

Understanding Trans-Sequential Ethnography and its Place in the Contemporary Sociological Landscape

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Contribution to the session »Jenseits innerfachlicher Pole: Die Transsequentielle Analyse und ihre Potentiale für eine transdisziplinäre Vergleichbarkeit«

What is trans-sequential ethnography? What defines it – both as a theoretical apparatus and as a way of practicing ethnography? Whereas others have addressed these questions by discussing trans-sequential ethnography (also known as trans-sequential analysis or TSA for short) itself (e.g. Scheffer 2007, 2013, 2019; Kolanowski 2023), we take something of an alternative approach. Specifically, we attempt to better understand TSA by situating it within some of the broader trends within the discipline of sociology as a whole. In particular, we argue that TSA should be seen as standing within a long line of social theory which recognizes that fundamental ontological condition of social life is one of, what we will term, excess. Second, we argue that TSA should also be seen as one effort, among others, to challenge and move beyond our traditional embrace of methodological foundationalism.

In what follows, we begin, first, by describing what we mean by ontological excess. We then sketch out what we see as having been both sociology's foundationalist methodological tendencies and its efforts to move beyond them. Finally, we discuss the basic assumptions of trans-sequential ethnography and its aims to be both uniquely sensitive to the excessive character of social life while also being largely non-foundationalist in its approach to ethnography.

Ontological Excess

Running as something of a throughline throughout much if not most of 19th and 20th Century social theory is a rather profound argument about the basic ontological character of social life. While Durkheim, Marx, Bourdieu, Goffman, Mead, and Freud, to cite just a few prominent examples, are all known for making sometimes radically different if not even conflicting sociological arguments, they can also be read, we think rather productively, as making similar observations about social life, and in particular, about the relationship between the individual and the social, the self and society. Durkheim famously declared society to be a phenomenon *sui generis*; a *social* reality that cannot be reduced to its individual or component parts. Marx maintained that under conditions of capitalism, there was always a deeper, more sinister more exploitative truth, lurking behind our everyday economic, cultural, and political affairs. Bourdieu saw our subjective perceptions of the world as being shaped, not by our individual psychology or calculations, but by our position within a broader set of objective relations. Goffman tirelessly

documented how there is always some more hidden, some more private “backstage,” that is separated from our more visible, more public, more frontstage performances. Mead argued that there is an “I” that is hidden beneath the more visible, more socially objectified “me” and that that “I” emerges as we anticipate how others will view “me” and its actions. And Freud insisted that not only are there deeper, darker desires that inform everything from the manifest content of our dreams to the ways in which we relate to others, but also that we actively work to keep these desires hidden both from ourselves and others.

Traditionally, within much of social theory, when such accounts are not seen as disparate if not even competing understandings of social life, they are often glossed as ones of and/or about the “irreducibility” of society and the social, on one hand, from the individual and psychological, on the other. And the conclusion that is typically made from this finding is that different methods or approaches must be used to study each of these distinct realms (i.e. psychology is committed to the study of the individual while sociology is committed to the study of society and the social). While none of this is wrong, insofar as it goes, there is a deeper, much more profound, more fundamental, ontological truth to these observations, one that often gets overlooked or altogether ignored both in these more theoretical debates but also in our discussions about the kinds of methods that are needed in order to know that reality. Namely, that society and the social, the individual and the psychological, while not entirely distinct, still nevertheless exist *in excess of* or *apart from* one other, such that it is impossible for the character of one to ever be *directly* deduced from or known from the point of view of the other.

To be sure, the idea that the social is always somehow *more than* or *other than* the individual and that the characteristics of one cannot be straightforwardly read off from the other is by now something of a sociological truism, one that hardly beggars much belief. Indeed, in today’s post-Kuhnian world, only the most hard-headed of hard-headed empiricists would ever argue that sociological knowledge is a *direct* product of our own observations or experiences. Be that as it may be, however, sociology has, nevertheless, we believe still generally been all-too unreflective about the distinct methodological challenges that arise from its desire to know and understand an excessive reality.

Methodological Foundationalism (and Beyond)

Traditionally, when sociologists talk about research methods they talk about certain pre-specified means by which to link theory and data. These means have certain starting, middle, and ending points. Researchers start by doing certain things, continue by doing others, and then end by doing still others. What is more, a method is further defined in the negative, that is by the list of practices that are deemed to be anti-scientific. While scholars almost always acknowledge that these abstract methodological understandings are always just that – abstract accounts that fail to perfectly reflect the reality of research as it is actually conducted – these exceptions to the rule are nevertheless thought to be generally unimportant for determining the quality of the research; so long as one does not depart too far from prescribed practice, the work that one is still doing is believed to be scientific.

