, commodity fetishism contains 'two interpretive moments: (1) the interpretation of goods as anonymous in exchange and (2) interpretations of commodity-exchange as natural'. Data fetishism consists of two comparable moments: (1) the interpretation of facts as objective in observation and (2) interpretation of facts as naturally occurring phenomena. We can also draw a parallel regarding comparability. Prices appear as innate in commodities—a folk theory if there ever was one—but Marx (1976 [1867]) demonstrates that prices are the apparent form of value, which is itself related to the exertion of abstract labor-power. Likewise, facts appear as innate in the social world—again, a folk theory—but it turns out that they are always related to the epistemological vision of the researcher. (Maybe this is why Desmond (2014) understands relational sociology as 'transactional'?) Finally, in the broadest sense, there is a parallel in terms of the attribution of agency. Commodity fetishism involves the attribution of agency to commodities, rather than conceptualizing them as the product of labor. Likewise, data fetishism attributes agency to facts themselves, rather than conceptualizing them as the product of the researcher. For more on the fetishization of facts, see Pugh and Mosseri (2023).
2. Comparison *in* ethnography should be distinguished from comparative ethnography. The former is universal to all ethnographic inquiry while the latter is specific to studies that assemble and relate distinctive (even if not 'substantialist') places, populations, periods, and so on. It helps clarify the positioning of actors, the unfolding of processes, and so on. While some, like Desmond (2014), have attempted to distinguish between 'comparative' and 'relational' ethnography, we hold that there can be no 'relations' without dis/similar units assembled by the analyst through either an explicit or implicit comparison.
3. Glaser and Strauss' (1967: 79) recommendation that formal theory should be crafted from substantive theory is nothing more than a horizontal extension, remaining firmly entrapped beneath the iron ceiling. To take one prominent example, Erving Goffman (1961) may develop a formal theory of 'total institutions' from a substantive theory of asylums. But his *horizontal* extension does not appear to inspire a *vertical* extension. It is little more than an extension of breadth beneath the iron ceiling—similar to how Glaser and Strauss (1967: 80) themselves move from a narrow substantive theory of 'doctors' and 'patients' to broader formal theory of 'professionals' and 'clients'. This tells us very little about the structure of the social space in which these actors move.
4. Interestingly, critics of the extended case method tend to be quite fond of the pragmatist tradition. Timmermans and Tavory derive their approach from Peirce, and while Eliasoph and Lichterman (1999)

never self-identify, they do repeat a pragmatist mantra three times over the course of two pages: ‘People can’t change anything until they consider it a problem’ (pp. 229–230). Meanwhile, Desmond’s (2014) approach is notable for the influence of symbolic interactionism. And finally, Glaser and Strauss (1967: 142, 237, 249–250) repeatedly invoke John Dewey to willfully conflate folk and analytical theory under a single rubric: ‘as John Dewey has clarified for us, grounded theory is applicable in situations as well as to them’ (p. 249). And again: ‘Social theory, as John Dewey remarked thirty years ago, is thereby enriched and linked closely with the pursuit and studied control of practical matters’ (p. 250). There is clearly more than an elective affinity at work between pragmatism and the iron ceiling.

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