At the same time that sociology has, thus, traditionally been premised on the assumption of ontological excessiveness, we can also say that it has been based on an overarching methodological foundationalism. Here, the adjective overarching is a significant one, for the discipline’s methodological foundationalism has transcended any *one specific* method. Perhaps most obviously, statistical methods, especially in their more positivistic expressions, have exhibited a foundationalist tendency. Researchers formulate an explicit hypothesis; they gather a random sample or collect important observational data; they run some statistical analysis (often a regression analysis of some kind) that allows them to control for confounding variables, and they then make certain conclusions about what factors are causing which

outcomes. But much the same, we believe, can be said for ethnography too. Ethnographers gain access to some interesting aspect of social life. They earn rapport with those who will become the focus of their research. They record their observations from the field in their fieldnotes, and the patterns that then emerge from those notes are then included in their final research reports.

All of this has become such a natural way of doing research, or to be more precise, a natural way of talking about how we (should) do research, that it can all sound rather unproblematic. Indeed, from a certain point of view, foundationalism is a rather natural part of knowledge production. As we go about the work of doing research, we cannot help but search for some stable basis from which to claim that the things that we think we have observed really are what we have observed. And insofar as any criticism ever calls those bases into doubt, it is a rather short path from there to those criticisms being seen as an attack on knowledge itself, regardless of whether or not they were ever intended to be. It is, thus, hardly surprising that when concerns about the foundationalist character of research methods have been raised, they have often been met with vehement denunciation and counter-accusations about the anti-scientific motivations that are supposedly lurking behind them.¹ And when such criticisms have not been seen as an attack on science, they have, generally, been dismissed as being counterproductive, insofar as they are believed to distract scientists from the real work of (foundationalist) research.²

Be all of that as it may, however, in glossing the details of our methodological practices in foundationalist ways, the risk that we continually run into is that we are not reflective enough about the degree to which our methods are or are not well-suited for the work that we ask them to do: capturing the character of an *excessive* reality. If all we are doing or all we say we are doing when we talk about our research is just some, cut-and-dried, foundationalist, color-by-number, off-the-shelf approach, then how can we be confident that our research practices are sensitive enough to document a reality whose features always are, at least to some degree, beyond our *immediate* grasp? And, what is more, how can we convince others, specifically our readers, that this is the case? How can we persuasively argue that we managed to get in touch with something which was not otherwise immediately apparent? How can we convince others that the excessive thing that we claim to have identified is in fact what we found and not just a product of our methodological narrow-mindedness?

The risk that foundationalist research, therefore always faces, is that however carefully it may have been produced, doubts will nevertheless persist as to the degree to which it was in fact capable of documenting an excessive reality. Indeed, much of the history of disciplinary and intra-disciplinary methodological debates can be read as a series of ongoing efforts to both highlight and move beyond our more foundationalist approaches to research. Quantitative scholars have, for example, criticized each other for the narrowness of the assumptions upon which their estimation techniques have been based (e.g. Abbott 1988; Lundberg et al. 2021). And ethnographers have, similarly, found fault with ethnographies whose over- and under-reliance on theory has led them to (potentially) pre-judge the results of their research (e.g. Wacquant 2002; Katz 2004, p. 287–295).

To overcome these obstacles, contemporary researchers, must develop research methods that allow them to: 1) identify the things that need to be identified in the course of research (without, at the same time, leading them to identify some things as being important that aren't actually important), and 2) to

¹ As seen perhaps most clearly in the polemics surrounding the so-called “science wars.”

² Coser (1975), to give one example, famously cautioned that ethnomethodology, and in particular, ethnomethodological critiques of traditional research methods were “a self-indulgent enterprise in which perpetual methodological analysis and self-analysis leads to infinite regress” (p. 698). And Geertz (1988), to give another, warned that arguments about the inherent positionality of ethnographers and the degree to which their claims reflected their own (Western) biases could induce “epistemological hypochondria” in ethnographers, leading them to worry about whether “anything one says ... is as a matter of fact so” (p. 71).

convince others that this is in fact what they have done. To put it somewhat differently, the methods that are needed for the work of sociological research today are ones that are demonstrably “sensitive” (i.e. flexible, nimble, nuanced, reasonably non-foundational), not “frigid” (i.e. strict, narrow, pre-defined, foundational) (Knorr-Cetina 2013, p. 17–20).

None of this, of course, is to say that there can ever be some perfectly neutral, perfectly non-foundational means by which to conduct research. Any method will always necessarily shape our point of view on the world and the types of evidence that are (and are not) available to us. But rather than just being blind to the extent to which this is the case, good research will always be that which looks to identify these limits and even tries to push beyond them so as to know what our methods are and are not capable of. For it is only in doing so that we can show that our research, rather than being some clunky, pre-fabricated, off-the-shelf method that may or may not be up to the task at hand, is instead a rather bespoke solution, one that is uniquely sensitive to the excessive features of the case-at-hand.

Trans-Sequential Ethnography

Trans-sequential ethnography, we believe, can best be understood as a set of theoretical and methodological understandings that sit squarely within each of these broader trends within sociology, and seeks to advance a form of ethnography that is both sensitive to ontological excess while also being reasonably non-foundationalist. Some forms of ethnography theoretically deduce in advance of research what the broader processes are that affect the setting being studied. Others, sometimes more, sometimes less blindly, assume that the processes that shape the object-of-investigation can (only) be observed in the immediate face-to-face interactions of the actors being studied. In contrast with either of these approaches, TSA is explicitly agnostic about questions of scale. Or rather, perhaps, more accurately, it looks to answer such questions abductively in and through the course of research by identifying the variable links that exist between situated happenings and the spatially and temporally extended processes that both inform those happenings and are informed by them.

Scheffer (2010), for example, shows how the happenings at English Crown Courts are determined not just by the interactions that take place within the courtroom nor by the institutional structures within which they unfold. They are “co-produced” by the legal work that is carried out, often well-in-advance of trial, by barristers whose case work anticipates and informs the later legal proceedings. Similarly, Liefke and Mahler (2023) find that the judgments that broadcast journalists make about better and worse forms of news are neither the subjective choices of individual journalists working alone at their computers nor decisions that are always already determined by the organizational structures or standards within which they are made. The judgments these journalists make are collective ones. They are assembled trans-temporally across the newsday and trans-spatially across the newsroom as different teams of journalists, and even, at certain points in the day, the newsroom as a whole, discuss how best to cover the stories that are covered on each of the broadcasters’ news shows.

Importantly, however, in neither of these examples, were the results pre-determined by some body of literature that pointed to the importance of pre-trial work, in the case of the first example, or the salience of collective decision-making, in the second. In both instances, each of these factors was identified as having a significant effect on the outcomes of interest in and through the ethnographers’ efforts to trace what it was that the subjects of their research were up to as those actors went about their workaday lives. But rather than simply taking-for-granted that everything that was relevant for understanding the goings on in the setting at hand was wholly circumscribed within the moment-by-moment interactions of the actors being studied, both studies were explicitly open to the possibility that there

may have also been events elsewhere (whether in the past or some imagined future) that were also somehow significant for understanding what was happening then and there. In Scheffer's work, this ultimately led him to the realization that case work at *time-t_i* was undertaken with an eye towards what might come later, whether at certain pre-trial hearings, or even possibly at trial itself, just as conversely, the events that did come later, were also interpreted and made sense of in light of what was known to have been done and decided earlier. For Liefke and Mahler, this sensitivity led them to recognize that in order to understand how and why the 19:00 show decides to cover a story a certain way, often required them to know something about a whole host of different spatially and temporally extended happenings. These might include, among many others: the choices that other journalists at other outlets are perceived to have made in coming to cover that same story exactly as they did, what the discussions were that took place at the 14:00 newsroom wide conference, the conversation that took place between two team members over lunch, and the various ways that the journalists believed their audience might interpret different versions of that story.

None of this is to suggest that TSA is and/or aims to be largely atheoretical. Far from it. While largely inspired by ethnomethodology, authors who have used TSA have drawn on a wide range of theoretical influences from Foucault, to Latour, Bourdieu to Goffman, and many others. But these influences have been used, not as a justification for certain pre-determined arguments about how certain situated events are linked to certain transcendent processes. They have been used as sensitizing concepts that highlight *some* of the different ways that these spatial and temporal associations *might* occur across different settings. For TSA, the ultimate proof-in-the-pudding lies, not in abstract theoretical arguments, but in the linkages that can be shown to exist between the here-and-now of actors' turn-by-turn interactions with the ongoing, transcendent processes of which their lives are a part.

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